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TIME AND CAUSALITY

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TIME AND CAUSALITY

Topic Editor:
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The problem of how humans and other intelligent systems construct causal representations from non-causal perceptual evidence has occupied scholars in cognitive science since many decades. Most contemporary approaches agree with David Hume that patterns of covariation between two events of interest are the critical input to the causal induction engine, irrespective of whether this induction is believed to be grounded in the formation of associations (Shanks & Dickinson, 1987), rule-based evaluation (White, 2004), appraisal of causal powers (Cheng, 1997), or construction of Bayesian Causal Networks (Pearl, 2000). Recent research, however, has repeatedly demonstrated that an exclusive focus on covariation while neglecting contiguity (another of Hume's cues) results in ecologically invalid models of causal inference. Temporal spacing, order, variability, predictability, and patterning all have profound influence on the type of causal representation that is constructed.

The influence of time upon causal representations could be seen as a bottom-up constraint (though current bottom-up models cannot account for the full spectrum of effects). However, causal representations in turn also constrain the perception of time: Put simply, two causally related events appear closer in subjective time than two (equidistant) unrelated events. This reversal of Hume's conjecture, referred to as Causal Binding (Buehner & Humphreys, 2009) is a top-down constraint, and suggests that our representations of time and causality are mutually influencing one another. At present, the theoretical implications of this phenomenon are not yet fully understood. Some accounts link it exclusively to human motor planning (appealing to mechanisms of cross-modal temporal adaptation, or forward learning models of motor control). However, recent demonstrations of causal binding in the absence of human action, and analogous binding effects in the visual spatial domain, challenge such accounts in favour of Bayesian Evidence Integration.

This Research Topic reviews and further explores the nature of the mutual influence between time and causality, how causal knowledge is constructed in the context of time, and how it in turn shapes and alters our perception of time. We draw together literatures from the perception and cognitive science, as well as experimental and theoretical papers. Contributions investigate the neural bases of binding and causal learning/perception, methodological advances, and functional implications of causal learning and perception in real time.

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EDITORIAL
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Time and causality: editorial

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Keywords: time perception, causality, causal inference, temporal adaptation, cognitive development, multimodal integration

It is my great pleasure to be able to introduce the research topic on Time and Causality. The topic had been hosted simultaneously on Frontiers in Perception Science and Frontiers in Cognitive Science. Doing so acknowledged that the human experiences of Time and Causality mutually constrain each other, and attracted high-quality submissions from a wide range of authors who might previously not have published in the same outlet.

The majority of research on Time and Causality in previous decades investigated how temporal information constrains causal inference (for an overview see Buehner, 2005). More specifically, such research is rooted in David Hume's assessment that causal knowledge must be inferred from non-causal input, in a manner where empirical cues of contingency, contiguity, and temporal priority elicit causal impressions in a bottom-up manner (Einhorn and Hogarth, 1986; Buehner and May, 2002). The first half of this volume includes articles from this tradition. Greville and Buehner (2012) pick up on the well-established finding that degrading cause-effect contiguity leads to concomitant decrements in causal learning. Their contribution asked whether the extent to which causal inferences are adversely affected by delay is related to temporal discounting, the phenomenon whereby rewards lose value over time. If causal learning is drawing on principles of associative learning (cf. Dickinson, 2001), then it would be reasonable to find such commonalities; Greville and Buehner (2012), however, do not evidence for such commonalities. Msetfi et al. (2012) revisit a classic phenomenon in covariation-based causal learning: Depressive Realism—the finding that dysphoric individuals appear to have a more realistic impression of the (absence of) cause-effect contingencies. In their contribution, Msetfi et al. (2012) show that dysphoric individuals are particularly sensitive to temporal shifts in contingency, i.e., momentary changes of action-outcome effectiveness.

Rankin and McCormack's (2013) is the first of two developmental articles in the volume and clarifies previously ambiguous or contradictory evidence regarding the understanding of the temporal priority principle—that causes must precede their effects. With improved and standardized methods, Rankin and McCormack (2013) find that even 3 year olds are sensitive to this principle, but also that there is developmental progression toward more consistent application of it. Schlottmann et al.'s (2013) contribution is from the domain of perceptual causality, concerning visual stimuli that lead to immediate and compelling impressions of causality, despite the impoverished nature of the stimuli. Schlottmann et al. (2013) examined the developmental progression of the distinction between physical and social causality, and find that spatio-temporal cues play an important role in making this distinction. Woods et al. (2012) also examined perceptual causality and its sensitivity to spatio-temporal manipulations. They find that context and prior experience heavily influences people's sensitivity to temporal as well as spatial violations of causal expectations.

The second block of articles represents research inspired by relatively recent efforts to examine how causal knowledge influences our perception of time. Temporal binding (Haggard et al., 2002) refers to the subjective shortening of time that occurs when a cause is followed by its effect (as opposed to an unrelated event), and/or subjective shifts in event perception whereby causes and effects mutually attract each other, resulting in delayed awareness of the former, and early awareness of the latter. Faro et al. (2013) open this section with a review of recent literature in this area. Moore et al. (2013) provide further evidence of temporal causal binding from merely observed actions, and argue that causal binding receives a boost when the cause is perceived to be an intentional action. Their study provides an important methodological improvement over previous work because it offered better control over the perceptual stimuli. Moore et al. (2013) also provide fMRI data that suggests that the intentionality/causality interaction is subserved by similar brain regions as those involved in agency. Rohde and Ernst (2013) demonstrate that temporal adaptation is symmetrical. People adapt to action-outcome sequences such that the point of subjective simultaneity (PSS) of action and outcome shifts forward following exposure to action—delay—outcome sequences. Importantly, when—in a clever experimental setup—participants experienced outcome—delay—action sequences, the PSS analogously shifted backwards. While at first this might appear to violate the causal asymmetry, this result actually fits with the unity assumption inherent in Bayesian accounts of perception. Parsons et al. (2013) challenge an internal-clock based interpretation of temporal causal binding and instead make a convincing case for a realignment of the sensory and motor timeline. Asai and Kanayama (2012, 2013) conclude the volume with a contribution on the cutaneous rabbit effect (CRE), a tactile illusion resulting from a causal interpretation of spatio-temporal stimulation of the skin. Asai and Kanayama (2012, 2013) show that the CRE is modulated by visual stimuli, when these "fit" with the causal interpretation of the experienced spatio-temporal pattern.

In sum, this volume is testament to convergence of research on time perception and causal inference, in two ways: Firstly, as the two thematic blocks of articles show, there is now a clear recognition that Time and Causality mutually constrain each other in human experience. Not only do temporal parameters influence our causal experience, but the construal of causal relations in the mind also affects the way we perceive and experience time. Importantly, the volume also highlights the convergence of methods and disciplines that is happening in this area. Time and Causality are now firmly on the agenda of cognitive, developmental, social, clinical, and applied psychologists, perception researchers and psychophysicists, as well as neuroscientists and philosophers. Future questions include what exactly the relation is between time, causality, and agency, and to what extent they share common neural markers, how perceptual adaptation relates to the experience of agency, causality, and temporal order, and how extant models of time perception (i.e., internal clocks) relate to causality-induced shifts in time perception.

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Assessing evidence for a common function of delay in causal learning and reward discounting

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Time occupies a central role in both the induction of causal relationships and determining the subjective value of rewards. Delays devalue rewards and also impair learning of relationships between events. The mathematical relation between the time until a delayed reward and its present value has been characterized as a hyperbola-like function, and increasing delays of reinforcement tend to elicit judgments or response rates that similarly show a negatively accelerated decay pattern. Furthermore, neurological research implicates both the hippocampus and prefrontal cortex in both these processes. Since both processes are broadly concerned with the concepts of reward, value, and time, involve a similar functional form, and have been identified as involving the same specific brain regions, it seems tempting to assume that the two processes are underpinned by the same cognitive or neural mechanisms. We set out to determine experimentally whether a common cognitive mechanism underlies these processes, by contrasting individual performances on causal judgment and delay discounting tasks. Results from each task corresponded with previous findings in the literature, but no relation was found between the two tasks. The task was replicated and extended by including two further measures, the Barrett Impulsiveness Scale (BIS), and a causal attribution task. Performance on this latter task was correlated with results on the causal judgment task, and also with the non-planning component of the BIS, but the results from the delay discounting task was not correlated with either causal learning task nor the BIS. Implications for current theories of learning are considered.

Keywords: causal learning, delay discounting, reinforcement delay, subjective reward value, utility

INTRODUCTION

The role of time is central to learning and behavioral processes. The precise temporal arrangements of when we perform actions, when the consequences of those action manifest, and when other events occur alongside these, can have a profound influence on the way in which such events are interpreted. Researchers in fields as diverse as neurology, computer science, and psychotherapy have long been interested in the ways in which our behavior is sensitive to time, and which psychological processes and underlying neurological structures govern such activity.

Reinforcers or rewards are stimuli that elicit a change in the behavior of an organism. Though virtually any stimulus has the potential to reinforce behavior, the typical conception of a reward is that which has a particular motivational significance or adaptive value to the organism, such as food. Rewards can in many cases be quantified (for instance, the volume of food received) and in this regard have an objective value. As one might expect, animals exhibit preference for larger rewards over smaller rewards. However, depending on the current situation (such as the animal's level of deprivation) the reward may also have a subjective value that differs from its objective magnitude. A factor of crucial importance in determining the subjective value is the time when a reward is received. Naturally, immediate rewards are preferred to delayed rewards, when the rewards are of equal magnitude; however, numerous studies have demonstrated that in certain cases, animals will choose a smaller, immediate reward over a larger, delayed reward. If we assume that the animal always selects the reward which it perceives has the greater value, then we may conclude that the subjective value of a reward declines with delay. Delays of reinforcement thus result in the objective value of the reward being discounted, hence the term delay discounting is used to describe this process.

The rate at which rewards are discounted as the delay increases varies between individuals. Those for whom the value of rewards declines steeply with delay are often identified as impulsive, since their routine preference for rapid reinforcement implies an inability to delay gratification in order to receive a larger reward. Studies have found differences in the rate of discounting between different age groups (Green et al., 1994, 1999) and cultures (Du et al., 2002). However, the general shape of the discounting function tends to be the same across individuals. A considerable effort has been made by a number of researchers (Mazur, 1987; Rachlin et al., 1991) to identify the mathematical relation that best describes the relationship between the delay until a reward is received and its subjective value. Initial work found that both an exponential decay function, $V = Ae^{-kD}$, and simple hyperbola, $V = A/(1 + kD)$, provided reasonable fits to discounting data, where V is the current subjective value, A is the nominal amount of the reward, D is the delay to reward, and k is a free parameter, representing the steepness of the discounting function. Myerson and Green (1995) concluded that the function most closely mapping how subjective value changes with delay is a hyperbola-like function with the addition of a scaling parameter: $V = A/(1 + kD)^s$, where the exponent s represents the non-linear scaling of amount and time; in other words, s has the effect of causing the curve to decline more slowly at long delays.

Obtaining a reliable measure of discounting can be problematic because of the lack of consensus over the mathematical function best suited to fit discounting data, and the difficulty involved in estimating the parameter k. To address this, Myerson et al. (2001), proposed the novel measure of obtaining the area under the curve (AUC) of the empirical discounting function. For this to be calculated, the points on a plot of the function are connected using straight lines and the area below the line can then be obtained using a fairly simple calculation. Further details of this procedure are provided in the Section "Materials and Methods" of this paper. AUC provides a simple, parameter-free measure of discounting that is not tied to a specific theoretical framework. It has the advantage of being applicable to individual or group data, and furthermore allows for direct comparison of discounting rates, whether between individuals or across tasks involving different amounts of reward or delay.

Delays also play a central role in conditioning, appearing to interfere with the acquisition process, with behavior taking longer to establish (Wolfe, 1921; Solomon and Groccia-Ellison, 1996) and being diminished either in magnitude or in rate (Williams, 1976; Sizemore and Lattal, 1978). Plots of the decline in response rate against time reveal similarly negatively accelerated functions as for delay discounting. Chung (1965) found in a signaled-delayed-reinforcement task that pigeons' response frequencies declined exponentially as a function of the delay interval. Other work (Herrnstein, 1970; Mazur, 1984) suggests that hyperbolic functions more accurately describe the trends in response data with delays. As with discounting, there is a lack of consensus regarding the precise shape of the function describing how response rates decline with delay. However, it is generally agreed that the relationship may be broadly described as a negatively accelerated decay function. A commonality between the process of temporal discounting and associative learning may thus be identified, raising the possibility that the two processes may have a shared cognitive basis. Indeed, some researchers (Dickinson et al., 1984; Dickinson, 2001) posit that many aspects of what is commonly referred to as higher-level human learning and cognition are fundamentally governed by simple associative mechanisms. Others adopt the viewpoint that processes such as induction and reasoning are based on more complex computational (e.g., Cheng, 1997) or symbol-manipulating (e.g., Holyoak and Hummel, 2000) architectures. However, such processes are still subject to the effects of time, as shall now be discussed.

Causal inference is the process by which we come to learn that an event has the capacity to produce or otherwise influence another event. Acquiring the knowledge that one event leads to another is fundamental not only to understand why events occur, but to direct our own behavior to intervene on the world and bring about desired outcomes. Causal inference is referred to as such because we cannot directly perceive a causal relation, and causality must therefore be inferred from the observable streams of evidence that are available to us. Hume (1888) identified three cues to causality: temporal precedence, contingency, and contiguity. To elaborate, causes must precede their effects, be followed by their effects with sufficient regularity, and be closely coupled in time (and space) with those effects.

Time is therefore a bedrock of causal induction according to the Humean doctrine, with contiguity essential for learning to take place. Initial research, approaching causal induction from an associative learning perspective, indeed supported this view. Shanks et al. (1989) found that in judging contingency between pressing a button and a triangle illuminating on a computer screen, human participants were unable to distinguish conditions involving delays of 4 s or greater from non-contingent conditions where the probability of the outcome was just as likely in the presence and absence of the cause. Such findings appear puzzling since both humans and animals demonstrate the ability in a variety of tasks to learn delayed causal relations. Recent research has demonstrated that there are a number of factors mitigating the effects delay such as prior knowledge or previous experience and resultant expectation (Einhorn and Hogarth, 1986; Buehner and May, 2003, 2004), awareness of causal mechanism (Buehner and McGregor, 2006), or structural information in the environment (Greville et al., 2010). Nevertheless, it is generally recognized that delays create difficulties for causal induction and that all other things being equal, a reasoner is more easily able to identify contiguous causal relations than those involving a delay. Studies such as those of Shanks et al. (1989; see also Shanks and Dickinson, 1991) show that causal ratings do tend to follow a pattern of decline with time that is similar to the decline of response rates in reinforcement learning with animals, with a sharp fall in ratings from immediate to delayed causal relations, with the steepness of the curve easing and flattening as delays extend.

Thus, there is a common effect of delays in associative learning, causal induction, and delay discounting. While it may be a stretch to posit that they are all essentially the same cognitive process, it seems reasonable enough to suggest that the way by which delays are recognized, interpreted, and represented may involve a common mechanism that forms a crucial part of all these processes. The effects of delay may vary from person to person, and from task to task, but it seems plausible that if delays are interpreted via a stable underlying process, then there should be some perceptible pattern in the way in which delays generally affect the behavior of an individual. Having then identified a common cognitive contribution of delay across learning processes, we now turn to consider evidence of how delays may be represented from a neurobiological perspective, and whether these processes all involve a common region of the brain that may be the site of temporal processing.

While the effects of reinforcement delay on behavior have been extensively studied, the neurobiological basis of such effects has received comparatively less attention (Evenden, 1999). However, it is well-established that the hippocampus plays an important role generally in learning and memory. Solomon et al. (1986) demonstrated that an intact hippocampus is required for trace conditioning but not delay conditioning in rabbits[1]. Beylin et al. (2001) demonstrated that hippocampal lesions in rats also impair delay conditioning when a longer inter-stimulus interval is used. This suggests that the hippocampus plays a role in the formation of associations between temporally discontiguous stimuli.

Bangasser et al. (2006) postulated that the hippocampus was responsible for forming an active representation of the CS that could then be associated with the US. Using a novel "contiguous trace conditioning" (CTC) paradigm, where the standard trace conditioning preparation was modified by representing the CS simultaneously with the US following the trace interval, Bangasser et al. demonstrated that hippocampal-lesioned rats could successfully condition with this procedure. Related findings by Woodruff-Pak (1993) concerning the patient HM, were interpreted by Bangasser et al. as evidence that existing association between the stimuli (as a result of previously experienced temporal contiguity) is required for trace conditioning with hippocampal damage. They speculate that the function of the hippocampus in conditioning is to bind stimuli that do not occur together in time.

Cheung and Cardinal (2005), however, obtained results that appear to directly oppose those of the above studies. In an action-outcome (i.e., instrumental) learning task, hippocampal-lesioned animals actually became better at learning (relative to shams) as the delay between action and outcome increased. Cheung and Cardinal explain this effect by suggesting that normal hippocampal function promotes the formation of context-outcome associations. In instrumental conditioning then, context-outcome associations compete with and thus hinder learning of response-outcome associations, so a disruption of contextual processing via hippocampal lesion will improve learning with delayed outcomes. Meanwhile during classical conditioning the CS may be considered part of the context and thus the reverse effect is obtained. In yet a further twist, Cheung and Cardinal found that the same lesioned animals were also poorer at choosing a delayed larger reward over an immediate smaller reward – despite their apparently superior ability at learning the predictive relationship between action and outcome when delays were involved. In other words, lesioned animals made more impulsive choices relative to shams.

Similar findings were obtained by McHugh et al. (2008) using a T-maze task. Rats chose between the two goal arms of a T-maze, one containing an immediately available small reward, the other containing a larger reward that was only accessible after a delay. Hippocampal lesions reduced choice of the larger delayed reward in favor of the smaller immediately available reward. McHugh et al. advanced the argument that the hippocampus assists normal temporal processing by acting as intermediate memory store that allows animals to associate temporally discontiguous events, and that insertion of a delay into tasks will result in abnormal performance in animals with hippocampal damage.

In summary then, the hippocampus has been implicated both in the process of choice between delayed rewards, and in conditioning processes. While the empirical evidence does not precisely elucidate the role of the hippocampus, there is clear indication that it is involved in processing temporal and contextual information. Specifically, the temporal processing that appears to be a necessity for trace conditioning or the delay of gratification to receive a larger reward is hippocampal-dependent. Thus, it seems logical to query whether both processes appeal to the same neural mechanism, and thus whether there may be a common process by which delayed rewards lose their subjective value and associative strength or impression of causality declines with delay.

Having reviewed a number of behavioral and biological findings, there seems to be mounting evidence that the processes of reinforcement learning and intertemporal choice behavior may well share a common foundation. We investigated the behavioral evidence that could lend credence to a hypothesis of shared function. More specifically, we pursued an individual differences approach, where we related an individual's performance in a standard causal learning task to their degree of temporal discounting to ascertain whether the two are correlated. It seems that whatever the outcome, there may be important implications for our understanding of timing behavior, in particular with regard to providing a unified theory of learning.

EXPERIMENT 1

Our goal for the first empirical study was to contrast behavior at the individual level on two well-established paradigms. Each participant completed two studies, a causal judgment task and a delay discounting procedure. It is important here to note that that the former, although an instrumental task, was evaluative rather than performance-based. In a typical instrumental performance task, the outcome has some appetitive value; such as a food reinforcer in animal reinforcement learning, or scoring points in a simple game context (Shanks and Dickinson, 1991) for tasks with human participants. Such tasks can often be complicated by the payoff matrix – that is the benefit of the outcome compared to the cost of responding. A causal judgment task meanwhile is free from such complications; the outcome is not assigned a particular value and the participant is given no motivation to try and make the outcome occur as much as possible. Rather, participants are simply given time to investigate and evaluate the causal relationship between response and outcome, selecting their own response strategy and providing a declarative judgment of contingency. Employing such a task thus enabled us to probe causal learning in an uncompromised manner.

MATERIALS AND METHODS

Participants

Ninety-one undergraduates from Cardiff University, 28 males and 63 females, with an average age of 20 years, volunteered to participate as part of a practical class. Participants did not receive any payment for participation. Due to computer malfunction, data for two participants was lost for the delay discounting task.

Design

The experiment consisted of two components, a causal judgment task, and a delay discounting task. The causal judgment task manipulated the independent variables contingency (or more accurately $P(e|c)$, the probability of an outcome given a response),

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[1] It is worth mentioning here that while trace conditioning involves a delay (trace interval) separating CS and US, counterintuitively, delay conditioning does not; CS either follows immediately or co-terminates with US. The "delay" in the term refers to that between CS onset and US onset.

and delay between response and outcome. Two levels of contingency (0.50 and 0.75) were factorially combined with three levels of delay (0, 2, and 5 s) to produce six experimental conditions, each of 120 s duration, in a 2 × 3 within-subjects design. With condition order counterbalanced across participants. The dependent measure was the causal rating (0–100) provided by participants at the end of each condition.

