NUMERICAL MAGNITUDE AFFECTS THE PERCEPTION OF TIME AND INTENSITY

by

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Abstract

The relative magnitude of an event (number magnitude) can have direct implications on timing judgments. Previous studies have found that large magnitude numbers are perceived to have longer durations than those of smaller numbers. This bias can be accounted for in several ways; first, the internal clock model theorizes that stimulus magnitude directly interacts with the components of a dedicated cognitive timer by increasing pacemaker speed. Another explanation posits that different quantitative dimensions (space, time, size, intensity and number) are all represented within a common cortical metric thus facilitating interactions within and across dimensions. I have expanded on this framework by proposing that perceived duration is inferred using flexibly applied rules of thumbs (heuristics) in which information from a more accessible dimension (e.g., number magnitude) is substituted for duration. Three paradigms were used to test this theory. First, commonalities in how the intervals separating discrete stimuli of different magnitudes were judged was examined across a variety of quantitative dimensions (number, size, and colour saturation). Perceived duration judgments increased systematically as the magnitude difference between the stimuli increased. This finding was robust against manipulations to sequence direction, and order, suggesting that interval duration was estimated by substituting information regarding the absolute magnitude difference. Second, the impact of number magnitude on sound intensity judgments was examined. When target sounds were presented simultaneously with large digits, they were categorized as *loud* more frequently, suggesting that participants substituted number magnitude when performing difficult sound intensity judgments in a manner similar to when judging duration. Third, the repetition of magnitude information presented in either symbolic (Arabic digits) or non-symbolic (numerosities) formats was manipulated prior to the presentation of a target number, whose duration was judged. The results

demonstrated that large numbers were judged to last for longer durations relative to small numbers. Furthermore, context had an effect in which a greater discrepancy in the target's numerical magnitude from the initial context sequence resulted in a longer perceived duration. The results across all three paradigms suggest that people generally employ information regarding one magnitude dimension (number) when making difficult perceptual decisions in a related dimension (time, sound intensity).

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Chapter I: Dissertation Introduction

Time, initially, is no more intrinsic to our mind than it is to an hourglass. Our sensations and our thoughts resemble the grains of sand that escape from the narrow opening.
- Jean-Marie Guyau (1890)

The means by which we come to comprehend temporal changes in our environment poses an interesting dilemma. How can we come to perceive something that is apparently undetectable? How is it that we can distinguish between "*what 5 minutes feels like*" and "*what an hour feels like*", when minutes and hours are simple abstractions that have no basis in physical reality. I will present a novel theoretical account for the perception of time that proposes that judgments about temporal extent – or duration – are directly connected to the ability to judge differences in number, and intensity level. To summarize, perceptual judgments of duration, quantity, or intensity are tied largely to the same underlying decision-making processes, with similar cognitive rules being applied – via top-down processes – across a wide-array of quantitative dimensions.

From a psychological perspective, the rate at which time is perceived to pass is dependent on a wide range of environmental and contextual factors, where an increase in some stimulus characteristic is associated with an increase in subjective duration. For example, a greater number of events (Fraisse, 1984), a higher rate of change across an interval (Brown, 1995; L. C. Leboe & Mondor, 2008; Poynter, 1989; Poynter, 1983), and greater stimulus complexity (Macar, 1996; Ornstein, 1969), all tend to induce longer reported interval durations. Similarly, increases in physical magnitude result in the same phenomenon, faster tempo/velocity stimuli (Kanai, Paffen, Hogendoorn, & Verstraten, 2006; Kaneko & Murakami, 2009; Zakay, Nitzan, & Glicksohn, 1983), and larger, brighter, more numerous stimulus events are all associated with a subjective increase in perceived duration (Matthews, Stewart, & Wearden, 2011; Mo, 1974; Oliveri et al., 2008; Xuan, Zhang, He, & Chen, 2007). Additionally, a variety of internal –

psychological – factors, such as an increase in controlled attention level (Underwood & Swain, 1973), whether attention is diverted to the left or right side of space (Ishihara, Keller, Rossetti, & Prinz, 2008), and emotional arousal (Droit-Volet, Fayolle, & Gil, 2011) are also tied in with variations in how duration is perceived. In the following set of studies, the means by which symbolic numerical magnitude information (i.e., Arabic digits) is implemented in forming decisions regarding duration (Chapter II, IV), and sound intensity level (Chapter III) are examined in depth, with the goal of developing a theoretical approach for explaining the perceptual interactions and biases witnessed across magnitude dimensions (time, number, intensity).

Models of Time Perception

A variety of scientific models have attempted to encapsulate the perceptual and cognitive mechanisms thought to underlie psychological timing. These have generally been subdivided into what John Michon (Michon, 1967, 1972) and Robert Ornstein (1969) have labeled *clock models* and *event-related theories*, or what Block (2003) aptly categorizes as *timing with a timer* and *timing without a timer* theories, respectively. While both frameworks make similar predictions regarding the subjective nature of the psychological experience of time (e.g., time flies when having fun, or drags on tediously when bored), they differ with regards to whether the distal stimulus representing time is attributed to internally generated mechanisms, or external (environmental) events. Clock models propose that perceived time is measured through the workings of a dedicated timer, while event-related theories suggest that perceived time is largely inferred, using event information related to the passage of time.

Internal clock models (timing with a timer)

Clock models of timing have generally dominated as a theory for non-human animal timing (Church, 1984; Gibbon, Church, & Meck, 1984; Thomas & Weaver, 1975; Treisman, 1963; Zakay, Nitzan, & Glicksohn, 1983). These models have proposed that the brain contains a pacemaker device that emits pulses at inconsistent/variable rates (Poisson variability) with the mean representation of the pulse rates across multiple trials representing the interval's duration. When the organism times an interval, a hypothetical switch is engaged, and pulses are counted in an accumulator device – allowing the internal clock to function in a run-stop timing mode. Reference memory stores a representation of the mean quantity of pulses that must be tallied before the current interval approximates the remembered interval length. When the ongoing pulse count matches the mean representation stored in reference memory (a process carried out through a comparator mechanism), the time-sensitive response is triggered (see Figure 1). Furthermore, it has been proposed that the internal clock can be switched from a run-stop (timing) mode to an event (counting) mode, in which each pulse is representative of an event (Allman, Pelphrey, & Meck, 2011; Meck & Church, 1983), providing the internal clock dual functionality as both a timer, and as a counter.

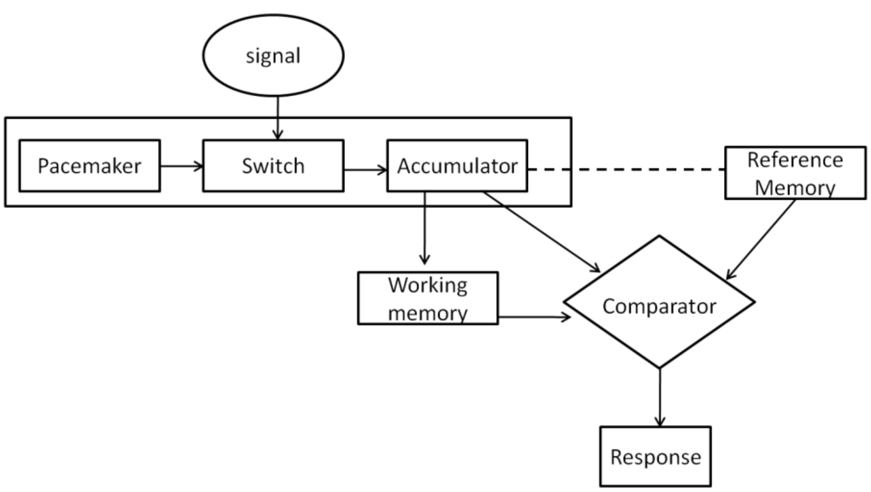

Figure 1. Scalar Expectancy Timing Model (Church, 1984; Gibbon et al., 1984)

According to Scalar-Expectancy Theory (SET), non-human animals respond on fixed interval reinforcement schedules by learning the value of a target interval – for example, a 5-minute period – by being positively reinforced for the first response occurring after 5 minutes have elapsed (usually with the administration of food). The scalar variance property (or Weber fraction) represents the variability ratio of perceived to actual time, and is measured by presenting the animal with a variety of intervals of varying length (some closer and some further away from the target interval). Accordingly, as the tested intervals take on values that are further away from the target interval, the probability that a response will occur decreases. This information can then be used to produce a temporal generalization gradient. The characteristics of this response gradient changes as different target interval lengths are employed, with flatter gradients occurring for longer target duration intervals indicating a reduction in temporal sensitivity. This scalar property holds for intervals ranging from 0.1 to 100 seconds in length (Lejeune & Wearden, 2006; Wearden & Lejeune, 2008), with counting operations exhibiting similar psychophysical functions (Meck & Church, 1983).The finding that Weber's law holds across a range of intervals is thought to be the result of the processes involved in comparing the total accumulated pulse rate held in working memory to a total held in reference memory (Block, 2003).

Building on the initial SET framework, arousal and attention have both been added to the original pacemaker-accumulator model as exogenous variables (Church, 1984; Gibbon et al., 1984; Gibbon, 1977; Meck & Church, 1983). According to clock models, pacemaker speed is theorized to be affected by one's state of arousal (Penton-Voak, Edwards, Percival, & Wearden, 1996; Wearden & Penton-Voak, 1995), where an increase in arousal is associated with longer subjective duration estimates, elicited by a simultaneous increase in the pacemaker's pulse production rate (Droit-Volet et al., 2011). This makes time feel subjectively slower than it actually is when aroused, a phenomenon sometimes reported during accidents, or fearful incidents (Arstila, 2012; Stetson, Fiesta, & Eagleman, 2007). A second factor shown to be highly predictable on time perception is that of attention, in which greater attentional focus on non-temporal tasks (e.g., word reading, visual search, arithmetic) tends to result in shortened subjective time estimates (Brown, 1985; Brown, 1997; Grondin & Macar, 1992; Hemmes, Brown, & Kladopoulos, 2004; Macar, Grondin, & Casini, 1994; Zakay et al., 1983). The internal clock model accounts for this phenomenon by stating that attention exerts its influence, not via pacemaker speed, but through the functioning of the switch – which is reimagined as a gateway. In this version, (attentional-gateway model) gate size is determined by how much attention is allocated to temporal or non-temporal tasks (Block & Zakay, 1996; Block & Zakay, 2006; Zakay & Block, 1998). In circumstances where attention is allocated away from temporal tasks, there is a narrowing of the gate such that fewer pulses are registered in the accumulator. This causes the interval to be experienced as shorter than its true length. Alternatively, when attention is devoted primarily to the passage of time, more pulses are registered in the accumulator, and perceived duration is processed as relatively long, a phenomenon often referred to as the *watched pot* illusion, named after the popular idiom “a watched pot never boils” (Cahoon & Edmonds, 1980).

As mentioned above – and discussed throughout – the effect of numerosity and symbolic magnitude on perceived duration is highly predicable, with greater quantities (Arlin, 1986; Hayashi, Valli, & Carlson, 2013; Mo, 1974) and larger Arabic digits (Oliveri et al., 2008; Vicario et al., 2008; Xuan, Chen, He, & Zhang, 2009; Xuan et al., 2007) eliciting systematically longer durations in comparison to small numbers. The internal clock/attentional-gate model can account for this bias in one of two ways: 1. larger numbers – as a function of their magnitude –

increase arousal level and thus enhance pacemaker speed, or 2. Processing larger numbers somehow requires fewer attentional resources, thus allowing the accumulation of more pulses. This second explanation, as research on size and distance effects have illustrated, would appear highly unlikely. For example, it takes longer to compare the magnitudes of two large numbers (8 vs. 9) versus two small numbers (1 vs. 2) despite both pairings exhibiting identical numerical distance (an arithmetic difference of 1). This indicates that it is overall more difficult – and therefore a greater drain on attentional resources – to process numerical differences at greater than lesser magnitudes (Moyer & Landauer, 1973; Moyer & Landauer, 1967; Restle, 1970). Therefore, the arousal explanation appears to be the only valid one.

Despite the popularity of internal clock model theories like SET (largely due to their predictive power) – these theories are scientifically unsatisfactory for a variety of reasons and should not be considered the definitive explanation for all time perception phenomena. First, the clock model speculates that physical time is somehow translated – at the level of the organism – into discrete temporal units or pulses. However, the production of these units is derived by directing attentional resources either to *time* or *non-time-related* processes. This *time focus* is seemingly implicit, and is not controlled in nature (e.g., counting the seconds as they pass), and yet (as noted above) it requires – and competes for – a high degree of attentional resources. Generally, implicit (i.e., automated) processes are thought to require few, if any attentional resources, and occur effortlessly (Hasher & Zacks, 1979; Logan, 1988; Posner & Snyder, 1975; Reber, 1989; Schneider & Shiffrin, 1977; Zacks, Hasher, Alba, Sanft, & Rose, 1984), thus 1. It is unclear what it even means to implicitly ‘attend to the passage of time’, and 2. If time processing does occur outside of our conscious awareness, how is it possible to direct attention to it? Finally, 3. How is it that an uncontrolled – *effortless* – process poses such a massive drain on the same attentional resources used to guide controlled, effortful processes such as those involved in arithmetical operations? Proponents of the internal clock model do not appear to address any of these questions.

Second, the internal clock model tends to be used to explain a variety of experimental findings, but the model is almost always applied *post hoc*. This limitation has been noted repeatedly over the years; Michon (1985) for instance noted 30 years ago "…In this approach it is impossible to distinguish qualitatively between the various factors that may cause fluctuations in the rate of the internal clock," (p. 17). For example, while some types of stimuli speed up the rate of the pacemaker (arousal), others exert their effect by shifting attentional resources away from the timer (e.g., visual object tracking). Simply because there is a lengthening in perceived duration when people are presented with a stimulus that may increase arousal level does not necessitate that increased arousal *caused* the increase in subjective duration. As Walsh (2003) more recently notes, "It is an additional problem that attention models do not make *a priori* predictions about attention and time." (p. 486). As such, any behavioral result can – in some manner of speaking – be interpreted as evidence in support of the theory.

Third, and as Block (1990) also points out, the internal clock model provides an oversimplified view of human timing abilities. It fails to consider that we often strategically manipulate environmental variables to convey information to us about the passage of time (e.g., using alarm clocks), or implement effortful strategies to measure time-in-passing (e.g., counting the seconds as they pass). Furthermore, these models fail to consider how highly complex external factors, including context, can impact time perception, focusing entirely on prospective timing (attending to time in the present) and never attempt to account for retrospective timing (how we reconstruct time after the fact). Additionally, if prospective time perception does involve consciously activating a switch between a pacemaker and an accumulator, then the model has difficulty accounting for experimental findings where people exhibit some degree of accuracy on implicit time perception tasks. For example, studies have demonstrated that temporal information is incidentally encoded without conscious intent (for a review see, Block & Zakay, 2001). In one case, memory regarding the temporal order of words presented in a list was found to be accurate – participants correctly identified the timing of a word in a list – despite forgetting which list the word was originally in (Hintzman, Block, & Summers, 1973). Further, participants' accuracy on the task was unaffected when information regarding temporal order was encoded under incidental conditions, over conditions where participants were told ahead of time to directly attend to the temporal order of the events (Auday, Sullivan, & Cross, 1988); while additional research has shown that people form implicit representations of interval length (Brown & Stubbs, 1992). This leads to the question, how is duration being encoded without consciously activating the switch between the pacemaker and accumulator?

Fourth, again as Block (2003) also pointed out, Weber's law applies to a wide array of quantitative stimulus dimensions beyond that of psychological time which includes judgments about quantity (Cordes, Gelman, Gallistel, & Whalen, 2001; Whalen, Gallistel, & Gelman, 1999). While the mode-control model previously discussed may be able to account for psychophysical similarities in the performance of time and number estimation, it is not clear why perceptual dimensions like sound intensity, brightness, and physical weight should also conform to Weber's law. As noted by Block, "With only slight modification (e.g., substituting external stimulus information for the pacemaker), scalar-timing models could easily become scalar-perceiving models" (p. 44).

Fifth, the neuroscientific evidence in support of a centralized timer is, at best, inconclusive. According to the distributed network perspective, the striatum may act as the locus for the internal clock, as striatal cells (which have firing rates that fluctuate from 10 to 40 cycles per second) may receive messages about when to begin timing an interval from cortical neurons (for a reivew see Grondin, 2010). At this point, their firing rates synch, becoming less synchronized over time, with their pattern of activation at the end of the timed interval being recorded in memory as a neural representation of the interval's duration. While this appears to be a reasonable physiological model of timing, there is a general lack of consensus in terms of the brain areas that are actually involved in time perception – as well as with the neurophysiological code thought to underlie duration. Other researchers have identified regions of the brain that may act as accumulators by ramping neural activation level with the progression of the interval; including the preSMA and anterior cingulate cortex (ACC) as areas that increase monotonically in neural firing rate with duration (Macar et al., 2002; Pouthas et al., 2005). Alternatively, other studies have identified regions in the inferior parietal cortex that monotonically ramp in firing rate with duration (Coull et al., 2004; Rao, Mayer, & Harrington, 2001). One possibility is that this ramping function could be a more general neural property involved in coding responses made to a wide array of quantitative environmental information; inclusive of brightness, sound intensity and number, all of which are coded by neurons with increasing monotonic functions.

Finally, there is strong evidence suggesting that auditory and visual stimuli are timed through modality-specific processes which lead to sounds being perceived as longer in duration than equivalent duration visual stimuli (Lhamon & Goldstone, 1974; Wearden, Edwards, Fakhri, & Percival, 1998). There is also evidence to suggest that there may be multiple clocks operating within sensory modalities. In several studies, it was found that visual adaptation to an area in visual space reduced the perceived duration of stimuli later presented in that location, but not in other proximal visual regions (Burr, Tozzi, & Morrone, 2007; Johnston, Arnold, & Nishida, 2006). Again, it is not clear how models that put forth that all psychological time perception phenomena are connected to the workings of a single internal clock could account for these findings.

Event-related theories (timing without a timer)

Modern event-related (*non-clock*) theories of time perception can be traced to the work of Paul Fraisse (Fraisse, 1963, 1984) and Robert Ornstein (1969), but their true origin can be found in the writings of French philosopher Jean-Marie Guyau (Guyau, 1890, 1988), who is considered to have written one of the earliest pre-modern cognitive psychological works on time perception (Michon, 1988). Quite generally, Guyau posited that psychological time is mentally constructed, and therefore is entirely dependent upon, recognized variations in the sensations and perceptions that are processed from the environment. Guyau is also the first to suggest that subjective time is closely associated with perceiving changes in intensity level, writing:

> Apart from the first three elements underlying the notion of time: differences, similarities, and number, consciousness soon puts us in possession of a fourth and extremely important one: intensity or degree. In my view there exists an intimate connection between intensity and the moment. (Guyau, 1890, 1988, p. 105)

He suggests that we are innately aware that time is marked by contextual changes demarked by differences, similarities and quantities, as well as variations in intensity, as he goes on to state, “If there were no division, no change and no gradation in activity or sensitivity, there would be no time.” (Guyau, 1890, 1988 p. 106). Paul Fraisse (1963) would conclude 70 years later that duration is derived from “successive changes and nothing else”, and therefore our perception of time is based upon "the number of changes observed." (p. 219). To summarize, for event theorists, the brain acts as an information processor, partitioning experiences into events and constructing a coherent sequential order from the structure of those events. It is through this process, two subjective sensations related to time perception are formed, which Michon (1985) categorizes as "*now*" – a sense of existing in the present moment, and "*flow*" – the sense that time is continually progressing into the future. The maintenance of these experiences he refers to as *tuning*:

> tuning can be described as the process of keeping track of the correspondence between events in the outside world and the events produced in an internal representation of that world: keeping the two series in synchrony is precisely what tuning is about. (Michon, 1985, p. 29)

As discussed by Michon (1985), because our consciously generated expectations for when an event *should* occur precede the actual event, the interval is evaluated as subjectively long, while time is perceived of as short when external events precede their expected occurrence. This is how event-related theorists reconcile temporal illusions such as *time-flying* and *the watched-pot* phenomena without relying on an internal clock. Michon (1985) argues that time perception is largely a controlled – effortful – process that requires attentional resources, and that the predominant distal stimulus for duration is non-temporal cues regarding event order, as well as "Other, intrinsically non-temporal cues" which "may be given a (quasi-) temporal interpretation" (p. 35). This provides a plausible explanation for a host of temporal illusions like the kappa effect – in which a greater distance (spatial interval) delineating successive events is often perceived as consisting of a longer temporal interval. In this instance, spatial (distance) cues are used to infer the duration of an interval (Alards-Tomalin, Leboe-McGowan, & Mondor, 2013; Casasanto & Boroditsky, 2008; Cohen, Hansel, & Sylvester, 1953; Henry & McAuley, 2009; Sarrazin, Giraudo, Pailhous, Bootsma, & Giraudo, 2004; Shigeno, 1993). This also accounts for why the close relations between changes in physical magnitude and time may also be taken advantage of when making inferences about duration. In these cases, I will argue that stimulus magnitude is substituted for time (Xuan et al., 2007).

It should be noted that not all event-related theories are the same, with some falling into and out of popularity since the late 1960s. In Ornstein's original *storage-size model*, memory is conceptualized as being similar to computer storage space, with subjective time being computed from the total amount of utilized memory storage-space. Thus, as this memory storage-space is filled (either by more events, or increased complexity), perceived time is lengthened. While this theory was the first modern account to oppose the idea of a centralized timer, it was found to be largely inaccurate, and difficult to replicate (Block, 1978). In a series of studies, Block found that it was actually the contextual nature of the experience itself that determined perceived duration, not the amount of memory used. For example, if participants studied words presented in a list using either shallow (structural level, e.g., font size) or deep (semantic level: word meaning) encoding strategies for a later recall test, the remembered presentation durations for the deeply processed words were equivalent to the shallowly encoded words, despite being recognized with greater accuracy and presumably occupying more memory space (Block, 1982; Block & Reed, 1978; Block, 1985).

Block found instead that it was *changes* in the context that influenced duration. For example, alternating word encoding tasks between shallow and deep strategies enhanced the subjective duration of the interval spent studying by 18% relative to those spent using a single strategy. However, there was no difference in the perceived durations of encoding tasks that were done entirely using shallow or deep encoding strategies despite the fact that the deep encoding strategy enhanced recall. Therefore, it cannot simply be concluded that enhanced memory for an event is synonymous with an increase in the perceived duration of that event.

As a result, Block and others (Brown, 1995; Poynter, 1989; Poynter, 1983) have suggested that perceived time may be reflective of the level of segmentation, or continuous change, within the experience, with a greater degree of change resulting in longer duration estimates. This phenomenon has been proposed to be the application of a more general change heuristic, where change is used to aid in making perceptual judgments that include, but are not limited to duration (L. C. Leboe & Mondor, 2008). For example, it has been found that changes over time are also used as a means of quantifying an event's magnitude – like judging the intensity of a sound (L. C. Leboe & Mondor, 2010).

Contextual-change theories seemingly have no explanation for why a large symbolic number (e.g., 9) should lead to a longer duration estimate over a smaller number (e.g., 1), as both are entirely static – unchanging – events. Rather than propose that time perception is determined by the application of a single heuristic (the level of processed change across the interval), I further propose that perceived duration is governed by the fluid application of a wide array of strategies and rule-sets, the application of which is largely contextually dependent.

A heuristic account of time perception

Heuristics are simple rules-of-thumb that when adhered to, generally produce correct answers/responses in a highly efficient manner; but, when over-relied upon, may yield systematic sources of error and bias. In many cases people rely solely on information that is only strongly associated with a target dimension rather than the target dimension itself. For example, the speed and subjective ease with which information is consciously accessed is typically used to index a variety of judgments, including event probability (availability heuristic, Tversky & Kahneman, 1973, 1974, 1981); recognition (fluency heuristic, J. P. Leboe & Whittlesea, 2002; Whittlesea, Jacoby, & Girard, 1990; Whittlesea & J. P. Leboe, 2000) and emotional preferences (J. P. Leboe & Ansons, 2006; Zajonc, 1968). Despite the wealth of research regarding the impact of heuristics on higher order cognitive processes; investigation into how they can lead to distortions on basic perceptual judgments has received far less scrutiny.

Despite this, heuristics have been widely applied as explanations for perceptual illusions. In an early example, Dees (1966) supported a heuristic account of the moon illusion – where the moon appears larger in size at the horizon versus when it's overhead – stating, "most 'perceptual logic' is subverbal and in large measure automatic and based upon learned premises which usually are true." (p. 2). In the moon-illusion, when viewed on the horizon, monocular depth cues (interposition, linear perspective), are used to make inferences about distance, which subsequently impact perceived size. When the moon is positioned overhead, the lack of those depth cues disrupts this inferential process.

Since then, heuristics have been used to account for other perceptual illusions, for example, perceiving depth in a two-dimensional object when the two-dimensional object is put into motion (Braunstein, 1962; Braunstein, 1976), determining the glossiness of an object's surface (Fleming, 2012); the performance of complex motor skills (Dienes & McLeod, 1996; Gigerenzer, 2004); how we taste our food, (for reviews on how colour impacts taste perception see, Delwiche, 2004; Spence, Levitan, Shankar, & Zampini, 2010), as well as our ability to identify the spatial and perceptual characteristics of a sound (e.g., pitch) (Leboe & Mondor, 2007). In all of the above cases, perception in one system was influenced by the application of a heuristic over an algorithm.

The question can be posed as to why heuristics would be used in perception at all when relying entirely on bottom-up – sensory-driven – mechanisms would decrease the probability of making systematic perceptual errors (i.e., illusions). This is likely a case of the benefits largely outweighing the costs, with a heuristic approach resulting in heightened processing speed, as well as enhancing one's ability to flexibly analyze degraded information (for a similar conclusion, see Braunstein, 1976). This explanation appears plausible for all instances where heuristics are used in perception, with the organism's survival being largely dependent on fast thinking, and adaptability. Despite this, the question remains as to why information about numerical magnitude would ever be informative and/or associated with the dimension of time?

Current imaging research has provided some support for the existence of a common neural metric (allocated to the Intraparietal Sulcus or IPS) for the representation of time, space and quantity, referred to as the generalized magnitude system (Bueti & Walsh, 2009; Walsh, 2003). The A Theory of Magnitude or ATOM framework – predicts that all incoming quantifiable information (whether spatial, temporal or numerical) will be represented using a common, analog code that is utilized by the action production (motor response) system of the parietal cortex. This code is invoked when performing general 'more/less' relative magnitude approximations. Finally, it is also thought that this common code may facilitate the various interactions witnessed between temporal/spatial/numerical dimensions (see Figure 2), which include kappa/tau effects (A.), filled duration illusion (B.) and SNARC (spatial-numerical association of response codes) effects (C., see Figure 2).