The delay discounting task combined two levels of the factor reward (£200 and £10,000) with seven levels of the factor delay (1 month, 3 months, 9 months, 2, 5, 10, and 20 years) in a 2 × 7 within-subjects design. The dependent measure in each case was the point of subject equivalence (see below for how this was determined). Taken together, the points of subjective equivalence at each level of delay (for a given reward amount) yielded the AUC (again see below) which was the main dependent measure we used.

Apparatus and procedure

The two tasks were programmed using Python version 2.4.1 for the causal learning task and E-Prime version 2.0 from Psychology Software Tools for the delay discounting task. Each participant used a PC running Windows XP with a 19" LCD widescreen display, using a standard mouse and keyboard to input responses. The experiment was conducted in a small computer lab, with participants seated at individual workstations which were screened off from each other.

The causal judgment task was closely modeled on Shanks et al.'s (1989) study. For each condition, an outline of a triangle was displayed on the computer screen and beneath this a button which could be pressed by clicking on it with the mouse. Participants engaged in a free-operant procedure (FOP), where they were permitted to respond at any point and as often as they wished, with each response subjected to the reinforcement schedule. Every press therefore had the specified probability (either 0.5 or 0.75) of generating an outcome. If an outcome was scheduled, the triangle illuminated (the gray background became red and a "glow" effect appeared around the outline) for 250 ms following the programmed delay (either 0 s, i.e., immediately, 2 or 5 s). For all conditions, the triangle also illuminated unprompted once every 10 s period at a random point within that 10 s period – in other words, the first such background effect could occur at any time between 0 and 10 s, the second between 10 and 20 s, and so on. These random background effects were included to add a degree of uncertainty as to whether a given outcome was indeed generated by a response made by the participant or due to unseen alternate causes, thus making the task non-trivial. Each condition lasted for 2 min, at the end of which participants were asked "how effective is pressing the button at causing the triangle to light up?" and instructed to provide a rating from 0 to 100.

The delay discounting task was essentially a replication of Du et al.'s (2002) experiment. Combination of the factors amount and delay provided 14 different conditions, presented in a different random order for each participant. On-screen instructions and three practice trials were presented prior to beginning the experiment. It was made clear to participants that the amounts of money were hypothetical and they would not receive any real money for participating in the study. Each condition comprised seven choices or trials. For a given trial, participants were presented with two boxes, one containing the smaller, sooner (SS) reward and one containing the larger, later (LL) reward, and required to indicate which of these rewards they would prefer to receive. The left-to-right placement of the two rewards was randomized from trial to trial. Participants pressed Q or P on the keyboard to select the left or right reward respectively.

The value of the LL reward was always fixed at the specified amount of either £200 or £10,000, and the time until its receipt was one of the seven delays. The SS reward could be obtained "now," and its value changed from one choice to the next. For the first choice within each condition, the value of the immediate reward was half that of the delayed reward (e.g., £5000 now vs. £10,000 in 10 years). If the SS reward was chosen, its value was decreased on the subsequent choice; if the LL was preferred, the value of the SS was increased. The amount by which the SS was adjusted was half of the difference between the two rewards (i.e., £2500 in the above example). Thus if the participant chose £5000 now, the next choice would be between £2500 now and £10,000 in 10 years; if they chose the £10,000, the next choice would be between £7500 and £10,000. The amount of the adjustment was rounded to the nearest integer. This "titration" procedure was designed to converge on the subjective value of the LL reward. The subjective value was calculated as the mean of the last immediate reward that had been chosen and the last immediate reward that had been rejected.

To calculate the AUC, we normalized delay and subjective value by expressing each delay as a proportion of the maximum delay (20 years, i.e., 240 months), and each subjective value as a proportion of the nominal amount (i.e., £200 or £10,000). These proportions were then used as x and y coordinates respectively to graph each individual's discounting function. Connecting the individual data points using straight lines effectively divides the graph into a series of trapezoids, with the sum of the areas of all the trapezoids providing the total AUC. These areas can be calculated without actually constructing the graph, by using the simple formula: $(x_2 - x_1) \times [(y_1 + y_2)/2]$ for each trapezoid, where x_1 and x_2 are successive delays and y_1 and y_2 are the corresponding subjective values. For the first trapezoid, the values of x_1 and y_1 are 0 and 1 respectively. Since the x and y values are proportions, the maximum AUC is 1 (i.e., no discounting) with smaller values representing steeper discounting.

RESULTS

Causal judgment task

Figure 1 shows mean causal ratings for all six conditions in the causal judgment task. As expected, ratings were considerably higher at $P(\mathrm{e|c}) = 0.75$ than at $P(\mathrm{e|c}) = 0.50$. Also in accordance with our expectations, ratings declined as the delay between cause and effect increased. A 2 × 3 repeated-measures ANOVA confirmed significant main effects effect of contingency, $F(1, 90) = 39.69$, $p < 0.001$, $MSE = 470$, $\eta_p^2 = 0.306$ and delay, $F(2, 180) = 52.61$, $p < 0.001$, $MSE = 640$, $\eta_p^2 = 0.369$ as well as a significant interaction between contingency and delay, $F(2, 180) = 4.12$, $p < 0.05$, $MSE = 437$, $\eta_p^2 = 0.044$ Closer inspection of **Figure 1** reveals that the difference between judgments at $P(\mathrm{e|c}) = 0.75$ and $P(\mathrm{e|c}) = 0.50$ was noticeably greater with 0 s delay than with 2 or 5 s, which is likely the driving force behind the significant interaction. The main effects are in accordance with

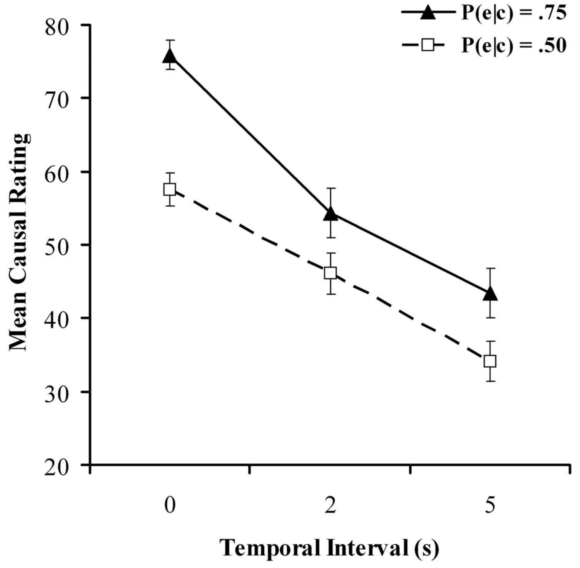

FIGURE 1 | Mean causal ratings for all conditions in the causal judgment task as a function of temporal delay for the causal judgment task in Experiment 1. Filled and unfilled symbols refer to $P(e|c)$ values of 0.75 and 0.50 respectively.

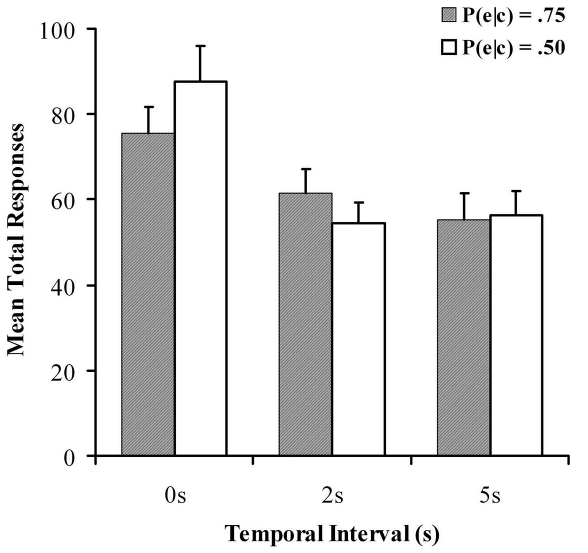

FIGURE 2 | Mean total responses for all conditions in the causal judgment task as a function of temporal delay for the causal judgment task in Experiment 1. Filled and unfilled bars refer to $P(e|c)$ values of 0.75 and 0.50 respectively.

several previous findings in the literature; a similar interaction meanwhile was also found by Shanks et al. (1989) when contrasting experimental conditions with $P(e|c) = 0.75$ against control conditions with $P(e|c) = 0$. Although our study instead used values of 0.75 and 0.5, this finding is broadly consistent with the idea that delays make it harder to recognize and differentiate between objective contingencies and that contingency and contiguity act in concert to influence perception of causality (Greville and Buehner, 2007).

Mean response rates per minute are reported in **Figure 2**, and a 2 × 3 within-subjects ANOVA was again used to examine differences between conditions. An analysis of response rates found no significant effect of contingency, $F(1, 90) = 0.483$, $p = 0.489$, $MSE = 1118$; however there was a main effect of delay on response rates, $F(2, 180) = 28.582$, $p < 0.001$, $MSE = 1305$, $\eta_p^2 = 0.241$ with fewer responses emitted during the delayed conditions, in line with existing findings (Reed, 1999; Buehner and May, 2003). The implication is that participants withhold further responding until the consequences of their actions are revealed, leading to fewer responses with longer delays. There was also a significant contingency × delay interaction, $F(2, 180) = 3.973$, $p < 0.05$, $MSE = 10597$, $\eta_p^2 = 0.042$ response rate with 0 s delay was significantly greater at $P(e|c)$ of 0.5 than at 0.75. These extra responses could account for the interaction observed in the causal ratings, in line with a negative outcome density effect. In an appetitively neutral task such as this however, response rate may not indicate much about causal beliefs and it is the causal judgment that should be focused on as the critical measure.

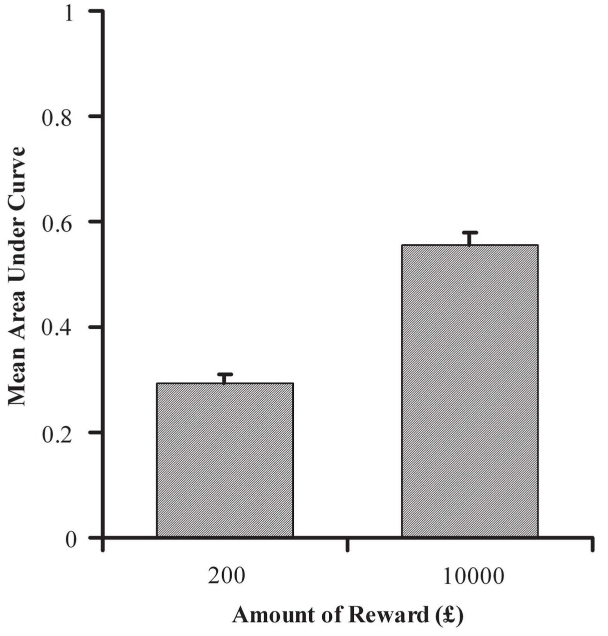

FIGURE 3 | Mean area under the curve (AUC) as a function of delayed reward amount for the delay discounting task in Experiment 1. AUC is calculated from participants' point of indifference at combinations of delay extent and value of immediate reward.

Delay discounting task

Using the points of subjective equivalence, the AUC was calculated at each amount of reward for all individual participants, as specified in the Section "Materials and Methods". Mean AUC for £200 and £10,000 are shown in **Figure 3**. AUC was significantly greater with delayed rewards of £10,000 than £200, $t(88) = 12.138$, $p < 0.001$, indicating that discounting was less severe with the larger reward and thus replicating established findings (e.g., Green et al., 1994). Although individuals tended to discount smaller amounts more steeply, they were consistent in the manner of their discounting across reward amounts, with a strong positive correlation between an individual participant's AUC at £200 and £10,000, $r = 0.541$, $n = 89$, $p < 0.001$. This supports the idea that individual discounting functions at different reward amounts differ by a scaling factor, rather than by any qualitative difference in function shape.

Cross-task comparisons

We created a novel metric for each participant that allowed us to relate their performance on the causal judgment task to their individual level of delay discounting task. More specifically, we represented the manner in which an individual's perception of causality declined with delay in a fashion analogous to AUC: we expressed each rating and each delay as a proportion of the maximum (100 and 5 s respectively). This allowed calculation of the AUC in the same manner as described for the delay discounting task, separately for judgments at $P(e|c) = 0.75$ and 0.50 as for £200 and £10,000. Henceforth we distinguish between the two measures using the terms AUC_c (for the causal judgment task) and AUC_d (for the discounting task).

AUC_c was significantly greater at $P(e|c)$ of 0.75 than 0.5, indicative of the higher ratings attracted by the stronger contingency. There was however a strong significant positive correlation between individuals' AUC_c at $P(e|c)$ of 0.75 and that at 0.50, $P(89) = 0.546$, $p < 0.001$, much like that between AUC at £200 and £10,000 in the discounting task. Once again this demonstrates that delays affected individuals' perception in a consistent manner, with causal judgments being devalued similarly within individuals across both levels of contingency. Males evaluated delayed causal relations more favorably than did female participants, both at $P(e|c) = 0.75$ (mean AUC_c were $M = 0.60$ and $M = 0.53$ for males and females respectively) and $P(e|c) = 0.50$ ($M = 0.49$ and $M = 0.43$), yet discounted delays slightly more steeply than females at both £200 ($M = 0.28$ and $M = 0.30$) and £10,000 ($M = 0.53$ and $M = 0.56$). However, none of these differences reached statistical significance (all $ps > 0.1$). The remainder of the analysis therefore collapses across gender.

Since representing the delay-induced decline in causal judgments is not an established standard, we also calculated the ratios of the delayed to the immediate scores; specifically, ratings at 2 s over ratings at 0 s and ratings at 5 s over ratings at 0 s, for both $P(e|c) = 0.75$ and 0.50. This gave four individual ratios, plus a mean ratio across levels of contingency. This provided several bases of comparison to the AUC_d from the discounting task. **Figure 4** shows a plot of all individual participants' mean AUC_d against their mean AUC_c. There was no correlation between these two scores, $r = -0.114$, $n = 89$, $p = 0.289$, nor was there a correlation between mean AUC_d and mean ratio of delayed to immediate judgments, $r = 0.026$, $n = 89$, $p = 0.809$. There was likewise no correlation between ratio and AUC_d for any of the possible eight comparisons between the AUC_d at the two nominal values and the four ratios (all $ps > 0.2$).

We set out to examine the relationship between delay discounting and delay-impaired causal judgment, aiming to identify whether individuals devalue delayed rewards in the same manner as they appraise delayed causal relations. Overall, our results replicate several well-established findings in the literature: both contingency and contiguity substantially impact perceived causality, with causal judgments increasing in line with the proportion of responses generating outcomes, and declining with response-outcome delay (Shanks et al., 1989); and larger rewards being discounted less steeply with delay than smaller rewards (Raineri and Rachlin, 1993). We also found good consistency at the individual level both between discounting functions at different amounts, as well as coherence between trends of delay-induced decline at different contingencies. We can thus have confidence both in the reliability of our paradigms and the measures adopted. However, of principle interest was whether a relationship existed between individual discounting function and evaluations of delayed causal relations. No such correlations were found with any of the comparisons we applied. The implication therefore is that the effect of a delay on an individual's perception of causality is not related to the rate at which a reward loses its subjective value with delay. An extension of such a conclusion is that the discounting of a delayed reward is not based on an inability to identify or recognize a causal agent or mechanism that may be responsible for generating this delayed reward.

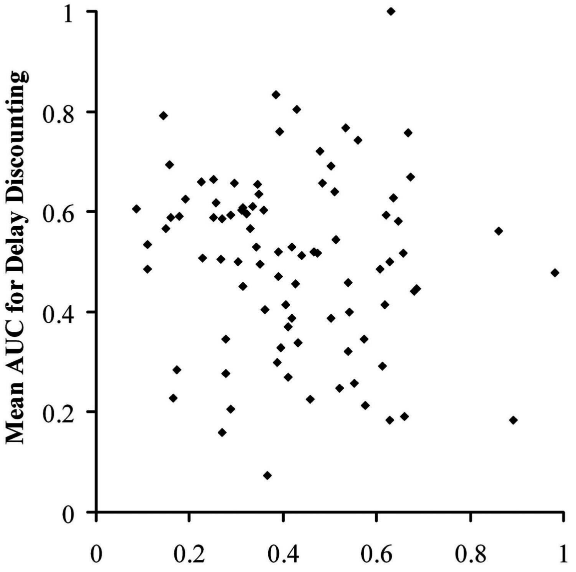

FIGURE 4 | Scatter plot of area under the curve for the discounting task against mean area under the curve for the causal judgment task for individual participants in Experiment 1.

EXPERIMENT 2

The failure to obtain a significant correlation in the first experiment is a result that is somewhat difficult to interpret. This could be taken as evidence that causal learning and choosing between concurrently available rewards are distinct processes, and do not share a common temporal processing mechanism. However, such a contention comes from the unenviable position of arguing from the null. Further investigation is therefore required. In the first instance, it seems most prudent to attempt to replicate these two studies and see if the same effect (or rather lack thereof) persists. At the same time, other measures can be introduced which may provide additional insight as to whether causal learning and discounting processes are in some way allied.

The following experiment then reprised effectively the same causal judgment and delay discounting tasks of the previous experiment (though both were streamlined as described in the Section "Materials and Methods"). In addition, two further tests were administered to participants. The first was a causal attribution task, in which participants had to select the true cause (or most likely cause) from three concurrently available causal candidates (further details will follow in the Section "Materials and Methods"). Performance in this task was used to compute a single metric that could be contrasted with the AUCs obtained from the causal judgment and discounting tasks. The second was version 11 of the Barrett Impulsivity Scale (BIS-11; Patton et al., 1995), a popular measure for assessing impulsive personality traits. Measures from all four tasks were then compared across participants.

MATERIALS AND METHODS

Participants

A total of 71 participants with a mean age of 20 years took part in the study (29 males, 41 females, with one participant declining to disclose gender). Of these 71 participants, five failed to complete the causal judgment task, with one of these five also failing to complete the delay discounting task. One additional participant failed to complete the BIS-11, while one further participant failed to complete the causal attribution task. This gave a total sample sizes of 66, 70, 70, and 70 for each of the individual tasks respectively.

Design

The general format of both the causal judgment and the delay discounting tasks remained identical to that in the first experiment. However, given the additional tasks being included, it was decided to streamline both tasks in order to reduce the demands on participants. For the causal judgment task, we removed the second level of $P(e|c)$, 0.5, making it a single-factor task with the same three levels of delay (0, 2 and 5 s), and $P(e|c)$ set at 0.75 for each of these three condition. For the delay discounting task, the value of the LL reward was fixed at £200, with the additional value of £10000 dispensed with. As a result, the delay discounting task no longer remained as an experimental study in itself (since there were no independent factors) but instead contributed a single measure, $\mathrm{AUC_d}$, for later comparison with other tasks. Both $\mathrm{AUC_c}$ and $\mathrm{AUC_d}$ were calculated in exactly the same manner as for the previous experiment, however as a result of streamlining, rather than there being two values for $\mathrm{AUC_c}$ (at $P(\mathrm{e|c}) = 0.75$ and $P(\mathrm{e|c}) = 0.5$) and $\mathrm{AUC_d}$ (at $\mathrm{LL} = £200$ and $\mathrm{LL} = £10000$), there were only single values of each (at $P(\mathrm{e|c}) = 0.75$ and $\mathrm{LL} = £200$ respectively).

For the causal attribution task, the independent variable was the interval separating cause and effect. The dependent measures was whether the participant selected the true cause (accuracy) and the time taken for them to make their selection (response time). By taking mean accuracy and response times over all three conditions, we have two single measures which can be contrasted with AUCs for the initial two tasks and scores from the BIS-11.

The BIS-11 provides a total score with a possible range of 30–120, with higher scores indicating greater impulsiveness. The scale can be further subdivided into three second order factors, cognitive, motor, and non-planning, which can additionally be included in our cross-task comparisons.

Apparatus and procedure

All four tasks were completed in the same computer laboratory using the same equipment as for Experiment 1. Both the new causal attribution task and the administration of the BIS-11 were programmed using Python.

The new causal decision making task was adapted from Young and Nguyen (2009). They used a first-person-shooter (FPS) video game in which the participants' task was to protect buildings that were being shot at by groups of three attackers. In each case, one attacker was an enemy and was firing explosive projectiles (the true cause, or target) while the other two were "friendlies" and firing duds (the foils). Participants could therefore protect the building by destroying the attacker that was causing the explosions. The key independent variable in Young and Nguyen's experiment was the temporal interval between the true cause and the effect. Essentially then, the task can be summarized as deciding which of three candidate causes was producing an effect (explosions) by observing the timing of when each attacker fired its weapon and when the explosion occurred at the building.

In the causal attribution task for the current paper, we transferred the essential features of Young and Nguyen's task from a 3D virtual environment to a simple experimental protocol using simple 2D stimuli, more closely resembling standard contingency judgment problems. Participants were presented with a triangle in the upper portion of the screen and below this was situated a row of three buttons. Alongside each button was a pointing hand, which would periodically press its adjacent button, constituting an instance of a candidate cause. The triangle illuminated contingent upon one of the buttons being pressed, with the other two buttons being foils. Buttons were labeled 1, 2, and 3 from left-to-right, and the position of the true cause on each condition was randomized on each condition. In our version of the task, participants were given three conditions in which the interval between the true cause and its effect was either 0, 2, or 5 s.