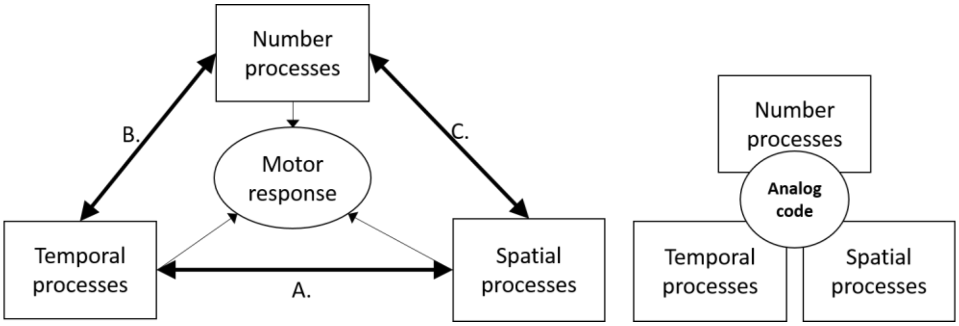

Figure 2. A Theory of Magnitude (ATOM) Framework (Walsh, 2003)

Guyau similarly supported the close association of time with space and number (as well as intensity), stating that the number of sensations (or variations in intensity) experienced over an interval could act as a cognitive index for duration, “The idea that number is perhaps sufficient to account for this case: a distance traversed seems longer when it gives rise to more sensations, while it seems shorter when it generates fewer sensory impressions.”, he further stated:

> I am not arguing that we count every individual sensation; neither do we have to measure the volume of two unequal mountains in cubic meters of earth, and yet we can tell at first sight that one is larger than the other… Numbers can exist in the absence of enumeration and one can estimate without detailed computations… we follow the example of animals and primitive tribesman, that is, we cast a glance and guess. The result of this evaluation represents simultaneously the apparent length of time and the spatial expanse traversed during that time. (Guyau, 1890, 1988, p 127).

Guyau – and more recently Walsh – have supported the conclusion that similar processes are used to perform approximations across a wide array of magnitude dimensions, thus allowing us to be quick and efficient at determining; whether or not one bush has more berries than another; whether a hunting party has been missing long enough to warrant a search; how hard a spear must be thrown to strike down a woolly mammoth; and whether or not a rival tribe is sufficiently far enough away to warrant ignoring them. To afford this level of fluidity of approximation across dimensions, it would make sense that all might be represented within a common representational, or decision-making metric. Time, however, unlike other environmental information, has no distal stimulus in the environment beyond that of change and therefore must always be inferred (or guessed at) by using information processed regarding the other dimensions. Similar to Guyau's proposition over 100 years, I suggest that time is inferred using the same implicit mental approximations used to guide judgements regarding differences in space, quantity and intensity.

Finally, if temporal judgments are based on perceived variations in other dimensions, what are the general cognitive mechanisms through which this is accomplished? I propose that space, number and intensity are substituted for duration through a process called attribute substitution (Kahneman & Frederick, 2002; Kahneman & Frederick, 2005). The general idea behind attribute substitution is succinctly stated by Kahneman and Frederick, "When confronted with a difficult question, people may answer an easier one instead and are often unaware of the substitution." (Kahneman & Frederick, 2005 p. 269). In one example, participants were asked to rate their current level of life satisfaction. If they were asked to first provide an estimate of how many dates they had been on in the last 6 months, it was found that their answer given to this question strongly correlated with their life satisfaction rating. However, if the questions were asked further apart, the answers provided for each were unrelated.

Attribute substitution occurs when the target feature is less readily assessed than some related attribute. When this happens, the more accessible feature is often incorporated into the decision without the decision-maker's conscious knowledge of the substitution. As previously noted, this occurs as a means to ensure speedy, uninterrupted decision-making that may be open to bias, as the authors note, "Whenever the heuristic attribute differs from the target attribute, the substitution of one for the other inevitably introduces systematic biases." (Kahneman & Frederick, 2005 p. 269). I propose that estimating duration poses a distinct challenge in that there is no environmental stimulus attribute representative of duration, and as such, we have learned to substitute a wide-array of related dimensional attributes when inferring duration.

Current Study

In the following set of experiments, I will test the heuristic account I have laid out above in several ways, using several different methodologies to answer several key questions. 1. If heuristics are used to infer duration, the perceived interval of time occurring between stimulus events (uninformative intervals) should rely on a different duration index than the duration of events themselves (informative intervals). In Chapter II (Number Magnitude, Size, and Colour Saturation on Time), I will employ an experimental framework that has been previously established for isolating the kappa effect – the influence of spatial extent on perceived duration – to answer this question. Previous work in this area has found that a greater variation in both physical space, as well as auditory spatial analogs – like pitch – across an interval tended to induce longer perceived durations for that interval (Cohen, Hansel, & Sylvester, 1954; Shigeno, 1986, 1993). As symbolic numbers are visual in nature but are not inherently spatial, I propose that kappa effects are – in part – driven by on-the-fly computations of the magnitude similarity of the events defining the interval separating their onsets. If the similarity in the magnitudes of successive events is high, this will be used to indicate that a relatively short interval must have occurred. However, if the similarity is low, this will be used to indicate that a relatively long interval must have occurred. If a flexible heuristic is being employed, similarity should exert analogous effects on perceived duration regardless of the dimension itself. If the system is largely inflexible (as would be suggested by the internal clock model), then an interval separating large numbers should be perceived of as longer in duration than the same interval separating small numbers.

2. If time-number interactions are the result of the application of a more general heuristic, then the same heuristic should be applied when making judgments in other modalities. In Chapter III (Number Magnitude and Sound Loudness) it is predicted that difficult decisions in another related modality (a difficult sound judgment regarding sound intensity) should be impacted by symbolic number magnitude in the same manner that duration judgments are influenced. In this chapter, I propose that people may anchor on the value of a symbolic number and subsequently use that information to guide perceptual judgments about sounds. There is evidence to suggest that the reverse is true, for instance, auditory tones facilitate judgments about the magnitude of later occurring symbolic numbers, when the tonal pitch and number magnitude are congruent (*high* pitch – *large* number, Oriet, Tombu, & Jolicoeur, 2005). I further propose that time-number interactions may constitute a form of numerical anchoring, where an irrelevant number's value is used as a launching point on a wide array of judgments.

3. If time-number interactions are driven by the application of perceptual heuristics; the resulting bias should be contextually dependent. For example, the degree of timing bias a number elicits will be based primarily on how large the context makes the number appear, rather than the number's absolute value, in the same way that contextual monocular distance cues influence how big the moon appears when on the horizon. Finally, in Chapter IV (Contextual Influence on Perceived Number Duration), I more generally examine how the tendency to perceive large numbers as lasting for longer durations is not entirely determined by digit magnitude, but rather, by the context preceding the presentation of that digit. For example, if the number 9 has the appearance of being *more substantial* when it follows a series of small numbers (1s and 2s)

versus large numbers (7s and 8s); will this feeling be captured and factored into duration estimates. If so, it indicates a high degree of flexibility in how numerical information might be imported into duration judgments.

In summary, the three experimental paradigms are all tests of cognitive flexibility. A heuristic account poses that rules which guide perception are generated via one's experiences and interactions with the environment, with later decisions about difficult dimensions (time) being weighted toward more easily accessible information (number). The more general goal of this work is aimed at expanding how we think about perception as a whole. The tendency to focus exclusively on bottom-up driven sensory processes in discussions of perception ignores the equally – if not more – important question regarding how top-down guided processes are factored into our experiences.

Alards-Tomalin, D., Leboe-McGowan, J., Shaw, J. D. M., & Leboe-McGowan, L.C. (2014). The effects of numerical magnitude, size, and colour saturation on perceived interval duration. *Journal of Experimental Psychology: Learning, Memory & Cognition*, *40*(2), 555-566. doi: 10.1037/a0035031

Chapter II: Number Magnitude, Size, and Colour Saturation on Time

The perception of time is driven by contextual and environmental cues that are often only indirectly associated with time itself. A variety of studies have shown that, in part, people infer duration through its interaction with experientially-related perceptual properties across multiple stimulus dimensions and modalities. For example, a duration is inferred as longer than a comparison interval when it contains more stimuli (Adams, 1977; Buffardi, 1971; Thomas & Brown, 1974) or exhibits dynamic properties (Brown, 1995; Casasanto & Boroditsky, 2008; Kanai et al., 2006; L. C. Leboe & Mondor, 2008). Additionally, higher order magnitude stimuli induce a subjective expansion of perceived duration. For example, in visual studies, bright lights are perceived as lasting longer than equivalent duration dim lights (Brigner, 1986; Kraemer, Brown, & Randall, 1995), while larger visual stimuli are perceived as lasting longer than equivalent duration small stimuli (Ono & Kawahara, 2007; Ono & Kitazawa, 2009). Likewise, in auditory studies, high frequency tones are reported as lasting longer than equivalent duration low frequency tones (Allan, 1984), while loud tones are perceived as lasting longer than equivalent duration quiet tones (Oléron, 1952).

Recently, a number of studies have determined that symbolic Arabic digits exert similar contextual biases on a wide array of different spatial and temporal tasks (Casarotti, Michielin, Zorzi, & Umiltà, 2007; Fischer, Castel, Dodd, & Pratt, 2003; Oliveri et al., 2008; Oliveri, Koch, & Caltagirone, 2009). To illustrate, a recent study revealed that, independent of their true duration, participants judged small magnitude digits (e.g., 1) to be shorter in duration; and large magnitude digits (e.g., 9) to be longer in duration, than intermediate digits (e.g., 5; Oliveri et al., 2008). This effect has since been replicated using a Stroop-like paradigm, finding that participants are more accurate at classifying the duration of a number when its magnitude is congruent with its presentation time (e.g., a small digit presented for a shorter time), than when it is incongruent with its presentation time (e.g., a small digit presented for a longer time; Lu, Hodges, Zhang, & Zhang, 2009; Xuan, Chen, He, & Zhang, 2009; Xuan, Zhang, He, & Chen, 2007).

The A Theory of Magnitude (ATOM) framework, proposed by Walsh (2003), is an influential theoretical framework accounting for the numerous interactions that have been demonstrated across various quantitative dimensions (e.g., number, quantity, size, duration). According to this approach, humans and animals possess a generalized analog magnitude system, in which space, time and quantity (or number), as well as other magnitudes (see Bueti & Walsh, 2009), are translated into an abstract magnitude code. This code represents the stimulus intensity in the form of an approximation (e.g., *a little* vs. *a lot*) which demonstrates a ratio-dependent property, such that, at greater stimulus intensities, effective discrimination will depend on an ever greater level of disparity between the compared stimuli (*Weber's law*; Bonn & Cantlon, 2012; Dehaene, Dehaene-Lambertz, & Cohen, 1998; Gallistel & Gelman, 2000; Piazza & Dehaene, 2004; Walsh, 2003).

Current brain-imaging research indicates the intraparietal sulcus as the neurological correlate associated with number processing, and the analog magnitude system (Cappelletti, Muggleton, & Walsh, 2009; Hubbard, Piazza, Pinel, & Dehaene, 2005). Research further indicates that numbers are represented according to a spatial format, with smaller magnitude numbers being associated with the left side and greater magnitude numbers the right side, resulting in a theoretical mental number line (Dehaene et al., 1998; Restle, 1970).

The concept of a mental number line receives further support from the Spatial-Numerical Association of Response Codes (SNARC) effect (Dehaene, Bossini, & Giraux, 1993). This line of research has demonstrated that people from Western cultures are faster at making left-handed responses to small magnitude numbers (e.g., 1) and right-handed responses to large magnitude numbers (e.g., 9), an effect that is prevalent even when numerical magnitude is irrelevant to the primary task (e.g., judging parity). This effect is robust and has been widely replicated using number words (e.g., one vs. nine), and auditorily-presented digits (Fias, 2001; Nuerk, Wood, & Willmes, 2005; Nuerk, Iversen, & Willmes, 2004).

Therefore, it appears that people impose spatial organization on numerical values by associating smaller numbers with the left side of space, and larger numbers to the right side of space. The imposition of spatial organization on numbers is, in part, experientially driven. The spatial layout, or direction of a person's writing system has a clear impact on how different magnitude dimensions are processed, including time (Fuhrman & Boroditsky, 2010), and numbers (Zebian, 2005). Zebian (2005), for example, demonstrated that while English and French readers (whose written languages move from left to right) are prone to a left-to-right oriented SNARC effect, Farsi and Arabic readers (whose written languages flow from right to left) demonstrate the opposite pattern; a right-to-left oriented SNARC effect.

Further support for the ATOM framework has arisen from a series of converging findings that suggest that other quantitative dimensions exhibit similar spatial biases. Similar SNARC-like effects — where responses to smaller magnitudes are facilitated by using the left hand, and larger magnitudes using the right hand — have been found using other abstract, continuous dimensions, including months, and alphabetic characters (Gevers, Reynvoet, & Fias, 2003); and a variety of quantitative perceptual dimensions, including physical size (Ren, Nicholls, Ma, & Chen, 2011), weight (Holmes & Lourenco, 2013) and time (Di Bono et al., 2012; Fabbri, Cancellieri, & Natale, 2012; Ishihara et al., 2008; Vallesi, Binns, & Shallice, 2008; Vicario et al., 2008). For example, people are faster at categorizing short duration intervals as "short" with a left-handed response, and long duration intervals as "long" with a right-handed response (referred to as the Spatial-Temporal Association of Response Codes, or STARC, effect). These observations further the theory that numerical magnitude, time, and other quantitative dimensions are all similarly organized, forming what has sometimes been termed a mental magnitude line (see also, Holmes & Lourenco, 2011, 2013).

While it has been established that different magnitudes are subject to spatial organization, the implications of this imposed organizational structure on various judgments has received less attention. A goal of the current study was to investigate whether the imposition of a left-to-right spatial framework on various quantitative dimensions (including, number, size, and colour saturation) would bias judgments regarding the duration of an interval separating two stimuli.

The reliance on common processes for representing time and numerical magnitude could account for the influence of numerical magnitude on temporal judgments previously demonstrated (Oliveri et al., 2008; Xuan et al., 2009, 2007). Additional support for this approach has emerged from demonstrations of various cross-dimensional interference effects between temporal and spatial perceptual processes. Early demonstrations of cross-dimensional interference (Abe, 1935; Benussi, 1913), found that if discrete stimuli (e.g., light flashes) were presented in a sequence, the amount of physical distance separating two events directly impacted subjective duration judgments of the separating interval. For example, the perceived duration of an interval was judged to become longer in its overall duration as a function of the distance separating the two stimuli. This perceptual illusion — called the kappa effect — has been widely reported for visual stimuli (Abe, 1935; Cohen et al., 1953; Lebensfeld & Wapner, 1968; Sarrazin et al., 2004), and in studies that have employed auditory spatial analogs, including sound frequency (Boltz, 1998; Cohen et al., 1954; Crowder & Neath, 1995; Henry & McAuley, 2009; Shigeno, 1986, 1993), and sound intensity (Alards-Tomalin et al., 2013). This bias is generally elicited through presenting participants with three sequentially presented stimuli (designated as AXB). In this sequence, A and B represent boundary elements defining a spatial interval within which the placement of the second occurring stimulus (X) is varied across trials. On a standard AXB task, judgments are formed about the relative durations of the blank intervals between A-X and X-B. The durations of these blank intervals are defined according to the stimulus onset asynchrony (SOA), the amount of time separating the onsets of two discrete stimuli. In the present study, SOAs are provided. Generally, as stimulus X nears stimulus A's spatial position, the tendency to classify the first blank interval (SOA: 1) as short, and second blank interval (SOA: 2) as long increases. Conversely, as stimulus X nears B's spatial position, the tendency to classify the first blank interval (SOA: 1) as long, and the second blank interval (SOA: 2) as short increases. Therefore, like the SNARC and STARC effects, kappa effects also demonstrate evidence for cross-dimensional interactions between different magnitudes (e.g., time and space).

In the experiments reported, we first investigated a role for variations in the sequential magnitudes of number, size, and colour saturation in contributing to the perceived duration of a blank interval. The goals of the following study were twofold. First, we wished to investigate if variations in magnitude contributed biases to interval duration judgments in a manner similar to changes in physical distance. We hypothesized that a smaller numerical magnitude difference (Experiments 1 and 2) would result in a shorter perceived duration between stimulus onsets.

Additionally, we hypothesized that a greater degree of perceptual similarity in both stimulus size (Experiment 3) and colour saturation (Experiment 4) will also bias participants to judge the interval separating two visual objects as shorter than when those two objects are less perceptually similar.

A possible alternative hypothesis is that the impact of stimulus magnitude on the subjective duration of discrete stimuli could also exert an influence on the perceived duration of an empty interval separating successive stimuli. This hypothesis makes the prediction that if two relatively large magnitude items are presented in succession (e.g., 8 –9), the interval separating them will be perceived as subjectively longer than the same duration separating two small magnitude items (e.g., 1–2). This alternative hypothesis was also tested in our study. In reporting these studies, our broader goal was to contribute to the increasing body of evidence demonstrating close associations between the comprehensions of magnitude across a variety of perceptual dimensions.

Experiment 1

An increase in the physical distance between two sequentially-presented stimuli (visual or auditory) leads participants to perceive an increase in the duration of the interval separating those stimuli (i.e., the kappa effect). In Experiment 1, instead of manipulating physical distance, we manipulated the relative numerical magnitudes of digits, such that the discrepancy between two items could be relatively large or small. On each of a series of trials, participants viewed a sequence of three digits, and then judged the relative durations of the SOAs separating the first digit from the second digit, and the second digit from the third digit. For half of the trials, SOA: 1 was longer than SOA: 2 (a *long-short* pattern), and for the other half, SOA: 1 was shorter than SOA: 2 (a *short-long* pattern). We hypothesized that a smaller magnitude discrepancy across

SOA: 1 should result in a tendency to categorize the sequence as “short-long” more often, while a greater discrepancy across SOA: 1 should result in a long-short response bias.

Method

Participants. Fifty-three University of Manitoba (Winnipeg, Manitoba, Canada) undergraduate students enrolled in an Introduction to Psychology course at the University of Manitoba participated in Experiment 1. They received partial course credit for participating. Twenty-eight participants were randomly assigned to the increasing digit magnitude condition (16 females, 12 males, mean age = 21.21 years) and 25 to the decreasing digit magnitude condition (n = 25, 15 females, 10 males, mean age = 19.32 years). The study was approved by the University of Manitoba, Fort Garry Campus Research Ethics Board. All participants provided informed consent.

Materials. The numbers were presented sequentially in increasing and decreasing magnitude configurations consisting of eight different Arabic digits (1, 2, 3, 4, 6, 7, 8, 9) presented in Times New Roman font. They were centrally presented on a computer monitor subtending a 4.25° visual angle, horizontally and a 5.13° visual angle, vertically. All materials were presented on a LG W2442PA Flatron LCD monitor with a screen resolution of 1920 × 1080 pixels with a response time of 2 ms. The monitor was connected to a PC utilizing an Intel Core 2 Duo CPU, 3.00 GHz, 3.18 GB RAM. The video card was an Intel Q45/Q43 Express Chipset display adapter. The images were displayed with 32 Bit Colour Depth, with a 60 Hz refresh rate. All of the digits were created using Microsoft PowerPoint 2010 and then converted into bitmap images (BMP files), which were displayed using E-Prime software Version 1.2 (Psychology Software Tools, 2002). The selected SOA durations were either relatively long (785 ms [47 frames], 768 ms [46 frames], 752 ms [45 frames], 735 ms [44 frames]), or relatively short (680 ms [41 frames], 660 ms [40 frames], 640 ms [39 frames], 620 ms [38 frames]).

Design and procedure. In this, and the following experiments, we used a standard AXB paradigm (e.g., Shigeno, 1986, 1993), which involves the presentation of three sequential events (a total trial duration of 1,420 ms). After receiving verbal instructions, an experimenter asked the participants to initiate the first and each subsequent trial by pressing the space bar. This button-press initiated the presentation of a fixation cross at the center of the screen for 500 ms, which was followed by an AXB sequence. The AXB paradigm involved the central presentation of three stimuli in succession, with each event appearing for an equal duration of 200 ms (12 frames). The first stimulus (A) was followed by an interval in which nothing was presented on the screen (i.e., a blank – uninformative – interval), followed by the presentation of the second stimulus (X). The second stimulus was then followed by a second blank interval, and finally a third stimulus (B). The durations of these blank intervals (SOAs) were manipulated across trials.

The interval between the onsets of the first and second stimuli (SOA: 1) and between the second and third stimuli (SOA: 2) varied in duration, so that either SOA: 1 was shorter (S-L pattern) or longer (L-S pattern) than SOA: 2. The SOA durations consisted of four L-S patterns (785-635 ms, 768-651 ms, 752-668 ms, 735-685 ms) and four S-L patterns (in which the long-short SOA durations were reversed). This information was further expressed as the interval difference (wherein SOA: 2 are subtracted from SOA: 1). This was done to provide a means of conceptualizing the overall saliency of the pattern’s timing, in which a smaller difference equated with an interval structure that was more difficult to detect. The 4 L-S patterns exhibited interval differences of 150, 117, 84, and 50 ms; while the 4 S-L patterns exhibited interval differences of -150, -117, -84, -50 ms.

Regarding the characteristics of the stimuli, the first and third events in the three-event sequence were unchanging boundary elements (the numbers 1 or 9), while the magnitude of the second stimulus varied across trials (spanning from 2 to 8). It should be noted that 5 was never included as a second stimulus value. Dependent on whether 1 was the first or third stimulus, each trial could be identified as either increasing (1-X-9) or decreasing (9-X-1) in magnitude.

After the offset of the third stimulus in the sequence, participants were prompted to classify the trial as either long-short (L-S pattern) or short-long (S-L pattern), depending on the perceived durations of SOA: 1 relative to SOA: 2. Participants made their responses by pressing keyboard buttons labeled SL or LS on the keyboard. The SL label was affixed to the keyboard's S-key (and pressed with the left hand); whereas the LS label was affixed to the keyboard's L-key (and pressed with the right hand).

By using this procedure, the six possible digits that could appear as the second stimulus (2, 3, 4, 6, 7, and 8) were combined with the eight levels of interval timing to generate 48 trial types for both the increasing and decreasing magnitude conditions. The magnitude difference between the second stimulus and the respective boundary elements generated six possible conditions: 1-7, 2-6, 3-5, 5-3, 6-2, and 7-1. According to this notation, the first number of each pair provides the arithmetic difference taken between the first and second digit, while the second number describes the difference taken between the second and third digit (see Table 1). The participants completed 10 repetitions of each of the 48 trial types in a randomized order. The session consisted of 480 experimental trials. Participants received no feedback on their responses and were asked to make their judgments as quickly and as accurately as possible.

Table 1
AXB Digit Values for the Increasing and Decreasing Magnitude Conditions of Experiment 1

Condition Type	**A**	**X** Magnitude Difference						**B**
		1-7	2-6	3-5	5-3	6-2	7-1	
Increasing	1	2	3	4	6	7	8	9
Decreasing	9	8	7	6	4	3	2	1

Results and Discussion

Table 2 displays the mean proportion of long-short responses for each condition of our design (in both increasing and decreasing configurations). Wherever means are provided in text, the standard errors are provided in parentheses. We submitted the proportion of long-short responses for each participant to a 2 (Digit Magnitude Direction: Increasing/Decreasing) × 6 (Digit Magnitude Difference: 1-7/2-6/3-5/5-3/6-2/7-1) × 8 (Interval Timing: ±150, ± 117, ± 84, and ± 50 ms) mixed-design analysis of variance (ANOVA), treating Digit Magnitude Difference and Interval Timing as within-participant factors, and Digit Magnitude Direction as a between-participants factor. In this, and subsequent experiments, when violations of the assumption of sphericity were observed, Greenhouse-Geisser estimates were used to correct the degrees of freedom. All analyses were conducted using SPSS Statistics for Windows, version 17 (SPSS Inc, 2008).

This analysis revealed a significant main effect of Interval Timing, $F(1.82, 92.66) = 159.96$, $p < .001$, $\eta p^2 = .76$. The proportion of long-short responses decreased systematically as the pattern timing was shifted from the most salient L-S pattern (150 ms; M = .77[.02]), to the most salient S-L pattern (–150 ms; M = .27[.02]). Critically, there was a significant main effect of Digit Magnitude Difference, $F(3.51, 179.20) = 14.74$, $p = .001$, $\eta p^2 = .22$, in which the proportion of long-short responses increased linearly as the magnitude difference shifted from

1-7 (a one digit different between the first and second numbers; *M* = .47[.02]), to 7-1 (a seven digit difference between the first and second numbers; *M* = .56 [.02]), $F(1, 51) = 32.54$, $p < .001$, $\eta p^2 = .39$. Additionally, digit magnitude direction (increasing vs. decreasing) had no influence on the overall trend (Digit Magnitude Direction × Interval Timing interaction, $p = .52$). Besides the main effects noted, none of the interactions were significant (remaining $p \geq .10$), and the between-participants main effect of Digit Magnitude Direction was non-significant ($p = .28$).

In Experiment 1, it was determined that variations in the magnitude of sequentially presented numbers can lead to biases similar to those witnessed when the physical space between discrete events was manipulated. We hypothesized that, because the representations of time, space and magnitude may rely on common mechanisms, a discrepancy in numerical magnitude should directly impact perceived duration; this hypothesis was confirmed. Additionally, the directionality of the sequence (increasing vs. decreasing) did not modulate the effect, replicating previous kappa effect studies (Alards-Tomalin et al., 2013; Henry & McAuley, 2009). This provides convergent support for our view that the present results represent a variant of the kappa effect based on the manipulation of a phenomenological distance (i.e., the space delineated on a mental number/magnitude line). Additionally, it provides evidence against the previously mentioned alternative hypothesis. This hypothesis predicts that the interval between greater magnitude stimuli will be perceived as “longer.” This account therefore predicts that decreasing magnitude sequences (e.g., 9-8-1) should exhibit a larger proportion of “long-short” responses than similarly structured increasing magnitude sequences (e.g., 1-2-9). This did not occur. In Experiment 2, we replicated Experiment 1 using a smaller set of second occurring (X) digits, while manipulating stimulus order, such that each sequence either conveyed an ordered (1-2-9) or non-ordered (2-1-9) sequence of digits.