In governing stimulus delivery, an underlying trial structure was used in the same manner as for Young and Nguyen's experiments, with the timeline divided into 4 s segments. This trial structure was not explicitly signaled to participants, and trials ran seamlessly from one into the next with each trial beginning immediately following the previous trial with no inter-trial interval. All the candidate causes (button presses) occurred during the first 3 s of each 4 s trial, randomly distributed within this 3 s. The effect then followed its true cause with the specified delay. The foils had no effect over the triangle. Trials continued until participants made their choice of which of the three candidates they felt was the true cause of the triangle lighting up.

The fourth and final task administered to participants was the BIS-11, a well known metric for assessing impulsiveness. The BIS-11 program presented all the items simultaneously on the same page. Participants clicked on labeled buttons to indicate the extent to which they agreed with each statement, and pressed a "submit" button to record these choices and calculate their score. All participants were administered all four tasks in the same fixed order, which was as follows: causal judgment, causal attribution, delay discounting, BIS-11.

RESULTS

Causal judgment task

Results for the causal learning task mirrored those of the previous experiment. Both causal ratings, $F(1, 130) = 23.477$, $p < 0.0005$, $MSE = 458.747$, $\eta_p^2 = 0.265$ and overall response rates, $F(1, 130) = 42.790$, $p < 0.0005$, $MSE = 751.475$, $\eta_p^2 = 0.397$ declined as a function of increasing cause-effect delay. The results are summarized in **Figure 5**.

Causal attribution task

Figure 6 shows mean accuracy and response times for each of the three conditions in the causal attribution task. Reaction time was significantly affected by delay, with longer intervals resulting in longer latencies. Binary logistic regression was meanwhile used to assess the impact of delay on accuracy. However, it has previously been reported that a relationship often exists between the speed and the accuracy with which a task is performed or a decision is reached (Garrett, 1922; Schouten and Bekker, 1967; Wickelgren, 1977) commonly referred to as the speed-accuracy tradeoff (SAT). Therefore, as well as being a dependent measure, response time also has the potential to be a determinant of accuracy of choice, and hence was entered into the regression model along with delay. Analysis confirmed that interval length was a significant negative predictor of accurate choice, Wald $\chi^2 = 19.796$, $\beta = -1.115$, $p < 0.0005$, in other words longer intervals resulted in poorer accuracy. This is consistent with the findings obtained by Young and Nguyen (2009), who also saw accuracy impairments as a consequence of increasing cause-effect intervals, and also with general findings in the literature regarding effects of delay in causal learning (e.g., Shanks et al., 1989). Reaction time was not in this case a significant predictor of accuracy, Wald $\chi^2 = 0.104$, $\beta = -0.002$, $p = 0.747$. Of principle interest for the current paper however was how performance in this experiment correlates with performance in the causal judgment task, delay discounting task, and the BIS. To this end, both mean accuracy and mean response time across the three conditions was calculated.

Cross-task comparisons

Neither the delay discounting task (this time) nor the BIS-11 yielded any individual results that can be analyzed in isolation, but instead produced scores for each participant that can be compared to performance in the causal learning tasks. Analysis once again revealed no significant correlation between the causal judgment and delay discounting task, nor was there a correlation between the causal attribution task and the delay discounting task. There was however a significant correlation between participants' $\mathrm{AUC_c}$ on the causal judgment task and their mean accuracy on the causal attribution task, $r = 0.362$, $n = 65$, $p < 0.005$. This indicates that participants whose assessments of response-outcome relations were less adversely affected by delay also showed greater ability to identify the correct causal candidate from a number of alternatives even when delays were involved. While many experiments in the literature have confirmed detrimental effects of delay across a wide range of studies, there has been considerably less (if any) research demonstrating that individuals show an internal consistency in the ways in which their causal decisions are affected by delays across learning tasks.

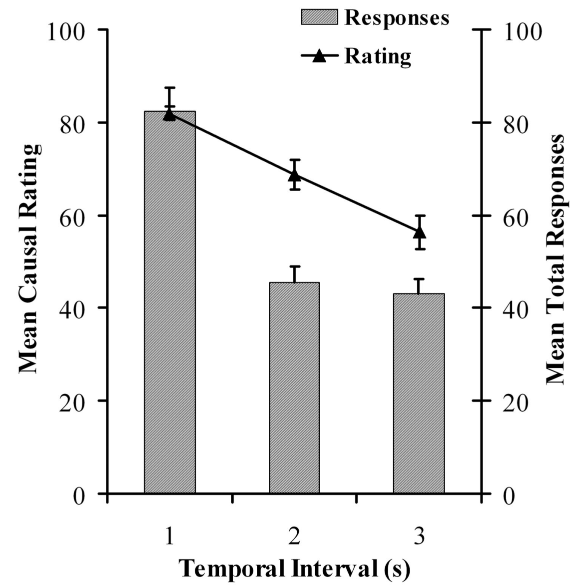

FIGURE 5 | Mean causal ratings and mean total response rates as a function of temporal delay for the causal judgment task in Experiment 2.

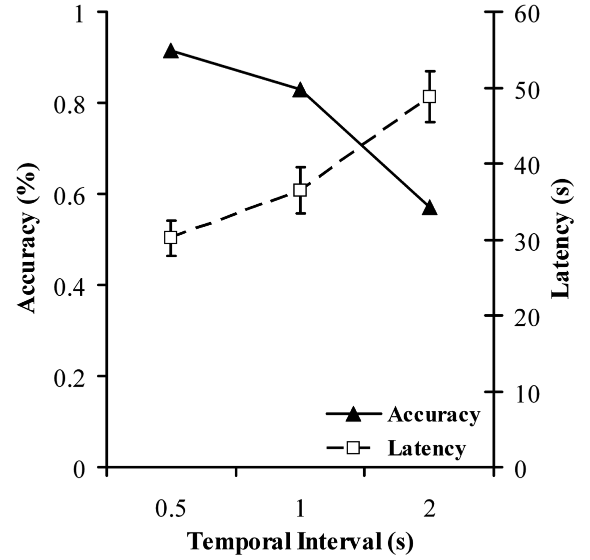

FIGURE 6 | Mean percentage accuracy and mean response times as a function of temporal delay for the causal attribution task in Experiment 2. Different symbol and line styles denote accuracy and response time.

Overall scores on the BIS-11 were not correlated with any of the metrics obtained from the three other tasks, including the delay discounting task, which may be seen as something of a surprise. There was however a marginally significant negative correlation between participants' scores on the non-planning factor of the BIS-11 and accuracy on the causal attribution task, $r = -0.229$, $n = 70$, $p = 0.057$. This suggests that higher scores on the non-planning second order factor (indicating a lack of planning) tended to result in a lower proportion of accurate choices in the causal attribution task. There were no further significant correlations between the other factors of the BIS-11 (cognitive and motor) and the other three tasks.

DISCUSSION

Summary and interpretation of results

The most notable finding from our second experiment is that there was a strong positive correlation between two very distinct forms of causal learning task. The first was an elemental causal judgment task, where participants were required to evaluate the extent of a putative causal relation by performing responses and observing the subsequent outcomes. The second was a causal attribution task where participants observed three candidates that were all potential causes of a single outcome, and identifying the most likely cause. The two tasks clearly both involve causal thinking, but the disparities between the two are evident, not least in the hypothesis space for each task (see, e.g., Griffiths and Tenenbaum, 2005, 2009). Yet despite their differences, performance in these two tasks was comparable for a given individual, with those doing well on one also tending to do well on the other.

This suggests that when engaging in causal learning, thinking, or reasoning, delays may have a consistent effect upon a given individual from one task to another. It therefore follows that an individual could potentially be categorized as being "delay susceptible" or "delay resistant" depending on their ability to recognize delayed causes or the extent to which they consider delayed effects to be evidence in favor of a causal relation. This is a strong parallel with evidence that has been obtained from studies of intertemporal choice and delay discounting, as reviewed in the Introduction, which suggests that individuals differ in the extent to which they discount delayed rewards. Some individuals (and indeed, cultures, age groups, and other social strata) have a strong preference for immediate rewards and steeply discount delayed rewards. This is often interpreted as an inability to delay gratification and an indication of impulsiveness compared to those who do not discount delayed rewards as steeply and are instead prepared to wait for rewards that are larger in magnitude.

Yet, despite this parallel, and many others earlier outlined in the introduction, we have now twice demonstrated that individual behavior in causal judgment and delay discounting tasks show no correlation. This replication of our earlier finding makes it increasingly likely that the results of this paper constitute evidence of an absence of correlation, rather than merely an absence of evidence. At the same time, the marginally significant correlation between non-planning impulsiveness and accurate causal attribution suggests that causal learning and impulsivity may yet indeed share a connection at some level. However, since the correlation involved the non-planning factor rather than cognitive aspects, then any connection is likely to be based on a lack of forethought or a failure to allow oneself adequate time to make an informed decision, rather than being founded on a common temporal processing mechanism.

On the surface this finding may seem somewhat surprising. The evidence reviewed in the introduction appeared to suggest that because of their inherent similarities, causal judgment and delay discounting may be governed by the same mechanisms. However, one may be even more inclined to suspect that the far more similar processes of delay discounting and probability discounting should share the same cognitive basis. Probability discounting is the process by which the subjective value of a reward declines as the likelihood of its delivery decreases, and therefore would seem to be very tightly connected to delay discounting. Recently, however, there has been an accumulation of data suggesting that a number of variables have different, even opposite, effects on temporal and probability discounting. Green and Myerson (2004) reviewed this evidence and concluded that despite the similarity in the mathematical form of the discounting functions, the patterns of results from their analyses strongly suggest that separate underlying mechanisms are involved for probability and temporal discounting. Hence, with potentially distinct cognitive pathways for these closely allied, perhaps we should not be surprised to find a lack of overlap between individual discounting functions and evaluation of delayed causal relations.

The lack of correlation between the BIS-11 and AUC from the delay discounting task is perhaps surprising, since the latter is often considered to be a behavioral measure of impulsiveness. Indeed numerous studies have previously found a strong positive correlation between BIS-11 and AUC (e.g., McHugh and Wood, 2008), though our study is not the first to show an absence of correlation between the two (Lane et al., 2003; Reynolds et al., 2008). These inconsistencies raise questions over the use of steep temporal discounting as an operational definition of impulsiveness.

Implications for theories of learning

Decision making in terms of choice between alternatives involving delays (such as performance on reinforcement schedules) is a direct reflection of the rate at which rewards are devalued by delays. The process of delay discounting however appears to be unconnected to a causal understanding. It would thus seem that this dissociation between discounting and causal learning indicates that simple associative learning cannot form the basis for both these processes. While for the sake of parsimony, a unified learning theory explaining such processes certainly offers appeal, based on the results of this and other studies, such a theory looks set to remain elusive.

To fully explore this conclusion, let us first review some of the essential concepts of learning theory. It is a fairly well-established finding in the behavior analysis literature that animals tend to respond more frequently during variable-interval (VI) reinforcement schedules compared to fixed-interval (FI) schedules (Herrnstein, 1964; Davison, 1969). It has also been demonstrated that animals prefer variable over fixed response-to-reinforcer delays when choosing between two concurrently available alternative response keys (Cicerone, 1976; Bateson and Kacelnik, 1997), thus indicating that the preference for variable reinforcement goes beyond task demands and reflects an inherent property of aperiodicity that makes it preferable. Researchers (McNamara and Houston, 1992; Bateson and Kacelnik, 1995) suggest that such preferences arise from foraging strategies or predatory behavior, which tend to benefit from variability of behavior. However this can also be explained from the perspective of temporal discounting.

If rewards lose their subjective value or associative strength as delays increase, then obviously an early reward contributes more, and a late reward contributes less, relative to a reward occurring at the midpoint of the two. However because of the negatively accelerated shape of the discounting function, the difference between the early and the intermediate reward is greater than the difference between the intermediate and the late reward. In other words, the gain from the early rewards is greater than the loss from the late rewards (compared to an intermediate reward). For an illustrative example, see Greville and Buehner (2010), **Figure 1**. Thus, a set containing an approximately equivalent number of both early and late rewards will have a greater net subjective value than a set where all the rewards are of intermediate latency. Therefore, assuming that on comparable interval schedules (where the variable schedule will have an even distribution of early and late rewards about the central midpoint of the fixed schedule), although the mean delay-to-reinforcement is approximately equivalent, the variable schedule results in the formation of stronger associations. Applying the temporal discounting principle to animal learning thus provides an account for the apparent preference for variability. The question which then arises, and which is most pertinent to the focal issues being explored in this paper, is whether human causal reasoning follows these same principles.

Greville and Buehner (2010) carried out a series of experiments comparing predictable and variable response-to-reinforcer delays in an instrumental causal judgment task, similar to that employed in the current paper. It was found that conditions where the interval was fixed were routinely judged as more causally effective than those with variable intervals, and furthermore that judgments tended to decline as variability increased. This is in direct opposition to the variability preference observed in animals. Greville and Buehner's results appear to complement those of the current paper, where we similarly show a dissociation between causal learning with delays and choice involving delayed rewards.

These results would appear to endorse view that causal induction cannot simply be reduced to associative learning. However, this interpretation rests on the assumption that preference for variable reinforcement is a reflection of the associative strength between response and reinforcer, which may not be entirely valid. If instead subjective value and associative strength are dissociable, this may in turn suggest that animals have the capacity to learn associations, or causal connections, without this necessarily resulting in an observable change expressed in behavioral preference. Indeed, a recent variant of associative learning theory, the temporal coding hypothesis (TCH; see, e.g., Miller and Barnet, 1993) posits exactly that. The TCH departs from the traditional associative view by arguing that the temporal relationship between events is encoded as part of the association; that is, the animal learns not only that the US will occur but also when it will occur. This temporal information plays a critical role in determining whether a response is made, and the magnitude and timing of that response. In other words, whether or not an acquired association will be expressed as observable behavior depends on the encoded temporal knowledge (Savastano and Miller, 1998; Arcediano and Miller, 2002). An extension of such an argument would be that an organism may be perfectly capable of recognizing a particular relation, and indeed identifying that relation as stable, but still exercise preference for another schedule that it perceives as perhaps less stable but offering greater potential for reward.

There remain, however, aspects of the design of the current study that could provide alternative explanations for the results obtained. While both the causal judgment task and the delay discounting task are well-established measures in the field, and therefore adequate to assess these respective processes, there are two important distinctions between them. Firstly, the outcome in the causal judgment task had no motivational significance for the participant. Greville and Buehner (2010) suggested that the facilitatory effect of temporal predictability in causal learning found in their studies, which contrasted with the long-established finding of animal preference for variable reinforcement, might in part be attributable to the fact that their causal judgment tasks were appetitively neutral. The same point may be raised here; although the amounts of money in the discounting task were hypothetical, subjects nevertheless tend to respond to such choices as though they were real amounts. In contrast, the outcome in the causal judgment task had no value, hypothetical or otherwise. It is therefore possible that adopting a causal learning task where outcomes provide more tangible reinforcement may have produced different results. However, Shanks and Dickinson (1991) have already contrasted causal judgment with instrumental performance (where participants engaged in a "points-scoring" task) and found that performance and judgment closely mirrored one another. Thus there is little to suggest that such an extension to the current study would yield a different result.

Secondly, while temporal delays in the causal learning tasks are directly experienced, the delay delays in the discounting tasks were merely described. There is considerable evidence that causal judgments made in described situations follow similar patterns to those made based on directly experienced events (Wasserman, 1990; Shanks, 1991; Lovibond, 2003; Greville and Buehner, 2007). We may therefore reasonably conclude that if a described causal learning task had been employed, similar results would have been obtained. However Hertwig et al. (2004) suggest that the same is not necessarily true for choice behavior; for instance, people tend to overestimate and underestimate the probability of rare events in decisions from descriptions and experience, respectively. Future research exploring this question then may seek to see if the same holds true in choosing between immediate and delayed rewards when delays are directly experienced.

Neurobiological correlates of associative and causal learning

The evidence from neurological studies reviewed earlier implicated the hippocampus in both the processes of trace conditioning and ability to delay gratification to select a larger delayed reward, and thus formed an important part of the motivation behind the study. We considered that the role of delay in both reducing the subjective value of reward and impairing causal attribution might be similar within individuals, but this proposition was not supported by our experimental findings. The primary implication of these results is that delays impact the two processes in different ways, and thus suggest that local temporal processing, rather than a common temporal processing structure, governs the impact of delays in these processes. We now turn to review further evidence from neurological research that can ballast this argument.

Turner et al. (2004) applied fMRI during a judgment task and found that right lateral prefrontal cortex (PFC) activation is sensitive to the magnitude of prediction error, which is a cornerstone of associative learning models such as the Rescorla-Wagner (Rescorla and Wagner, 1972) model, thus providing direct evidence for a neural basis of the mechanisms proposed in associative learning. However, it is debatable whether the task employed during their experiment constituted a valid test of causal, rather than just simple associative, knowledge. Fugelsang and Dunbar (2005) applied fMRI to a more complex causal judgment task and found that the brain responds differently to incoming data depending on the plausibility of the theory being tested. Specifically, given a plausible causal theory, evaluation of data consistent with that theory recruited neural tissue in the parahippocampal gyrus, whereas evaluating inconsistent data recruited neural tissue in the anterior cingulate, left dorsolateral PFC, and precuneus. Perhaps most relevant to the current article, Satpute et al. (2005) carried out fMRI during a relation-judgment paradigm (see Fenker et al., 2005). Their results revealed that although causal and associative processing shared several regions of activity in common, accessing causal knowledge produced patterns of activity in left dorsolateral prefrontal cortex (DLPFC) and right precuneus that were absent during associative judgment, even after correcting for task difficulty. They concluded that evaluating causal relations involves additional neural mechanisms relative to those required to evaluate associative relations. Indeed, further dissociations can be made at the neurological level even between basic associative processes. Myers and Davis (2002) report that acquisition and extinction appear to be governed by fundamentally different neural mechanisms in different learning paradigms, evident particularly when comparing extinction of fear conditioning and conditioned taste aversion. Each appears to recruit its own configuration of cellular mechanisms, perhaps as a function of task difficulty or the nature of the CR, CS, or US.

These findings appear to support a distinction between causal and associative learning. While there are certainly elements in common between the two processes, both in their neurological bases and at a cognitive or computational level, a number of important contrasts remain. Specifically, it appears that causal learning involves an additional layer of complexity, and recruits additional neurological structures, than associative learning.

Critique and future directions

This study is the first, as far as we are aware, to experimentally explore a potential link between causal learning and discounting. As such, the work was largely exploratory in nature and the methodology untested, so it is entirely possible that superior methods of comparing discounting functions with causal judgment data may be constructed. While the AUC has become a standard measure for temporal discounting, no such universally accepted measure exists for the causal judgment task. Our application of this procedure to our causal judgment task may thus be open to some criticism. For instance, the delays in the two tasks differ greatly in duration; seconds for the causal judgment task and months and years for the discounting task; moreover, as mentioned earlier, delays are experienced in the former while described and imagined in the latter. In addition, we only used three delays in the causal judgment task, rather than the seven in the delay discounting task; studying judgments over a much broader range of delays may provide a more finely tuned measure of the causal judgment data.

An alternative suggestion might be to contrast fixed and variable delays, as in Greville and Buehner's (2010) studies, and investigate whether preference for fixed vs. variable relations has any connection to temporal discounting. Despite the general trend of preference for predictability shown in Greville and Buehner's studies, individual participants may deviate from this trend and exhibit preference for variability. It would be interesting to see if such a preference arises from a contiguity bias, whereby the potential for immediate reinforcement in variable conditions overrides the impression of stability provided by fixed conditions, and whether such a contiguity bias is correlated with steep temporal discounting and impulsivity.

Finally, steep discounting has often been linked in the literature with impulsivity (Richards et al., 1999), which in turn has been linked to a number of socially maladaptive behaviors, including violence, drug abuse, and pathological gambling (Steel and Blaszczynski, 1998; Fishbein, 2000). As a result, considerable effort has been devoted to the development of potential interventions for impulsive behavior, and the current line of research may help provide further insights. For instance, although overall we found little evidence of any relationship between impulsivity and causal understanding, there was a marginally significant negative relationship between non-planning impulsiveness and correct identification of causes. An adaptation of this paradigm into a training game designed to improve causal attribution might therefore be a potential means of reducing the propensity for non-planning impulsivity. Though the current study does not yet provide strong enough evidence that could inform clinical practice, future research might suggest avenues for the development of new therapeutic strategies.

CONCLUSION

To summarize, this study represents an early step in exploring the potential relationship between areas of learning that have previously tended to be somewhat shielded from one another in the literature. Our results indicate that delays have a consistent influence in tasks involving causal learning, and that a given individual may be affected in the same way by delays across different causal learning tasks. However no such correlation exists between the manner in which delays hamper causal learning and the rate at which the subjective value of delayed rewards is discounted. Taken together with the results of Greville and Buehner (2010) and those of Satpute et al. (2005), the implication is that there is a dissociation between reinforcement learning and causal inference, and the effects of time in these learning processes cannot be attributed to a common temporal processing mechanism. The results have valid implications for current theories of learning as well as considerations for interventions in problem behavior and psychotherapy. It is our hope that further research will shed more light on this topic and more precisely identify those facets of learning processes that are idiosyncratic and those that form common elements between processes.

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Dysphoric mood states are related to sensitivity to temporal changes in contingency

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A controversial finding in the field of causal learning is that mood contributes to the accuracy of perceptions of uncorrelated relationships. When asked to report the degree of control between an action and its outcome, people with dysphoria or depression are claimed to be more realistic in reporting non-contingency (e.g., Alloy and Abramson, 1979). The strongest evidence for this depressive realism (DR) effect is derived from data collected with experimental procedures in which the dependent variables are verbal or written ratings of contingency or cause, and, perhaps more importantly, the independent variable in these procedures may be ambiguous and difficult to define. In order to address these possible confounds, we used a two-response free-operant causal learning task in which the dependent measures were performance based. Participants were required to respond to maximize the occurrence of a temporally contiguous outcome that was programmed with different probabilities, which also varied temporally across two responses. Dysphoric participants were more sensitive to the changing outcome contingencies than controls even though they responded at a similar rate. During probe trials, in which the outcome was masked, their performance recovered more quickly than that of the control group. These data provide unexpected support for the DR hypothesis suggesting that dysphoria is associated with heightened sensitivity to temporal shifts in contingency.

Keywords: causality, contingency, reinforcement, matching, maximization, learning, depression, depressive realism

INTRODUCTION

Our perception of the effectiveness of our actions to elicit their consequences shapes our sense of volition (Neuringer and Jensen, 2010) and personal agency (Bandura, 1982) and may be related to mental health. People who are depressed often possess symptoms that resemble personal helplessness, yet it has been claimed, perhaps paradoxically, that they may also be more sensitive to the causal consequences of their behavior than others (Alloy and Abramson, 1979; Martin et al., 1984; Alloy et al., 1985; Benassi and Mahler, 1985; Vasquez, 1987). The strongest evidence for this depressive realism (DR) effect, involves data from the contingency judgment task (Dobson and Franche, 1989). This task requires the participant to make an arbitrary response and make a verbal or written judgment of the statistical relation between the response and an arbitrary outcome. However, there are possible problems with this method as an objective measure of people's understanding of causal relationships. Here, we test mood related differences in causal learning using a less subjective measure of causal learning, a behavioral test of contingency and contiguity sensitivity measured over time, a method often used to study contingency sensitivity (e.g., Thomas, 1981; Dickinson et al., 1992) and timing sensitivity (Chiang et al., 2000) in animals. First, we briefly review DR studies before describing how the present experiment will address interpretative issues with extant measures.

The term DR originates from a series of studies carried out by Alloy and Abramson (1979). Student participants were given opportunities to press or not to press a button and observe whether an outcome (light) was temporally contingent upon their actions (key press). The programmed contingency between the action and the outcome can be formally described by the ΔP measure (Allan, 1980). ΔP expresses the strength of the relationship in terms of a number between -1 and $+1$, allowing for negative relationships. It is calculated as the difference between the conditional probabilities of the outcome following an action [p(light|press)] and following no action [p(light|no press)]. In most experiments, participants' numeric judgments of their control over the outcome are then analyzed for consistency with the programed contingency. Indeed, much research has been conducted to determine the extent to which ΔP, as a formal model of contingency learning, is an accurate predictor of judgments (e.g., Jenkins and Ward, 1965; Allan and Jenkins, 1980; Chatlosh et al., 1985; Wasserman et al., 1993).

However, Alloy and Abramson's (1979) aim was not so much to test the model but to assess the relative accuracy of judgments made by student participants, who were either categorized as mildly depressed or not depressed. A range of conditions and manipulations were tested across a series of experiments, though it was two critical conditions that engendered differences between the two mood groups. These were conditions where the frequency of outcomes was varied (25 versus 75% of trials included outcomes) but the difference between the two conditional probabilities and degree of control was always zero ($\Delta P = 0$). Judgments made by the depressed participants reflected this contingency and in both conditions were close to zero, suggesting that they recognized their lack of control. Judgments made by the non-depressed participants, although low in the 25% condition, were higher in the 75% condition and were consistent with the perception of a moderate degree of control.

Based on these findings (Alloy and Abramson, 1979), and subsequent replications of the effect (Martin et al., 1984; Alloy et al., 1985; Benassi and Mahler, 1985; Vasquez, 1987), the general conclusions were that depressed people were realistic about control whereas the non-depressed were optimistic in their perceptions of causal efficacy. This evidence is considered to be strong largely because ΔP is regarded as an accurate objective measure of control against which to assess people's ratings (Dobson and Franche, 1989; Ackermann and DeRubeis, 1991; Haaga and Beck, 1995).

However, the interpretation of such findings as indicating realism is based on the assumption that the experimentally programmed ΔP (ΔP_{Prog}) and the ΔP experienced by the participant (Experienced DP: ΔP_{Exp}) are the same or at least very similar. This may not be the case (Msetfi et al., 2005, 2007; Murphy et al., 2005). For example, ΔP_{Prog} is defined as the difference between two conditional probabilities. The first, p(Outcome|Action), is clear as it is defined by the participants' responses. However, the second probability, p(Outcome|no Action), is ambiguous to manipulate experimentally because it is determined by the frequency of not responding. It is certainly not clear how non-events are defined over time and therefore their psychological frequency, as opposed to that which the experimenter programs and counts, is unknown (Msetfi et al., 2005, 2007). Thus the ΔP_{Exp} is also unknown in the case of contingencies where the p(Outcome|noAction) > 0. A stronger test of DR might involve conditions in which the experienced conditional probabilities and ΔP_{Exp} were under greater experimenter control.

Furthermore, other factors, such as response rate variability, can influence the ΔP_{Exp}. ΔP_{Exp} is determined, to some extent, by the relative tendency to respond and to withhold responding. For instance, during 20 possible action opportunities if a participant responds 18 times and withholds responding 2 times, this sets limits on the range of possible contingencies that might be experienced. Some participants tend to respond more while others respond less even when instructed to sample both situations similarly. In extreme cases, the participant might experience only the p(Outcome|Action) or the p(Outcome|noAction) rather than both as intended by the experimenter (Matute, 1996) and a skewed ΔP_{Exp} depending on the programming method used (Matute, 1996; see also Hannah and Beneteau, 2009). This is a crucial issue for any experiment designed to test sensitivity to actual relationships. In fact, Matute (1996) has argued that the DR effect might occur simply because the depressed respond less than the non-depressed who respond at high rates and experience a more positive contingency. This interpretation suggests that the DR effect is a result of response rate differences changing the ΔP_{Exp}.

Another obstacle to assessing perceptions of contingency involves the dependent variable in these studies. Many studies rely on participants' explicit verbal or written judgments about their perception of control over outcomes using Likert or similar numerical scales. An alternative method we explore here is a performance measure (see also Hannah and Beneteau, 2009). There is some reason to suspect that the two measures might not elicit the same judgment. Verbal judgments may be more sensitive to disruption (Allan et al., 2005) and representative of people's willingness to predict that an outcome will occur rather than their perception of the overall contingency (Allan et al., 2007).

In summary, we have identified three aspects of the standard contingency learning procedure used in DR studies, that may lead to equivocal or possibly misleading data with relation to the DR hypothesis. These are (i) the ambiguity of ΔP_{Prog} where p(Outcome|no Action) > 0; (ii) response rate variability which may affect ΔP; and (iii) reliance on explicit judgments of control as dependent measures which are assumed to reflect contingency sensitivity.

The current study was designed to address these issues. The primary change involved examining not the accuracy of judgments of contingency but how effective participants' responses were in causing an outcome to occur. The procedure is based on a standard instrumental or free-operant procedure in which participants are instructed to cause an outcome to occur (e.g., flash of light on a computer screen) as many times as possible. Note that in this procedure the outcome never occurs in the absence of the response and, no matter what the response rate, the p(light|no press) is always zero. Also rather than assess contingency as the difference in perceived effectiveness of responding or not responding, the procedure required participants to learn which of two responses was more effective and that this would vary over the course of the trial. This means that responding should be maintained on a given action while it is more contingent, but that it should shift between responses as the contingencies change.

However, in the two-response procedure we used in the present experiment, there are two possible behavioral strategies that people might employ. Previous research indicates that under similar conditions, people tend to "match" their responses to the outcome contingencies (e.g., Chatlosh et al., 1985; Koehler and James, 2009). This is consistent with Herrnstein's (1961) Matching Law in which the relative probability of responding on each of the two behavioral choices matches the probability of reinforcement. Alternatively participants might employ an all or nothing maximization strategy which might actually be more effective in producing outcomes. In other words, when $p(\text{flash}|\text{response}_1) > p(\text{flash}|\text{response}_2)$, then the most effective means of producing the maximum number of light flashes is to make only response_1 and not response_2. In the conditions tested here, the maximization strategy could thus be thought of as the most effective and normative option in comparison to contingency matching strategies.

In addition, behavior that tracks changes in the probability of the light flash might be claimed to reflect sensitivity to correlated and uncorrelated actions as well as adjustment speed. Successful performance on the task involves being sensitive to which of the two actions is more contingent with the outcome and then changing behavior to reflect the change in contingency over time. Indeed, sensitivity to shifts in the temporal predictiveness of actions for individual cues are argued to be an important cue to causality (Greville and Buehner, 2010). It spite of this, studies exploring the effects of depression on causal learning have not so far included time based responses to contingency. Given, however, that mild depression has been linked to a slowing down in the perception of time itself (e.g., Gill and Droit Volet, 2009), possibly through attentional mechanisms (Msetfi et al., 2012), participants with depression may be less sensitive to temporal shifts in reinforcement.

This leaves us with some interesting and testable predictions. Across a number of conditions, mildly depressed people are more sensitive to uncorrelated contingencies than controls (e.g., Alloy and Abramson, 1979). However, there is little evidence to suggest that sensitivity leads to more effective behavior. For example, related research on learned helpless suggests that depression is associated with passive behavior in the face of positive contingencies (e.g., Hiroto, 1974; Maier and Seligman, 1976). Therefore, we might hypothesize that, if realistic, mildly depressed participants' response rate probabilities will be more similar to the programmed contingencies than those of the control group. However, we would also hypothesize that the response rates of controls will be greater and produce a greater frequency of light flashes. Based on Matute and her colleagues' work on the link between response rates and DR (Matute, 1996; Blanco et al., 2009), controls will respond at higher rates or adopt a maximization strategy, and, consequently, experience more light flashes than the dysphoric group. Finally, based on research into the slowing effects of depression on time perception, we hypothesize that depressed participants will respond later to the switch in contingencies than the control group (e.g., Tysk, 1984; Bschor et al., 2004; Gill and Droit Volet, 2009; Msetfi et al., 2012).

MATERIALS AND METHODS

PARTICIPANTS

University students completed the Beck Depression Inventory (BDI; Beck et al., 1961) before being invited to participate and again during participation. All participants gave consent after being informed as the nature of the study. The final sample comprised forty-eight participants who were assigned to the dysphoric ($n = 24$) or control groups ($n = 24$) on the basis of their BDI scores. As with the majority of DR research (e.g., Alloy and Abramson, 1979; Msetfi et al., 2005), scores of 9 or above indicated dysphoric mood and scores of 8 or below indicated no depression and membership of the control group. The groups were matched on demographic variables, gender, handedness, age, years of education, pre-morbid IQ measured by the National Adult Reading Test (NART; Nelson, 1982), and short term memory capacity (Digit span; Lezak, 1995). All participants were right handed and all between group t-tests carried out on demographic were not were not reliable (all $t < 1.14$). However, as expected, the dysphoric group had significantly higher BDI scores ($M = 15.6$, SE = 1.6) than the control group ($M = 4.4$, SE = 0.5: $t(46) = 6.56$, $p < 0.001$).

PROCEDURE

Participants were briefed about the nature of the experiment and read an information sheet. Participants completed the digit span test, the NART, and the BDI. Instructions for the experimental task were then presented on a computer screen, which participants were able to read self-paced. The full instructions are shown in the Appendix. In the instructions, participants were asked to maximize the occurrence of a brief light flash on the computer screen by pressing two on-screen buttons as many times as they chose to during each experimental trial. The button on the left could be pressed using the left "tab" key and the button on the right using the "return" key on the computer keyboard. For half the participants, response_1 was the left on the computer keyboard with response_2 on the right. For the other half of the sample, this positioning was reversed. Buttons were not to be pressed simultaneously or held in the on position.

Each trial was 50 s long and separated by a 10 s inter-trial interval. During the first 25 s of each trial, 85% of presses on response_1 – the "early" button – were reinforced immediately with a light flash, while 15% of presses on response_2 – the "late" button were reinforced. Reinforcement sequences were generated randomly for each participant. In addition, the outcome contingencies switched buttons after 25 s in the middle of each trial. All light flashes lasted 100 ms with no delay between the button press and the flash. Dependent measures were response rates and the probability of pressing the late button during each 5 s time segment of every experimental trial [$p(\text{late}) = F(\text{late})/F(\text{early}) + F(\text{late})$].

There were a total of 18 trials in the procedure. However, participants were told that there would be some probe trials where the light would be hidden from them, but that they should use what they had already learned in order to make the light flash as many times as possible (trials: 9, 12, 15, and 18). An on-screen message at the end of each trial recorded the number of light flashes during that trial. Finally, participants were debriefed, and paid a nominal fee for their participation.

RESULTS

The probability of pressing the late button – $p(\text{late})$ – was calculated for every 5 s time segment for each participant, across reinforced learning trials and also across masked probe trials. The analyses of reinforced and probe trial data are reported separately below, and an alpha level of 0.05 was used in all analyses unless stated otherwise.

REINFORCED LEARNING TRIALS

The $p(\text{late})$ for each 5 s time segment was analyzed using a mixed $(14 \times 10) \times 2$ factorial analysis of variance with trial (1–14) and time segment (5–50 s) as within subjects variables and mood (dysphoric, control) as the between subjects variable. For brevity and simplicity, data are shown in **Table 1** for each time segment averaged over experimental trials.

Response probabilities did change across time segments, $F(9, 414) = 324.51$, $p < 0.001$, $\eta^2 = 0.88$, MSE = 0.273. Although the effect of trial was not reliable, $F(13, 598) = 1.495$, $p = 0.114$, MSE = 0.048, the trial by time segment interaction was significant, $F(117, 5382) = 6.93$, $p < 0.001$, $\eta^2 = 0.13$, MSE = 0.024, with response probabilities increasingly matching programmed contingencies as learning progressed. This trend did not depend on mood group however, as the three-way interaction between trial, time segment, and mood group was not significant ($F < 1$). However, there was a significant main effect of mood $F(1,$

Table 1 | Mean probability of responding on the late button [p(late)] for the control and dysphoric groups during each 5 s time segment averaged over 14 reinforced trials.

Time (s)	M	SE	Contingency comparison 0.15 or 0.85		Maximization comparison 0 or 1	
			t	p	t	p
CONTROL GROUP						
5	0.173	0.028	0.819	0.421	6.182	<0.001
10	0.135	0.027	−0.563	0.579	4.921	<0.001
15	0.126	0.027	−0.874	0.391	4.65	<0.001
20	0.112	0.027	−1.379	0.181	4.128	<0.001
25	0.119	0.026	−1.194	0.245	4.513	<0.001
30	0.574	0.019	−14.605	<0.001	−22.532	<0.001
35	0.861	0.027	0.42	0.678	−5.094	<0.001
40	0.879	0.027	1.062	0.299	−4.448	<0.001
45	0.89	0.025	1.579	0.128	−4.326	<0.001
50	0.896	0.023	1.964	0.062	−4.477	<0.001
DYSPHORIC GROUP						
5	0.211	0.040	1.538	0.138	5.291	<0.001
10	0.177	0.035	0.753	0.459	5.016	<0.001
15	0.154	0.036	0.125	0.902	4.463	<0.001
20	0.158	0.033	0.252	0.803	4.789	<0.001
25	0.171	0.035	0.587	0.563	4.779	<0.001
30	0.627	0.015	−15.246	<0.001	25.487	<0.001
35	0.874	0.026	0.942	0.356	−4.918	<0.001
40	0.882	0.027	1.178	0.251	−4.403	<0.001
45	0.884	0.025	1.404	0.174	−4.716	<0.001
50	0.868	0.025	0.716	0.481	−5.34	<0.001

These comparisons were made using single sample t-tests and the alpha level was ameliorated to $\alpha = 0.00125$ for 40 comparisons.

Table 2 | Absolute frequency of response for the control and dysphoric groups during each 5 s time segment averaged over the early and late buttons and the 14 reinforced trials.

Time (s)	Control group		Dysphoric group	
	M	SE	M	SE
5	9.73	0.844	11.49	0.701
10	11.9	0.900	13.2	0.787
15	11.7	0.826	12.96	0.749
20	11.57	0.789	12.73	0.718
25	11.36	0.768	12.71	0.712
30	11.67	0.812	12.86	0.737
35	11.94	0.772	13.22	0.731
40	11.64	0.715	13.13	0.787
45	11.56	0.707	13.13	0.804
50	11.46	0.703	13.11	0.801

46) = 4.43, $p = 0.041$, $\eta^2 = 0.088$, MSE = 0.221. Dysphoric participants p(late) was higher ($M = 0.501$, SE = 0.01) than controls ($M = 0.476$, SE = 0.01) throughout the reinforced learning trials.

The main effect of mood on response probabilities does not indicate whether participants were using a contingency matching strategy or a maximization strategy. Consider that if responses are distributed across buttons in a manner consistent with the programmed contingency (0.15 and 0.85) or maximization (0 and 1), then the response probability should average out at 0.50 over the course of each experimental trial. As dysphoric participants responded on the late button with a probability of 0.500 and this was significantly higher than controls, who responded at a probability of 0.476, this is evidence of responding which is closer to one of those strategies. In order to explore this further, the p(late) was compared to the DP programmed at the same time points and to values consistent with a maximization strategy (see **Table 1**) using a series of single samples t-tests. The alpha level for these tests was ameliorated to account for multiple comparisons, where $\alpha = 0.05/40$ comparisons $= 0.00125$.

Table 1 shows that for both groups, response probabilities during 9 of the 10 time segments were not significantly different from the programmed contingencies but were significantly different from maximization probabilities throughout. Thus participants' responses matched contingencies rather than being consistent with the more effective maximization strategy. However, as the dysphoric group achieved an average response probability of 0.500 overall (see above), which was significantly higher than that of controls ($p = 0.04$), this is suggestive of a general increased probability sensitivity in the dysphoric group. Greater sensitivity could also be indicated by a more rapid switch in response probabilities between the two buttons at 25 s. However, as there were no significant interactions involving mood and time segment, $F < 1$, there was no evidence for any mood related changes in this type of contingency sensitivity.

In order to check whether the group effect on response probabilities reported above was related to mood related changes in response propensity, we also examined absolute response frequencies. The data were then analyzed using a similar analysis of variance strategy to that described above, with trial (14), time segment (10), and button (early, late) as within subjects variables and mood as the between subjects variable and are shown below in **Table 2** averaged over trials and button. Overall response frequencies increased over trials, from an average of 140.50 (SE = 11.10) on Trial 1 to 279.5 (SE = 11.31) on Trial 14, $F(13, 598) = 45.17$, $p < 0.001$, MSE = 76.42, and over time segments, $F(9, 414) = 21.20$, $p < 0.001$, MSE = 20.29. The dysphoric group responded on average 257.1 times during each trial (SE = 15.1), while controls made fewer responses ($M = 229.1$, SE = 15.1). However, the mood effect was not reliable, $F(1, 46) = 1.73$, $p = 0.195$.

Finally, response frequency data was used to calculate a measure of the effectiveness of responding over reinforced trials in terms of light flashes produced [effectiveness = First 25 s (F Early × 0.85) + (F Late × 0.15); Second 25 s (F Early × 0.15) + (F Late × 0.85)]. These data were analyzed using a mixed analysis of variance, with trials (14) and trial half (first 25 s, second 25 s) as the repeated measures factors. Mood was the between subjects factor. Response effectiveness improved over trials, $F(13, 32) = 19.35$, $p < 0.001$. Although the dysphoric group produced more flashes ($M = 93.72$, SE = 5.1) than controls ($M = 84.9$, SE = 5.1), the

Table 3 | Mean probability of responding on the late button [*p*(late)] for the control and dysphoric groups during each 5 s time segment and on average (labeled *M*) for each the four masked probe trials.