Table 2
Mean proportion of "long-short" responses in Experiment 1

Digit Magnitude Direction	Digit Magnitude Sequence	Interval Timing Difference (in ms)															
		150		**117**		**84**		**50**		**-50**		**-84**		**-117**		**-150**	
		M	***SE***	***M***	***SE***	***M***	***SE***	***M***	***SE***	***M***	***SE***	***M***	***SE***	***M***	***SE***	***M***	***SE***
Increasing	1-7	.78	.03	.66	.03	.60	.03	.63	.04	.38	.04	.35	.04	.24	.03	.25	.03
	2-6	.70	.04	.71	.03	.72	.04	.61	.04	.44	.03	.30	.03	.30	.04	.24	.03
	3-5	.79	.03	.74	.04	.66	.03	.62	.03	.44	.03	.37	.03	.31	.03	.27	.03
	5-3	.73	.04	.73	.03	.68	.03	.65	.04	.54	.03	.37	.03	.35	.03	.29	.04
	6-2	.80	.02	.78	.03	.70	.04	.67	.04	.52	.04	.44	.04	.40	.04	.36	.04
	7-1	.79	.03	.74	.03	.72	.03	.66	.03	.58	.04	.41	.03	.35	.03	.31	.03
Decreasing	1-7	.73	.05	.69	.04	.59	.05	.57	.04	.32	.04	.30	.04	.21	.04	.17	.03
	2-6	.76	.04	.69	.04	.59	.05	.60	.04	.46	.05	.32	.04	.25	.04	.24	.05
	3-5	.75	.04	.72	.04	.68	.04	.58	.05	.38	.05	.42	.04	.30	.05	.24	.04
	5-3	.80	.03	.74	.03	.72	.03	.62	.04	.46	.03	.36	.04	.33	.05	.30	.04
	6-2	.76	.03	.73	.03	.68	.03	.62	.05	.38	.04	.40	.04	.32	.05	.32	.05
	7-1	.81	.04	.76	.04	.70	.04	.65	.04	.46	.04	.36	.04	.37	.05	.31	.05

Experiment 2

The results of Experiment 1 demonstrated that discrepancies in the magnitudes of numerical digits exert biases on interval duration judgments. In an earlier auditory kappa effect study, it was found that the degree of pitch-distance between sequential tones biased interval duration judgments regardless of stimulus order (Crowder & Neath, 1995). In this study, Crowder and Neath (1995), used sound sequences that did not follow ordered ascending, or descending pitch trajectories (e.g., the target sound [X] frequency did not fall directly between the frequencies of the boundary tones [A and B]). If the effect of magnitude on time perception is analogous to the auditory kappa effect, then the effect should be robust to stimulus order manipulations. As in Experiment 1, our goal was to determine whether differences in numerical magnitude, would influence participants' judgments regarding the durations of each SOA, and whether or not this effect was determined in whole, or partially, by the presence of an ordered stimulus trajectory.

Method

Participants. Forty-two University of Manitoba undergraduate students enrolled in an Introduction to Psychology course at the University of Manitoba participated in Experiment 2. They received partial course credit for participating. Twenty-one were randomly assigned to the ordered digit sequence condition (nine females, 12 males, mean age = 20.52 years), and 21 to the non-ordered sequence condition (10 females, 11 males, mean age = 20.67 years). This was done to reduce the quantity of experimental trials to lower the likelihood of participant fatigue.

Materials. In Experiment 2, only the digits 1, 2, 8, and 9 were used. Despite this manipulation, the mode of their presentation was identical to that of Experiment 1.

Design and procedure. The procedure for Experiment 2 followed that of Experiment 1 with the exception that sequence order was manipulated as a between-participants variable. One group of participants completed a version of the experiment that contained non-ordered numerical sequences. These sequences were still marked by large and small magnitude boundary stimuli, but did not covey a consistent increase or decrease in number magnitude. Participants in this condition encountered the digits 1 or 9 as the second occurring digits in four possible AXB sequences: 2-1-8, 8-1-2, 2-9-8, and 8-9-2. This allowed us to maintain the same digit magnitude differences of 1-7 (for sequences 2-1-8 and 8-9-2) and 7-1 (for sequences 8-1-2 and 2-9-8), while allowing the sequence order to be disrupted (see Figure 3 for the increasing and decreasing number magnitude stimuli in the ordered condition, and see Figure 4 for the increasing and decreasing number magnitude stimuli in the non-ordered condition). Participants in both ordered and non-ordered digit sequence conditions responded to sequences that either progressively increased or decreased in magnitude, presented in separate blocks of trials. The presentation order of these blocks was counterbalanced across participants. The four AXB sequences were presented with each of the eight interval timing conditions for a total of 32 trial types. Each trial was repeated 10 times, for a total of 320 trials (160 trials per block).

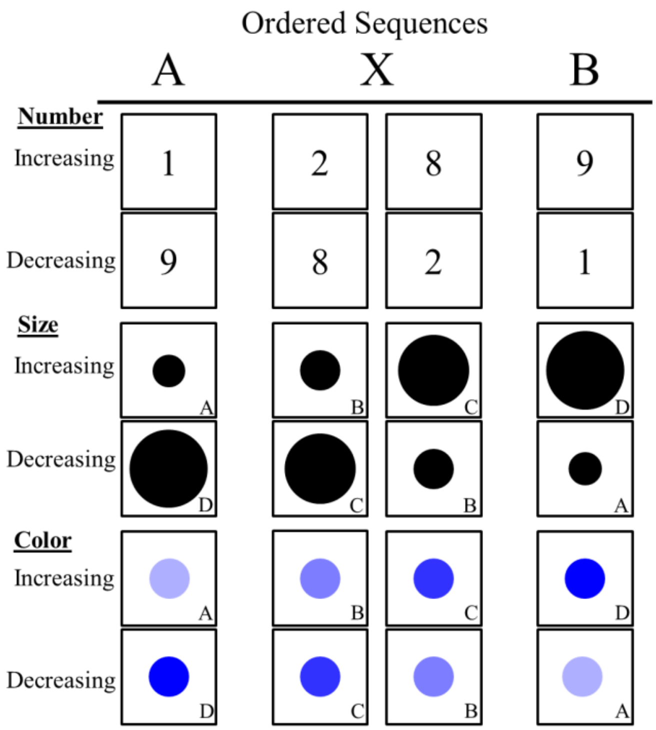

Figure 3. The stimuli used in Experiments 2, 3 and 4 (which include numbers [Experiment 2], discs of varying size [Experiment 3]. And discs of varying colour saturation [Experiment 4]) in increasing and decreasing directions for ordered sequences.

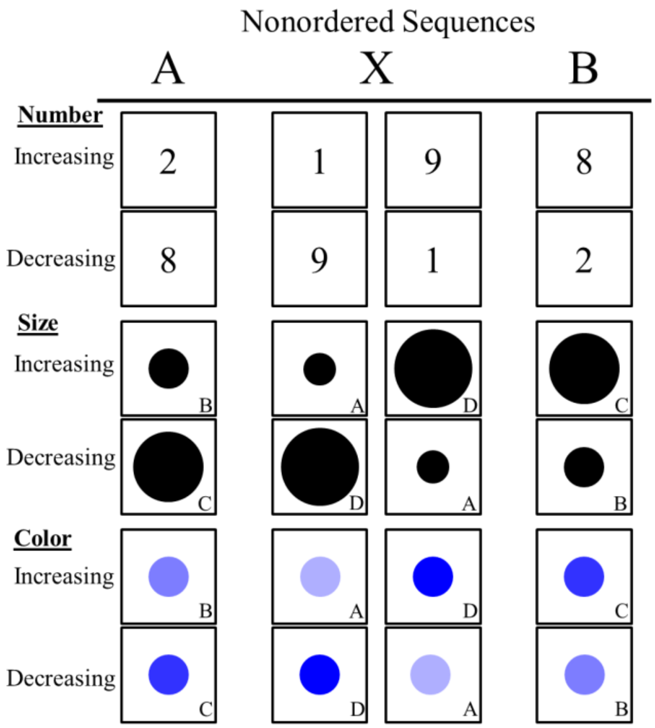

Figure 4. The stimuli used in Experiments 2, 3 and 4 (which include numbers [Experiment 2], discs of varying size [Experiment 3], and discs of varying colour saturation [Experiment 4]) in increasing and decreasing directions for non-ordered sequences.

Results and Discussion

In Figure 5, the effect of Interval Timing on participants' long-short judgments is displayed for both ordered and non-ordered digit sequence conditions. Additionally, the effect of Digit Magnitude Difference on the proportion of long-short judgments is displayed in Figure 6. We submitted these data to a 2 (Digit Sequence Order: Ordered/Non-ordered) × 2 (Digit Magnitude Difference: 1-7/7-1) × 2 (Digit Magnitude Direction: Increasing/Decreasing) × 8 (Interval Timing: ± 150, 117, 84, and 50 ms) mixed-design ANOVA, treating Digit Magnitude Difference, Digit Magnitude Direction, and Interval Timing as within-participant factors and Digit Sequence Order as a between-participants factor.

This analysis revealed a significant main effect of Interval Timing, $F(2.02, 80.66) = 100.88$, $p < .001$, $\eta p^2 = .72$. The proportion of long-short responses decreased systematically as the pattern shifted in timing from the most salient L-S pattern (150 ms; *M* = .69 [.03]), to the most salient S-L pattern (150 ms; *M* = .27 [.02]), replicating Experiment 1.

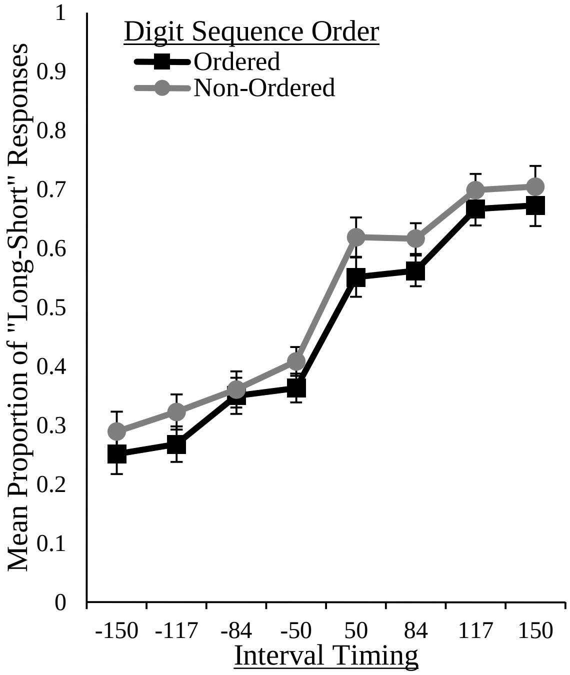

Figure 5. The mean proportion of ‘long-short’ responses for the ordered and non-ordered sequence conditions as a function of the eight timing interval conditions in Experiment 2. Error bars represent the standard error of the mean for each condition.

Critically, there was the significant main effect of Digit Magnitude Difference, $F(1, 40) = 14.92$, $p < .001$, $\eta p^2 = .27$. Replicating Experiment 1, the proportion of long-short responses was significantly smaller on 1-7 trials (*M* = .44 [.02]) than on 7-1 trials (*M* = .52 [.02]) further revealing that the degree of numerical magnitude difference across SOA: 1 and SOA: 2 acted a source of bias on the participant’s interval timing judgments (see Figure 6).

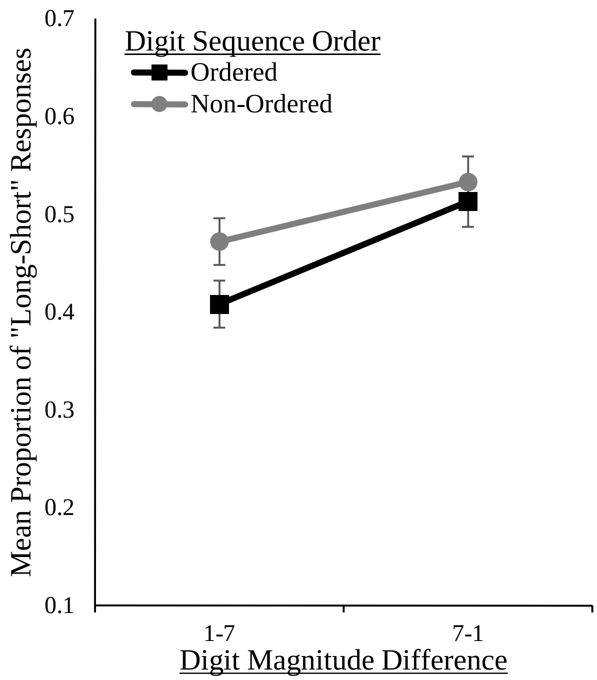

Figure 6. The mean proportion of ‘long-short’ responses for the ordered and non-ordered sequence conditions as a function of the two magnitude difference levels in Experiment 2. Error bars represent the standard error of the mean for each condition.

As was the case in Experiment 1, there was no main effect of Digit Magnitude Direction (p = .98). Additionally, the between-participants main effect of Digit Sequence Order (Ordered/Non-ordered) was non-significant (p = .14). Additionally, as was the case in Experiment 1, none of the interactions achieved statistical significance (remaining $p \geq .07$). To summarize, Experiment 2 replicated the findings of Experiment 1; interval duration judgments were directly biased by the numerical magnitude difference of the bounding digits, with a greater difference translating into a longer subjective interval. The goal of Experiments 3 and 4 was to determine whether variations in the magnitudes of other dimensions, including size (Experiment 3), and colour saturation (Experiment 4) would induce similar biases on interval duration judgments.

Experiment 3

In Experiments 1 and 2, it was discovered that differences in numerical magnitude contributed to participants' perception of interval length. The goal of Experiment 3 was to determine whether differences in the physical size of sequentially-presented stimuli would induce interval duration judgment biases similar to those witnessed for numbers (Experiments 1 and 2). Specifically, we replaced the digits used in our previous experiments with black discs that varied in overall diameter. It was hypothesized that interval timing would again be biased by the level of magnitude discrepancy of the stimuli bounding that interval regardless of direction, or order.

Method

Participants. Forty-four University of Manitoba undergraduate students enrolled in an Introduction to Psychology course participated in Experiment 3. They received partial course credit for participating. Twenty-three students were randomly assigned to the ordered disc sequence condition (15 females, eight males, mean age = 19.52 years), and 21 to the non-ordered condition (15 females, six males, mean age = 20.14 years).

Materials. All materials and equipment used in Experiment 3 were identical to those used in Experiments 1 and 2, except that the digits were replaced by four black discs. These discs were varied in overall diameter. In order from smallest to largest, the discs subtended 5.32° × 3.56° (25 mm × 15 mm; Disc A), 7.97° × 5.34° (45 mm × 30 mm; Disc B), 17.44° × 13.43° (140 mm × 95 mm; Disc C), and 21.06° × 8.36° (160 mm × 100 mm; Disc D) of visual angle.

Procedure. The procedure of Experiment 3 was identical to Experiment 2. In the ordered sequence condition, intermediate sized discs (B and C) were always the second occurring stimulus, while the largest (D) and smallest discs (A) acted as the boundary stimuli. This resulted in four AXB sequences that were analogous to the sequences used in the ordered sequence condition of Experiment 2. Specifically, sequences A-C-D and D-B-A exhibited a large (115 mm - 80 mm) difference in the diameters of the first two discs (A-C or D-B) and a small (20 mm - 15 mm) difference in the diameters of the second two discs (C-D or B-A), and are referred to throughout as a Large-Small disc size difference patterns. By contrast, sequences A-B-D and D-C-A constituted trial types in which the diameter difference between the first two discs (A-B or D-C) was small (20 mm - 15 mm) and the diameter difference between the second two discs (B-D or C-A) was large (115 mm - 80 mm) and are referred to as a Small-Large disc size difference patterns. Increasing and decreasing sequences were presented in separate blocks, the order of which was counterbalanced across participants.

In the non-ordered sequence condition, the middle occurring disc in each sequence was either the smallest or largest diameter disc (Discs A and D, respectively) with the intermediate-sized discs acting as boundary stimuli. As in Experiment 2, this manipulation allowed us to further examine whether these biases were robust to manipulations to the pattern's overall trajectory (e.g., whether it's ordered vs. non-ordered; See Figure 3 for the increasing and decreasing size stimuli in the ordered condition, and Figure 4 for the increasing and decreasing size stimuli in the non-ordered condition). In this case, the four AXB sequences included, two Large-Small difference sequences (B-D-C and C-A-B), and two Small-Large difference sequences (B-A-C and C-D-B).

Results and Discussion

Figure 7 displays the effect of Interval Timing on participants' long-short judgments for both ordered and non-ordered disc sequence conditions. Additionally, in Figure 8 we display the effect of Disc Size Difference on the proportion of long-short judgments. We submitted the proportion of long-short responses for each participant within each condition to a 2 (Disc

Sequence Order: Ordered/Non-ordered) × 2 (Disc Size Difference: Large-Small/ Small-Large) × 2 (Size Direction: Increasing/Decreasing) × 8 (Interval Timing: ± 150, 117, 84, and 50) mixed-design ANOVA, treating Disc Size Difference, Size Direction and Interval Timing as within-participant factors, and Disc Sequence Order as a between-participants factor. This analysis revealed a significant main effect of Interval Timing, $F(2.92, 122.48) = 74.86$, $p < .001$, $\eta p^2 = .64$. As in previous experiments, the proportion of long-short responses decreased systematically as the pattern shifted in its timing from the most salient L-S pattern (150 ms; M = .66 [.02]), to the most salient S-L pattern (150 ms; M = .34 [.02]).

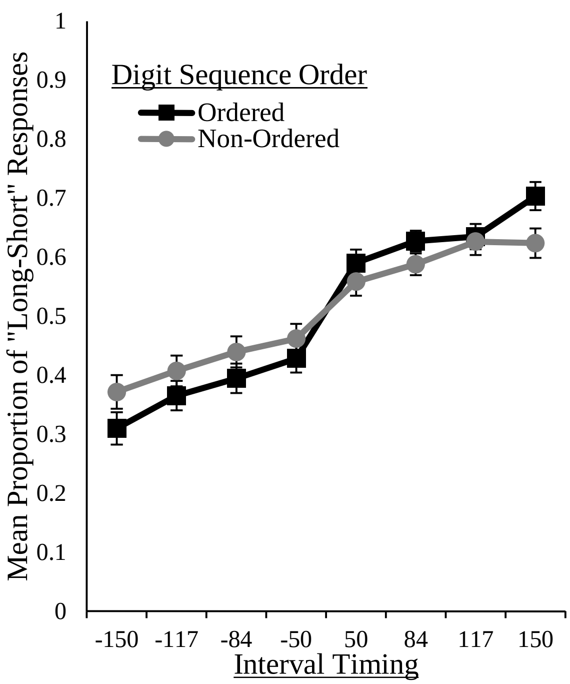

Figure 7. The mean proportion of ‘long-short’ responses for the ordered and non-ordered sequence conditions as a function of the eight timing interval conditions in Experiment 3. Error bars represent the standard error of the mean for each condition.

Critically, there was a significant main effect of Disc Size Difference, $F(1, 42) = 63.50$, $p < .001$, $\eta p^2 = .60$. As defined by the relative difference in the diameters between the discs that defined SOA: 1 and SOA: 2, the proportion of long-short responses was lower on Small-Large difference trials (M = .42 [.02]) than on Large-Small difference trials (M = .60 [.02]). This finding confirmed the hypothesis that a relative difference in the size of the stimuli used to form the boundaries of SOA: 1 and SOA: 2 exerted a similar bias for participants' interval duration judgments as numerical magnitude (see Figure 8). An interval defined by sequentially presented discs that were relatively close in their overall diameters was more prone to being labeled as "short," while the same interval defined by discs relatively discrepant in their overall diameters was more prone to being labeled as "long." Additionally, there was no main effect of Size Direction (p = .11), nor was there a between-participants main effect of Disc Sequence Order (p = .89).

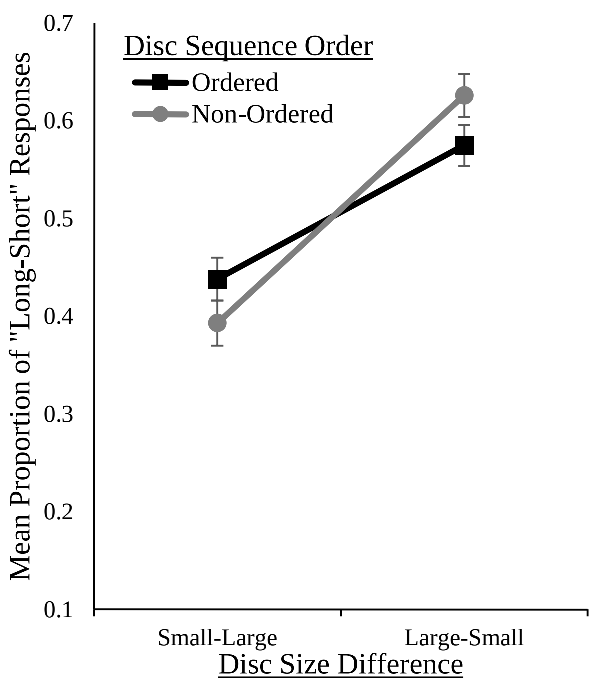

Figure 8. The mean proportion of 'long-short' responses for the ordered and non-ordered sequence conditions as a function of the two size difference levels in Experiment 3. Error bars represent the standard error of the mean for each condition.

There were several significant two-way interactions. First, there was a narrowly significant Disc Sequence Order × Disc Size Difference interaction, $F(1, 42) = 4.23$, $p = .05$, $\eta p^2 = .09$; to deconstruct this interaction, a simple effects analysis comparing Ordered to Non-ordered groups at each level of Disc Size Difference (Small-Large [S-L] and Large-Small

[L-S]) was performed. While none of these pairwise comparisons achieved statistical significance, there was a tendency for Disc Size Difference to bias responses to a greater degree when the stimuli were presented in a non-ordered configuration. For example, participants in the non-ordered group were more inclined to categorize an S-L pattern as "short-long" (resulting in a smaller proportion of long-short responses; *M* = .39 [.02]) than the ordered group (*M* = .44 [.02]; *p* = .16). Likewise, participants in the non-ordered group were also more likely to label an L-S pattern as "long-short" (*M* = .63 [.02]) than the ordered group (*M* = .58 [.02]; *p* = .12). While non-significant, the result suggests that disc size difference may be more likely to bias interval timing judgments when the stimuli are presented following a non-ordered trajectory. This reduction in temporal sensitivity may result from an inability to predict the upcoming location of the stimulus as it is perceived to move within three-dimensional space (e.g., looming vs. receding); or responses might be driven by changes in the perceived momentum of the stimuli. This is further supported by a significant Disc Sequence Order × Interval Timing interaction, $F(2.92, 122.48) = 3.02$, $p = .01$, $\eta p^2 = .07$.

To deconstruct this interaction, the simple effects were analyzed comparing Ordered versus Non-ordered groups at each of the eight Interval Timing levels. None of these Pairwise comparisons achieved significance ($p = .126$), except when there was a 150 ms interval difference, $F(1, 43) = 5.28$, $p = .05$. These patterns were more likely to be classified as L-S when they followed an ordered trajectory (*M* = .70 [.02]) than a non-ordered trajectory (*M* = .62 [.03]). While further studies would need to be done to elucidate this finding, it suggests that temporal interval sensitivity may be reduced when attending to approaching/receding stimuli that have unpredictable trajectories.

Last, there was a significant Disc Size Difference × Size Direction interaction, $F(1, 42) = 12.23$, $p < .001$, $\eta p^2 = .23$. The simple effects comparing Size Direction (increasing vs. decreasing) at each level of Disc Size Difference revealed that S-L patterns were more likely to be classified as "short-long" when the pattern decreased in size (*M* = .37 [.02]) than when it increased in size (*M* = .46 [.02]), $F(1, 43) = 11.03$, $p = .01$. Similarly, L-S patterns were more likely to be classified as "long-short" when the sequence decreased in size (*M* = .62 [.02]) versus when it increased (*M* = .58 [.02]), $F(1, 43) = 4.16$, $p = .05$. The impact of directionality on interval duration judgments suggests that a decreasing size trajectory (i.e., receding stimuli) is more prone to demonstrating a magnitude difference bias than increasing size trajectory (i.e., approaching stimuli). It is not entirely clear why this result was obtained, and further experimentation is required to determine if it is reliable, however other studies have shown similar asymmetries for looming and receding sounds, where it has been suggested that there is an adaptive bias for the heightened processing of approaching environmental events (Neuhoff, 2001). Therefore, discrepancies in the measured biases for increasing/decreasing disc sizes may be attributed to similar phenomenon. Furthermore, there are perceptible limits in terms of how small a disc could get, but no theoretical upper limit. None of the remaining interactions were significant (remaining $p \geq .09$).

Experiment 4

In Experiment 4, colour saturation levels were manipulated across three sequentially-presented discs. Colour saturation was selected as the primary variable because, like changes in size, it constitutes a different magnitude dimension from that of number; however, unlike size, colour saturation conveys no inherent spatial cues regarding visual movement in depth. In Experiment 3, the manipulation of disc diameter may have been interpreted by the participants as conveying variations along a three-dimensional spatial trajectory (e.g., approaching vs. receding object). This potential confound was addressed by selecting colour saturation as the primary variable in Experiment 4.

Method

Participants. Thirty-nine University of Manitoba undergraduate students enrolled in an Introduction to Psychology course completed Experiment 4. They received partial course credit for participating. Twenty-two were randomly assigned to the ordered (15 females, seven males, mean age = 19.52 years), and 17 to the non-ordered (15 females, two males, mean age = 20.14 years) sequence conditions. While participants were not screened for colour vision deficiencies, the stimuli were colour constant with only saturation level being modulated.

Materials. The stimuli in the ordered and non-ordered sequence conditions consisted of four blue discs that were centrally presented against a white background. The RBG levels were manipulated in Microsoft PowerPoint 2010 to create different levels of colour saturation. The corresponding CIE Lab values for each coloured disc can be found in Table 3. The stimuli included: (a) Dark Blue (Disc A), (b) Medium Dark Blue (Disc B), (c) Medium Light Blue (Disc C), and (d) Light Blue (Disc D) coloured discs. The saturation difference (given as a percentage) between the Dark Blue and Medium Dark Blue discs, and Light Blue and Medium Light Blue discs, was 29%. The saturation difference between the Dark Blue and Medium Light Blue discs, and the Light Blue and Medium Dark Blue disc was therefore 71%. All the discs subtended a visual angle of 18.53° × 11.03°.

Design and procedure. The procedure was identical to Experiment 3. By using discs that varied in colour saturation level, we controlled the relative difference between successive discs in the same way that we controlled relative size difference in Experiment 3. Specifically, in the ordered sequence condition, the medium blue discs (B and C) were always the second occurring stimuli, while the lightest (A) and darkest (D) blue discs were presented as the boundary stimuli (i.e., A-C-D, D-B-A, A-B-D, D-C-A). In non-ordered sequences, the medium blue discs were presented as the boundary stimuli, whereas the lightest and darkest blue discs were used as the second occurring stimulus (i.e., B-D-C, C-A-B, B-A-C, C-D-B). See Figure 3 for a graphical comparison of increasing and decreasing colour saturation stimuli in the ordered condition and Figure 4 for the non-ordered condition.

Table 3
CIE LaB values for coloured discs

Colour	**L**	**a**	**B**
Light blue	74	18	40
Medium light blue	58	34	65
Medium dark blue	38	66	98
Dark blue	32	79	108

Results and Discussion

Figure 9 displays the main effect of Interval Timing on participants' long-short judgments for both the ordered and non-ordered disc sequence conditions. Additionally, Figure 10 displays the effect of Disc Saturation Difference on the proportion of long-short judgments. We submitted the proportion of long-short responses for each participant to a 2 (Disc Sequence Order: Ordered/Non-ordered) × 2 (Disc Saturation Difference: Large-Small/Small-Large) × 2 (Saturation Magnitude Direction: Increasing/Decreasing) × 8 (Interval Timing: ± 150, 117, 84, and 50 ms) mixed-design ANOVA, treating Disc Size Difference, Saturation Magnitude Direction, and Interval Timing as within-participant factors and Disc Sequence Order as a between-participants factor. This analysis revealed a significant main effect of Interval Timing, $F(2.97, 109.93) = 105.74$, $p < .001$, $\eta p^2 = .74$. This main effect was characterized by a systematic decrease in the proportion of long-short responses as the pattern was shifted in its timing from the most salient L-S pattern (150 ms; *M* = .74 [.02]), to the most salient S-L pattern (150 ms; *M* = .28 [.02]).

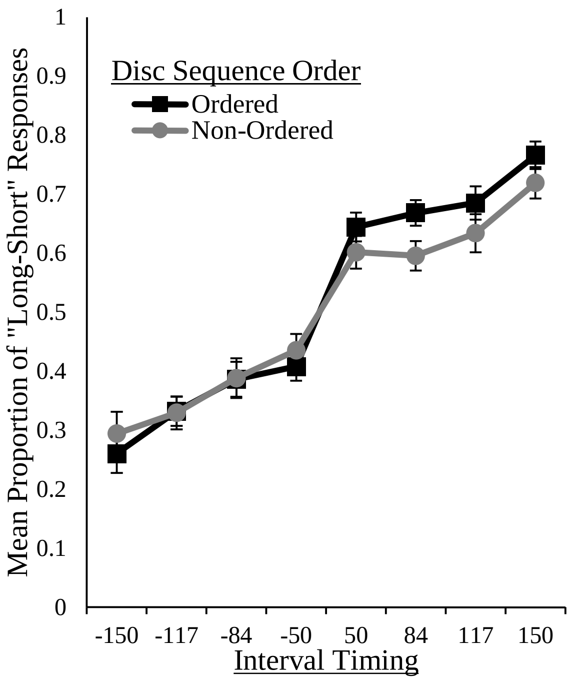

Figure 9. The mean proportion of "long-short" responses for the ordered and non-ordered sequence conditions as a function of the eight timing interval conditions in Experiment 4. Error bars represent the standard error of the mean for each condition.

Critically, there was a significant main effect of Disc Saturation Difference, $F(1, 37) = 17.54$, $p < .001$, $\eta p^2 = .32$. When the difference in the colour saturation of the discs bounding SOA: 1 were greater than the difference bounding SOA: 2, there was a higher proportion of long-short responses (*M* = .55 [.02]) than when the saturation difference of the discs bounding SOA: 1 was small (*M* = .47 [.02]), replicating Experiments 1, 2, and 3 (see Figure 10). Interestingly, while Colour Saturation Direction had no impact on the proportion of L-S responses ($p = .56$), and there was no between-participants main effect of Disc Sequence Order ($p = .43$), there was a significant Magnitude Difference (Disc Saturation Difference) × Disc Sequence Order interaction, $F(1, 37) = 5.27$, $p < .05$, $\eta p^2 = .13$.

To deconstruct this interaction, the simple effects were examined comparing Ordered versus Non-ordered sequences at each level of Disc Saturation Difference (29-71, 71-29). The analysis revealed a significant difference between how Ordered and Non-ordered groups responded to 29-71 (Small-Large saturation differences) patterns, $F(1, 38) = 4.19$, $p = .05$. In this circumstance, participants in the Non-Ordered condition were more inclined to classify the pattern as S-L ($M = .43$ [.12]) than participants in the Ordered condition ($M = .50$ [.02]). The same analysis conducted for 71-29 (Large-Small saturation differences) was non-significant ($p = .38$). To some degree, this result mirrors the Disc Size Difference × Disc Sequence Order interaction discovered in Experiment 3; participants were slightly more inclined to rely on magnitude difference when making a subjective interval timing judgment, when the pattern followed a non-ordered versus ordered trajectory. None of the remaining interactions were statistically significant (remaining $p \geq .18$).

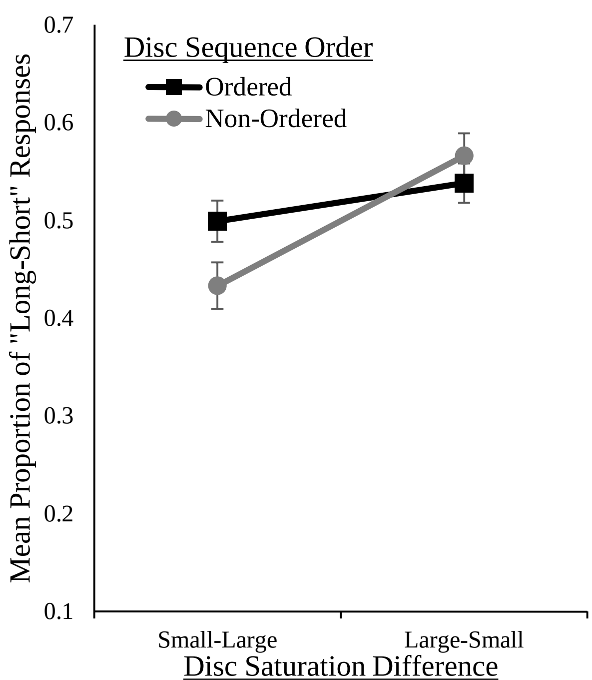

Figure 10. The mean proportion of “long-short” responses for the ordered and non-ordered sequence conditions as a function of the two saturation difference levels in Experiment 4. Error bars represent the standard errors of the mean for each condition.

General Discussion

The primary goal of the study discussed in Chapter II was to establish whether variations in the magnitude of numbers—as well as the size, and colour saturation of visual discs—between sequentially-presented stimuli – impacted judgments of interval duration. Previous work in this domain has revealed that numbers may be represented spatially, along a mental number line, and organized according to numerical magnitude. For example, studies on the SNARC effect provide compelling evidence in favor of peoples' tendency to mentally represent increasing digit quantity spatially, from left-to-right (Dehaene et al., 1993; Gevers et al., 2003). There is also a wealth of evidence that judgments of blank interval duration are dependent on the amount of physical distance used to separate sequentially presented stimuli, resulting in the kappa effect (Cohen et al., 1953). While it is known that physical distance can interact with and influence timing judgments, it has yet to be established how magnitude directly biases interval timing. The current experiments addressed this question. In Experiments 1 and 2, the degree of magnitude discrepancy between sequentially-presented numbers was found to contribute a bias to the perceived duration of the SOA. This took the form of increased "long" interval duration judgments for intervals marked by stimuli exhibiting a greater degree of magnitude difference. Moreover, this effect is analogous to previously demonstrated kappa effects in that it did not depend on the directionality of the sequence (i.e., whether it increased or decreased); and occurred regardless of whether the sequence followed an ordered or non-ordered stimulus trajectory.

Experiments 3 and 4 further established that this phenomenon occurs when other magnitude dimensions (size and colour saturation, respectively) are similarly manipulated. In total, these findings support the theory that an experientially determined, mental magnitude line not only impacts spatial processing (i.e., SNARC-effect) but also influences judgments of duration (i.e., kappa effect). Additionally, we found evidence against the hypothesis that a blank interval between greater magnitude events (e.g., 9-8) will be judged as "longer" in duration than one separating lesser magnitude events (e.g., 1-2). There is also some evidence to suggest that stimulus order can have an impact on interval timing in some magnitude dimensions (e.g., size and colour saturation), with non-ordered trajectories leading to enhanced response biases based on magnitude similarity.

The interaction witnessed in this study between stimulus magnitude and perceived interval duration is likely a component of a broader class of cognitive phenomena, the implications of which could be very useful for guiding our understanding about the way we organize information to take advantage of dimensional overlap to maximize efficiency. In the early stages of processing, stimuli that exhibit perceptual similarities, or are closer in spatial proximity, may become integrated, enhancing processing efficiency. For example, Lamy, Segal, and Ruderman (2006) found than an unattended background pattern (composed of discrete white squares) facilitated the detection of a target symbol "ɔ," "c" or "u") when the pattern and target symbols visually matched. Similarly, as proposed by Gestalt psychologists (Köhler, 1947; Wertheimer, 1961), elements presented in succession that exhibit close temporal proximity may also bind together to enhance pattern detection, a process that may have unintended consequences on temporal judgments. For example, the automatic binding of dimensionally-related information can reduce reaction times when attempting to parcel out one of those dimensions resulting in Garner interference (e.g., judging line length will be influenced by the line's width; Garner, 1976; Pomerantz & Garner, 1973).

Similar interference may occur when people are making judgments about stimulus magnitude. For example, when judging visual brightness, a concurrent, incongruent sound (e.g., bright object–low pitch) can impede reaction times (Marks, 1987). Similarly, people have difficulty disentangling information about the frequency of a sound (e.g., when making pitch judgments) from its vertical location (L. C. Leboe & Mondor, 2007). When participants are asked to make judgments about the different font sizes of digits, they also have difficulty discounting information about the digit's magnitude (Henik & Tzelgov, 1982) and are unable to discount information about the relative size of circles when making judgments about the number of circles in an array (Hurewitz, Gelman, & Schnitzer, 2006).

We propose that cross-dimensional interference effects may be reflective of a process in which the close associations formed between magnitude dimensions, as a function of experience, are used to facilitate processing. For example, under normal circumstances, the presence of "More" on any one dimension often tends to co-occur with "More" on an interrelated dimension. For example, a person approaching from some distance who is also speaking will simultaneously cast a progressively increasing retinal image concurrently with an increase in the relative pitch and volume of their voice. Therefore, if one dimension is absent, or inaccessible, an increase in any of the other dimensions can still be used to draw inferences about the target's location in space. Additionally, people may substitute information from a more precise stimulus dimension (e.g., variations in space) when judging a less precise domain (e.g., approximating the passage of time). As a result, kappa and SNARC effects could both be reflective of a phenomenon, in which information from one dimension is cognitively imported to fill in the gaps of a missing or imprecise dimension.

Theoretical accounts that treat stimulus properties across multiple dimensions as relying on the same underlying neural structures already exist. Most relevant for the current purposes is Walsh's (2003) ATOM framework. By that account, time, distance and quantity are all represented according to their relative magnitudes within the same cortical metric. The role of the intraparietal sulcus in processing different quantitative dimensions has been well established in the brain-imaging literature. However, it is unresolved as to whether this system converts all incoming quantitative information — including information about the magnitude of numbers— into a common, underlying abstract magnitude code. Present brain-imaging research generally supports this interpretation, for example, there is overlapping intraparietal activation when participants make quantitative comparisons across a variety of magnitude dimensions, including Arabic digits, line length, degree of angle, size and luminance (Fias, Lammertyn, Reynvoet, Dupont, & Orban, 2003; Pinel et al., 2004). Additionally, an fMRI adaptation paradigm found that repeated presentations of both symbolic (digits) and non-symbolic (item arrays) quantities suppress activation in overlapping intraparietal regions (Piazza, Pinel, Le Bihan, & Dehaene, 2007). However, it would be presumptuous to assume that similar behavioral effects witnessed across different magnitude dimensions are indicative of a single abstract magnitude code. The inability to distinguish distinct neural populations encoding for specific magnitudes may largely be due to equipment insensitivities and experimental paradigms that lack the statistical power to be able to elucidate these differences (see, Cohen Kadosh & Walsh, 2009).

As a counterpoint, some evidence has demonstrated differences in how the brain represents numerical information presented in different notations and modalities (Barth, Kanwisher, & Spelke, 2003; Campbell & Epp, 2004). Therefore, we do not suggest that our results are indicative of a singular abstract magnitude code that uses the same underlying neural architecture to represent all magnitude dimensions, but rather a similar representational format, in which magnitude information across different modalities and dimensions is similarly organized and structured.

Conclusion

The current study demonstrates that variations in magnitude (numerical representation, size, and colour saturation) can bias judgments of interval duration similarly to variations in physical distance. We therefore conclude that both SNARC and kappa effects are rooted in the same cognitive phenomenon, in which dimensional interference extends across spatial, temporal and quantitative dimensions.

Perceptual Interactions between Dimensions

In Chapter II, I proposed that people will factor in information regarding absolute magnitude similarity for the events bordering a blank – uninformative – interval when judging the duration of that interval. A process analogous to the application of Gestalt grouping principles based on form similarity – a perceptual organizational skill developed by the age of three months (Quinn, Bhatt, Brush, Grimes, & Sharpnack, 2002). Firstly, and most importantly, it was noted that there were similar biases across all 4 experiments despite manipulating different stimulus dimensions in each, where a greater magnitude difference translated into a greater proportion of longer responses when judging interval duration. This tendency was present when the sequence of events progressively increased or decreased in magnitude, or followed ordered/non-ordered trajectories.

The findings further suggested that kappa effects may – at least partially – be the result of people employing a similarity heuristic when judging duration. Furthermore, if the aforementioned interactions between time and magnitude, in which larger numbers are perceived as lasting for longer durations than small numbers, is driven by an increase in pacemaker rate during the presentation of greater magnitude events, then there should have been an increased proportion of L-S responses on decreasing (9-X-1) trial types. This is predicted because the onset event in a decreasing sequence (9) should have increased the pacemaker's output, which then should have extended the blank interval immediately following. Instead, it was found that a 1-digit difference between successive numbers generated a greater proportion of S-L (short-long) responses regardless of the magnitude of those events. Furthermore, if kappa effects are entirely bottom-up in nature, the bias should have been limited to only non-symbolic – physical magnitude dimensions, we would not have anticipated analogous perceptual phenomena across both symbolic (number) and non-symbolic dimensions (size, colour saturation).

The results suggest the application of a similarity heuristic when judging the intervals of time between successive events by computing the degree of absolute magnitude difference between the events bounding the interval and weighting the duration judgment based on that difference.

In Chapter III, I examined whether the tendency to use number magnitude to index the duration of an informative (i.e., filled) interval is a temporal bias that is elicited through variations in the functioning of a pacemaker, or if it is reflective of a more general, flexibly applied, heuristic in which number magnitude is incorporated into, and used to anchor, judgments regarding other magnitude dimensions. In Chapter III, I tested the effect that number magnitude has on basic sound intensity judgments. If a standard heuristic is being applied, a sound should be categorized as *louder* more frequently when paired with a large number versus a small number.

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Alards-Tomalin, D., Walker, A. C., Shaw, D. M., & Leboe-McGowan, L.C. (2015). Is 9 louder than 1? Audiovisual cross-modal interactions between number magnitude and judged sound loudness. *Acta Psychologica*, *160*, 95-103. doi:10.1016/j.actpsy.2015.07.004

Chapter III: Number Magnitude and Sound Loudness

The environment continually poses demands on our basic cognitive and sensory subsystems, in which information must be organized, and integrated across modalities and dimensions to form coherent, representative percepts of the world in which we live. For example, our ability to process motion depends on the integration of information across multiple sensory streams (Soto-Faraco, Kingstone, & Spence, 2003); our sense of taste is impacted by colour (Spence et al., 2010); while speech comprehension is impacted by concurrent visual information (McGurk & MacDonald, 1976; Munhall, Gribble, Sacco, & Ward, 1996). While examples of cross-modal influences on perception are numerous, in all cases, transfer effects have demonstrated that information from one modality can be used to make efficient inferences about a related – but discrepant –stimulus dimension, presented in a different modality.

In Chapter III, I was interested in how people might adaptively use magnitude information from visually-presented symbolic numbers (i.e., Arabic digits) when judging the intensity of a sound. While sound intensity may appear largely unrelated to number, it should be considered that numbers – when presented as Arabic digits – are typically associated with changes in sound intensity in our environment. Whether it be on a volume knob attached to an amplifier, or the digital read-out when adjusting your computer's internal speakers – larger numbers are typically indicative of increased volume, and small numbers, decreased volume.

If it is the case that through experience we form a wide array of mental short-cuts to reduce cognitive load, and improve efficiency; the presence of numerical information under regular – unconstrained – situations, is likely to be useful when reporting on the intensity of a sound. Therefore, the presence of relatively large numbers should elicit biases toward reporting greater sound intensity.

Generalized Magnitude System

The potential interaction between symbolic numbers and sound intensity, may be the result of both dimensions sharing a common representational neural metric, within a generalized magnitude system (see A Theory of Magnitude or ATOM) (Bueti & Walsh, 2009; Gallistel, 2011; Holmes & Lourenco, 2011; Walsh, 2003). Some evidence in favor of this perspective implicates the intraparietal sulcus (IPS) as the locus for a generalized magnitude system, which is commonly activated when people process magnitude information in a variety of formats, including non-symbolic (numerosities) and symbolic (Arabic numerals, number words) numbers (Eger, Sterzer, Russ, Giraud, & Kleinschmidt, 2003; Piazza, Izard, Pinel, Bihan, & Dehaene, 2004; Piazza, Pinel, Le Bihan, & Dehaene, 2007). Based on these findings, it has been proposed that the IPS hosts a generalized, notation independent representation of number (Dehaene, 2008; but see, Lyons, Ansari, & Beilock, 2015). Additionally, this metric is thought to subserve the representations of a wide array of other continuous and discrete magnitude dimensions, which include (but are not limited to) the dimensions of time and space.

To understand how this abstract magnitude code works, we must first understand that magnitude is coded by neurons with monotonic rate-intensity output functions (i.e., they exhibit spiking rates that increase with stimulus intensity). The outputs of these neurons are thought to feed forward onto neurons that are tuned to respond to specific distal values (e.g., numerosity detectors, Dehaene, 2008; Nieder, Freedman, & Miller, 2002; Verguts & Fias, 2004). The tuning curves of the neurons that preferentially respond to a specific value flatten as the distal value increases, resulting in reduced perceptual sensitivity at higher intensity levels (Allman, Pelphrey,

& Meck, 2011; Cordes, Gelman, Gallistel, & Whalen, 2001; Dehaene, 2003, 2008). One aftereffect of this organization is that one's ability to detect a perceptible disparity between two stimuli (the *just noticeable difference)* worsens as the intensity levels of the compared stimuli are increased (see Weber's law, Dehaene, 2003). In support of ATOM, a wide array of perceptual dimensions have been found to conform to Weber's law including: duration (Gibbon et al., 1984; Meck & Church, 1983), non-verbal numbers (Cordes et al., 2001; Gallistel & Gelman, 1992, 2000; Whalen et al., 1999), symbolic number (Dehaene, Dehaene-Lambertz, & Cohen, 1998; Moyer & Landauer, 1973; Moyer & Landauer, 1967), and even sound loudness (Knudsen, 1923; Miller, 1947; Riesz, 1928), suggesting that they may all be organized using a common representational framework.

In further support of this perspective, a variety of cross-dimensional transfer effects have been found between symbolic numbers and other magnitude dimensions. For example, the magnitudes of task-irrelevant symbolic numbers can impact judgments about physical size or length (de Hevia, Girelli, Bricolo, & Vallar, 2008; Henik & Tzelgov, 1982; Viarouge & de Hevia, 2013), numerosity (Naparstek & Henik, 2010), duration (Alards-Tomalin, Leboe-McGowan, Shaw, & Leboe-McGowan, 2014; Kiesel & Vierck, 2009; Oliveri et al., 2008; Vicario et al., 2008; Xuan et al., 2009, 2007), and luminance (i.e., brightness) (Cohen Kadosh et al., 2008; Cohen Kadosh, Henik, & Walsh, 2007; but see Pinel et al., 2004). Furthermore, they may also interfere with basic spatial-motor actions, including the performance speed of left-vs. right-handed responses (Dehaene, Bossini, & Giraux, 1993; Nuerk, Wood, & Willmes, 2005), and precision motor responses (pinch vs. whole hand grasps), as well as grip aperture (Andres, Ostry, Nicol, & Paus, 2008; Lindemann, Abolafia, Girardi, & Bekkering, 2007). Therefore, proponents of the ATOM framework, might suggest that cross-dimensional biases elicited from numbers on sound intensity judgments would demonstrate evidence in favour of a cross-dimensional representational framework for magnitude.

Numerical Anchoring

Another interpretation that may be able to account for the potential transfer effect of numerical magnitude when judging sound amplitude comes from the heuristics and biases approach to cognition. Numerical anchoring is a basic cognitive phenomenon, wherein task irrelevant numbers are used as referents (or starting points) for making various decisions. Assimilative anchoring is said to have occurred when an estimate is pulled in the same direction of the irrelevant number's magnitude. For example, when participants are asked to estimate the number of African countries in the United Nations, the estimates provided tend to be greater if the anchor value was initially a larger number (e.g., *higher/lower than 100*) versus a smaller number (e.g., *higher/lower than 10*) (Tversky & Kahneman, 1974). Interestingly anchoring occurs even when it is obvious that the anchor is unrelated to the target task. For example, the magnitude of numerical anchors may be generated through obviously random events, like a wheel-of-fortune spin (Chapman & Johnson, 1999; Tversky & Kahneman, 1974), or may be entirely incidental, like the numbers of one's social insurance number impacting product valuations (Ariely, Loewenstein, & Prelec, 2003), or an athlete's jersey number impacting athletic performance judgments (Critcher & Gilovich, 2008). In either case, attending to large numbers tends to facilitates higher overall estimates. Furthermore, for anchoring effects to occur, all that is generally required is that the person pay sufficient attention to the anchor value. For example, participants that first made an unrelated magnitude judgment about a number (e.g., judging an ID number as *lower/higher than 1920*), prior to making an estimation judgment (e.g., estimate the number of physicians in the phonebook) were influenced by the numerical magnitude of the anchor despite not directly comparing their estimate against it (Wilson, Houston, Etling, & Brekke, 1996).

Researchers have further suggested that assimilative numerical anchoring is a phenomenon driven by people relying on the absolute value of any numerical information stored in short-term memory (Kahneman & Knetsch, 1993; Wilson et al., 1996; Wong & Kwong, 2000). Furthermore, the Anchoring as Activation approach posits that the mere presence of the anchor in short-term memory will facilitate the activation of features that are held in common with the anchor; a form of confirmation bias. As noted by Chapman and Johnson, (1999) "…anchors have their effect because decision makers consider reasons why their value for the target item is like the anchor, but show relative neglect for reasons why their value for the item is unlike the anchor" (p. 121). Therefore, the anchor will likely bias target estimates when people have reason to attend to the numerical anchor, while storing some aspect of that value in short-term memory. We therefore propose that when participants have sufficient reason to attend to, and process the magnitude of a number presented prior to the target sound (e.g., the number and sound occur simultaneously [Experiment 5], or the participant is required to hold the number in short term memory [Experiment 7]), then the number will function as an anchor and bias judgments in the same direction as the numerical value. However, when participants have no reason to attend to the digit (e.g., it occurs prior to the sound's presentation [Experiment 6]), people will actively discount or ignore it, thus reducing/eliminating any anchoring effects. Furthermore, the fact that the target task is perceptual in nature (judging sound intensity) is largely irrelevant, as anchors have been found to bias a wide variety of judgments that range from estimating weights, to general/factual knowledge estimates (e.g., estimate length of Mississippi river), probability estimates, legal judgments (e.g., length of a prison sentence), purchasing decisions, and self-efficacy assessments (for a recent review see, Furnham & Boo, 2011).

Current Study

Interestingly, while interference effects between number magnitude and other visual dimensions have been widely demonstrated, fewer studies have examined the presence of these kinds of interactions using a cross-modal experimental, paradigm, and to our knowledge, no studies have been published to date on whether sound intensity judgments are influenced by visually presented numbers. In one recent, noteworthy study, it was found that participants tended to spontaneously generate a higher proportion of large magnitude numbers when listening to high intensity versus low intensity sounds (Heinemann, Pfister, & Janczyk, 2013).

In the current study we examined the opposite interaction, whether or not visual numbers elicited biases on the perceived intensity of an otherwise unrelated sound. We predicted that visual magnitude information (in the form of symbolic numbers) would exert cross-modal biases on a basic sound intensity judgment task – despite being task irrelevant – in a manner consistent with assimilative anchoring. Furthermore, we have attempted to set the foundation for a new account of these phenomenon by contrasting the generalized magnitude framework against a theoretical account that emphasizes the adaptive use of numerical information as anchors on perceptual tasks. To this end, participants were asked to compare the intensity of a target sound against an earlier heard reference sound.

Experiment 5

In Experiment 5, the reference event consisted of a steady tone, while the target event consisted of a tone that was either 10% higher or 10% lower in intensity that was paired (occurred concurrently) with a symbolic number. The primary task was to categorize the target tone as either *louder* or *quieter* than an earlier reference tone. In this case, it was predicted that, due to the close temporal proximity of the number with the sound, it would be difficult for the participants to ignore the numerical value, allowing them to use it as an anchor when judging a sound's intensity-level.

Method

Participants. Twenty-nine University of Manitoba undergraduate students enrolled in an Introduction to Psychology course participated in the experiment in exchange for partial course credit, 20 of which were female (9 male). The mean age of this group was 19.21 (SD = 3.51). The participants self-reported normal, or corrected-to-normal hearing and vision. The study received prior approval by the University of Manitoba, Fort Garry Campus Research Ethics Board. All participants provided informed consent prior to participating.

Materials.