Time segment in s	Probe 1		Probe 2		Probe 3		Probe 4	
	M	**SE**	***M***	**SE**	***M***	**SE**	***M***	**SE**
CONTROL GROUP								
5	0.028	0.022	0.079	0.044	0.071	0.041	0.032	0.020
10	0.144	0.039	0.164	0.052	0.134	0.052	0.098	0.048
15	0.134	0.041	0.26	0.07	0.142	0.052	0.135	0.049
20	0.201	0.051	0.381	0.082	0.179	0.059	0.214	0.068
25	0.322	0.078	0.519	0.084	0.343	0.079	0.332	0.058
30	0.505	0.081	0.702	0.066	0.642	0.072	0.75	0.068
35	0.668	0.056	0.774	0.066	0.809	0.07	0.886	0.047
40	0.724	0.064	0.727	0.069	0.905	0.044	0.892	0.055
45	0.719	0.063	0.726	0.066	0.921	0.037	0.914	0.046
50	0.74	0.062	0.708	0.076	0.828	0.07	0.873	0.053
M	**0.419***	**0.023**	0.504	0.025	0.498	0.03	0.513	0.021
DYSPHORIC GROUP								
5	0.068	0.039	0.167	0.061	0.164	0.054	0.14	0.052
10	0.271	0.066	0.214	0.07	0.111	0.045	0.148	0.052
15	0.221	0.056	0.179	0.065	0.138	0.052	0.18	0.063
20	0.291	0.061	0.245	0.057	0.19	0.06	0.189	0.063
25	0.491	0.07	0.433	0.076	0.37	0.074	0.259	0.069
30	0.651	0.069	0.62	0.084	0.645	0.071	0.606	0.073
35	0.698	0.061	0.786	0.059	0.76	0.069	0.731	0.073
40	0.707	0.068	0.853	0.042	0.809	0.056	0.844	0.048
45	0.76	0.051	0.793	0.06	0.839	0.059	0.873	0.043
50	0.769	0.065	0.79	0.054	0.849	0.056	0.816	0.059
M	**0.493***	**0.020**	0.508	0.021	0.488	0.026	0.479	0.022

*Between group differences are emphasized in bold, *indicates the level of significance = 0.018.*

mood effect was again not significant, $F(1, 44) = 1.47$, $p = 0.231$, nor were any of the interactions involving mood.

Response rate and response effectiveness data from reinforced trials therefore shows that controls did not produce more responses than the dysphoric group or receive more light flashes.

MASKED PROBE TRIALS

As with reinforced trials, we calculated p(late) values for each time segment of the four masked probe trials. These data are shown in **Table 3**, along with the average p(late) across each probe trial. Inspection of these data shows that, in spite of the light flashes being masked, participants were able to maintain appropriate performance and switch responding from the early to the late buttons midway through the each trial.

The results of the mixed factorial analysis of variance were consistent with these observations, with the effects of trial, time segment and the trial by time segment interaction being significant, all $F > 2.62$, all $p < 0.04$. Of more interest, however, were mood effects. The trial by mood interaction was significant, $F(3, 138) = 3.28$, $p = 0.023$, $\eta^2 = 0.07$, $\text{MSE} = 0.08$. Tests of the simple effects of mood on p(late) during each probe trial showed that the control group responded significantly less than the dysphoric group overall during the first probe trial, $p = 0.018$, but not the second third or fourth probe trials, all $ps > 0.26$.

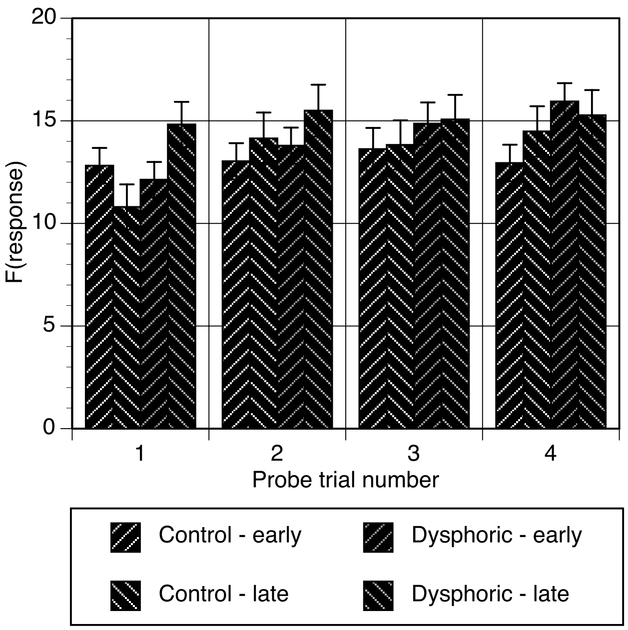

FIGURE 1 | Response frequency on the early and late buttons during each probe trial averaged over time segment for the control and dysphoric groups. Error bars correspond to the standard error of the mean.

Response frequencies on each button were also examined for each time segment of each probe trial and are shown in **Figure 1** averaged over time segment. The data were analyzed with trial (4), time segment (10), and button (early, late) as within subjects variables and mood as the between subjects variable. As expected, response frequencies increased over the four probe trials, and also depended on button, and time segment, all $Fs > 5.31$, all $ps < 0.001$. Of interest here were the effects involving mood. **Figure 1** suggests that mood effects were present in the first probe trial but not subsequent trials. This observation was consistent with the significant mood by trial by button interaction, $F(3, 138) = 4.29$, $p = 0.006$, $\eta^2 = 0.085$, $\text{MSE} = 116.61$. Further analysis of this interaction showed that the mood by button interaction was only significant in the first probe trial, $F(1, 46) = 7.42$, $p = 0.009$, $\eta^2 = 0.139$, $\text{MSE} = 178.92$, and not the subsequent probe trials, all $Fs < 1.11$, all $ps > 0.29$. The source of the mood difference in Probe trial 1 was revealed through simple effects analyses, and showed that the dysphoric group responded significantly higher on the late button than the early button, $p = 0.03$, and that this was at a significantly higher level than controls, $p = 0.013$.

Response effectiveness scores were calculated for each probe trial (see **Figure 2**). These data were analyzed with a mixed analysis of variance with time segment (first 25 s, second 25 s), and trial number (1–4) as repeated measures factors. Mood was the between subjects factor. Response effectiveness increased significantly over

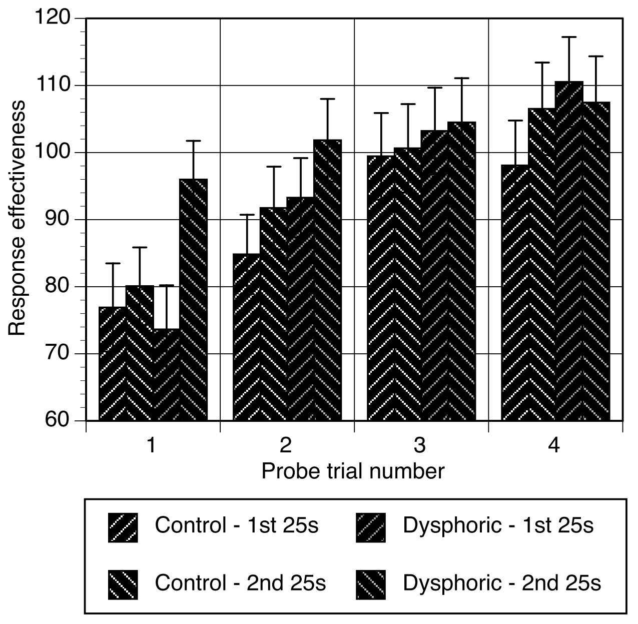

FIGURE 2 | Response effectiveness scores during the first 25 s and second 25 s of each probe trial for the control and dysphoric groups. Error bars correspond to the standard error of the mean.

the four probe trials, $F(3, 138) = 29.49$, $p < 0.001$, $\eta^2 = 0.39$, MSE = 372.03, but depended on whether responses were made in the first or second half of the trial, $F(3, 138) = 4.22$, $p = 0.007$, $\eta^2 = 0.084$, MSE = 156.24, and mood group, $F(3, 138) = 6.15$, $p = 0.001$, $\eta^2 = 0.118$, MSE = 156.24. Further analysis of this three-way interaction between trial, time segment, and mood involved examining the simple interactions between time segment and mood group for each probe trial. The time by mood interaction was only significant for the first probe trial, $F(1, 46) = 6.82$, $p = 0.012$, $\eta^2 = 0.129$, MSE = 322.79, but not subsequent probe trials, all $ps > 0.18$. Thus, probe trial 1 was the source of mood effects on response effectiveness, where there was no difference between mood groups in the first 25 s of the first probe trial, $p = 0.73$. However, the dysphoric group significantly improved their effectiveness by the second 25 s of the first probe trial, $p < 0.001$, and the difference between their effectiveness and that of the control group approached the level of significance, $p = 0.058$. The control group's performance did not improve during the first probe trial, $p = 0.54$. This data indicates that the dysphoric group recovered from the detrimental effect on performance of masked trials more rapidly than controls.

The data from masked probe trials shows that the effects of mood were only evident on the first probe trial. The probability that the control group pressed the late button during this trial was lower in comparison to the dysphoric group. This between groups difference was due to the dysphoric group returning to higher levels of response frequency and response effectiveness in the second half of the first trial with their responses on the late button.

DISCUSSION

Participants in this study "matched" their responding to the programmed reinforcement contingencies (e.g., Chatlosh et al., 1985). They did not use what would have been a more effective, but more effortful, all or nothing maximization strategy (Koehler and James, 2009). In other words, they would have produced more outcomes had they made all responses on one button in the first half of each trial [$p(\text{early}) = 1$, $p(\text{late}) = 0$] and then switched to the other button during the second half [$p(\text{early}) = 0$, $p(\text{late}) = 1$]. In general, on reinforced trials, the dysphoric group matched their behavior more closely to the contingencies than controls. Although there was no evidence that the mood groups differed in the extent to which they responded to the temporal shift in reinforcement. When reinforcement was masked during the probe trials, both groups performance suffered initially, but the dysphoric group recovered more quickly. These findings will be discussed in more detail below in relation to the DR hypothesis and the methodological issues raised in the introduction.

In the introduction, we noted several possible methodological issues with DR research, carried out by ourselves and others (e.g., Alloy and Abramson, 1979; Msetfi et al., 2005), that has utilized standard contingency judgment procedures. These included the ambiguity of the ΔP_{Prog}, especially in conditions with a non-zero $p(\text{light}|\text{no press})$ or long periods of inter-trial interval (Msetfi et al., 2005), and of the ΔP_{Exp}, as a result of response rate fluctuations (e.g., Matute, 1996). Finally, explicit verbal or written judgments of control may produce a biased test of contingency sensitivity and the DR hypothesis (Allan et al., 2007).

We found that when participants were exposed to the reinforcement of the light flash during learning trials, the dysphoric group responded overall in a manner that was more consistent with the programmed contingencies than the controls. Consider that the programmed contingencies on each lever were $\Delta P = 0.85$ and $\Delta P = 0.15$, the average of which is 0.50 over the course of each experimental trial. Both groups behavior "matched" the programmed contingencies, but the dysphoric group produced an average $p(\text{late})$ of 0.5, consistent with the programmed average, and significantly higher than controls. In order to produce this pattern of probabilities, participants were required, not only to be sensitive to the programmed contingencies, but also to match their behavior rapidly and closely to the switch between contingencies midway through the trial. Although there is some evidence that people with depression are less sensitive to time passage (Msetfi et al., 2012), and perceive time to be passing more slowly (e.g., Gill and Droit Volet, 2009), there was no evidence here that dysphoria was associated any temporal insensitivity to reinforcement contingencies. In fact, to the contrary, it could be argued that improved matching is evidence of improved contingency sensitivity and perhaps DR in the dysphoric group.

This evidence for DR could however have been based on fluctuations in response frequency rather than any particular propensity to realism. It has been argued previously that response rate variability could produce deviations in the ΔP_{Exp}, as well as the effectiveness of behavior. Indeed, helplessness and behavioral passivity is one of the classic symptoms of depressed mood. In this study, it was not possible for fluctuations in levels of behavior to influence the ΔP_{Exp}, but it was possible that controls would respond at higher levels, and that their responses would therefore be more effective in producing outcomes, even if they were less realistic than the dysphoric group. However, we found no evidence that controls either responded at higher levels or received higher levels of reinforcement (Blanco et al., 2009). In fact, overall, the control group responded at a lower level than the dysphoric group. On reinforced trials, this difference was too subtle to produce a significant effect on response effectiveness. Thus, whereas the dysphoric group was more successful at contingency matching than controls, there were no between group differences on reinforced trials in how effective behavior was in producing as many light flashes as possible.

It was also important to examine responding in the absence of direct exposure to reinforcement. On masked probe trials, both groups responded appropriately and effectively, though performance did suffer initially from the absence of reinforcement. As hypothesized, between group differences were amplified in the absence of reinforcement. We had expected that realism, or an improved awareness of the causal effectiveness of actions, would result in less of a decrement in performance when reinforcement was withdrawn. However, although response effectiveness dropped for both groups in early probe trials, the speed of recovery from it depended on mood. For the control group, response effectiveness steadily improved over the four probe trials. The dysphoric group recovered rapidly by the second half of the first probe trial to formerly high levels. Essentially, the dysphoric group required less time to recover from the withdrawal of direct reinforcement. These results have several theoretical implications and suggest avenues that require further exploration.

For example, these data provide no support for the idea that depression is consistent with low response levels and reduced exposure to reinforcement (e.g., Lewinsohn and Libet, 1972; Lewinsohn and Graf, 1973). In fact, both dysphoric and control groups responded at equally high levels. This finding is also inconsistent with Blanco et al. (2009), who found that dysphoric participants, exposed to a zero contingency procedure with a high frequency of outcomes, responded less and made lower contingency ratings than controls. In the present experiment, participants were exposed to positive outcome probabilities in comparison to the zero contingencies Blanco et al. (2009) tested. Thus it might be that zero contingency conditions are a special case that produce lower response rates in dysphoric groups, which might itself be a particular form of the DR effect.

It is also interesting to note that, in the present study, dysphoric participants were less affected than controls by the withdrawal of reinforcement in probe trials. Although response effectiveness was reduced just like with controls, the effect did not last so long in the dysphoric group. The effectiveness of their behavior improved significantly by the second half of the first probe trial. One possible reason for this might be because people with dysphoria are less responsive or sensitive to reinforcement in the first place and less affected by its absence. This suggestion is consistent with negative relationships, reported in the normal population, between mood and reinforcer sensitivity (Glautier et al., 1998), antidepressant administration and increased sensitivity to outcomes (Murphy et al., 2012), and theoretical reinforcement sensitivity approaches to psychopathology (e.g., Gray, 1982).

Thus far, we have interpreted the current findings as evidence for DR. It should be noted however that, rather than evidence of good learning, probability matching as observed in the general population has been characterized as a non-normative tendency (West and Stanovich, 2003). In comparison to a considered and effective maximization strategy, contingency matching could be seen as a "mistake" based on a rapid response to the situation (Koehler and James, 2009). In fact, we hypothesized that controls might actually adopt a maximization strategy based on higher response rates. This was not the case. In this study, controls "matched" their responses to the programmed contingencies but not quite as consistently as the dysphoric group. However, it could be argued that the current results are not suggestive of improved learning in dysphoria but perhaps a stronger tendency toward less than normative responses.

In summary, we have found dysphoria to be associated with improved response-outcome contingency sensitivity using a time based contingency procedure. Participants in the dysphoric group produced responses that were more consistent with the programmed contingency over time than participants in the control group. The effectiveness of their responses also recovered more rapidly from the withdrawal of reinforcement. There was no evidence for a link between dysphoria and reduced response levels, experienced reinforcement contingencies or reduced sensitivity to temporally marked changes in contingency. The findings from this behavioral task are novel, as effective performance must involve sensitivity to contingencies that change over time and depression effects on causal learning have not previously been studied in this manner. These findings provide support for the DR hypothesis, though realism may not necessarily be indicative of normative behavior.

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APPENDIX

INSTRUCTIONS

The task instructions appeared on the computer screen, and participants were able to self pace through the instructions using the "carry on" button.

Screen 1

In this game you will see a picture of a light bulb appear on the screen beside two different buttons.

It will be your job to try to make the light switch on as many times as possible. You will be able to press the buttons next to the light bulb as many times as you wish in order to try to make the light come on.

Screen 2

When the light does switch on, it will appear as a brief flash and will switch off again immediately. We want this to happen as many times as possible.

There are several rules to this game however!

∗You must not press both buttons at once.∗

∗You must not press and hold the buttons down.∗

Screen 3

When the game starts, you will see the light bulb first of all.

Then after waiting for a few seconds, the two buttons will appear.

When you can see them, you can press them!

You can press the button on the left using "the tab key."

You can press the button on the right using the return key.

Screen 4

There is no limit to how many times you can press the buttons (as long as you follow the rules).

Just keep in mind that your job is to make the light switch on as MANY times as possible.

Screen 5

When your first go at pressing the buttons is finished, the buttons will disappear from the screen.

There will be a short delay, after which the buttons will reappear and you can have another go. Each go at pressing the buttons is called a trial.

You will have quite a lot of trials in this button pressing game, and during each trial, you must follow the same rules.

Screen 6

During each trial, you will learn a lot about how to make the light flash. We will need to check what you have learned.

Therefore on some trials a message will appear on the screen saying "This is a test trial." On a test trial, you will not see the light bulb on the screen. Even though you cannot see the light, you must carry on and press the buttons exactly as if the light bulb were on the screen.

Screen 7

Even though you will not be able to see the light on test trials, the experimenter will monitor how often the light flashes in order to collect data. Your aim on test trials is to use what you have learned to make the light flash as many times as possible.

There will only be a few test trials in the whole game, which will take approximately 15 min. When the game is finished, a message will appear on the screen. If you have any questions, please ask the experimenter now or say that you are ready to start the game.

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The temporal priority principle: at what age does this develop?

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The temporal priority principle states that all causes must precede their effects. It is widely assumed that children's causal reasoning is guided by this principle from early in development. However, the empirical studies that have examined children's use of the principle, most of which were conducted some decades ago, in fact show inconsistent findings. Some researchers have argued that 3-year-olds reliably use this principle, whereas others have suggested that it is not until 5 years that children properly grasp the inviolability of the principle. To examine this issue, 100 children, 50 three-year-olds, and 50 four-year-olds, took part in a study in which they had to judge which of two causes yielded an effect. In the task, children saw one event (A), an effect (E), and then another event (B). The events A and B involved the rolling of balls down runways, and the effect E was a Jack-in-a-box popping up. The extent to which E left a visible trace was also varied, because comparisons across previous studies suggested that this may affect performance. As a group, 3- and 4-year-olds performed at above-chance levels, but performance improved with age. The nature of the effect did not have a significant impact on performance. Although some previous studies suggested that 3-year-olds may be more likely to choose B rather than A as a cause due to a recency effect, we found no evidence of this pattern of performance in the younger group. Potential explanations of the age-related improvement in performance are discussed.

Keywords: causal reasoning, time, development

INTRODUCTION

Much of the recent research on children's causal learning has focused on their ability to use statistical information to make causal inferences, with such learning modeled using the causal Bayes net approach (e.g., Gopnik and Schulz, 2004; Gopnik et al., 2004; Sobel et al., 2004; Gopnik, 2012; see Lu et al., 2008; Griffiths and Tenenbaum, 2009 for more recent Bayesian approaches to causal learning). Researchers in this tradition have typically emphasized the good performance of young children in causal learning tasks (see Gopnik and Schulz, 2004; Gopnik, 2012 for review), suggesting developmental continuity in causal cognition. However, important questions still remain about whether there may be qualitative differences between younger and older children's causal abilities, with such differences being a theme of the body of research on children's causal cognition that pre-dated the Bayesian approach (e.g., Shultz and Mendelson, 1975; Bullock, 1985; Corrigan and Denton, 1996; Schlottmann, 1999). In this paper, we focus specifically on whether there are developmental changes in children's grasp of the principle that causes must precede effects.

The temporal priority principle in causal reasoning states that causes always come before their effects in time, even though there may appear to be some circumstances in which cause and effect appear perfectly contemporaneous (for example, when we press a button on a TV remote, or turn the volume up or down on the radio). Although some philosophers have argued for the possibility of backward causation (Dummett, 1954), in everyday life there would never be a circumstance in which causal attributions would knowingly be in breach of the temporal priority principle, i.e., adults would never choose the succeeding event as the cause of a given effect. As this principle, and causal cognition more generally, is fundamental for success in the world which surrounds us, it is important that we determine the age at which the inviolability of the principle is properly appreciated.

Most of the studies that examined children's use of the temporal priority principle did so in the 1970s and 1980s (Shultz and Mendelson, 1975; Kun, 1978; Bullock and Gelman, 1979; Sophian and Huber, 1984). However, in recent years there has been a resurgence of interest in investigating temporal cues to causation in both adults' and children's judgments (Buehner and May, 2004; Lagnado and Sloman, 2004, 2006; Buehner, 2005; Buehner and McGregor, 2006; Burns and McCormack, 2009; Greville and Buehner, 2010; Frosch et al., 2012). Some of these studies have addressed whether children and adults will infer the structure of events in a causal system, based on the temporal pattern in which events occur (Lagnado and Sloman, 2004, 2006; Burns and McCormack, 2009; Frosch et al., 2012), whereas others have examined how the temporal contiguity of an event and an outcome affect causal strength judgments (Buehner and May, 2004; Buehner, 2005; Buehner and McGregor, 2006). In both of these sorts of studies, it is assumed that participants' causal judgments will respect the temporal priority principle. However, we can query whether such an assumption is appropriate with respect to young children.