Sounds and images. Adobe Audition 3.0 (Adobe Systems Incorporated, 2007) software was used to synthesize the sounds used throughout the current study, which were generated with a sampling rate of 44,100 Hz (16 bit). The sounds used were based on a sine wave, and were 500 ms in duration, with a frequency of 250 Hz. The reference tones had three possible intensity levels (**REF1**: 81.96 dB, **REF2**: 82.96 dB, **REF3**: 83.96 dB), while the target tone was either 10% louder, or 10% quieter than the reference tone, resulting in 6 possible target tone intensities. All of the sounds had 10 ms amplitude ramps to eliminate onset/offset clicks. The numbers were all single digit Arabic digits that were either *less than 5* (1, 2, and 3) or *greater than 5* (7, 8, and 9). For each trial, these numbers were presented at the center of the screen, and subtended 3.3° × 2.6° degrees of visual angle.

Computer system. All of the sounds were presented through Maxell HP/NC-II noise-cancellation headphones with a sensitivity of 102 dB, and a frequency response range of 10 Hz – 28 kHz. The stimuli were presented using E-prime 2 software (Psychology Software Tools, 2012) run on 5 PCs. These PCs were installed with Intel Core 2 Duo E8400 @ 3.00 GHz CPUs, with 3.18 GB of RAM and Intel Q45/Q43 Express Chipset video cards. Connected to each PC was an LG W2442 PA Flatron LCD monitor with a screen resolution of 1920 x 1080 pixels, 32 Bit Colour Depth and a 60 Hz screen refresh rate.

Design and procedure. The participants were given instructions on the task, and then self-initiated the experiment by pressing the spacebar (see Figure 11A). On each trial, following the presentation of a 500 ms fixation cross, a reference sound was heard (without any accompanying visual stimuli). Following the offset of the reference sound, there was a 750 ms inter-stimulus interval (ISI), followed by a synchronized target sound/number pairing which lasted for 500 ms (total trial duration = 1,750 ms). After the offset of the target sound/number pairing, the participants were prompted to categorize the target sound's intensity as: *louder* or *quieter* than the reference sound by using the *L* and *Q* keys on the keyboard, respectively.

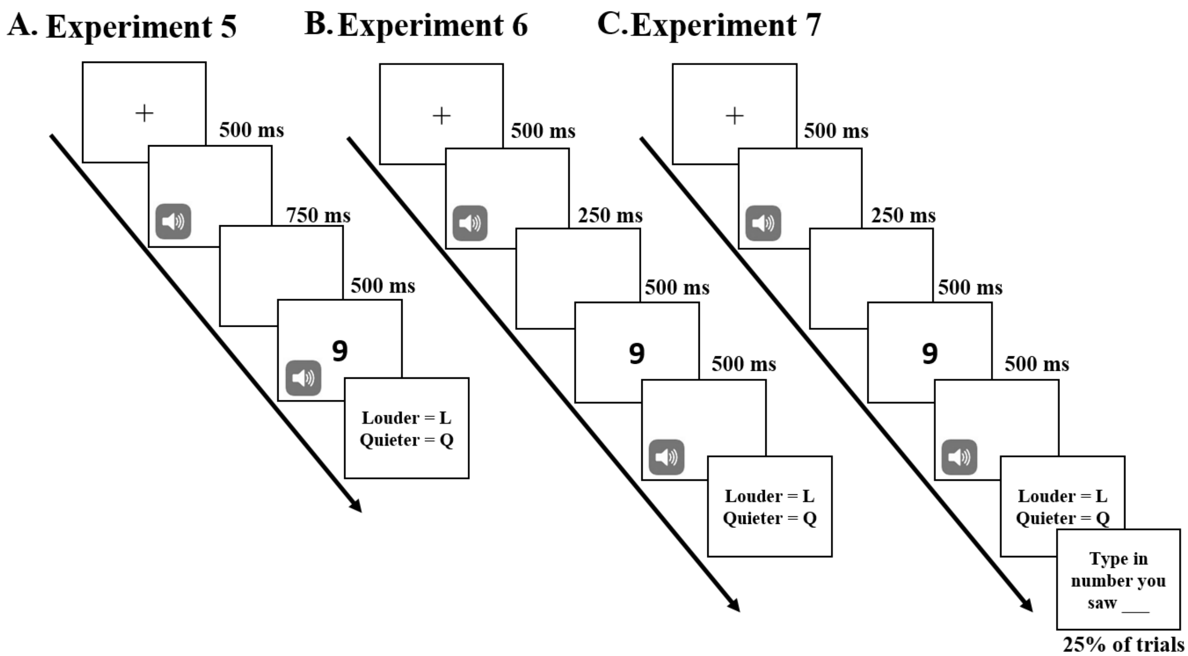


Figure 11. Experimental procedures for Experiments 5, 6 and 7.

The 6 target sounds were presented with small (1, 2, 3) or large (7, 8, 9) magnitude numbers. This resulted in 36 total trial types, which were repeated 5 times resulting in a total of 180 randomized trials per block. The experiment included two counterbalanced blocks of trials. In one block (Experimental), the target sounds were paired with numbers, while in the other block of trials (Control), the target sounds were paired with jumbled number images (see Figure 12). There were a total of 360 trials per session. SPSS Statistics package version 17 was used to analyze all of the reported data (SPSS Inc, 2008).

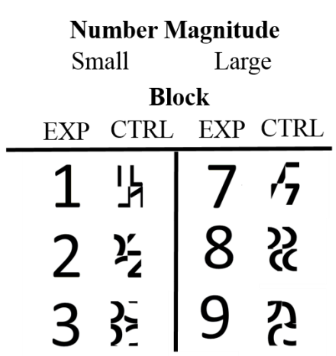


Figure 12. Experimental stimuli (EXP) and Control stimuli (CTRL).

Results and Discussion

Table 4 displays the average proportion of *loud* responses for each Reference Intensity (REF1, REF2, REF3) × Target Intensity (10% louder vs. 10% quieter) × Number Magnitude (Small [1, 2, 3] vs. Large [7, 8, 9]) × Block (Experimental, Control) factor. These values were submitted to a 3 × 2 × 2 × 2 within-participants, repeated-measures Analysis of Variance (ANOVA). Wherever means are provided in-text, standard errors are given in brackets. Greenhouse-Geisser estimates were used to correct the degrees of freedom when violations to the assumption of sphericity were observed throughout the study.

Table 4
Mean Proportion of "Louder" Responses in Experiment 5

			Target Intensity			
			Loud (10%)		Quiet (10%)	
			Number Magnitude			
Block	**Reference Intensity**		Large	Small	Large	Small
Exp.	REF1	*M*	.77	.68	.17	.15
		SE	.03	.03	.03	.03
	REF2	*M*	.89	.87	.34	.30
		SE	.02	.02	.03	.03
	REF3	*M*	.96	.90	.58	.55
		SE	.01	.02	.04	.03
Control	REF1	*M*	.74	.73	.16	.13
		SE	.03	.03	.03	.03
	REF2	*M*	.88	.90	.31	.28
		SE	.02	.02	.04	.04
	REF3	*M*	.93	.94	.57	.56
		SE	.02	.02	.04	.04

Note. *M* = mean, *SE* = standard error of the mean

First, there was a significant main effect of Number Magnitude $F(1, 28) = 11.30$, $p = .002$, $\eta p^2 = .29$. When target sounds were presented simultaneously with large numbers, those sounds were judged as *louder* more frequently (*M* = .61 [.02]) than target sounds presented with small magnitude numbers (*M* = .58 [.02]). Furthermore, there was a significant Number Magnitude × Block interaction, $F(1, 28) = 8.84$, $p < .01$, $\eta p^2 = .240$ (see Figure 13).

To deconstruct this interaction, Post-Hoc pairwise contrasts were run comparing the mean proportion of *louder* responses provided for Large versus Small magnitude digits separately for Experimental and Control blocks. In the Experimental block, the proportion of *loud* responses were significantly greater when the target sound was paired with a large number (*M* = .62 [.01]) versus a small number (*M* = .57 [.02]), $F(1, 28) = 28.76$, $p < .001$, $\eta p^2 = .51$. In the Control block, there was no significant difference in the proportion of *loud* responses provided to small versus large magnitude jumbled numbers ($p = .48$). Therefore, when the number and target sound were synchronized, sound intensity judgments were biased in the direction of the magnitude of the task irrelevant number.

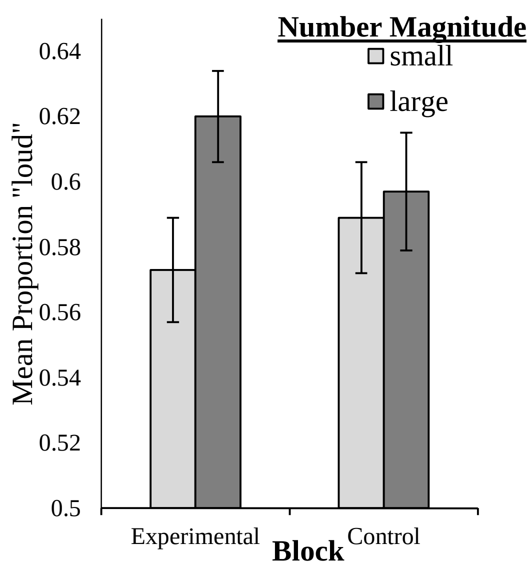


Figure 13. Experiment 5: Number Magnitude × Block interaction. Small (numbers 1, 2), large (numbers 8, 9). Error bar represent the standard error of the mean (SEM).

Second, the analysis revealed a significant main effect for Reference Intensity $F(1.27, 35.44) = 153.71$, $p < .001$, $\eta p^2 = .85$. This main effect was characterized by a proportional increase in *loud* responses as reference sound amplitude was increased: REF1 (M = .44 [.02]), REF2 (M = .60 [.02]), REF3 (M = .75 [.02]). There was also a main effect of Target Intensity $F(1, 28) = 297.09$, $p < .001$, $\eta p^2 = .91$, caused by participants producing a greater proportion of *loud* responses for 10% louder targets (M = .85 [.02]) versus 10% quieter targets (M = .34 [.03]) This indicates that the participants were successful at distinguishing between the selected sound intensity levels.

Third, there was a significant Reference Intensity × Target Intensity interaction $F(2, 56) = 55.98$, $p < .001$, $\eta p^2 = .67$ (see Figure 14A). To deconstruct this interaction, we converted the mean proportion of *louder* responses into proportional Target Intensity difference scores and then collapsed across all of the variables except for Reference Intensity (REF1, REF2, REF3), which were compared. The difference scores were calculated by subtracting the mean proportion of louder responses provided to 10% quieter targets (Q) from the mean proportion of louder responses provided to 10% louder targets (L). The resulting metric is reflective of sound intensity categorization sensitivity, with larger difference scores indicating a higher level of sensitivity when discriminating between quiet and loud targets. The Target Intensity difference scores for each of the three levels of Reference Intensity were compared using a one-way repeated measures ANOVA.

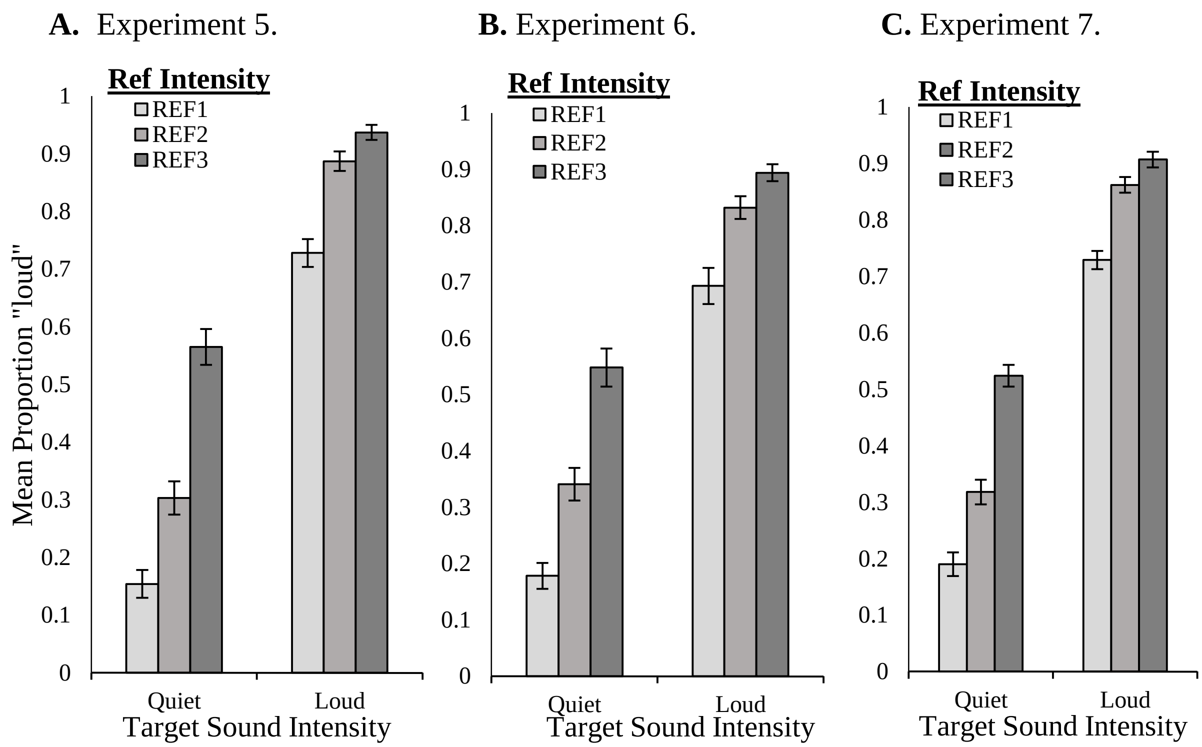


Figure 14. Reference Intensity × Target Intensity interactions in Experiments 5, 6 and 7. Error bar represent the standard error of the mean (SEM).

There were significant differences in the Target Intensity difference scores for each level of Reference Intensity, $F(2, 56) = 55.98$, $p < .001$, $\eta p^2 = .67$ (see Figure 15). Post-Hoc contrasts revealed that these differences were such that REF1 ($M = .57$ [.03]) and REF2 ($M = .58$ [.03]) were statistically identical ($p = .66$). There were however significant differences between REF1 and REF3 ($M = .37$ [.03]) $F(1, 28) = 89.09$, $p < .001$, $\eta p^2 = .76$, and between REF2 and REF3, $F(1, 28) = 73.94$, $p < .001$, $\eta p^2 = .73$. This indicates a significant drop in sound intensity categorization sensitivity when the reference intensity was increased from 82.96 dB (REF2) to 83.96 dB (REF3), a result predicted by Weber's law. The remaining main effects and interactions were non-significant (remaining $p \geq .08$).

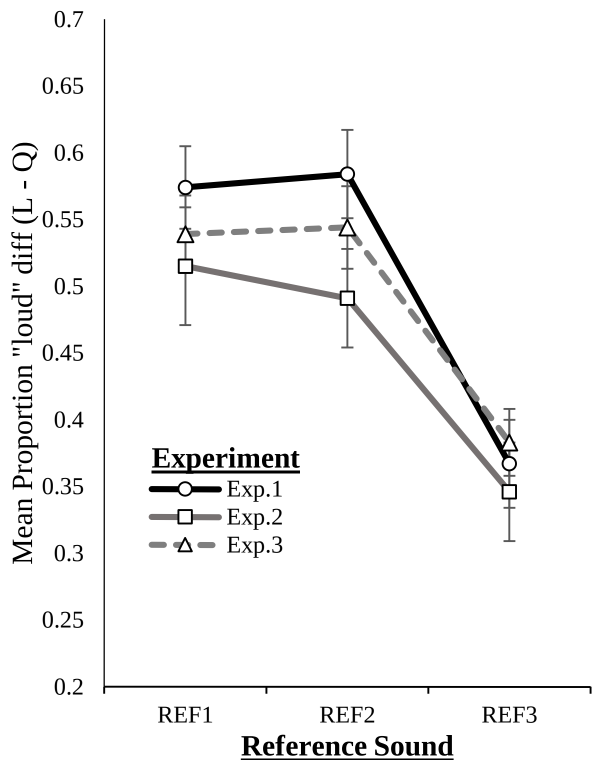


Figure 15. Intensity difference scores: the mean proportion of loud responses for 10% Quieter targets (Q) subtracted from the mean proportion of loud responses for 10% Louder targets (L); lower values indicate reduced discriminability.

While it is interesting that number magnitude can exert cross-modal biases on sound intensity, it is not entirely clear what the source of this bias is. While the ATOM perspective may propose that it is reflective of a common underlying magnitude code that is representative of both numerical magnitude and sound intensity, it is also possible that people may be using the irrelevant number as an anchor when making perceptual judgments about sound intensity.

In Experiment 6, we attempted to disentangle these accounts by modifying the experimental procedure, presenting the number immediately prior to the target sound rather than having them occur simultaneously. In this case, by having the number and sound occur independently, the participants could selectively ignore the numerical information and attend only to the target sound.

Experiment 6

In Experiment 5, we used a procedure similar to a basic priming paradigm. In doing so, a task-irrelevant symbolic digit was presented immediately prior to the target sound, acting as a prime. Previous work using similar priming procedures have found that responses to symbolic numbers (judging magnitude, or parity) tend to be facilitated when primed by congruent magnitude information presented in another notation, like a number word (TWO → 2) (Naccache & Dehaene, 2001a; Naccache & Dehaene, 2001b), or even by an animal name that is conceptually congruent with the number's magnitude (LION → 9, Gabay, Leibovich, Henik, & Gronau, 2013). These findings have all been used to support the theory of a common abstract code for representing various magnitude dimensions. If sound intensity and number magnitude are relying on a common magnitude code, sound intensity responses should be primed in the same direction as the magnitude of the earlier viewed number. Alternatively, if information held in short-term memory is being used as an anchor, separating the events should enable the participants to discount the irrelevant numerical information; allowing the sound intensity judgment to be performed unbiased.

Method

Participants. Twenty-six University of Manitoba undergraduate students enrolled in an Introduction to Psychology course participated in the experiment in exchange for partial course credit, 24 were female, and 2 were male. The mean age of this group was 18.73 (SD = 1.34).

Materials. The same PC and software used in the previously reported experiment were used throughout.

Design and procedure. Experiment 6 used the same procedure as Experiment 5 with one change, the synchronization of the irrelevant number and target sound events. In Experiment 6, the events were offset so that the number directly preceded the onset of the sound on every trial, and thus acted as a prime, instead of as a co-occurring event (see Figure 11B).

Results and Discussion

Table 5 displays the average proportion of *louder* responses for each Reference Intensity (REF1, REF2, REF3) × Target Intensity (10% louder vs. 10% quieter) × Number Magnitude (Small [1, 2, 3] vs. Large [7, 8, 9]) × Block (Experimental, Control) factor. These values were subsequently submitted to a 3 × 2 × 2 × 2 within-participants, repeated-measures ANOVA.

Table 5
Mean Proportion of "Louder" Responses in Experiment 6

			Target Intensity			
			Loud (10%)		Quiet (10%)	
			Number Magnitude			
Block	**Reference Intensity**		Large	Small	Large	Small
Exp.	REF1	*M*	.70	.69	.19	.18
		SE	.04	.04	.03	.03
	REF2	*M*	.86	.81	.33	.36
		SE	.03	.03	.04	.04
	REF3	*M*	.90	.89	.56	.56
		SE	.02	.02	.04	.04
Control	REF1	*M*	.69	.69	.15	.19
		SE	.04	.03	.03	.03
	REF2	*M*	.82	.84	.33	.35
		SE	.02	.03	.04	.03
	REF3	M	.92	.87	.52	.55
		SE	.02	.03	.04	.04

Note. *M* = mean, *SE* = standard error of the mean

Firstly, the presence of a task-irrelevant number presented prior to the target sound, had no impact on target sound intensity judgments (p = .98). Secondly, as in Experiment 5, there was a significant main effect for Reference Intensity where the proportion of *louder* responses increased progressively with reference sound intensity, *F*(2, 50) = 177.29, $p < .001$, $\eta p^2 = .88$. To illustrate, target sounds following REF1 intensity reference sounds exhibited the smallest overall proportion of loud judgments (*M* = .44 [.02]), followed by REF2 (*M* = .59 [.02]), and REF3 (*M* = .72 [.02]). Similar to Experiment 5 the main effect of Target Intensity was also significant *F*(1, 25) = 166.61, $p < .001$, $\eta p^2 = .87$. Participants categorized 10% louder targets as *louder* more frequently (*M* = .81 [.02]) than 10% quieter targets (*M* = .36 [.03]).

Thirdly, there was a Reference Intensity × Target Intensity interaction similar to that found in Experiment 5, *F*(1.32, 32.88) = 17.03, $p < .001$, $\eta p^2 = .41$ (see Figure 14B). To deconstruct this interaction, difference scores were formed by subtracting the proportion of *loud* responses provided to quieter targets from the proportion of *loud* responses provided to louder targets for each participant. These difference scores were then compared across the three levels of Reference Intensity (REF1, REF2, REF3) using a one-way repeated measures ANOVA. The analysis revealed a significant main effect for Reference Intensity *F*(2, 50) = 17.03, $p < .001$, $\eta p^2 = .41$ (see Figure 5). Post-Hoc pairwise contrasts further revealed no difference between REF1 (*M* = .52 [.04]) and REF2 (*M* = .49 [.04]) ($p = .40$). There were, however, significant differences between REF1 and REF3 (*M* = .35 [.04]), *F*(1, 25) = 17.08, $p < .001$, $\eta p^2 = .41$ and REF2 and REF3 *F*(1, 25) = 42.41, $p < .001$, $\eta p^2 = .63$. The results further confirmed the findings of Experiment 5, that there was a significant reduction in sensitivity when categorizing target sound intensity at the higher reference sound intensity levels. The remaining effects were non-significant (remaining $p \geq .32$).

To summarize, in contrast with the predictions of the generalized magnitude account the results of Experiment 6 did not indicate any form of numerical priming on sound intensity judgments. This leads us to theorize that when the auditory and visual events occurred simultaneously, it was difficult for the participants to discount the numerical information while actively attending to the sound, despite its being irrelevant to the task. This facilitated the number being used as an anchor when judging sound intensity. When the events were temporally displaced, because the number was irrelevant and did not need to be attended to, it became easier to parcel out, or ignore; thus keeping it from entering short-term memory and biasing the later sound intensity judgment. In Experiment 7, to test this short-term memory hypothesis, the same procedure as Experiment 6 was used, however, despite the number still being task irrelevant, participants were required to hold the number in short-term memory.

Experiment 7

In Experiment 7, the procedure was modified so that participants were required to hold whatever number they saw prior to the presentation of the target sound in short-term memory. This ensured that the numerical information was being processed rather than ignored or actively suppressed. As has been previously shown, simply attending to a number is sufficient to bias later target judgments, even when the two sources of information are completely unrelated (Wilson et al., 1996). If the bias witnessed in Experiment 5 is a form of numerical anchoring on a perceptual judgment regarding sound intensity, it is expected that the number should go on to exert assimilative anchoring biases when held in short-term memory, despite not occurring simultaneously with the target sound.

Method

Participants. Thirty-four University of Manitoba undergraduate students enrolled in an Introduction to Psychology course participated in Experiment 7 in exchange for partial course credit. Twenty-three of the participants were female and 11 were male. The mean age of this group was 19.29 (SD = 2.34).

Materials. The same PC, software and stimuli as Experiments 5 and 6 were used.

Design and procedure. The procedure used in Experiment 7, was identical to Experiment 6 with one key difference, on 25% of the Experimental block trials (which were selected randomly) after making the sound intensity judgment, the participants were prompted to reproduce the digit they had seen in the interval between the offset of the reference sound and the onset of the target sound (see Figure 11C). As it was unpredictable which trials would have this added secondary task[1], therefore, the participants had to hold the target number in short-term memory during the presentation of the target sound on all of the trials.

Results and Discussion

Table 6 displays the average proportion of *louder* responses for each Reference Intensity (REF1, REF2, REF3) × Target Intensity (10% louder vs. 10% quieter) × Number Magnitude (Small [1, 2, 3] vs. Large [7, 8, 9]) × Block (Experimental, Control) factor. These values were submitted to a 3 × 2 × 2 × 2 within-participants, repeated-measures ANOVA.

[1] Number reproduction task accuracy for 34 participants was $M = 82.35$ (SD = 16.9).

Table 6
Mean Proportion of "Louder" Responses in Experiment 7

			Target Intensity			
			Loud (10%)		Quiet (10%)	
	Reference		**Number Magnitude**			
Block	**Intensity**		Large	Small	Large	Small
Exp.	REF1	*M*	.78	.70	.20	.15
		SE	.02	.03	.04	.03
	REF2	*M*	.92	.80	.34	.23
		SE	.02	.04	.04	.03
	REF3	*M*	.94	.87	.51	.44
		SE	.01	.04	.04	.03
Control	REF1	*M*	.72	.72	.22	.20
		SE	.03	.03	.03	.03
	REF2	*M*	.85	.87	.36	.35
		SE	.02	.02	.04	.03
	REF3	*M*	.91	.90	.56	.58
		SE	.02	.02	.03	.04

Note. *M* = mean, *SE* = standard error of the mean

As in Experiment 5, there was a main effect of Number Magnitude $F(1, 33) = 7.36$, $p = .01$, $\eta p^2 = .182$. This effect was characterized by a tendency for participants to categorize target sounds that followed the presentation of a large magnitude numbers as *louder* more frequently ($M = .61$ [.01]), than targets sounds following the presentation of small numbers ($M = .57$ [.01]). Also similar to Experiment 5, there was a significant Number Magnitude × Block interaction $F(1, 33) = 6.16$, $p = .018$, $\eta p^2 = .157$ (see Figure 16). The interaction indicated a significant effect of Number Magnitude for Experimental block trials, $F(1, 33) = 7.70$, $p = .009$, $\eta p^2 = .19$, that did not extend to the Control condition which featured jumbled digits ($p = .85$). In the Experimental Block, participants judged target sounds that followed the presentation of large magnitude numbers as *louder* more frequently ($M = .62$ [.02]) than sounds following small magnitude numbers (*M* = .53 [.02]). Thus, the effect witnessed was similar to other anchoring demonstrations, in that it was dependent on the participant attending to the number, and storing that number in short-term memory. The number in this case actively biased responses despite: 1. having no connection to the sound intensity task, and 2. not occurring simultaneously with the target sound. These findings support an anchoring interpretation of the above results.

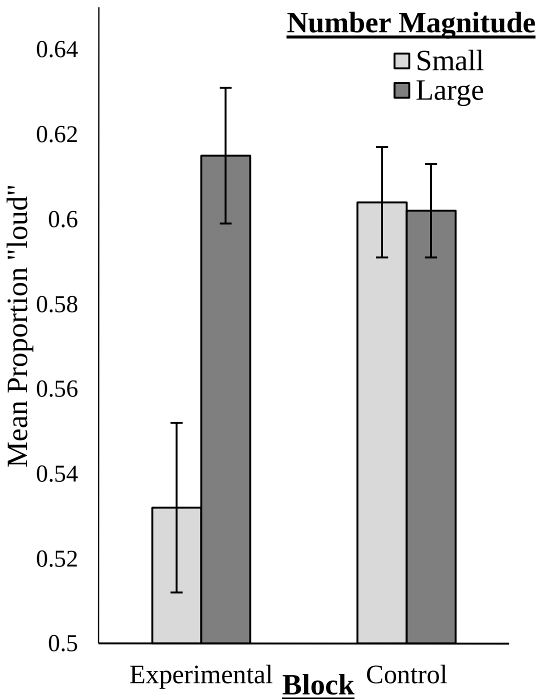


Figure 16. Experiment 7: Number Magnitude × Block interaction. Small (numbers 1, 2), large (numbers 8, 9). Error bar represent the standard error of the mean.

Additionally, similar to Experiments 5 and 6, there was a main effect of Reference Intensity level $F(1.55, 51.27) = 176.89$, $p < .001$, $\eta p^2 = .84$, where the proportion of *louder* responses increased linearly with reference sound intensity, with targets that followed REF1 sounds exhibiting the smallest proportion of *louder* responses (*M* = .46 [.01]), followed by REF2 (*M* = .59 [.01]), and REF3 (*M* = .72 [.01]). Furthermore, as in Experiments 5 and 6, there was a significant Reference Intensity × Target Intensity interaction $F(1, 33) = 6.16$, $p = .018$, $\eta p^2 = .16$ (see Figure 4C). To deconstruct this interaction, as in Experiments 5 and 6, target intensity difference scores were compared across the levels of Reference Intensity using a one-way repeated measures ANOVA.

There was a significant main effect of Reference Intensity $F(2, 66) = 31.22$, $p < .001$, $\eta p^2 = .49$ (see Figure 5). Post-Hoc pairwise comparisons further revealed no difference between REF1 ($M = .54$ [.03]) and REF2 ($M = .54$ [.03]). However, REF1 significantly differed from REF3 ($M = .38$ [.03]) $F(1, 33) = 38.22$, $p < .001$, $\eta p^2 = 54$. Similarly REF2 also significantly differed from REF3, $F(1, 33) = 46.15$, $p < .001$, $\eta p^2 = .58$, demonstrating the same reduction in categorization sensitivity at higher overall intensity levels witnessed in Experiments 5 and 6.

General Discussion

The results firstly supported that sound intensity categorization responses conform to Weber's law, with discrimination judgments demonstrating a marked reduction in sensitivity as the reference sound intensity level was scaled upwards. This was particularly evident for a reference intensity change from 82.96 dB (REF2) to 83.96 dB (REF3). It is clear that regardless of the experimental manipulation, the perceptual discriminability of sound loudness decreases as the overall intensity levels of the compared sounds is increased.

Secondly, the results observed in the current experiment have established that visually presented numbers elicited biases on sound loudness categorization responses. These biases however only occurred under two conditions 1. when the number occurred simultaneously with the sound, or 2. The number and sounds were presented independently, but the number was held in short term-memory when the sound was presented. However, in conditions where the number preceded the sound by a short delay and participants had no reason to remember it, the bias was eliminated. This finding was more in keeping with an anchoring explanation of this phenomenon versus one that speculates on the existence of a generalized magnitude system. As noted, highly fluid, conceptual priming phenomena have been observed between numerical notations (Words/Numbers, Gabay et al., 2013; Naccache & Dehaene, 2001a; Naccache & Dehaene, 2001b); as such, we do not discount that symbolic and non-symbolic numbers may be represented beneath a common metric, however, our findings do not support the conclusion of a common code underlying the representations of sound intensity and number magnitude.

To summarize, the anchoring as adjustment account poses that once a number is held in short-term memory, its presence will be sufficient to induce anchoring effects regardless of its relationship to the target task. Furthermore, this effect can be rather superficial, being determined entirely by the anchor’s absolute value (Kahneman & Knetsch, 1993; Wilson et al., 1996; Wong & Kwong, 2000). Interestingly, anchoring effects are not be limited to numbers, and theoretically, any information conveying some form of inherent magnitude, once held in short-term memory, should induce assimilative anchoring biases – even when the estimated dimension is in a completely different modality. For example, participants that drew longer lines estimated the Mississippi river to be longer, and the average temperature of Hawaii to be higher (Oppenheimer, LeBoeuf, & Brewer, 2008). Therefore, one future application of studies looking into numerical anchoring on perception, might examine how these other conceptual magnitude dimensions may induce similar effects on perceptual judgments. Participants drawing longer lines for instance may also estimate later occurring sounds as *louder*, than those drawing shorter lines. As suggested by the authors, the activation of the representation for line length may have served as a prime for a seemingly, unrelated magnitude dimension, “Hence cross-modal effects of anchors may arise, with a large anchor in any one modality leading to a large judgment in any other modality.” (p. 15) (see also Newell & Shanks, 2014). If the design of the current experiment were modified, such that participants had to either judge the magnitude (larger/smaller than 5), or parity (odd/even) of the intervening digit on each trial, prior to judging a sound's intensity level; it seems likely that this would be a sufficient modification for inducing the participants to hold enough numerical information to induce a similar anchoring effect, despite being temporally displaced from the sound.

Additionally, cross-modal interactions between numbers and other sound properties are likely to exist. Prior studies have found that high/low pitched sounds were categorized faster using congruent high/low vertically oriented response keys (Rusconi, Kwan, Giordano, Umiltà, & Butterworth, 2006), and participants were faster at responding to high/low pitch sounds when they originated from high/low sources respectively (L. C. Leboe & Mondor, 2007). This suggests that some basic sound attributes are represented according to a spatialized cognitive template. The same can be said of numbers: small magnitude numbers (e.g., 1 and 2) involuntarily initiate downward saccadic eye movements; while large magnitude numbers (e.g., 8 and 9) initiate upward saccadic eye movements (Gevers, Lammertyn, Notebaert, Verguts, & Fias, 2006; Schwarz & Keus, 2004). Converging evidence has further shown that the passive displacement of the body in a upward vertical direction leads to the random generation of more large magnitude numbers, and downward vertical direction, small numbers (Hartmann, Grabherr, & Mast, 2011). This suggests that sound pitch and number magnitude are likely to exert interactional effects based on this shared spatial framework. For instance, people should be faster to categorize a sound's pitch as "high" in frequency when first primed with a large number, and "low" in frequency when first primed with a small number.

Conclusion

To conclude, while some of the results could potentially be framed as evidence in support of a generalized magnitude system for number and sound intensity, it would seem unlikely that visual numbers and sounds would share a common representational format in the IPS considering that sound intensity coding has been largely attributed to the primary auditory cortex. Studies using fMRI for example, have correlated hemodynamic response functions in the superior temporal gyrus of the primary and secondary auditory cortices which correspond directly to increases in perceived sound loudness (Jäncke, Shah, Posse, Grosse-Ryuken, & Müller-Gärtner, 1998). Furthermore, studies using fMRI adaptation paradigms have found that stimulus repetition causes reduced neural activity in the sub-populations responsible for coding that attribute (Grill-Spector, Henson, & Martin, 2006). Therefore, if numbers are represented via an abstract code, adaptation effects should occur within and across numerical notations. In one study, an adaptation response was found in the right IPS for Arabic digits, but no reduction in activation for magnitude repetition was found using number words, or mixed notations (e.g., 2 – six) (Cohen Kadosh, Cohen Kadosh, Kaas, Henik, & Goebel, 2007; Cohen Kadosh & Walsh, 2009). Another study found no evidence of repetition suppression at all for neurons in the IPS when presented with repeated numerical representations (Shuman & Kanwisher, 2004). Therefore, it has recently been suggested that IPS neurons generally code, not for the magnitudes themselves, but rather, the decision processes involved in comparing magnitudes; and that overlapping computational constraints may account for the cross-talk witnessed between magnitude dimensions (Van Opstal, Gevers, De Moor, & Verguts, 2008; Van Opstal & Verguts, 2013). Hence, rather than sharing a representational code, they may instead share a common comparison process (DeWind & Brannon, 2012; Feigenson, 2007). This however, will require further investigation.

There is another theoretical alternative that may account for the current set of findings, which unlike ATOM, maintain distinct representational systems across dimensions instead of suggesting a common representational code. The theory currently best suited for this purpose is the *neural reuse* model (Anderson & Penner-Wilger, 2013; Anderson, 2010; Anderson, 2014). According to this theory, pre-existing – evolutionary older – neural circuitry is reused to support more phylogenetically recent capacities (e.g., language, and mathematics), predicting that a newly emerging skill (e.g., number representation), will be supported across a greater variety of structures, exhibiting a higher degree of distribution throughout the brain, over established skills (e.g., attention). For example, the pre-existing neural circuitry involved in finger gnosis (finger localization) is recombined for number representation (Anderson & Penner-Wilger, 2013; Penner-Wilger & Anderson, 2008; Penner-Wilger et al., 2007). Similar models have suggested that the neural circuitry in place for distinguishing differences in physical size are also activated by numbers (Cantlon, Platt, & Brannon, 2009; Henik, Leibovich, Naparstek, Diesendruck, & Rubinsten, 2012). Therefore, another potential explanation for our results is that the neural circuits involved in sound intensity perception are similarly being reused in the representation of number, thus facilitating cross-modal interactions across magnitude dimensions.

It should be noted that these theories as of yet do not fully explain why – in the current study – cross-modal interactions were elicited only when the number and sound occurred concurrently, or when the number was held in short-term memory (Experiment 5 and 7, respectively), but not in a procedure where the number acts as a prime (Experiment 6). We suggest that – at least in some instances – the interactions witnessed between numbers and other perceptual dimensions may constitute examples of numerical anchoring, and that people might adaptively use information held in short-term memory to guide a wide variety of decision processes, which include basic perceptual judgments (e.g., sound intensity).

Contextual Influences on Time/Number Interactions

In Chapter II, I demonstrated that relying on a similarity heuristic when judging the duration of a blank interval between events can result in kappa-like effects across a wide array of stimulus dimensions including: symbolic number magnitude, size, and colour saturation level. The perceived duration of the blank interval separating two events is in part arrived at through the substitution of information computed about the absolute magnitude difference across the interval. In Chapter III, it was further demonstrated that task irrelevant symbolic numbers bias perceptual judgments of the intensity of an unrelated sound, in a manner consistent with numerical anchoring. For example, participants factored numerical information into sound intensity estimates only when the number co-occurred with sound (and thus could not be ignored), or when the number was presented ahead of the sound, but held in short-term memory. Alternatively, the number’s magnitude ceased to have any impact when presented prior to the target sound’s onset. This could be the result of participants actively ignoring – task-irrelevant – numerical information. Alternatively, a contextual-change in the mental processes required for number magnitude to sound intensity, may have led to the unintentional forgetting of the number’s magnitude (Mulji & Bodner, 2010). Despite the lack of priming found for number on sound intensity; when the events co-occurred, it became difficult to eliminate the impact of numerical magnitude information on the sound intensity task. This first finding is very similar to prior demonstrations of time/number interactions as in these cases, people estimated the durations of numerical events in which the number’s presentation duration actually is the time interval that is estimated (Oliveri et al., 2008). One result of this design is, as was evident in Experiment 5 when a sound and number were presented simultaneously, it is difficult to disentangle highly proximal – co-occurring – dimensions, and thus it is impossible to discount the effect of number on the duration of the interval.

It therefore must be asked, to what degree is the absolute magnitude of the number important in driving this bias versus the bias being contextual in nature? Lu et al. (2009), for example, found that time-number interactions were enhanced by attaching a weight unit suffix to a target digit that emphasized the magnitude difference between the compared digits (1 kg vs. 9 kg). This finding suggests that time-number interactions are more about how large the number is subjectively perceived to be, as determined by the context in which it is presented. This further suggests a large role of top-down processes in driving time-number interactions.

In Chapter IV, I employed a paradigm similar to Oliveri et al. (2008) and Lu et al. (2009) in that participants judged the presentation durations of target numbers. In other words, like in Experiment 5, the target (duration) and interfering (number) dimensions occurred simultaneously. However, in the study described in Chapter IV, these target numbers were always presented at the end of preceding numerical context sequences. The magnitude of these context sequences either matched or mismatched the approximate magnitude of the target.

The internal clock model does not rule out the impact of external factors such as arousal-level, in affecting pacemaker output. It has been suggested that if one’s expectations are violated – for example by the presentation of a novel (oddball) stimulus in a series of repeated standards – this often tends to elicit prolonged duration estimates for the novel stimulus (Birngruber, Schröter, & Ulrich, 2014, 2015). It has been suggested that the occurrence of an infrequent stimulus is proposed to heighten arousal level, which in turn increases pacemaker speed (Ulrich, Nitschke, & Rammsayer, 2006). Therefore, a final prediction was tested regarding the influence of context and expectations. If a sequence of numbers – all similar in magnitude –

are presented prior to a target number, the target should be experienced as *longer* in subjective duration the further it is in distance from the preceding context sequence. If expectation does influence pacemaker speed in the predicted way, the number 9 should be perceived of as *longer* when immediately following a series of small numbers (1, 2, 3) versus when it follows a sequence of large numbers (7, 8, 9). Conversely, a 1 should be perceived of as *longer* when it immediately follows a series of large numbers versus a series of small numbers. In Chapter IV this account was tested, and a novel anchoring approach is provided as a potential alternative explanation for the impact of contextual manipulations on time-number interactions.

Alards-Tomalin, D., Walker, A. C., Kravetz, A., & Leboe-McGowan, L. C. (2015). Numerical context and time perception: Contrast effects and the perceived duration of numbers. *Perception*. Advance online publication. doi:10.1177/0301006615594905

Chapter IV: Contextual Influence on Perceived Number Duration

Time perception is a fundamental skill, widely demonstrated to be susceptible to bias from numerous sources of environmental and contextual information (Fraisse, 1984). For example, intervals containing more events (e.g., visual, auditory, or tactile) are judged as longer in duration (Adams, 1977; Buffardi, 1971; Dong & Wyer, 2014; Javadi & Aichelburg, 2012; E. C. Thomas & Brown, 1974), as are stimuli that exhibit higher complexity (Ornstein, 1969), and dynamic characteristics including, looming, or flickering (for vision see: Aubry, Guillaume, Mogicato, Bergeret, & Celsis, 2008; Brown, 1995; Casasanto & Boroditsky, 2008; Grassi & Pavan, 2012; Kanai, Paffen, Hogendoorn, & Verstraten, 2006; van Wassenhove, Buonomano, Shimojo, & Shams, 2008), (for audition see: DiGiovanni & Schlauch, 2007; Eisler & Eisler, 1992; Grassi & Darwin, 2006; Leboe & Mondor, 2008; Schlauch, Ries, & DiGiovanni, 2001).

A subset of the time perception literature has focused on how contextual information regarding magnitude can impact perceived duration judgments (i.e., time-magnitude biases). These studies have found that increases in size, luminance, volume (Brigner, 1986; Cantor & Thomas, 1976; Kraemer, Brown, & Randall, 1995; Matthews, Stewart, & Wearden, 2011; Ono & Kawahara, 2007; Ono & Kitazawa, 2009; Thomas & Weaver, 1975), and quantity (Arlin, 1986; Hayashi, Valli, et al., 2013; Mo, 1974), all induce phenomenological time dilation— a subjective lengthening in perceived duration. Perhaps of greater interest is that this bias has also been demonstrated for symbolic magnitude. For example, the perceived durations of relatively small numbers (e.g., 1, 2) tend to be underestimated, while large numbers (e.g., 8, 9) are overestimated (Chang, Tzeng, Hung, & Wu, 2011; Kiesel & Vierck, 2009; Oliveri et al., 2008; Vicario, 2011; Vicario et al., 2008; Xuan et al., 2009, 2007). This finding implies that time-magnitude biases are not purely sensory in nature, but emerge at a higher semantic level of analysis. In the current study, we examined how manipulating meaningful information about the numerical context preceding the presentation of a number can further impact a number's perceived duration. To put it generally, we examined how the level of contrast between the target number's magnitude and its preceding context may further bias temporal judgments, and how this informs current theories regarding cognitive representations of time and number.

ATOM Framework

There are currently few theories which posit satisfactory explanations for time-magnitude perceptual biases. The "A Theory of Magnitude" (ATOM) framework is a set of predictions which has attempted to bridge this gap. To summarize, ATOM hypothesizes that the brain (specifically the right intraparietal sulcus [IPS]) contains a common representational metric for space, time, and number, and that these dimensions exert their influence via sensorimotor transformations that are unique to the action system of the parietal lobe (Bueti & Walsh, 2009; Gallistel, 2011; Walsh, 2003). This system, sometimes referred to as the analogue magnitude system, is thought to convert magnitude information from multiple dimensions (including number) into a modality-free, notation-independent code that exhibits scalar variance (i.e., the standard deviation of estimation responses increase proportionally with the intensity of the estimated stimulus) and thus conforms to Weber's law. Theoretically, this system would allow people to formulate quick and efficient *more-than* versus *less-than* approximations regarding differences in object size, weight, brightness, numerosity, and duration, with the efficiency of the comparison process being determined by the ratio with which the compared stimuli differ. This analogue system has since been proposed to provide the phylogenetic foundation upon which more advanced arithmetical skills are based (Cantlon et al., 2009; Henik et al., 2011); hence, we first learn that continuous magnitudes have additive and subtractive properties, and only then are these qualities subsequently mapped onto symbolic magnitude referents (i.e., symbolic numbers). It is at this point in development where symbolic numbers are presumed to gain entry into the analogue magnitude system, exhibiting scalar variability (Whalen et al., 1999).

One theory accounting for time-magnitude biases proposes that time and number become conflated as scalar values during the conversion process, allowing information from one dimension (number magnitude) to effectively spill-over into the other (time) at the representational stage (Gallistel & Gelman, 2000; Whalen et al., 1999); with a recent transcranial magnetic stimulation study providing some evidence in support of this perspective (Hayashi, Kanai, et al., 2013). Despite this, the majority of the physiological evidence is less supportive of the theory that time and number share a common representational metric. For example, while number representation appears to involve primarily prefrontal and posterior parietal areas (Nieder & Dehaene, 2009), time perception is largely determined by a distributed thalamic-basal ganglia circuit (Merchant, Harrington, & Meck, 2013). Furthermore, there may be hemispheric lateralization of time and number cognitive processes. For example, patients with right hemispheric lesions display time perception impairments but are spared any deficit in number skills (Cappelletti, Freeman, & Cipolotti, 2009). Similarly, a transcranial magnetic stimulation study found that virtual lesions to the left IPS selectively impaired numerosity estimation but left time perception intact (Dormal, Andres, & Pesenti, 2008). Furthermore, asymmetries in the interactions between time and number have been noted, with number being far more likely to bias timing judgments than the reverse (Dormal & Pesenti, 2007; Droit-Volet, Clément, & Fayol, 2008). As such, the ATOM framework requires careful future consideration as to whether it truly provides an undistorted perspective of the cognitive systems subserving number and time comprehension, and if it can in fact serve as an explanation for the various perceptual interactions found to occur between magnitude dimensions.

Neural Amplitude Hypothesis

Another theory of time and number perception that has recently gained some traction has suggested that both properties arise from temporal variations in the activation patterns of distributed neural networks (Ivry & Spencer, 2004). For example, psychological time (at the level of subsecond intervals) may be represented as a function of neural spiking characteristics and individual membrane potentials (Buonomano & Merzenich, 1995; Buonomano, 2000; Karmarkar & Buonomano, 2007). Further building upon these state-dependent network models, current computational models have shown that information about event duration (as well as event frequency) may be derived monotonically from overall neural activity level, and then maintained within a neural circuit (Bancroft, Hockley, & Servos, 2014). We will refer to this perspective more generally as the neural amplitude hypothesis (NAH; Curran & Benton, 2012; Eagleman & Pariyadath, 2009; Eagleman, 2008; Matthews, 2011; Pariyadath & Eagleman, 2007, 2012; Sadeghi, Pariyadath, Apte, Eagleman, & Cook, 2011).

The NAH makes two predictions regarding magnitude. The first prediction is that stimulus intensity is coded as a linear function of neural activity (i.e., neural spiking rates that increase monotonically with stimulus intensity). This has received physiological support from studies examining the neural responses to brightness (Barlow, Snodderly, & Swadlow, 1978; Tikhomirov, 1983), size (Murray, Boyaci, & Kersten, 2006), numerosity (Roitman, Brannon, & Platt, 2007; Verguts & Fias, 2004) and has also been found to be representative of single neurons coding for duration in the lateral parietal regions of primates (Janssen & Shadlen, 2005; Leon & Shadlen, 2003). The second prediction is that reduced neural amplitudes, resulting from the repetition of a stimulus property (i.e., neural repetition suppression, see Wiggs & Martin, 1998) will correspond with a reduction in perceived duration. The NAH, for example, proposes that the perceived durations of repetitive stimuli will contract relative to an earlier occurring target causing the earlier target to appear to have the longer duration (Rose & Summers, 1995). In further demonstration of this phenomenon, Pariyadath and Eagleman (2007) presented participants with number sequences wherein the duration of the first number (the target) was compared against the durations of a string of numbers that immediately followed it. The sequences were either repetitive (1-1-1-1-1), ordered (1-2-3-4-5), or randomized (1-4-3-5-2). The researchers found that the target's duration (e.g., ''1'') was overestimated relative to the standards for both the repeated and ordered conditions, but was unaffected on random number sequence trials. This was taken to indicate that contextual symbol repetition, and the predictable ordering of the numerical stimuli, were both sufficient to induce physiological neural repetition suppression, which then caused a contraction of their subjective durations relative to the initial target, thus making the target appear longer in duration (Schindel & Arnold, 2011).

The NAH is not incompatible with ATOM, and in fact, may provide the mechanisms by which magnitudes are coded in the analog system. Furthermore, it poses a set of testable predictions regarding the neural representation of time and number that are informative and can be used to further test the validity of the ATOM framework. For example, repetition suppression is a general physiological phenomenon common to all neural assemblies and should be evident for neurons that represent magnitude commonly across formats.

Current Study

In the current study, the impact of contextual magnitude repetition on timing judgments for numerical targets was examined with the intent of addressing three empirical questions, (a)

Will the repeated presentation of similar magnitude numbers cause a later occurring target number to be perceived of as subjectively shorter in duration? If the NAH is correct, contextual magnitude repetition should result in shorter duration judgments when numerical targets are similar in magnitude to their preceding context. (b) Will target magnitude continue to bias duration judgments despite manipulations to the preceding context? To date, most studies have examined number magnitude-time biases in the absence of further contextual manipulations (for an example to the contrary, see Lu, Hodges, Zhang, & Zhang, 2009). (c) Finally, if symbolic and non-symbolic (i.e., numerosity) numbers are processed under a common representational metric, cross-notation repetition suppression should be evident. In other words, the repetition of magnitude — as conveyed by numerosity — should reduce activation levels for similar magnitude numerical symbols causing them to be perceived of as shorter in duration.

Experiment 8

Experiment 8a

In Experiment 8a, the target number was preceded by a context sequence composed of six numbers. A third of the context sequences were composed of repetitive large magnitude numbers (randomly intermixed 7s, 8s, and 9s), while another third were composed of repetitive small magnitude numbers (intermixed 1s, 2s, and 3s), the final third were composed of randomly intermixed small and large numbers. Target magnitude (small: 1, 2; large: 8, 9), and whether the target was similar in magnitude to the context sequence (e.g., large context: 7-9-8-7-9-8; large target: 9) or dissimilar (e.g., small context: 2-1-3-1-2-3; large target: 9), were manipulated across trials. The mixed context sequences (e.g., mixed context: 1-8-7-3-9-2; large target: 9) were included as a control condition to ensure that the participants would attend to the magnitude of each number in the context sequence.

Method.

Participants. Thirty-nine undergraduate students enrolled in an introductory to psychology course at the University of Manitoba participated in Experiment 8a in exchange for partial course credit. Thirty-four of the participants were female (five male), with a mean age = 18.79 years, (SD = 1.82). Individual participants with accuracy rates on the context sequence task that fell below the accuracy cutoff on mixed contexts (below the average subtracted from 2 × SD of the mean) were treated as outliers and removed from further analysis. This led to the removal of seven participants (final n = 32). All of the experiments received prior approval by the University of Manitoba Fort Garry Campus Research Ethics Board.

Materials. In Experiment 8a, the numbers 1, 2, 8, and 9 were presented as targets, while the numbers 1, 2, 3 and 7, 8, 9 were presented in the context sequences. The Arabic digits subtended 3.3° × 2.6° of visual angle and were presented on a PC using E-prime 2.0 release candidate software (ver. 2.0.8.74; Psychology Software Tools, 2012).

Design and procedure. Each experimental trial was initiated with a space-bar press. The participants first rated whether each of the six context sequence numbers were ‘‘smaller’’ or ‘‘larger’’ than five. For half of the participants, ‘‘smaller’’ judgments were made by pressing a key labeled S affixed to the S-key of a standard keyboard and ‘‘larger’’ judgments by pressing a key labeled L affixed to the L-key (Key Mapping A). The other half of the participants used the reverse mapping (‘‘S’’ affixed to the L-key, and ‘‘L’’ affixed to the S-key; Key Mapping B). Both conditions were employed to control for the possibility that participants would map small magnitude responses to the left side of the keyboard, and large responses to the right. The context sequence task was self-paced, with each number remaining onscreen until a response was registered. There was a 500 ms blank inter-stimulus interval (ISI) separating the presentation of the final context sequence stimulus and the target. The duration of the target number was categorized by the participants as short, medium, or long in its duration (see Figure 17). All three context sequence categories (large, mixed, and small) were composed of 48 trials, which included two repetitions of each target number (1, 2, 8, 9) at each of the six possible target intervals (340, 360, 380, 420, 440, and 460 ms); resulting in 144 randomly intermixed experimental trials.