As we have mentioned, most of the empirical research into children's use of the temporal priority principle was conducted some decades ago, conclusions were drawn on the few studies conducted, and then the issue was then evidently laid to rest. Based on these studies, review papers such as that of Bullock et al. (1982) have drawn clear conclusions about the age at which young children respect the principle when making causal judgments (Bullock et al., 1982; Burns and McCormack, 2009). The study that is most frequently referred to is that of Bullock and Gelman (1979), which found that children as young as 3 years of age chose a temporally prior event as the cause of an effect more often than would be expected by chance. Based on their findings, researchers will typically assume that 3-year-olds are capable of understanding the fact that causes always precede effects in time (Bullock et al., 1982). However, on closer inspection of studies investigating the temporal priority principle, it is apparent that despite the principle's fundamental nature, the empirical evidence regarding children's ability to adhere to this principle in their causal judgments is mixed (Shultz and Mendelson, 1975; Kuhn and Phelps, 1976; Kun, 1978; Bullock and Gelman, 1979; Shultz et al., 1986). Indeed, we would argue that on the basis of the current evidence available, it is not clear at what age children properly appreciate the principle when making causal judgments.

One possible reason for the mixed pattern of findings is that the most common studies referred to in the literature have included relatively small sample sizes. For example, in Bullock and Gelman's (1979) study, there were only 16 children in each age-group and in Shultz and Mendelson's (1975) study there were only 18 in each age-group. Given the potential variability in performance by young children, differences in findings between studies could reflect characteristics of children in the samples. Arguably, these sample sizes are potentially not sufficient to provide a true or detailed representation of performance of the age-groups examined, and we included much larger sample sizes in the current study. Moreover, it is clear that, looking across the studies, researchers who have investigated children's use of the temporal priority principle have sometimes used very different methodologies. Most notably, some researchers asked children questions about pictures of familiar event sequences, whereas others used novel mechanical events and asked children to make causal attributions. This makes it difficult to directly compare each study, and draw firm conclusions based on their findings.

One technique used to examine children's use of the temporal priority principle involves two events, event A and event B, with children being required to choose the event that caused the other event to occur (Sophian and Huber, 1984; Shultz et al., 1986). Shultz et al. (1986) found that 3- to 4-year-olds failed to do this correctly when making causal judgments. However, one problem with the methodology in Shultz et al.'s (1986) study may be that, for some of the trials, A caused B to occur, while in other trials the causal order was reversed. Thus, there was not one standard cause and one standard effect. This could have been confusing for young children, and may have led to random responding. In many everyday scenarios, the roles of cause and effect cannot be reversed; for example, pressing a switch will cause the light to turn on but the light turning on does not cause the switch to be pressed. This suggests that the technique may not be the most appropriate one to use when investigating temporal priority.

A second technique involved presenting children with pictures depicting sequences of events, and children were then asked questions about causality (Kun, 1978; Das Gupta and Bryant, 1989). For example, Kun (1978) presented 4.5-, 6-, 7-, and 8-year-olds with 10 sets of three picture cards in the form "A caused B caused C," where A was the antecedent of B and C was the consequent of B. For instance, one set of cards depicted a child pulling a dog's tail (A); the dog biting the child (B); the child crying (C). Children were then asked three questions: what happened next (pointing at B), why did B happen, and a non-sense question. They simply had to point to the picture that answered the question. Kun (1978) found that children as young as 4 years answered these questions in a way that suggested that they understood the temporal priority principle: that is, they were able to choose the correct antecedent and consequent of B. A potential issue with this experiment is that many of the sequences presented to the children may have been familiar to them due to previous experiences and exposure to the events. Hence, the children may have already developed schemas about the events and the consequences of many actions, which might affect how they respond in this experiment.

A third technique that overcomes the problems identified with both of the other techniques, and is the one which was adopted by the current study, is what we have termed the A–E–B paradigm. This technique has been used in a number of previous studies (Shultz and Mendelson, 1975; Bullock and Gelman, 1979; Bullock et al., 1982; Sophian and Huber, 1984). It requires children to draw inferences about which of two possible events (A or B) caused an outcome. For example, children are shown a potential cause A, followed by effect E, and then potential cause B. In this case, the correct response is to choose A as the cause as it precedes effect E. Advantages of this paradigm are that, first, the events and sequences used in the paradigm can be chosen to be novel to children. Thus, it removes the opportunity for children to draw upon their knowledge of the causal power of familiar events. Moreover, the roles of cause and effect are never switched, unlike in the studies of Shultz et al. (1986) and Sophian and Huber (1984, Experiment 1). Finally, the paradigm draws very little on verbal abilities and so it cannot be argued that children's comprehension and verbal ability are confounding their performance.

Bullock and Gelman (1979) and Shultz and Mendelson (1975) adopted this technique in their studies. Shultz and Mendelson (1975) used three different pieces of equipment in their study, one of which we will focus on here. This piece of equipment was a wooden box with two holes on top at either end, with events A and B being the dropping of marbles into the holes and the effect being a bell ringing. Bullock and Gelman (1979) only used one piece of apparatus in their study, but it was very similar to the piece of equipment used by Shultz and Mendelson (1975). Bullock and Gelman's (1979) apparatus also involved a wooden box with two holes on top of the box, at either end. The experimenter would drop two balls into the holes (A or B) and children were then required to decide which ball caused the effect (E). The sequence always took the form A–E–B or B–E–A. Although in Shultz and Mendelson's (1975) study the effect (E) was a bell which rang inside the box, in Bullock and Gelman's (1979) study the effect was a teddy bear which popped up, giving a "Jack-in-the-box" effect.

Shultz and Mendelson (1975) found that 6- to 7-year-olds and 9- to 11-year-olds chose the preceding event as the cause whereas 3- to 4-year-olds were as likely to attribute the cause to the following as well as the preceding event. In fact, Shultz and Mendelson (1975) compared the performance of younger 3-year-olds (aged 3 years 0 months to 3 years 7 months) with the performance of older 3-year-olds (3 years 8 months to 3 years 11 months) and, interestingly, found that the younger 3-year-olds tended to erroneously choose the event that occurred most recently as the cause more often than would be expected by chance. This in itself raises an issue: why did younger 3-year-olds choose the most recent event as the cause, rather than simply responding at random? Shultz and Mendelson (1975) suggested that if young children do not understand the temporal order of the cause and effect process they will choose the event that is most salient to them, which in this case would be the most recent event. In contrast, Bullock and Gelman (1979) found that children as young as 3 years of age consistently chose the preceding event as the cause. Thus, unlike the conclusion drawn by Shultz and Mendelson (1975); Bullock and Gelman (1979) argued that their results suggest that children as young as 3 years old can and do rely on temporal ordering when making causal judgments (Bullock and Gelman, 1979). Also, like adults, they understand that temporal priority dominates over all other cues, for example, spatial contiguity (Bullock et al., 1982).

Considering both these studies used similar apparatus and a similar methodology, it is surprising that they found such contradictory results. Moreover, from these results, very different conclusions have been drawn about the age when children fully comply with the temporal priority principle. This prompted the current study to investigate this issue further, in an attempt to determine not only the age at which young children comply with the temporal priority principle, but also to discover why Bullock and Gelman (1979) and Shultz and Mendelson (1975) found conflicting results.

There are small methodological differences that could potentially provide an explanation for the differences in the results. One notable difference is in relation to how the experimenters obtained responses from children. Shultz and Mendelson (1975) asked children a series of quite complex questions whereas Bullock and Gelman (1979) opted for a more simple response method, using questions that young children may have been more likely to understand. In addition to this, although the apparatus used in both studies included runways, it was only in Bullock and Gelman's (1979) apparatus that the runways were actually visible to children. Thus, there was a difference in spatiotemporal and mechanism information between the two studies

There were two other methodological differences that may have potentially affected the amount of attention that children paid to the B event in the sequence. In Bullock and Gelman's (1979) paper, they stated that event B occurred "coincident with the start of the Jack's action," i.e., that there was no delay between event E and B, whereas in Shultz and Mendelson's (1975) study, there was an obvious delay between event E and event B. Moreover, the studies differed in the extent to which the effect (E) left a visible trace once it occurred in the sequence of events. In Shultz and Mendelson's (1975) study, the effect was a bell, which rang. There was no visible trace of the effect, and the bell had stopped ringing before the second potential cause occurred. By contrast, in Bullock and Gelman's (1979) study, once the "Jack" popped up, it remained up even when the second ball was released into the hole. It could be argued that these variations – in the delay between B and E and in the persistence of the causal consequence – may potentially explain the difference in results obtained across these studies. In particular, these differences may have had an influence on children's attention while they observed the sequence of events. In Bullock and Gelman's (1979) study, once the "Jack" popped up and remained up, attention may have been so focused on the "Jack" that children may not have witnessed the second ball being released into the hole. Therefore, when asked to choose the ball that made the "Jack" pop up, they may have chosen the first ball because this was the only ball that they had attended to. Conversely, as mentioned earlier, in Shultz and Mendelson's (1975) study the bell stopped ringing before the second ball was released into the hole and so this may have allowed children to concentrate on the second ball and increased the likelihood it was chosen as being causally relevant to the effect. Alternatively, it could be argued that the persistent visibility of the "Jack," and its intrinsic interest to children, increased the salience of the effect which in turn may have made it easier for the children to correctly recall the order of events, and as a result, choose the correct event as the cause.

It seems plausible that these variations in Shultz and Mendelson's (1975) and Bullock and Gelman's (1979) methodologies could explain the difference in results, and that they could be explored in an attempt to explain the variation in results. However, more importantly, with regards the issues regarding existing studies that were raised earlier, it is appropriate to return to the question of when children reliably appreciate the temporal priority principle in their causal reasoning. Thus the primary aim of the current study was to attempt to determine the age at which young children adopt the principle of temporal priority when making causal judgments. Additionally we were interested in whether Shultz and Mendelson (1975) were correct to argue that younger 3-year-olds will tend to answer systematically incorrectly in this type of task, by showing a recency effect in their causal judgments. The current study used a large sample of children aged 3 and 4 years, with participants assigned to one of two conditions. In one condition, the "Jack up" condition, the effect involved a teddy popping up and staying in view, whereas in the other condition, the "Jack down" condition, the teddy bear popped up and then went down again. The key difference between these conditions was in whether there was a persistent visually available effect when the second possible cause occurred.

MATERIALS AND METHODS

PARTICIPANTS

One hundred children took part in the study, fifty 3-year-olds ($M = 43$ months; range = 36–47 months) and fifty 4-year-olds ($M = 53$ months; range = 48–59 months). There were 64 females and 36 males in total. Half of each age-group was randomly assigned to one of two experimental conditions. Children were recruited in local schools and preschools. All the children were

FIGURE 1 | Diagram of the front view of the apparatus.

tested individually in the child's school or preschool and each child received a sticker for taking part.

APPARATUS

The apparatus consisted of a wooden box, 70 cm long, 21.5 cm high, and 23 cm wide. On the left and right sides of the box were plexiglass windows showing two runways inside the box. One runway was painted red (the location of event A) and the other runway was painted white (the location of event B) so children could discriminate between the two. The runways were 30 cm long, began at openings on the top outer corners of the box and dropped at a 30° angle toward the center of the box. The runways were a mirror image of each other (see **Figure 1**).

White and red wooden balls (2 cm in diameter) were placed onto holders situated at the edge of the openings of the box. A button could be pressed at each holder to release the ball into the opening, and begin the sequence of events. When the balls were released they rolled down the runways, disappeared from view, and silently rolled into a compartment at the back of the box that was easily accessible to the experimenter and out of sight from the child.

On the top middle section of the apparatus was a 10 cm × 10 cm opening to which a lid was attached, and a teddy bear Jack-in-the-box (event E) was located beneath this lid and could pop up from it. Even though the first event was manually instigated by the experimenter, unbeknownst to children, the teddy bear popping up and release of the second ball was driven by hidden motors and built-in timers. This allowed for maximal control over the sequence of events and their timing. The timing between A and E, E and B (in an A–E–B sequence), and B and E, and E and A (in a B–E–A sequence) was fixed at 1.5 s between each event. By means of a digital video recording, we estimated that the amount of time a ball was visible as it rolled down a runway was 0.38 s. Thus, once the ball was released into the runway (event A), it took 0.38 s for it to roll down the runway and disappear from view. Then after another 1.12 s, the teddy bear popped up (event E). Once the brief Jack-in-the-box event had finished, after another 1.5 s, ball B was released into the runway (event B), and 0.38 s later, ball B disappeared from view.

DESIGN

There were two experimental conditions, labeled "Jack up" condition and "Jack down" condition. The design was between subjects with children in each age-group randomly assigned to one of the two experimental conditions. In both conditions, the sequence of events was initiated by the experimenter pushing the button that released the ball into the hole. In the "Jack up" condition, after 1.5 s, the Jack popped up and remained up (effect E), and after another 1.5 s, the second ball was released into the hole. In the "Jack down" condition, after 1.5 s, the Jack popped up and then disappeared back down again under the lid. After another 1.5 s the second ball was released into the hole.

PROCEDURE

At the beginning of the testing session, children were invited to take a seat facing the box and were told that they were going to play a game with the experimenter. They were introduced to the "special box," and then asked to name the colors of the runways and the wooden balls. The children were then asked to watch very carefully as the experimenter placed a wooden ball onto the holder and released it into one of the runways. They watched the experimenter initiate four training demonstrations (A–E, A–E, B–E, B–E, or B–E, B–E, A–E, A–E, with order counterbalanced across the groups) after which they were asked two closed, forced choice questions. One was about ball A and the other about ball B: "Did the white ball make the teddy bear pop up?" and "Did the red ball make the teddy bear pop up?" This training phase was included in order to demonstrate that either ball could potentially cause the "Jack" to pop up. Thus, correct judgments at test had to be based solely on the temporal order cues available to children. If children answered these questions correctly the experimenter moved onto the test trials, but if children answered incorrectly the experimenter repeated the training phase again.

During the test trials, children saw four sequences of events. They observed one ball being released into the runway, disappear, the teddy bear popping up and then the second ball being released into the runway. Although it looked to children that the experimenter released the ball, because the experimenter's hand was held directly behind the ball on its holder, in fact it was released by a timing mechanism. Children saw the A–E–B sequence twice and the B–E–A sequence twice. Thus, twice the correct answer was to choose the red ball, and twice the correct answer was to choose the white ball. The order in which the trials were presented was randomized for each child. After each trial children were asked to choose the ball that "made the teddy bear pop up." No feedback was given. Once participation was complete, children were thanked for their participation, given a sticker and were then returned to their classroom.

RESULTS

Table 1 shows the mean number of times children chose the temporally prior event in each of the two conditions. It is clear that 4-year-olds performed better in both conditions. The table also shows that, for both age-groups, there appears to be very little difference between how children performed in each of the two conditions. **Figure 2** shows the frequencies of correct responses of 3- and 4-year-olds collapsed across both conditions. The majority of the 4-year-olds chose the temporally prior event in each of the four trials: 41 performed perfectly compared to only 22 three-year-olds. An analysis of variance (ANOVA) was conducted with between groups factors of condition and age-group. The

Table 1 | Number of times children chose the temporally prior event in each of the two conditions.

Age-group	Jack-up		Jack-down	
	Mean	SD	Mean	SD
3 years	2.84	1.31	2.92	1.15
4 years	3.64	0.91	3.76	0.60

Scores can range from 0 to 4.
There were 50 children in each age-group.

Table 2 | Age in months of children achieving each score.

	Number correct				
	0	1	2	3	4
Jack up	$N = 3$	$N = 2$	$N = 6$	$N = 8$	$N = 31$
Mean age	46.33	44.50	45.67	48.50	50.35
SD	4.51	3.54	2.73	6.48	5.82
Range	42–51	42–47	41–49	42–59	36–59
Jack down	$N = 1$	$N = 1$	$N = 10$	$N = 6$	$N = 32$
Mean age	47	39	41.16	46.17	49.81
SD	–	–	6.53	6.68	5.42
Range	–	–	36–57	39–58	39–59

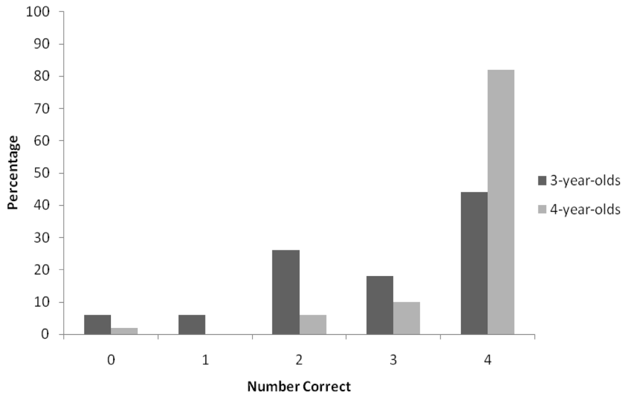

FIGURE 2 | Frequencies of correct responses by 3- and 4-year-olds across the four experimental trials collapsed across both conditions.

results revealed no significant main effect of condition and the interaction between age-group and condition was also not significant, both *Fs* < 1. There was however a significant main effect of age-group $F(1,99) = 15.90$; $p < 0.001$. This result indicates that 4-year-olds chose the temporally prior event significantly more often than 3-year-olds. However, although 3-year-olds did not perform as well as 4-year-olds, a one-sample *t*-test (with a test value of 2, because children completed four trials) revealed that 3-year-olds' performance was significantly above chance, $t = 5.09$; $df = 49$; $p < 0.001$. As would be expected, a one-sample *t*-test revealed that 4-year-olds' performance was also significantly above chance, $t = 15.76$; $df = 49$; $p < 0.001$.

In light of Shultz and Mendelson's (1975) findings it was decided to investigate 3-year-olds' performance in more detail to examine if the younger 3-year-olds were likely to attribute the cause to the event that followed the effect rather than the event that preceded the effect. For this analysis, the responses of the 23 youngest 3-year-olds (3 years, 1 month to 3 years, 7 months, *M*: 3 years, 4 months) were compared with those of the 27 oldest 3-year-olds (3 years, 8 months to 3 years, 11 months, *M*: 3 years, 9 months). The mean number of correct responses for the younger group was 2.78 in the Jack up condition and 2.64 in the Jack down condition; means for the older group were 2.88 and 3.27, respectively. A two-way ANOVA with factors of age (young versus old 3-year-olds) and condition revealed no significant main effects or interactions, all *Fs* < 1. Therefore, there was no significant difference in how the younger 3-year-olds responded compared to how the older 3-year-olds responded. In addition, we examined the age of children who achieved each of the five possible scores; **Table 2** shows the mean and range of these ages. It can be seen from the table that the children who were likely to erroneously choose the most recent event as the cause (those scoring 0 or 1) were not notably younger than those who simply responded at chance levels (scoring 2), and indeed that the groups of children with the higher scores included some of the youngest children. Thus, there was no indication in our data of a developmental shift from below-chance to above-chance performance. In fact, **Figure 2** clearly shows that within the group as a whole, relatively few 3-year-olds systematically chose the most recent event as the cause.

DISCUSSION

Adopting the A–E–B paradigm, and using similar apparatus to that used in two influential studies, we attempted to discover whether the causal judgments of both 3- and 4-year-olds would reliably reflect the principle that causes must always precede their effects in time. The key findings were that while children in both age-groups judge that a preceding rather than a succeeding event was the cause of an effect more often than would be expected by chance, there was a significant developmental improvement in the numbers of correct responses between the two ages. In the study, we also manipulated between two conditions whether or not the outcome was one that was visually present at the time at which the succeeding event occurred, because we had hypothesized that this may affect the likelihood that children would perform well on this task. However, there was no significant difference in levels of performance across the conditions.

The performance of the 4-year-olds was very good in both conditions, with children's judgments rarely defying the temporal priority principle. Three-years-olds' judgments were much less likely to be correct. Although as a group they performed significantly above chance, **Figure 2** shows something resembling bimodal performance in this age-group, with around a quarter of 3-year-olds performing at chance levels (two correct) and just below a half of the group getting all questions correct. The findings suggest that while some 3-year-olds seem to have firmly grasped the temporal priority principle, others are not yet reliably incorporating this principle in their causal judgments. Following

Shultz and Mendelson's (1975) analyses of younger versus older 3-year-olds' performance, we examined whether the performance of our 3-year-olds differed depending on their age. However, there was no difference between how younger 3-year-olds performed compared to older 3-year-olds. Moreover, there was no tendency for young 3-year-olds to choose the succeeding event as the cause, and thus, these findings are not consistent with those of Shultz and Mendelson (1975).

In fact, our findings are not completely consistent with those of either of the early studies that our paradigm is based on. Unlike Bullock and Gelman (1979), we found that 4-year-olds performed significantly better than 3-year-olds. However, unlike Shultz and Mendelson (1975), we found that the younger group as a whole performed at above-chance levels. As it was found that the difference in the visibility of the "Jack" did not influence how children responded, this suggestion can be discarded as an explanation for the reason why these two previous studies found such contradictory results. It may be the case that, as discussed earlier, there are other procedural differences between the two previous studies that could potentially account for the variance in their results. Moreover, it should also be highlighted that we found marked individual differences in children's performance levels in our 3-year-old group. Thus, the differences in findings between the previous studies could also reflect the varying ability levels of young children in the relatively small samples they tested.

It should be noted that the current study opted for a procedure that differed from both Bullock and Gelman's (1979) and Shultz and Mendelson's (1975) procedures (i.e., neither condition in our study was an exact replication of either of their methodologies). We used simple questions like those of Bullock and Gelman's (1979), as we felt that Shultz and Mendelson's (1975) questioning procedure was too complex. However, our procedure did differ from that of Bullock and Gelman's (1979) in that we left an obvious delay between event E and event B, in order to match the delays between A and E and E and B (assuming an AEB sequence). We note that in our procedure, even though these delays were identical, once B occurred the ball took a further 0.38 s to pass down the runway and reach the Jack. Thus, the causally relevant component of the B event (the ball reaching the Jack) was more temporally separated from the effect than that of the A event. We deliberately introduced a clear delay between E and B so that children's attention would not be divided between E and B (which occurred in different spatial locations). However, it might be argued that the consequence of this – the different temporal contiguity between E and the causally relevant components of the A and B events – may have biased participants in favor of choosing A. One way to avoid this problem would be to cover the runways up completely, so that children simply see the balls being dropped into the box, and do not see any additional visuo-spatial information. In fact, we have carried out such a study (Rankin and McCormack, unpublished), and found extremely similar results to those reported here.

An important question that remains is why the ability to consistently apply the temporal priority principle improves significantly between 3 and 4 years. We can distinguish between at least two possible explanations of the age effect: that changes reflect improvements in information processing efficiency (a processing explanation), or that changes reflect a new appreciation and understanding of the temporal priority principle itself (a reasoning explanation). With regard to the former explanation, a likely candidate process may be that of memory. Indeed, it has been argued that memory limitations may contribute to the difference in performance between 3- and 4-year-olds (Shultz and Mendelson, 1975; Kun, 1978; Koslowski and Masnick, 2002). It could be the case that 3-year-olds cannot remember the order of the event sequence in some trials, and, although they may be trying to use the temporal priority principle in their judgments, they are more likely to make errors than 4-year-olds because they mis-remember the sequence.

It seems likely that remembering event order and using this information to make an inference places demands on young children's working memory resources. While the development of working memory has been extensively explored across childhood, we are only aware of a single study that has directly examined how it may affect children's causal judgments. McCormack et al. (2013) measured working memory abilities alongside 4- to 7-year-old children's causal learning in the context of a quite different causal learning task (one examining the cue competition effect of blocking). They found that children's performance on the task was predicted by their working memory abilities over and above chronological age and verbal ability. Thus, there is some evidence that even relatively basic causal judgments might be affected by children's working memory skills. We are currently exploring whether this is also the case in a causal task similar to that used in the present study.

It may also be the case that 3-year-olds' understanding of causal principles is not as advanced as 4-year-olds'. This suggestion draws upon research that has investigated children's use of temporal cues to make causal inferences (McCormack and Hoerl, 2005, 2007; McColgan and McCormack, 2008). The results from these studies suggest that children's may be able to represent or remember the temporal order of events before they properly understand of the causal significance of this order. For example, McCormack and Hoerl (2007) conducted a study in which children were required to judge the outcome of a sequence of events based on the order in which events A and B occurred. Children were introduced to two dolls, John and Peter, and they were told that the dolls take turns to do things but one doll (e.g., John) always goes first and the other doll (e.g., Peter) always goes last. Children were then told that the dolls were going to go into a room to brush their hair. The experimenter closed the door of the room so that the children could not see what was happening, although the experimenter told children that one doll brushed his hair, placed the hairbrush in one cupboard in the room when he was finished and that the other doll retrieved the brush and then placed it in the other cupboard. After this, the test phase commenced where the experimenter placed each doll beside the picture of the cupboard that he had put the brush into, and children were required to decide which cupboard the brush was in now. McCormack and Hoerl (2007) were interested to see if children could make the appropriate inference about the current location of the brush based on the temporal order in which the dolls had taken their turn, even though children did not directly see the dolls take their turn in front of them.

McCormack and Hoerl (2007) found that 5-year-olds were successful at choosing the correct location, but 4-year-olds were at chance, suggesting that 4-year-olds' ability to reason about the temporal order of the events was not as advanced as that of 5-year-olds. This finding suggests that it may be the case that young children can only perform successfully when they can see the events unfold in front of them. McCormack and Hoerl (2007) provided some evidence for this suggestion by repeating the experiment with 3-year-olds, with the apparatus set up in such a way so that children could see the events unfold in front of them. In this condition, even 3-year-olds chose the correct location. From these findings, McCormack and Hoerl (2007) argued that it is actually viewing sequences of events occur in front of them unfolding in a certain order that allows young children's judgments to reflect the order in which events occurred, rather than their ability to represent and then reason about event order (see also McCormack and Hoerl, 2005; McColgan and McCormack, 2008; Hoerl and McCormack, 2011)

McCormack and Hoerl (2007) put forward a suggestion that may explain how young children perform successfully on reasoning tasks where they can view the sequence in front of them. They suggested that when young children (around the age of 3–4 years) view causal sequences, such as the A–E–B sequence of events, they make a causal judgment without necessarily attending to or reflecting on the entire event sequence. Young children may operate along the lines of a default: when making causal judgments, they ignore any event which occurs after the effect E. It may be the case that once children see the effect E, they may no longer encode the rest of the sequence as causally relevant. Thus, when asked causal questions they may not even consider event B in the sequence, and so will automatically choose event A as the cause. McCormack and Hoerl (2007) termed this process an encoding default process. They argued that this process is non-insightful and does not require an explicit understanding about the role of temporal order in determining the causal structure of events. It usually leads to successful performance because the temporal order in which they see the events is also the causal order. However, this account would assume that young children do not have an explicit grasp of why one event is the cause and another event is not the cause, that is, they have not grasped the significance and logical force of causal order.

McCormack and Hoerl (2007) distinguished between such an encoding default process and making temporal priority judgments by reasoning about order. They suggested that older children and adults reflect on the whole sequence, recall the order based on their memory for the event sequence, and choose the causally efficacious event based on an understanding about temporal priority and the temporal order of events. If children possess this ability, then they should never make errors when it comes to choosing the causally efficacious event providing they can remember the order in which the events happened.

Thus, in relation to the findings from the current study, it may be the case that 3-year-olds are operating along the lines of an encoding default process. As a group they are above chance in choosing the correct event as the cause because the temporal order is the same as the causal order, and because they can see the events unfolding in front of them. In order to account for developmental improvements in performance, we need to assume that this encoding process does not always function optimally (i.e., that children do not always encode A as a causally relevant event). If this is correct, 3-year-olds, when faced with the test question, will sometimes find themselves with no previously encoded information about what is causally relevant. Moreover, if they lack an explicit grasp of the temporal priority principle, they will have no basis on which to make an inference, even if they can recall the order in which the events occurred, and will have to guess. In contrast, 4-year-olds are more successful as they are able to reason about event order, putting to work the principle that causes always precede effects, which will inevitably yield the correct answer. This could be further explored by covering the entire event sequence, and then informing children afterward about the order in which the balls had been dropped. If children are basing their judgments on something like an encoding default process, then they should struggle to choose the temporally prior event if they cannot see the events unfold in front of them. By contrast, if children are reasoning about event order based on their explicit understanding that causes precede effects, then we would expect to find that children would successfully choose the temporally prior event even under these circumstances.

We have distinguished between two possible reasons for some 3-year-olds' poorer performance: that they may have poorer memory skills or that they may lack an explicit grasp of the nature of the temporal priority principle. In the current study, we did not actually ask children to recall the order in which the events had occurred (though see Sophian and Huber, 1984), but assessing children's memory for the event sequence along with their causal judgments may in fact be critical in distinguishing between these two explanations. If memory problems underpin children's difficulties in the task, then we would expect to see a close relationship between the accuracy of memory for the event sequence and causal judgments, such that when children choose B as the incorrect cause they also are likely to erroneously report the order in which events occurred. Such a pattern of performance would suggest that children do understand the temporal priority principle but have difficulty putting it to work because of problems remembering event order. Alternatively, it may be that, at least for younger children, there is no close relationship between memory performance and performance on the causal task: e.g., younger children may get causal questions wrong but get memory questions correct. This second pattern of performance would suggest that children do not fully appreciate the temporal priority principle. Indeed, if young children's causal judgments are underpinned by an encoding default process, we might expect there to be no close relationship between memory for event order and causal judgments, because the latter would not be based on the former. Rather, children's causal judgments would reflect the extent to which they encoded each of the previous events as causally relevant at the time at which the events unfolded.

The fact that we have found development improvements in such a basic aspect of causal learning indicates that there may be important age-related changes in causal cognition that may be overlooked within the causal Bayes net approach that currently dominates developmental research in this area (Gopnik and Schulz, 2004; Gopnik et al., 2004). The Bayesian account is a computational approach that does not aim to describe the psychological processes involved in causal inference, therefore, as it stands, the issue of whether 3-year-olds' problems stem from memory difficulties or conceptual problems is not one that Bayesian theorists need take a stance on. Indeed, Gopnik (2012) argues that even preschoolers' causal inferences typically resemble those of an idealized Bayesian learner. Nevertheless, it could be argued that the very existence of developmental improvements in performance, such as those reported here, makes it pressing to identify what the important underlying processing changes are.

In conclusion, it has been shown that there is a difference between how 3- and 4-year-olds perform in a simple causal paradigm, with 4-year-olds performing significantly better than 3-year-olds. However, unlike findings from previous research, we found that even young 3-year-olds are unlikely to show a recency effect in their causal judgments. It is the more random performance of a sub-group of 3-year-olds that requires a developmental explanation. Our finding suggests that either that 3-year-olds' understanding about the temporal priority principle is not as advanced as that of 4-year-olds, or that they fail to remember the order in which the events have occurred.

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Domain-specific perceptual causality in children depends on the spatio-temporal configuration, not motion onset

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Humans, even babies, perceive causality when one shape moves briefly and linearly after another. Motion timing is crucial in this and causal impressions disappear with short delays between motions. However, the role of temporal information is more complex: it is both a cue to causality and a factor that constrains processing. It affects ability to distinguish causality from non-causality, and social from mechanical causality. Here we study both issues with 3- to 7-year-olds and adults who saw two computer-animated squares and chose if a picture of mechanical, social or non-causality fit each event best. Prior work fit with the standard view that early in development, the distinction between the social and physical domains depends mainly on whether or not the agents make contact, and that this reflects concern with domain-specific motion onset, in particular, whether the motion is self-initiated or not. The present experiments challenge both parts of this position. In Experiments 1 and 2, we showed that not just spatial, but also animacy and temporal information affect how children distinguish between physical and social causality. In Experiments 3 and 4 we showed that children do not seem to use spatio-temporal information in perceptual causality to make inferences about self- or other-initiated motion onset. Overall, spatial contact may be developmentally primary in domain-specific perceptual causality in that it is processed easily and is dominant over competing cues, but it is not the only cue used early on and it is not used to infer motion onset. Instead, domain-specific causal impressions may be automatic reactions to specific perceptual configurations, with a complex role for temporal information.

Keywords: perception of causality, social causality, physical causality, causal reasoning, domain-specificity, agency, animacy, cognitive development

INTRODUCTION

Humans, including infants from 6 months, perceive causality when one geometric shape moves briefly after another, on a linear path. Motion timing is crucial in this and causal impressions disappear with short delays between the motions. However, the role of temporal information in perceptual causality is more complex than this: it provides not only cues to causality but is also a processing factor. It affects not only ability to distinguish causality from non-causality, as commonly emphasized, but also to distinguish social from mechanical causality. Here we consider this wider role of temporal information with children aged 3 to 7 years and adults.

Perceptual causality in motion sequences obviously lacking real causality has been the topic of much research since Michotte's (1963) seminal work, which continues to attract interest because it promises one simple solution to the complex problem of how we know about cause and effect. Michotte argued that in some cases we do not need to know, but can simple see causality. Rather than requiring much experience with relevant events, and complex reasoning to link the experienced events to another, causality appears as a Gestalt property of particular motion sequences. Just as we see a triangle when shown three appropriately configured corners (Kanizsa, 1976), we see causality, one event producing another, when shown two motions in appropriately configured sequence. This provides us with a perceptual identification of what cause is that does not require any conceptual knowledge or understanding.

Michotte's prime example was the launch event, in which shape A moved up to and contacted a stationary shape B and stopped, while B began to move away immediately (**Figure 1A**). For this sequence people typically report seeing A launch B, i.e., physical causality, confirmed by many independent studies (e.g., Schlottmann et al., 2006). A temporal delay of half a second or less at the point of contact destroys this impression in adults, as does a gap of a few millimeters. However, the overall impression depends on the configuration, affected also by speed and other factors, e.g., small gaps are tolerated at high speeds with small delays (Schlottmann and Anderson, 1993).

Later on, Kanizsa and Vicario (1968) also found perceived social causality in reaction events, very similar to launch events, but without contact (**Figure 1B**): a moves up to B, but B begins to move away before A could reach it. Both move simultaneously for a fraction of a second, then A stops and B moves away. Now people report social causality, that A is chasing B and B is running away, also confirmed independently (e.g., Schlottmann et al., 2006). Michotte and others, in particular White (e.g., White and Milne, 1997, 1999), have provided many other examples of what White calls interaction impressions, e.g., impressions of pulling,

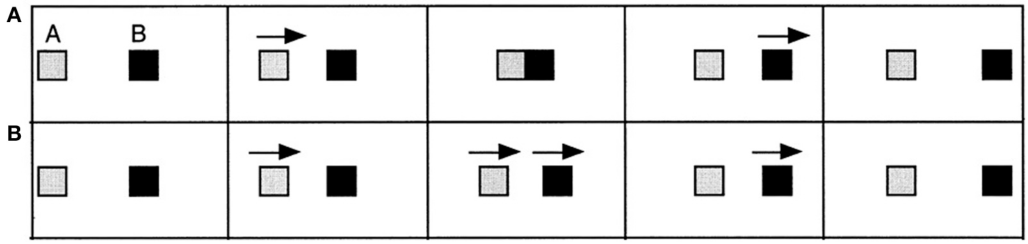

FIGURE 1 | Michotte's (1963) launch event (top—A) and Kanizsa and Vicario's reaction event (bottom—B).

bursting, etc., typically for interactions in the physical domain, while work in the Heider and Simmel (1944) tradition suggests extensions to the social domain. In the present study, however, we focus on launch and reaction causality.

Perceptual causality independent of reasoning and learning, dependent only on particular structural features, as claimed since Michotte (1963), remains controversial for adults. After all, adults have relevant experience and a conceptual understanding of cause that they could bring to bear on interpretation of these simple motion sequences, which for the most part can be seen as representations of real causal events. Thus, White (2006) argues that causal interaction impressions arise when an event triggers a matching representation in memory, and the involvement of memory in adults' impression is difficult to rule out.

On the other hand, there have been multiple demonstrations of perceptual causality in infants (Leslie and Keeble, 1987; Oakes, 1994; Cohen and Amsel, 1998; Schlottmann and Surian, 1999; Schlottmann et al., 2009, 2012). Infants have little relevant experience and presumably lack an a priori understanding of cause that would allow them to identify particular sequences as potential cause and effect sequences. These demonstrations make a claim that causality is perceived more plausible, and many infancy researchers take this view (Leslie and Keeble, 1987; Schlottmann et al., 2009, 2012, see Scholl and Tremoulet, 2000).

In modification of Michotte's original claims, the view that causality is perceived need not imply that infants' and adults' experience does not contribute. Instead of a modular reading (Scholl and Tremoulet, 2000), the claim might be merely that there is a perceptual core to causal structure on which learning can build: if from infancy we see certain instances of causality even without relevant knowledge, this would support the acquisition of knowledge relevant to these causal events. Perception links the events together, and children can then figure out at their leisure why the events go together. These more rational and experience-dependent analyses of perceived causal links may well come to affect the perceptual impression subsequently. On this non-modular reading one can hold that causality is perceived, without denying that it is affected by experience and knowledge, thus sidestepping the controversy (Schlottmann, 2000; Schlottmann et al., 2009, 2012).

While perceptual causality has been much studied with adults and with infants, there is less work with talking-age children. Yet this is important—in infants perceptual causality can be inferred only indirectly from how long they look at launch and reaction events, but for direct evidence we need some form of perceptual report requiring language and therefore older children. The problem is, however, that children find it difficult to freely express their perception, and that the drawn-out, age-related changes found in early studies of children's verbal reports of launch and related events (Olum, 1956, 1958; Lesser, 1974, 1977; Thommen et al., 1998) could reflect development of perceptual causality or simply language development. That the latter is substantially involved follows from a study reporting perceived launch and reaction causality from age 3, when language requirements were reduced by asking children whether a picture of Postman Pat engaging in a physical or social or non-causal interaction fit various motion events (Schlottmann et al., 2002); the present study also adopts this picture methodology.

When we use a methodology other than free verbal reports, it is conceivable that observers may not report their spontaneous causal perception, but that the use of social and physical causal language is metaphorical, and that observers merely draw analogies between motion patterns on the screen and memories of real world events triggered, in this case, by stationary pictures or instructions. In the extreme, observers may not represent the causality of motions on the screen at all, but merely use such language after matching screen motion and event memory on lower level features. Adults, in contrast to this view, do spontaneously report causality in these types of screen events, and there is good agreement between results found with free report and structured responses, as typically used in contemporary studies (for review, see Scholl and Tremoulet, 2000; Schlottmann et al., 2006).

This worry is unfounded with children as well: it is already clear that even preverbal infants represent the causal, not just spatio-temporal structure of launch and reaction events, as reviewed above, and there is no reason to assume that this representation is lost in talking-age children. Moreover, that young children report analogies prompted by the instruction rather than spontaneous perception is even less likely than for adults: while pre-schoolers are capable of analogical inference, they are not prone to do so unless there is clear agent similarity between domains or knowledge of the underlying causal relations (see Goswami, 1992; Rattermann and Gentner, 1998; and discussion in Schlottmann et al., 2002). Note also that even the strongest Michottian claim is not that observers confuse these screen events with physical/social causation in the real world, it is merely that observers see one shape launching/chasing the other, being aware at the same time that these are all just shapes on a screen. Use of a structured methodology with children therefore would not seem to fundamentally change the nature of the task, but merely increases its sensitivity.

When such a structured picture methodology was used instead of free verbal report, pre-schoolers easily recognized basic forms of perceptual causality, but there were also age-related changes in the role of various perceptual cues (Schlottmann et al., 2002). From 3 years, children reliably identified launch events as instances of physical causality and reaction events as social causality, distinguishing these from non-causal events with a delay, but only 5-year-olds were as accurate in identifying non-causal events as they were in identifying causal events. Younger children often over-attributed causality, in part due to a causal response bias. This study thus suggests, firstly, that children's facility with the causal-noncausal distinction develops over the preschool range, while, secondly, the domain distinction seems well-established by age 3.

On the first point, it may seem surprising that children have difficulty with delayed events, given that infants perceive causality in causal but not delayed sequences. However, this makes sense when we consider that causal perception of some events does not imply non-causal perception of others. While launch and reaction events may be perceptually "special" with a relatively automatic meaning, this does not hold for delay events, which are perceptually neutral, without meaning. Children have to think about their interpretation, which is more difficult at younger ages.

The second point, that even the youngest children had no difficulty at all distinguishing domains of perceptual causality, agrees well with the standard position that from infancy interactions in the physical and social worlds are distinguished by whether the agents make contact or not (Premack, 1990; Mandler, 1992, 2004; Baron-Cohen, 1994; Leslie, 1994, 1995; Baron-Cohen and Ring, 1995; Carey, 2009): this is because mechanical interactions require spatial contiguity while social agents can also interact from afar. The absence of contact indicates that an action was self-initiated, and only social agents are capable of this. Concern with contact thus ultimately reflects concern with domain-specific mechanisms of causation.

For adults, of course, spatial contiguity is not the only cue to the causal domain. In perceptual causality, for instance, adults attend to animacy cues as well, attributing social causality more when the shapes move in apparently animate manner (Schlottmann et al., 2006), but this did not affect children, even though they recognized the movement as animate (Schlottmann et al., 2002). On face value, these data thus suggest late developmental change in how domains of perceptual causality are distinguished, with children, like adults, attending to contact relations from very early on, while other cues are attended only much later. The present studies evaluate this view.

Our first experiment reconsiders the previously found neglect of animacy cues: is this a true developmental difference, or could it be merely a secondary consequence of children's difficulties with temporal delays, discussed above? The inclusion of delay events in perceptual causality tasks may tax children's processing resources, and as a result children may not be able to attend to all cues available. On this view, if the task is simplified, by elimination of difficult non-causal events, this may free resources to attend to animacy. But this should continue to be neglected if there is a developmental difference in how children and adults distinguish domains of perceptual causality.