The timing task was a duration scaling method referred to as *category-rating*. According to this method, the different interval durations are categorized based on the boundaries of a predetermined response scale (Allan, 1979). Prior to beginning the experiment, participants were trained to categorize 340, 400, and 460 ms intervals, as short, medium, and long, respectively. To accomplish this, 12 practice trials, with accuracy feedback, were completed. The ‘‘short’’ label was always affixed to the G-key of a standard keyboard, the ‘‘medium’’ label to the H-key, and the ‘‘long’’ label to the J-key, thus the duration categorization response options were independent of the response options used to categorize number magnitude during the context sequence task. The key-label mappings used for duration categorization responses were upheld across all participants.

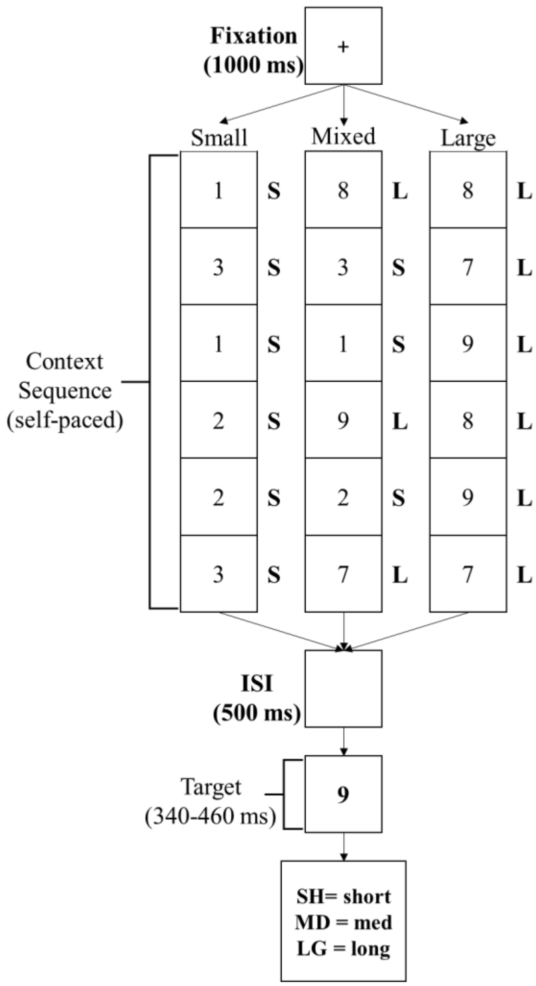


Figure 17. Experiment 8a procedure.

***Data analysis*.** In analyzing participants' responses, the total number of ‘‘short’’ responses were multiplied by 0, ‘‘medium’’ responses by 1, and ‘‘long’’ responses by 2. The sum of these three products was then divided by the sum of each participant's total responses. This data conversion produced average duration ratings for each participant that ranged between 0 and 2, with scores approaching 0 indicating a higher proportion of short responses, and scores approaching 2 indicating a higher proportion of “long” responses (for similar analyses see, Aubry et al., 2008; Masson & Caldwell, 1998; Mattes & Ulrich, 1998; Rammsayer & Ulrich, 2001). SPSS Statistics Package version 17 was used in the analyses of all of the reported data (SPSS Inc, 2008).

Results and discussion. First, to assess whether the key mapping impacted context sequence task accuracy, a 3 (sequence type: small, mixed, and large) × 2 (Key Mapping: A and B), mixed-measures analysis of variance (ANOVA) was run, with sequence type included as a within-participants variable and Key Assignment, a between-participants variable. Sixteen participants were randomly assigned to each of the Key Assignment conditions. This analysis revealed a main effect of sequence type, $F(1.223, 36.70) = 63.919$, $p < .001$, $\eta p^2 = .681$. Participants performed with higher overall accuracy when categorizing repetitive small (M = .995 [.002]) and repetitive large (M = .997 [.001]) magnitude patterns versus mixed magnitude patterns (M = .956 [.005]), $F(1, 31) = 74.130$, $p < .001$, $\eta p^2 = .705$. This drop in accuracy was likely the result of having to alternate responses in an unpredictable manner for mixed sequence types. Furthermore, the Key Assignment condition did not interact with sequence type (p = .848), and the main effect of Key Assignment was non-significant (p = .242). As it was clear that the response mapping itself did not impact on accuracy, this variable was excluded from further analyses.

Second, the mean duration ratings were submitted to a 3 (context sequence: small, mixed, and large) × 6 (target duration: 340, 360, 380, 420, 440, and 460 ms) × 2 (target magnitude: small and large) repeated-measures ANOVA. This revealed a significant main effect of target duration, $F(5, 155) = 73.79$, $p < .001$, np2 = .704, in which mean duration ratings increased across the target intervals $F(1, 31) = 123.403$, $p < .001$, $\eta p^2 = .799$ (see Figure 18).

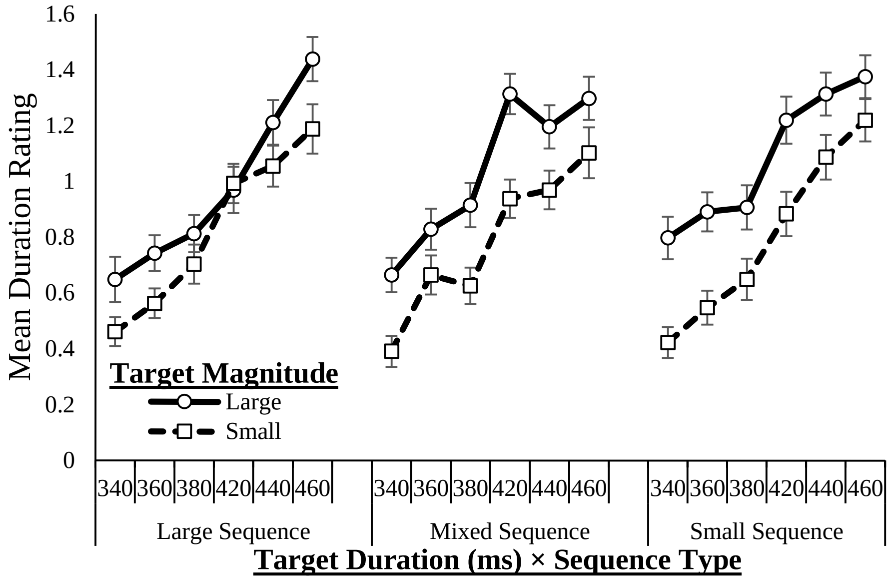


Figure 18. Experiment 1a mean duration ratings for small and large magnitude targets at each of the six durations (340, 360, 380, 420, 440, and 460 ms) for each of the three context sequence types (large, mixed, and small). The error bars represent the standard error of the mean.

There was also a significant main effect of target magnitude, $F(1, 31) = 66.88$, $p < .001$, $\eta p^2 = .683$, in which large target numbers (8 and 9) were categorized as long in duration more frequently ($M = 1.03$ [.044]) than small numbers (1 and 2; $M = .803$ [.040]). Additionally, there was a significant context sequence × target magnitude interaction $F(2, 62) = 7.93$, $p < .001$, $\eta p^2 = .204$ (see Figure 19a). This interaction was due to a main effect of context sequence type for large magnitude targets $F(2, 62) = 4.492$, $p = .015$, $\eta p^2 = .127$, that did not extend to small targets ($p = .514$). None of the remaining interactions were significant (remaining $p \geq .056$).

The interaction was further deconstructed using post-hoc pairwise contrasts that compared the mean perceived durations provided to large magnitude targets across the three context sequence types. This analysis revealed that large targets preceded by repetitive small context sequences were rated to have longer durations ($M = 1.083$ [.051]) versus when they were preceded by repetitive large magnitude contexts ($M = .970$ [.051]), $F(1, 31) = 16.88$, $p < .001$, $\eta p^2 = .353$ (remaining $p \geq .102$).

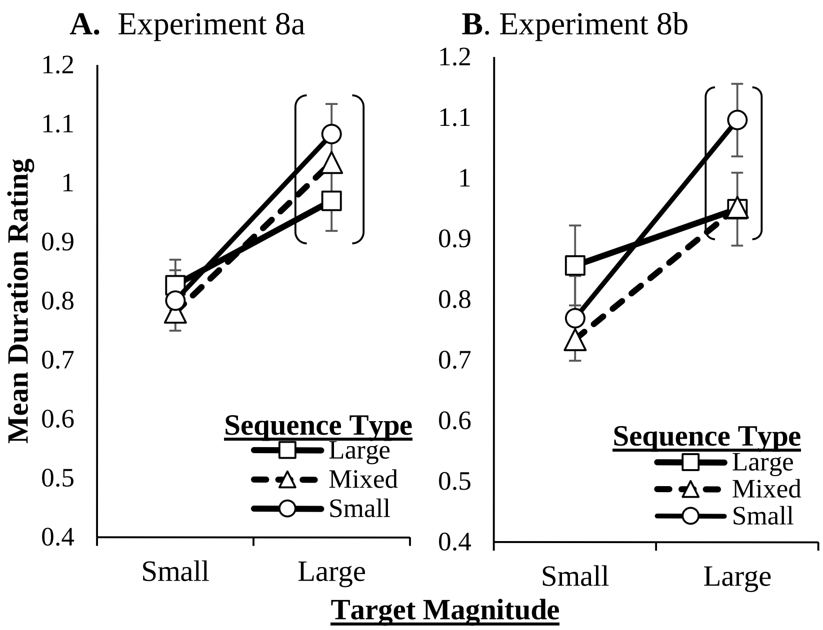


Figure 19. Target magnitude × sequence type interactions: Experiment 1a (A) large vs. small sequences significant at p < .001. Experiment 1b (TM group = 13). (b) Large vs. small sequences significant at p = .005. The mean duration scale minimum = 0, maximum = 2. Error bars represent the standard error of the mean.

To summarize, (a) despite contextual manipulations, large target numbers were categorized to have longer durations relative to small targets. (b) There was a contrast effect, in which large target numbers were judged as longer in duration when they followed small number contexts, and as shorter when following large magnitude contexts. While this finding partially fits with the predictions of the NAH account — that attribute repetition should induce shorter duration estimates via repetition suppression — the effect was asymmetrical, occurring only for large magnitude targets.

One possible explanation for this, is that the contrast effect was being caused by the repeated presentation of a numerical symbol as a context sequence number, and as the target, and not by manipulations to the magnitude of the context sequence. If this were true, a further prediction can be made, the mixed sequences always contained a single repetition of the target number (1-3-8-**9**-7-2 → **9**), while repetitive magnitude sequences that were similar in magnitude to the target always contained two repetitions of the target symbol (7-**9**-8-7-8-**9** → **9**). If the contrast effect were purely the result of symbol repetition, targets following repetitive matching magnitude sequences should be judged as shorter relative to targets following mixed contexts. We tested this by collapsing the variables into Repetitive-Match and Mixed context sequence categories and compared the perceived duration ratings using a one-way repeated measures ANOVA. The results revealed no significant difference between the categories (p = .512), supporting the idea that symbol repetition was not behind the contrast effect observed for large magnitude numbers.

A second possibility is that the contrast effect may be unique to symbolic numbers like Arabic digits, and may not be present for representations that tap into numerosity (i.e., physical quantity). If the witnessed asymmetric repetition effect is specific to symbolic numbers, then this asymmetry should be eliminated when symbolic numbers are replaced with numerosities. In Experiment 8b, this hypothesis was tested.

Experiment 8b

In this experiment, Experiment 8a was replicated using numerosities (in the form of canonical dot patterns) as opposed to Arabic digits.

Method.

Participants. Twenty-six new participants recruited from the same participant pool took part in Experiment 8b. The mean age of the sample was 19.27 years (SD = 2.47), which included 17 females and 9 males.

Materials. Numerosities were presented as canonical arrangements of black dots on a white background framed by a black border (see Figure 20). The total surface area within each border subtended 5.6° visual angle, while each dot in the pattern subtended 0.9° of visual angle.

Design and procedure. The basic procedure for Experiment 8b was identical to Experiment 8a; however, Arabic digits were substituted with numerosities.

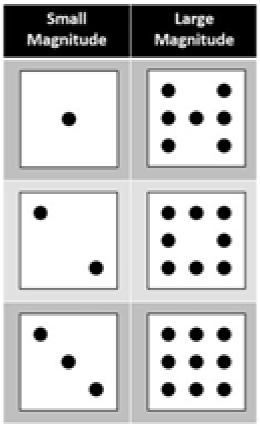


Figure 20. Patterns used in Experiment 8b.

Results and discussion. The mean duration ratings were submitted to a 3 (context sequence: small, mixed, and large) × 6 (target duration: 340, 360, 380, 420, 440, and 460 ms) × 2 (target magnitude: small and large) repeated-measures ANOVA. The results revealed a significant main effect of target duration $F(3.008, 75.195) = 81.780$, $p < .001$, $\eta p^2 = .766$, indicating that perceived duration ratings increased systematically across the target intervals, resulting in a significant linear trend $F(1, 25) = 162.678$, $p < .001$, $\eta p^2 = .867$. However, the main effect of target magnitude was not statistically significant ($p = .154$). This was a surprising result since it was previously found that time-magnitude biases for canonical dot patterns were as robust as those found for symbolic numbers (Xuan et al., 2007). To determine why the time-magnitude bias was eliminated in this instance, we collapsed across all other factors and compared the mean duration ratings across the four numerical targets (1, 2, 8, and 9) using a one-way repeated measures ANOVA.

This analysis initially revealed a non-significant main effect of number ($p = .156$); however, an interesting pattern emerged; for half of the participants (n = 13), time categorization responses were biased by structural differences in the allocation of the dots composing the patterns. The main effect of number for these participants, $F(3, 36) = 4.147$, $p = .013$, $\eta p^2 = .257$, was driven by a tendency to perceive the duration of 1 (a single, focally presented dot) as subjectively longer (*M* = 1.066 [.041]) than 2 (two dots; *M* = .947 [.063]), $F(1, 12) = 4.589$, $p = .053$, $\eta p^2 = .277$; 8 (eight dots; *M* = .936 [.067]), $F(1, 12) = 6.942$, $p = .022$, $\eta p^2 = .366$; or 9 (nine dots; *M* = .889 [.05]) $F(1, 12) = 21.405$, $p < .001$, $\eta p^2 = .641$. Furthermore, there were no significant differences in the perceived duration ratings for the numbers 2, 8, and 9 ($p = .558$). This set of participants are referred to as the central presentation (CP) group because the appearance of a single, focally presented dot was a more salient feature to them than the pattern's magnitude. The other half of the participants (n = 13) exhibited the predicted time-magnitude bias. This group demonstrated a main effect for number $F(3, 36) = 14.724$, $p < .001$, $\eta p^2 = .551$, in which 1 was rated to have the shortest duration (*M* = .782 [.054]), followed by 2 (*M* = .791 [.049]), 8 (*M* = .970 [.047]), and 9 (*M* = 1.028 [.046]), generating a significant linear trend $F(1, 12) = 28.804$, $p < .001$, $\eta p^2 = .706$. These participants are referred to as the target magnitude (TM) group because their results were in keeping with the hypothesis that dot pattern magnitude was the more salient feature. When responses to the four target items were compared, grouping the participants based on their response bias type (CP vs. TM), there was a significant interaction $F(3, 72) = 14.736$, $p < .001$, $\eta p^2 = .380$ (see Figure 21).

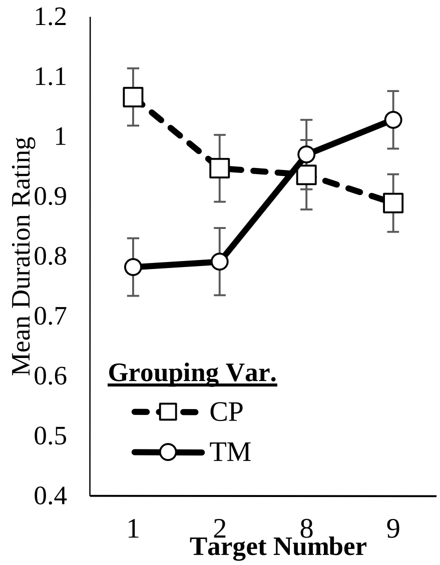
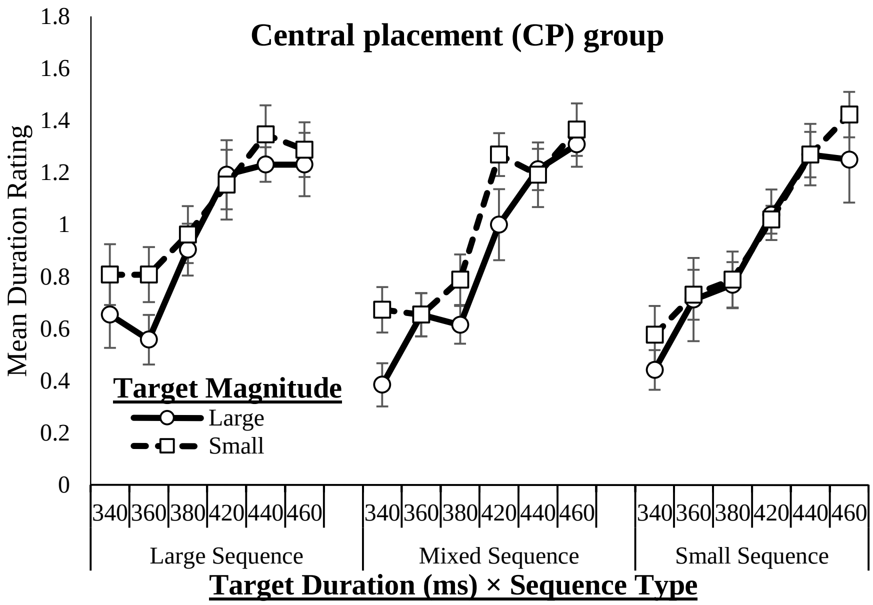


Figure 21. Target Number × Grouping Variable interaction. CP = Central Placement, TM = Target Magnitude. The mean duration scale minimum = 0, maximum = 2. Error bars represent the standard error of the mean.

Individual, 3 (context sequence: small, mixed, and large) × 6 (target duration: 340, 360, 380, 420, 440, and 460 ms) × 2 (target magnitude: small and large) repeated-measures ANOVAs were run on the CP (n = 13), and TM (n = 13) groups. For the CP group, the main effect of target magnitude $F(1, 12) = 6.404$, $p = .026$, $\eta p^2 = .348$, was characterized by a tendency to report small magnitude targets (dot pattern representing 1) as longer in perceived duration ($M = 1.006$ [.046]) than large magnitude targets ($M = .912$ [.054]). Furthermore, the main effect of target duration was significant, $F(3.03, 36.36) = 43.685$, $p < .001$, $\eta p^2 = .785$ as further demonstrated by a significant linear trend $F(1, 12) = 92.897$, $p < .001$, $\eta p^2 = .886$ (see Figure 22a).

A.



B.

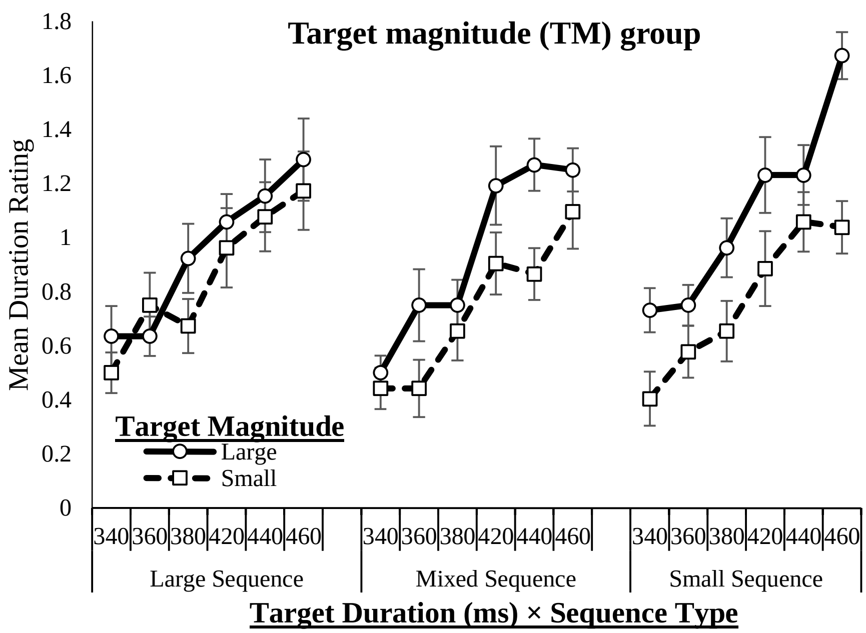


Figure 22. Experiment 8b mean duration ratings for small and large magnitude targets at each of the six durations (340, 360, 380, 420, 440, and 460 ms) for each of the three context sequence types (large, mixed, and small), for the central presentation group (n =13) (a), and the target magnitude group (n = 13) (b). The error bars represent the standard error of the mean.

However, for these participants, there was no influence of the context sequence on duration ratings (remaining $p \geq .092$). For the TM group, the main effect of target magnitude revealed that small magnitude numerosities were rated to have shorter durations (*M* = .786 [.045]) relative to large (*M* = 1.00 [.044]), $F(1, 12) = 30.443$, $p < .001$, $\eta p^2 = .717$. There was also a significant main effect of target duration $F(2.546, 30.552) = 36.524$, $p < .001$, $\eta p^2 = .753$ characterized by a linear trend $F(1, 12) = 67.107$, $p < .001$, $\eta p^2 = .848$ (see Figure 22b).

Finally, as was the case in Experiment 8a, there was a significant Context Sequence × Target Magnitude interaction $F(2, 24) = 4.986$, $p = .015$, $\eta p^2 = .294$ (see Figure 19b). As was the case in Experiment 1a, for the TM group, large target numerosities (8 and 9) that followed small context sequences were rated to have longer durations (*M* = 1.096 [.061]) versus when they followed large contexts (*M* = .949 [.060]) $F(1, 12) = 11.680$, $p = .005$, $\eta p^2 = .493$. Also similar to Experiment 1a, there was no difference in the perceived duration of small target numerosities following small (*M* = .769 [.068]) versus large contexts (*M* = .856 [.066]) ($p = .176$).

To summarize the results: (a) half of the participants demonstrated a tendency to judge a single, focally presented, dot as having a longer duration relative to the remaining numerosities. The other half demonstrated time-magnitude biases that were consistent with those demonstrated in Experiment 8a, with smaller numerosities (1 and 2) being categorized as short in duration and larger numerosities (8 and 9) as long.

One possible reason for this split was that for half of the participants, a single, focally presented dot may have constituted a more perceptually salient feature over the pattern's quantity, thus overriding the impact of pattern magnitude. As symbolic numbers were always presented in the center of the screen in Experiment 8a, this explains why a similar division did not occur in that instance. (b) For the participants that exhibited the predicted time-magnitude bias for numerosity, large magnitude target numerosities were judged as longer when they followed a small magnitude context and as shorter when they followed a repetitive large context (a contrast effect). This finding mirrored Experiment 8a. Experiments 8a and 8b confirmed that contextual magnitude can have an influence on the perceived duration of a number; however, this effect was limited to large magnitude targets. Some possible explanations for this will be further proposed in the General Discussion section. However, first we will address the third question posed, whether the effects found in Experiments 8a and 8b will occur when the context and target numbers were presented in separate numerical notations (numerosity/number).

Experiment 9

In Experiment 9, we examined whether magnitude repetition in one notation (numerosity) had an influence on the perceived duration of a numerical target presented in a different notation (symbolic number). The presence of this effect would support ATOM's proposal of a common analog metric for representing number. The absence of this finding would suggest either (a) that numbers in different formats are independently represented; or (b) one of either the context sequence stimuli, or the target are not being processed using an analog format. For example, the participants may rely on a controlled or effortful enumeration strategy when judging the magnitude of a dot pattern, and an automated strategy when processing symbolic number magnitude. Relying on an effortful enumeration strategy over the course of the context sequence may cause a temporary reduction in the attentional resources necessary to process the target number's magnitude, eliminating or reducing time-magnitude biases in the process.

Method

Participants. Twenty-eight undergraduate new participants recruited from the same participant pool took part in Experiment 9 for partial course credit. Of the 28, 16 of the participants were female, and12 were male. The participants had a mean age = 18.82 years (SD =

1.44). Two participants were eliminated from the final analysis for failing to meet the accuracy criterion (final n = 26).

Materials. The targets used in Experiment 9 were symbolic numbers identical to those in Experiment 8a. The context sequences used in Experiment 9 were the six numerosities used in Experiment 8b.

Design and procedure. The procedure was identical to Experiment 8. The participants were asked to judge whether each of 6 context sequence numerosities was larger or smaller than 5. Immediately following this task, they rated the perceived duration of a target number as short, medium, or long. The target's magnitude, and whether it was similar or dissimilar to the preceding context sequence's magnitude were manipulated across trials.

Results and discussion

Experiment 9 mean duration rating analysis. The mean duration ratings were submitted to a 3 (context sequence: small, mixed, and large) × 6 (target duration: 340, 360, 380, 420, 440, and 460 ms) × 2 (target magnitude: small and large) repeated-measures ANOVA. Consistent with Experiment 8, there was a main effect of target duration $F(5, 125) = 105.783$, $p < .001$, $\eta p^2 = .809$, in which mean duration ratings increased progressively across the target intervals resulting in a linear trend $F(1, 25) = 218.877$, $p < .001$, $\eta p^2 = .897$ (see Figure 23). There was also a significant main effect of target magnitude $F(1, 25) = 18.527$, $p < .001$, $\eta p^2 = .426$, in which large numbers were rated to have longer durations ($M = .953$ [.042]) than small numbers ($M = .807$ [.038]) (see Figure 24). Additionally, there was a significant, main effect of context sequence type $F(2, 50) = 4.538$, $p = .015$, $\eta p^2 = .154$ (remaining $p \geq .554$).

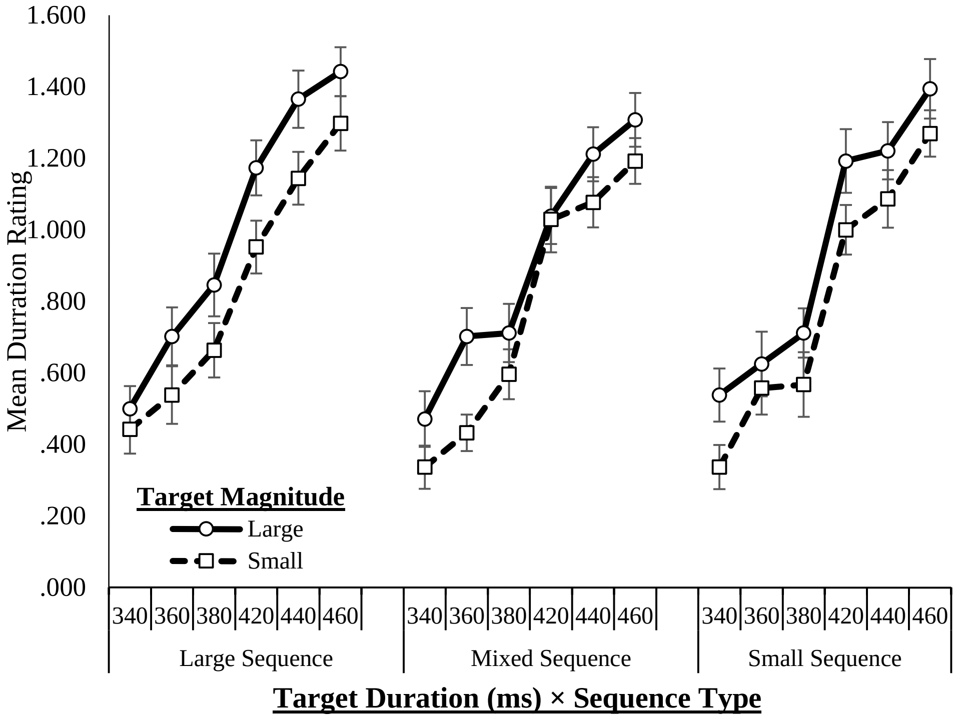


Figure 23. Experiment 9 mean duration ratings provided for small and large magnitude targets at each of the six durations (340, 360, 380, 420, 440, and 460 ms) for each of the three context sequence types (large, mixed, and small). The error bars represent the standard error of the mean.

The main effect of context sequence type was further analyzed with post-hoc pairwise contrasts comparing differences between the sequence types across the collapsed target magnitudes (see Figure 24). These comparisons revealed that target numbers preceded by repetitive large context sequences were rated as longer in duration (M = .922 [.044]), than targets following mixed sequences (M = .842 [.036]) $F(1, 25) = 10.302$, $p = .004$, $\eta p^2 = .292$, and approached significance when compared with small contexts (M = .875 [.038], $p = .061$; mixed vs. small, $p = .295$). The results indicated that there may have been an increase in cognitive load on mixed patterns, as these included a combination of having to alternate responses, and process the switch in numerical notation from the context sequence to the target. Noteworthy was the absence of a magnitude contrast effect for large numbers.

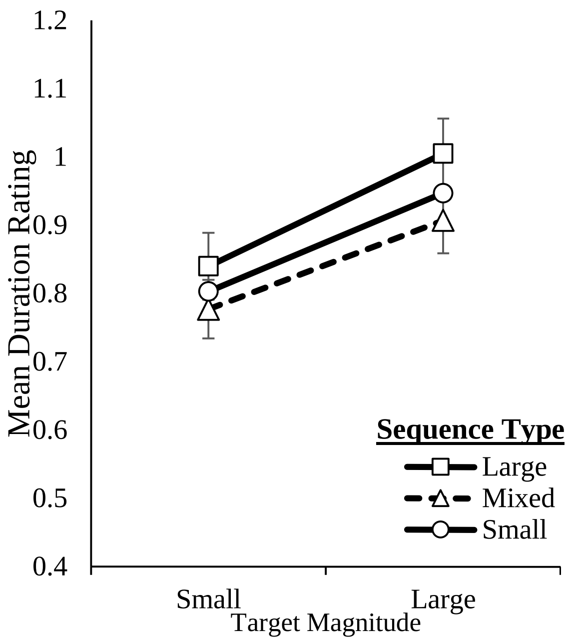


Figure 24. Main effects of target magnitude sequence type in Experiment 9. The mean duration scale minimum = 0, maximum = 2. Error bars represent the standard error of the mean.

As previously discussed, there are several explanations for the lack of contrast effect witnessed previously for numbers and numerosities: (a) the context task strategy may have differed depending on notation (numerosity—effortful vs. symbolic number—automatic). If this were the case, by adopting an effortful strategy for the context task in Experiments 8b and 9 (enumeration), there may have been fewer attentional resources left over to process target magnitude; alternatively, (b) a change in notation may be more perceptually salient than a change in magnitude, which may have overshadowed the effect of the context sequence's magnitude on the perceived duration of the target.

Median reaction time analysis across Experiments 8a, 8b, and 9. To address the first possibility, we looked at participant reaction times across Experiments 8a, 8b, and 9 when correctly categorizing the magnitudes of the items in mixed context sequences. If the participants were relying on a controlled (i.e., effortful) strategy for processing numerosity over symbolic number, participants should be slower overall when categorizing numerosity context sequences (Experiments 8b and 9) versus Arabic digit sequences (Experiment 8a).

Alternatively, if the participants were attending to magnitude using similar comparison processes for both symbolic and non-symbolic number representations, then categorization responses should be slower, the closer the context stimuli are to the benchmark value (5; distance effect) regardless of format. To investigate this, the median response times (RTs) for accurate categorization responses on mixed sequence types in Experiments 8a, 8b, and 9 were compared using a 3 (magnitude difference: 4 Diff, 3 Diff, 2 Diff) × 3 (Experiments 8a, 8b, and 9) mixed measures ANOVA treating magnitude difference as a within-participants factor, and experiment as a between-participants factor (see Table 7). The magnitude difference categories were created by collapsing the responses to the numbers 1/9 into an arithmetic difference category of 4, 2/8 into a difference category of 3, and finally, 3/7 into a difference category of 2. If a standard distance effect were present, then median RTs should increase as a function of the arithmetic difference from the benchmark.

This analysis revealed a significant main effect of magnitude difference, $F(2, 170) = 47.990$, $p < .001$, $\eta p^2 = .361$ where RTs increased as arithmetic difference decreased resulting in a significant linear trend $F(1, 85) = 90.138$, $p < .001$, $\eta p^2 = .515$. Furthermore, the experiment factor did not interact with magnitude difference ($p = .594$), and there were no between-participants differences in the RTs ($p = .648$). These findings suggest that participants did not rely on a slower, more effortful, enumeration strategy in Experiments 8b and 9, and used similar comparison processes regardless of the notation.

Table 7

Median RT values in milliseconds (ms) for correct context sequence categorization responses to each of the three arithmetical distances from the benchmark value (5)

	Magnitude Difference					
	4 Diff (1+9)		3 Diff (2+8)		2 Diff (3+7)	
Experiment 1a	606.76	(19.72)	618.76	(22.47)	647.30	(21.86)
Experiment 1b	593.86	(17.74)	615.87	(19.96)	645.40	(24.52)
Experiment 2	621.59	(16.59)	647.41	(19.15)	664.61	(20.93)
Total	607.40	(10.40)	627.35	(12.22)	652.44[a]	(13.05)

Note. RT = response times.

[a]All possible pairwise comparisons across collapsed magnitude differences were significant at a $p < .001$.

The second possibility is that notation switching (in Experiment 2) may have some unforeseen impact on attention, namely participants may have attended more to the switch, than the target's magnitude. If this were the case, a cross-notational switch from context to target should disrupt the efficiency with which the target's magnitude is processed thus reducing time-magnitude biases. This was examined in the next section.

Experiment 8a (number context-number target) and 9 (numerosity context-number target) simple effects test. To determine any possible costs of switching context sequence notation on time-number magnitude biases, the mean perceived duration ratings were submitted to a 2 (Target Magnitude: large, and small) × 3 (Sequence Type: large, mixed, and small) × 2 (Experiment: number context sequence [8a], numerosity context sequence [9]) mixed-measures ANOVA, treating Target Magnitude and Sequence Type as within-participant variables, and Experiment as a between-participant variable.

First, there was a standard time-magnitude bias, where small numbers were categorized as having shorter durations (M = .805 [.028]) than large numbers (M = .991 [.031]) $F(1, 56) = 73.842$, $p < .001$, $\eta p^2 = .569$. Second, there was a significant three-way target magnitude context sequence experiment interaction $F(2, 112) = 4.908$, $p = .009$, $\eta p^2 = .081$ (Figure 25). The remaining main effects and interactions were non-significant (remaining $p \geq .07$).

In deconstructing this three-way interaction, two simple effects tests were used, the first compared the mean duration ratings provided to large magnitude targets across the three context sequences for the two experimental conditions (number context vs. numerosity context) independently. In the second, large targets were compared between the two experimental conditions for each of the three context sequence types (large, mixed, and small) independently. It was clear that neither context notation (numerosity/number) nor context type (large, mixed, and small) had any impact on the time-magnitude bias elicited by small magnitude numbers (see Figure 25), as such they were excluded from the test.

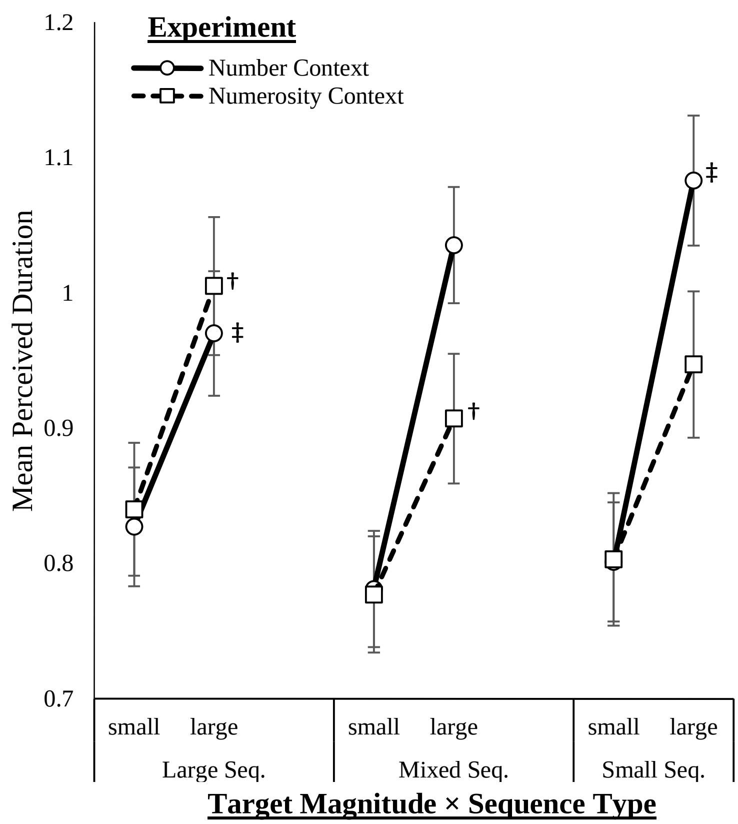


Figure 25. Three-way Target Magnitude (small, large) × Sequence Type (large, mixed, small) × Experiment (Number Context [8a] vs. Numerosity Context [9]) interaction. Error bars represent the standard error of the mean. † $p = .002$, ‡ $p < .001$.

In the first test, (a) a significant difference was found for targets that followed number context sequences (Experiment 8a) across the three context sequence types $F(2, 62) = 4.492$, $p = .015$, $\eta p^2 = .127$. Participants rated large targets to have longer durations when they followed small contexts (*M* = 1.083 [.051]), followed by mixed (*M* = 1.035 [.046]), and lastly large contexts (*M* = .970 [.051]). The difference between large and small contexts was significant $F(1, 31) = 16.877$, $p < .001$, $\eta p^2 =.353$; however, neither large ($p = .102$) nor small sequences ($p = .297$) differed significantly from mixed. Additionally, (b) a significant difference for targets following numerosity context sequences across the context sequence types was found $F(2, 50) = 4.555$, $p = .015$, $\eta p^2 = .154$, with the longest mean perceived duration occurring for targets following large sequences (*M* = 1.005 [.045]), then small (*M* = .947 [.05]), and finally mixed (*M* = .907 [.043]). The difference between large and mixed was significant $F(1, 25) = 12.603$, $p = .002$, $\eta p^2 = .335$, while the difference between large versus small ($p = .094$) and mixed versus small were not ($p = .281$).

In the second test, no difference was found in the mean perceived duration ratings for large numbers following large contexts, regardless of experimental condition ($p = .616$). Interestingly, large targets were rated to have shorter durations when they followed mixed numerosity contexts (*M* = .907 [.054]) versus number contexts (*M* = 1.035 [.043]) $F(1, 56) = 3.986$, $p = .051$, $\eta p^2 = .066$. Lastly, the difference between numerosity (*M* = .947 [.054]) and number (*M* = 1.083 [.048]) approached significance in the small context sequence condition $F(1, 56) = 3.546$, $p = .065$, $\eta p^2 = .06$. To summarize (a) a switch in the context sequence notation modified the context effect, a finding not in keeping with the ATOM framework’s suggestion of a common analogue code. (b) Categorizing mixed dot patterns context sequences may increase cognitive load overall compared to categorizing Arabic digits, and thus reduce the impact that 9 has on a subsequent target duration judgment. This is likely not the effect of using a different strategy for categorizing dot patterns, but likely the effect of the notation switch in Experiment 9.

General Discussion

Previous studies have demonstrated that magnitude information, regardless of its presentation format, induces systematic biases on perceptual judgments regarding time; however, the extent to which contextual manipulations will modulate these biases has yet to be fully investigated. The findings of current study are as follows: (1) on average, both large numbers and numerosities were rated as lasting for longer durations than small numbers and numerosities (a standard time-magnitude bias). (2) Context effects were evident, but were asymmetrical, occurring only for large magnitude targets, with the type of effect being determined by whether the context was presented in the same notation as the target, or a different notation. (A) When presented in the same notation (number or numerosity), larger magnitude values were rated to have longer durations when they followed small contexts versus large contexts. (B) When presented in different notations, larger values were perceived as shorter when following mixed sequences. In the case of point A, the effect could be interpreted as being supportive of the NAH (i.e., a reduction in subjective duration due to repetition suppression); however, the account cannot satisfactorily explain why a similar form of repetition suppression was not evident for small magnitude targets. We introduce a theory in the Contrastive anchoring and time–number distortions section that may be able to account for this asymmetry.

In the case of point B, there appears to have been a compounding effect of the higher cognitive load of the mixed-sequence categorization task, in addition to the notation switch (from numerosity to number) in Experiment 2, which may have contributed to a reduction in perceived target duration. In a recent meta-analysis on 117 time perception studies, it was found that across prospective timing paradigms an increase in cognitive load was associated with a decrease in the subjective-to-objective duration ratio (Block, Hancock, & Zakay, 2010), a finding which supports this theory. (3) A contrast effect was only evident for large numbers, and only evident when context and target were in the same notation, indicating the possible presence of separate mechanisms behind the representations of symbolic number and dot patterns. Additionally, the results of previous studies (Pariyadath & Eagleman, 2007) have shown that the decision-making processes regarding magnitude may contribute to time perception biases. Therefore, because in the current study, participants provided magnitude judgments to all context sequence numbers, it is possible that the presence of these magnitude judgments further contributed to the biases observed in the current study by influencing participants' duration judgments at a later decision-making stage.

Notation-Independence versus Notation-Dependence

An ongoing debate in the numerical cognition literature, touched upon in previous chapters, has focused on whether there are regions of the IPS that code for number using a notation-independent representation common across digits, number words, and dot patterns. One theoretical account holds that neurons in the parietal cortex respond generally to the property of ‘‘number,’’ and are insensitive to presentation format. Studies have found, for example, that the IPS plays a role in processing symbolic magnitude (e.g., Arabic digits and number words (Eger et al., 2003; Naccache & Dehaene, 2001; Pinel, Dehaene, Rivière, & Le Bihan, 2001) as well as non-symbolic (e.g., numerosity) magnitudes (Piazza et al., 2004). In an earlier study, it was found that the repetition of symbolic numbers (in Arabic digit format), elicited neural response suppression in the IPS (Naccache & Dehaene, 2001). Therefore, if this area constitutes a notation-independent analog system for the representation of number, it should demonstrate similar adaptation responses to repeated values, regardless of notation (dot patterns, symbols).

The neurophysiological data in support of this has been mixed. In one study, physiological neural adaptation was found despite changes to the numerical notation (from symbolic to non-symbolic) suggesting that these neural assemblies were coding the general property of number over-and-above presentation format (Piazza et al., 2007). By contrast, other studies have cast doubt on the conclusion that symbolic and non-symbolic numbers share a common abstract representation in the IPS. Shuman and Kanwisher (2004), for example, found no evidence of neural adaptation in regions of the IPS thought to be responsible for coding symbolic number when numerosities were repeatedly presented (see also, Cohen Kadosh & Walsh, 2009).

Contrastive Anchoring and Time-Number Distortions

As an alternative to the NAH and ATOM frameworks—which suggest that time-magnitude biases arise at representational stages of analysis, alternatively, time-number magnitude biases arise at a later decision-making stage and constitute a unique instance of the cognitive phenomenon known as anchoring (Newell & Shanks, 2014). It has been shown that many complex judgments can be influenced by the presence of irrelevant numbers, which are subsequently used as benchmarks when forming an estimate. In most anchoring studies, participants performed a numerical estimation on a subject they were not knowledgeable about (e.g., estimating the number of African countries in the United Nations). Anchoring was said to have occurred when people's ratings were biased in the direction of the number they were making the comparison against (e.g., more/less than "100" will generate a larger estimate than more/less than "10"). Interestingly, anchoring effects are highly robust, and occur even when one is aware that the numerical anchor was generated from a random external event, like a wheel-of-fortune style spin (Tversky & Kahneman, 1974), or generated internally by passively looking at the last two digits of one's social security number before making an estimate (Ariely et al., 2003).

Anchoring effects can occur even when participants are presented with incidental numerical information; for example, participants rated the performance of athletes with larger jersey numbers as better, and were willing to pay a higher price for food from restaurants with larger numbers in their name (e.g., "Studio 97" vs. "Studio 17"; Critcher & Gilovich, 2008). Furthermore, it is beginning to appear that anchoring is not limited to the use of numerical information, but can transfer across a wide variety of dimensions. For example, as previously noted, participants who illustrated longer (as opposed to shorter) line segments also estimated the Mississippi river to be longer, and estimated the average temperature of Hawaii to be higher (Oppenheimer et al., 2008).

While anchoring generally elicits biases on magnitude judgments in the same direction of the anchor (assimilative anchoring, see Chapter III), the reverse (contrastive anchoring), in which ratings or estimates are biased in the direction opposite the anchor, have also been demonstrated. Interestingly, the form of anchoring elicited is often determined by minor manipulations to the experimental task (Mussweiler, 2003). For example, if people judge their own attractiveness against a highly attractive standard (anchor), if the task enhances perceived similarities between the individual and the anchor, assimilative anchoring tends to occur (e.g., seeing oneself as more attractive); however, if differences are highlighted, contrastive anchoring is more likely to occur, and the person will rate themselves as less attractive (Brown, Novik, Lord, & Richards, 1992).

Frederick and Mochon (2012) demonstrated further evidence for contrastive anchoring (also referred to as *scale distortion*) for magnitude judgements performed on a numerical scale.
In their experimental task, the participants first estimated the weight of a wolf (which elicited estimates far smaller than 1,000 pounds) or performed a control task. Afterwards, they were presented with a list of 15 animals varying in weight from light (mouse) to very heavy (elephant) and were asked to select the animal closest to 1,000 pounds. Interestingly, if the participants had first estimated a wolf's weight (which had anchored them at a lower value), they were inclined to select an animal that was significantly larger than 1,000 pounds (mean = 2,170 pounds) as an exemplar of a 1,000-pound animal versus the control group (mean = 1,385 pounds).

In essence, by anchoring on a smaller magnitude in the context task (a wolf's weight), the test weight (1,000 pounds) felt subjectively greater than its true value. This finding is in keeping with the contrast effects demonstrated in Experiments 8a and 8b, in which large numerical targets (8, 9) were judged to longer when they followed small magnitude contexts. This, however, opens up another issue, why were no contrast effects evident for small target numbers (1, 2)? Why, for example, were they not perceived of as even shorter when they followed a large context? Objective, numerical scales (e.g., weight, height, and quantity) are scales that have meaningful zero points, while subjective scales (e.g., rating heaviness on a scale of 1–10) do not. Values that approach zero on an objective scale are likely to be less open to contextual influences (e.g., contrast), because their meaning is objectively associated with the lower-boundary limit of the scale (Mussweiler & Strack, 2000; Mussweiler, 2003). In the case of quantity, the numbers 1 and 2 are representative of the scale's lower boundary limit, while the numbers 8 and 9 simply represent relatively large quantities, which could further extend onwards from 10 to infinity.

We propose that because the large values were not associated with an absolute scale boundary, the interpretation of their relative magnitude was also open to contextual sources of bias (e.g., contrast). This theory will require further testing by using a larger range of values, for example, the number 9 should feel by contrast relatively large when it follows a series of 1's, 2's, and 3's but also relatively small in the presence of 51's, 52's, and 53's. As such, 9 should be perceived as shorter in duration in the second example.

Conclusion

Perceived time is impacted by the magnitudes of symbolic and non-symbolic numbers. Additionally, manipulations to the contextual information preceding the presentation of a target number can further modify its perceived duration. Only time will tell whether the impact of number magnitude on perceived duration is itself the result of an inadvertent mixing of analogue magnitudes across dimensions, or is the result of biases at the decision-making stage.

Chapter V: Dissertation Conclusion

The conclusion that our sense of time is largely inferred from the events that occur in our environment is hardly a new one (as discussed in-depth in Chapter I), however, the general idea that heuristics are fluidly applied, and used – at a preconscious level of analysis – to strategically guide our perceptual responses to the environment is still largely in its infancy. As noted by some researchers – the application of heuristics in the real world not only improves response efficiency, but is often associated with increased accuracy as well (Gigerenzer & Brighton, 2009), with the bounded rationality approach positing that our minds have evolved the capacity to fluidly exploit regularities in the environment in order to devise processing shortcuts (Gigerenzer & Todd, 1999; Todd & Gigerenzer, 2003). The concept of attribute substitution (Kahneman & Frederick, 2002, 2005) describes the mechanics through which perceptual biases arise across a wide array of decisions, tasks, and stimuli. In the current set of studies, I have proposed that difficult judgments regarding less accessible environmental properties – such as time – are more heavily weighted in favour of related properties that are more accessible.

The heuristic approach highlighted above is consistent with the recent Neural Reuse model (Anderson, 2014; Penner-Wilger & Anderson, 2008) touched upon in Chapter III, which posits that the neural architecture of phylogenetically older skillsets are subsequently adapted to serve novel (i.e., modern day) functions. The ATOM framework is largely consistent with this approach as it supports the notion that an evolutionarily older, manual action production system, has adapted to subserve the representations of space, time and number (Walsh, 2003), a conclusion supported as far back as Guyau (1890, 1988), who stated with respect to the existence of psychological time in consciousness:

> It [time] is there in the form of force, effort and also as intention, at least when the organism begins to realize what it wants, but even then time is completely imbedded in sensibility and motor action, and consequently it merges with space. (Guyau, 1890, 1988, p. 111).

In both theories, the proposition is that the mind is organized to fit the requirements of the environment, with the bounded rationality/attribute substitution approach focusing on how this is accomplished within the organism, across its own lifespan, as that organism learns to apply rule-sets in a top-down guided manner, based on experience; and the ATOM/Neural Reuse approaches focusing on how this is accomplished on an evolutionary scale. Therefore, the interactions observed between time and number could be driven by a more commonplace sharing of neural resources across the representations of space, time, number and intensity. This resource-sharing approach has also been adopted to explain within-dimensional interactions that involve variations in notation, and have included the suggestion of a common pool of cognitive resources shared for processing the magnitude of symbolic numbers, and physical (non-symbolic) quantities (Cantlon et al., 2009; Corbett, Oriet, & Rensink, 2006; S Dehaene et al., 1998; Fias et al., 2003, but see Rousselle & Noël, 2007). Alternatively, these biases may arise at the output (decision-making) stage, based on the substitution of information that is experientially related to duration (e.g., number/intensity). Further research will certainly be required to completely disentangle these approaches.

Overall, the current set of experiments largely indicated that the effect that number magnitude exerts on time perception is largely contextual. If judging an interval between the presentations of two successive numerical events, the absolute magnitude difference taken between those numbers is used as an index for interval duration. Furthermore, number magnitude exerts biases on sound intensity judgments that are similar to the effect they exert on perceived duration. Finally, numbers can convey a relative magnitude which is based on their meaning within a specific context. For example, if a large target number followed a series of small numbers (vs. large numbers), the degree of contrast from the context made that target feel even larger inducing a longer duration estimate. These suggest that number/time interactions are indeed flexible, and contextually modifiable, and therefore are likely top-down in nature.

While the current findings cannot disentangle ATOM/Neural Reuse and Bounded rationality/Attribute substitution approaches, the results are decidedly not in favour of the internal clock model interpretation. Firstly, In Chapter II it was found that the absolute magnitude of the numbers bounding the interval had no impact on interval duration, but rather, the magnitude difference was the deciding factor. If the internal clock model were correct, an interval separated by two large magnitude stimulus events (9-8) should have resulted in a longer duration versus an interval separated by two small magnitude events (1-2). In reality, they were perceived to be identical in duration. Secondly, in Chapter III, it was found that numbers exerted the exact same effect on sound intensity judgments that they have been repeatedly shown to have on duration estimates. Again, as noted by Block (2003), the only way to reconcile these kinds of analogous findings across dimensions is to rework the basic internal clock model format, into a more general internal "perceiving" model.

Finally, in Chapter IV, contrast effects were unique to large magnitude target numbers (8, 9). For example, 8 or 9 tended to be judged as *longer* in duration when following a series of small digits (1, 2, 3) versus large digits (7, 8, 9). The internal clock model predicts that similar oddball-type effects are driven by enhanced arousal caused by expectancy violations. This however seems extremely unlikely, as the measured contrast effect did not occur for small magnitude number targets. For example, 1 and 2 were not perceived of as any longer in duration when they followed large context sequences than when they followed small context sequences. If expectancy violations speed up the pacemaker's output, then they should do so regardless of the target's magnitude.

Across all three experimental paradigms there were findings that internal clock models are not equipped to explain, and as such, these models should no longer be applied as potential explanations for time perception biases and temporal illusions, or they should be extensively revised, or eliminated entirely. While our ability to sense time seems mysterious and intangible in relation to our other more concrete perceptual processes like that of spatial and numerical processing, an understanding of what drives temporal illusions can help further illuminate – not just our understanding of how our sense of time is created in the brain – but also the regularities that drive perception in general. Interestingly, and perhaps perplexingly, it is through studying illusions and biases elicited in the lab that we can come to understand how the application of heuristics can be so effective in a natural context.

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