Should children be able to consider other than spatial information in Experiment 1, this raises the question whether the already established distinction of reaction from launch causality is best described as reflecting attention to contact relations (Premack, 1990; Mandler, 1992, 2004; Baron-Cohen, 1994; Leslie, 1994, 1995; Baron-Cohen and Ring, 1995; Carey, 2009), or whether temporal information plays a role as well. The issue arises because launch and reaction events differ spatially, but also temporally: one has contiguous, the other simultaneous motion. Thus, both types of information could underlie the earliest domain distinction in perceptual causality. Difficulty grasping the causal implications of temporal delays, described above, need not imply difficulty grasping the implications of temporal information more generally. Accordingly, Experiment 2 studies children's causal impression for displays varying temporal and spatial information independently, to assess whether temporal cues contribute to it as well. In this study, therefore, we move from considering temporal information as a processing factor that can impede or facilitate processing, to considering the cues to causality that it might provide.

Two further experiments consider at what level spatio-temporal cues might affect children's causal impression, in particular, whether children use these cues for inferences about the mechanism of causation. If children mainly consider whether a motion is self- or other initiated, as under the standard proposal, then they may treat reaction events with occluded motion onset as less social than standard reactions, inferring the possibility that contact might have occurred out of sight in the former (Experiment 3). Similarly, if displays have both simultaneous motion-at-a-distance and contiguous contact motion (Experiment 4), children may treat motion-at-a-distance preceding contact as more social than contact motion preceding motion-at-a-distance, because the latter is not ultimately self-initiated. If, on the other hand, children's causal impressions are relatively automatic reaction to particular perceptual configurations, then they might treat the two motion orders or occluded/non-occluded motion onsets similarly.

In sum, our experiments revolve around the domain-distinction in perceptual causality. While the standard view emphasizes the importance of spatial cues for a distinction between physical and social events, we consider in two experiments whether temporal and animacy cues may play a role as well. The standard view also holds that children use the perceptual information to determine whether motion-onset was self- or other initiated, and this is assessed in two further experiments.

We report the data as 4 experiments. However, one group of children participated in Experiment 1, while another group saw events relevant to Experiments 2 to 4, which are separated for convenience of analysis and argument. A third group of children provided additional data in Experiment 3 and 4, as noted below. Thus, we have four conceptually, but not always materially different studies.

EXPERIMENT 1

The first experiment considers whether children can use other than spatial information in determining domains of perceptual causality. As discussed, previous work highlights children's difficulty with identifying non-causal delayed events. Here we consider whether this difficulty might affect children more widely: if it is processing-intensive to focus on whether events are non-causal, this may reduce ability to process other task components relying on the same resources. Elimination of the need to attend to non-causality may thus allow children to consider cues previously neglected.

We assess, in particular, if with reduced processing requirements, children's causal impressions are affected by motion-style cues to the causal domain. Michotte (1963) first reported that a non-rigidly moving shape that rhythmically expands and contracts gives a strong impression of animate motion (**Figure 2**), and this appears for adults (Schlottmann et al., 2006), children (Schlottmann et al., 2002), and infants (Schlottmann and Ray, 2010). This animate motion also strongly affects causal impressions in adults (Schlottmann et al., 2006), but not children or infants (Schlottmann et al., 2002, 2009, 2012). Here we test whether this reflects processing limitations or a true developmental difference in perceptual causality.

MATERIALS AND METHODS

Subjects

Sixteen 3-, 4-, 5-, and 7-year-olds (mean ages 3 years 7 months, 4 years 4 months, 5 years 3 months, 7 years 6 months; ranges 3 years 2 months to 3 years 11 months, 3 years 10 months to 4 years 9 months, 4 years 11 months to 5 years 10 months, and 6 years 9 months to 7 years 9 months), from London nursery and primary schools. Children consented orally and also had written consent from one parent.

Materials

Children chose from two A4 sized drawings to illustrate the target concepts. The same pictures were used as in Schlottmann et al. (2002), with the physical causality picture showing Postman Pat having kicked a football, while the social picture showed Pat chasing another man. Neither picture involved contact between Pat and ball/other agent.

Each child saw 4 different computer-animated motion events, made with Macromedia Director and shown on a Macintosh laptop attached to a 12 inch color monitor. Each event involved two squares (50 × 50 pixels, about 2.5 × 2.5 cm). Red (A) always started on the left, moved toward Green (B) in the middle of the screen and Green moved away toward the right. The motions repeated continuously, with about 0.6 s (30 frames) black screen separating cycles.

Two events were launch events, in which A moved toward B, contacted it mid-screen, and stopped, while B began to move as soon as A touched it. The other two were reaction events, in which A and B remained about 3 cm (60 pixels) apart. A moved alone for 30 frames, then A and B moved together for 30 frames, then A stopped, and B continued for another 30 frames. In one event of each type A and B moved rigidly at a rate of 4 pixels/frame (about 9.5 cm/s) over 60 frames. In the other event, the shapes moved non-rigidly, as in **Figure 2**. The square first extended over 10 frames at a rate of 8 pixels/frame with the left edge stationary, then it contracted at the same rate with the right edge stationary. After repeating these steps twice more, the non-rigid shape had covered the same 240 pixels distance in the same time as the rigidly moving shape. Each event took just under 5 s.

Procedure

Children were tested individually at their school. Children were first introduced to the response pictures and the target meanings were explained by questioning about their content. If children did not make appropriate statements, the experimenter (E) described the pictures to them as in Schlottmann et al. (2002). This was typically necessary for 3- and 4-year-olds.

Next children were shown the stationary squares on the screen. Children were told they would see different movies in which these squares would move, and to watch carefully so that they could explain afterwards what was happening in the movie; children were also told that the pictures would help them with this. Then the first movie was shown. After watching for an uninterrupted cycle, E pointed to the physical picture asking "does the green move because the red has hit, like in this picture?" She then pointed to the social picture asking "Or does the green move because it wants to run away from red, which is chasing it, like in this picture?" The event kept cycling until the child made a choice. Questions were typically not needed anymore after a couple of movies, with children pointing spontaneously, but questions were repeated as necessary. Sessions took about 10 min, with most of this time spent on initial discussion of the pictures. Movies were presented in individually randomized order.

Results

Table 1 gives the percentage of physical or social attributions to four events. The data replicate previous findings that all ages see contact events (rows 1 and 2) as largely involving physical causality, while non-contact events involve psychological causality (rows 3 and 4). However, in contrast to previous work,

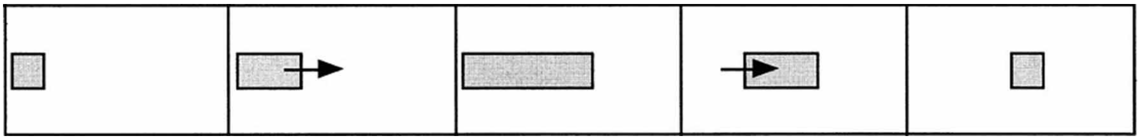

FIGURE 2 | Michotte's (1963) caterpillar stimulus. The square appears to move itself by first rhythmically expanding from the right edge, then contracting from the left edge.

Table 1 | Percentage of physically and socially causal attributions, in 4 age groups, for launch events with contact and reaction events without, each involving rigid, inanimate, or non-rigid, animate motion in Experiment 1.

Row No.	Event		Age							
	Spatial feature	Motion style	3 years		4 years		5 years		7 years	
			Phy	Soc	Phy	Soc	Phy	Soc	Phy	Soc
1	Contact	Rigid	**88**	12	**94**	6	**100**	0	**100**	0
2	Contact	Non-rigid	**56**	44	31	**69**	50	50	31	**69**
3	No Contact	Rigid	**56**	44	25	**75**	6	**94**	19	**81**
4	No Contact	Non-rigid	19	**81**	6	**91**	25	**75**	0	**100**

Modal values in bold.

agent motion affected children's attributions: when contact events involved non-rigidly moving shapes they more often appeared as social causality, while non-contact events appeared more often as physical with rigid shapes (rows 2 and 3). The effect was strongest for the youngest children, who were split in their attributions.

Statistical analysis agreed with the visual impression. To enable factorial ANOVA, physical attributions received a score of 1, social attributions of -1. High proportions of physical attributions thus produce positive scores up to 1, while high proportions of negative attributions produce negative scores up to -1. Mixed responses move scores toward the chance level of 0. ANOVA effects can thus reflect the choice patterns in **Table 1** (Lunney, 1970; Rosenthal and Rosnow, 1984; see Schlottmann et al., 2002).

The ANOVA here found not only a main effect of spatial information, $F_{(1,\,60)} = 101.76$, $MSE = 0.15$, $p < 0.01$, $\eta^2_{\text{partial}} = 0.62$, with more physical attributions to contact event, but also of motion style, $F_{(1,\,60)} = 42.21$, $MSE = 0.17$, $p < 0.01$, $\eta^2_{\text{partial}} = 0.41$, with more social attributions to non-rigid motion, and an interaction, $F_{(1,\,60)} = 22.26$, $MSE = 0.11$, $p < 0.01$, $\eta^2_{\text{partial}} = 0.27$, with non-rigid motion reducing physical attributions to contact events more than it increased social attributions to non-contact events. This asymmetry appeared for all but the 3-year-olds, leading to an age × motion style × spatial contiguity interaction, $F_{(3,\,60)} = 3.74$, $MSE = 0.11$, $p < 0.02$, $\eta^2_{\text{partial}} = 0.15$, and an age main effect, $F_{(3,\,60)} = 2.84$, $MSE = 0.13$, $p < 0.05$, $\eta^2_{\text{partial}} = 0.12$.

When the age groups were considered separately, all ages had spatial main effects, $F_{(1,\,15)} > 8.44$, but, as described above, there was no interaction for 3-year-olds, with $F_{(1,\,15)} > 10.00$ for the other ages. Five-year-olds had no motion style main effect, with $F_{(1,\,15)} > 8.44$ for the other ages, due to a minor inversion in the data: at age 5, but not 3, 4, and 7, social impressions for non-contact events were slightly more frequent with rigid (94%) than non-rigid motion (75%). The reason is unclear, but the effect is small, with 5-year-olds, as all other ages, typically treating both non-contact events as social.

Discussion

This study for the first time found animacy effects on children's impressions of perceptual causality, with events involving non-rigidly moving shapes moving in an animal-like pattern generally producing more social, less physical responses from age 3. Overall, the pattern was similar to that found for adults, including that non-rigid motion affects launch causality more strongly than reaction causality (see Schlottmann et al., 2006). This asymmetry appeared for all but the youngest children.

The finding has two implications: first, there is not, after all, a developmental difference in domain-specific perceptual causality, such that this is affected by animacy only in adults. Rather, previously reported lack of animacy effects in children (Schlottmann et al., 2002) may reflect processing limitations. When the task requires thought about what delayed events mean, children may not have the resources to consider animacy cues at the same time, but when there are no delays, they do use animacy information.

Second, our finding highlights the different processing demands of spatial, temporal, and motion-style cues in perceptual causality tasks. An alternative view is that the animacy effects found here reflect more general changes in the task: they could be due to a reduction in response complexity, with two, not three response options. Or they might appear whenever children have to attend only to two cues rather than three. However, eliminating the need to attend to spatial information by presenting only contact or only non-contact events did not improve children's performance even though there were only two cues and two response options (Schlottmann et al., 2002; Experiment 3). This argues against these more general reasons for children's improvement here.

Instead, we propose that contact information is processed automatically in non-delayed motion events, even by young children, in contrast to delays and to motion-style cues, so there is little gain when the need to attend to spatial cues was omitted in Schlottmann et al. (2002; Experiment 3), but greater gain when delays were omitted here. This view fits with ceiling-level distinction of contact from non-contact causality from age 3, when children of the same age find it more difficult to distinguish causal from non-causal, delayed events (Schlottmann et al., 2002; Experiment 1 and 2), and when they attend to animacy only under simplified conditions, as studied here. Note that when both attention to delays and motion-style was possible (Schlottmann et al., 2002; Experiment 1 and 2), children attended to delays, not motion-style, even though one could argue that at a rational level the latter should be more important, with the non-rigid shapes appearing as self-propelled animate agents capable of social interaction, and there is no reason why such interactions should not contain delays. Nevertheless, delays seem more intrinsically important to perceptual causality than the nature of the agents.

In sum, the most important finding was that there is no developmental difference in perceptual causality between adults and children after all: all ages can consider animacy information in roughly similar manner. The experiment also further supports a view that some aspects of launch/reaction events are processed automatically, while others require attention, which may help explain why children are not always affected by all the same informers as adults.

EXPERIMENT 2

If young children can attend to other than spatial cues in perceptual causality, then this raises the question whether their distinction of reaction from launch events really depends mainly on spatial information, as theories of infants early ontological distinctions suggest, following Premack (1990), or whether temporal information contributes as well, because launch and reaction events differ in both.

To study this, we varied temporal and spatial information independently. Each child saw 6 events, in which A and B moved simultaneously, contiguously or after a delay, either with or without contact. Contiguous contact events are standard launch events (**Figure 1A**), simultaneous non-contact events are standard reaction events (**Figure 1B**). Simultaneous contact events correspond to Michotte's entraining events (**Figure 3A**). Yela (1952) has previously described contiguous events with sizable gaps between the shapes (**Figure 3B**) as launching without collision, but Schlottmann et al. (2006) found that adults do not see such events as clearly physical. Gap events have not yet been studied with children.

As discussed, preschoolers have difficulty interpreting delay events. They do not lack perceptual sensitivity to the delay: infants as young as 2 months can detect delays of less than half a second (Lewkowicz, 1996), and 6-months-olds treat events with delays between 600 ms and 1 s as non-causal in habituation-of-looking-time studies (Leslie and Keeble, 1987; Oakes, 1994; Schlottmann et al., 2009, 2012). Nevertheless, 3-year-olds often ignore delays of that magnitude in causal attributions. Here, we increased the delay, to over 2 s, in an attempt to make it more salient to children.

MATERIALS AND METHODS

Participants

Three child groups, 30 children each, participated, as well as 22 adults. The nursery group (18 girls) had children aged 3 and 4. The year 1 group (15 girls) had children aged 5 and 6, and the year 3 group (15 girls) had children aged 7 and 8. Children were from a London nursery and two primary schools. The adults (15 females) were typically undergraduates in their early twenties.

Materials

Children chose from three A4 sized drawings to illustrate the target concepts (**Figure 4**), featuring Postman Pat pushing a post cart, Postman Pat standing while another walks by for non-causal, independent motion, and Postman Pat chasing someone who runs away. The agents did not make contact in any of these pictures. We switched the physical picture from the previously used football picture, to fit the entraining event better.

Events involved the same animated shapes, speeds and distances as before, but this time each child saw 6 different motion events, all involving rigid motion. In 3 events, A contacted B mid-screen, in the other 3 events, A and B remained about.6 cm (60 pixels) apart. In contiguous motion events, with and without contact, A moved first, and B began to move as soon as A contacted it. In simultaneous motion events, with and without contact, A moved alone for 30 frames, then both moved together for 30 frames, then A stopped, and B continued for 30 frames. In delayed motion events, A contacted B, and B began to move after 120 frames (about 2.5 s). The contiguous and simultaneous

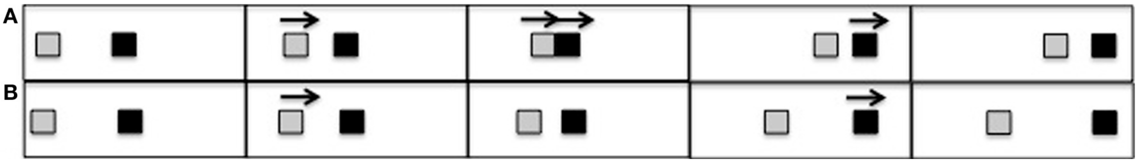

FIGURE 3 | Michotte's (1963) entraining event (A) and launching with a gap (B).

FIGURE 4 | Choice pictures for physical causality, non-causality, and social causality.

motion events took 240 frames in total (just under 5 s), with stationary periods at beginning and end adjusted; the delayed events took 260 frames.

Procedure

The procedure was as before. When the first movie was shown children were not just asked about the physical and social picture, but also about the non-causal picture: "Or does the green move on its own, not because of anything red has done, like in this picture?" Sessions took about 15 min, including the initial discussion. Movies were presented in individually randomized order.

Results

In **Table 2**, contact events without delay (rows 1 and 2) received about 90% attributions of physical causality, with no apparent age differences, and with no apparent difference between entraining and launch events. Thus, simultaneous motion *per se* is not a cue to social causality. Delayed contact events (row 3), in contrast, received far less causal attributions at all ages, and were typically seen as non-causal.

For no contact events, in contrast, it made a difference whether motion was contiguous or simultaneous: standard reaction events, with simultaneous motion (row 4), were treated as socially causal at all ages, while gap events with contiguous motion (row 5) received more non-causal, less social attributions. Delayed events without contact (row 6) were treated as non-causal, slightly more so than delayed contact events. Performance of the child groups on delay events was generally better than in previous work.

In line with the visual impressions, the ANOVA (as before, with score 0 for non-causal responses) found main effects of Spatial and Temporal Information, as well as an interaction, with the smallest $F_{(2,\ 216)} = 8.53$, $MSE = 0.41$, $p < 0.01$, $\eta^2_{\text{partial}} = 0.07$. Follow-up tests showed no significant differences between attributions to simultaneous and contiguous contact events, $F < 1$, with high positive, physical scores for both. In contrast, simultaneous motion without contact received more social attributions, with more negative scores, than contiguous motion without contact, gap events, $F_{(1,\ 108)} = 14.08$, $MSE = 0.56$, $p < 0.01$, $\eta^2_{\text{partial}} = 0.11$, which in turn had higher negative scores than delay motion without contact, $F_{(1,\ 108)} = 6.85$, $MSE = 0.42$, $p = 0.01$, $\eta^2_{\text{partial}} = 0.06$. Delayed events with and without contact did not differ, with scores closer to 0, $F_{(1,\ 108)} = 2.14$, $MSE = 0.31$, $p = 0.14$. There were no age effects in any analysis, all $F < 1$.

Discussion

Experiment 2 shows clearly that temporal information is important for the domain distinction in perceived causality, not just spatial information. As in previous work, contact events were seen as physically causal, regardless of whether they involved contiguous launch or simultaneous entraining motion. Events without contact did not appear physical, and here the temporal structure mattered: while simultaneous motion without contact appeared as social causality, contiguous motion without contact appeared ambiguous, with more non-causal attributions. Physical reports of launching-at-a-distance appeared at no more than baseline level found for all stimuli, in contrast to Yela (1952). The same pattern appeared at all ages, including adult controls. It also replicates a recent study on adult free verbal report as well as ratings (Schlottmann et al., 2006). The difference to Yela's (1952) early results may reflect differences in stimuli, as well as instructions that allowed for both types of causality in the newer work.

The delay events here had 2+ rather than 1 s delay as in previous work. The extra long delay may have helped children, with even half of the 3-year-olds considering delayed events non-causal, an improvement over prior work, but as in previous work, children still had clearer impressions of causal than non-causal events. Age effects within this study did not reach significance, but non-causal attributions to delayed events still increased slightly with age, and appeared more frequently for delayed non-contact than contact events, also as in prior work.

Overall, Experiment 2 agreed with Experiment 1 that even young pre-schoolers attend to more than just contact relations when determining the domain of causality, in contrast to the standard view developed since Premack (1990). In particular, temporally overlapping, simultaneous motion is crucial for making non-contact events appear to show social causality.

EXPERIMENT 3

Under the standard view, contact or its absence is so important for the domain distinction because this indicates whether the motion is self-initiated, and only agents are capable of this (e.g., Premack, 1990). On this reading, reaction events are seen as involving social

Table 2 | Percentage of physically causal, non-causal, and socially causal attributions, in 4 age groups, for 6 events varying spatial and temporal features factorially in Experiment 2.

Row No.	Spatial feature	Temporal feature	Event name	Age											
				3 years			5 years			7 years			Adult		
				Phy	Non	Soc	Phy	Non	Soc	Phy	Non	Soc	Phy	Non	Soc
1	Contact	Simultaneous	Entraining (**Figure 3A**)	**87**	3	10	**90**	3	7	**93**	0	7	**91**	0	9
2	Contact	Contiguous	Launch (**Figure 1A**)	**90**	7	3	**80**	7	13	**97**	3	0	**91**	0	9
3	Contact	Delayed	Non-causal	23	**57**	20	40	40	20	27	**63**	10	18	**73**	9
4	No contact	Simultaneous	Reaction (**Figure 1B**)	17	7	**77**	0	20	**80**	20	10	**70**	18	14	**68**
5	No contact	Contiguous	Gap (**Figure 3B**)	20	30	**50**	17	40	**43**	23	**40**	37	18	**45**	36
6	No contact	Delayed	Non-causal	17	**70**	13	20	**60**	20	3	**90**	7	14	**77**	9

Modal values in bold.

causality, mainly because without contact, B is seen to self-initiate motion. The Experiment 2 finding that contiguous motion without contact does not appear as social causality is already at odds with this view. The next two experiments assess more directly the extent to which children's reaction to perceptual causality displays reflect concern with the onset of motion.

Experiment 3 assesses children's reactions to motions-at-a-distance with occluded onset. The logic here is that not all motions-at-a-distance are self-initiated, sometimes onset by contact may simply have occurred earlier, out of sight. Occlusion of the onset of motion thus allows for the possibility of earlier, unseen contact behind the occluder, and if the onset of motion is children's main concern, this manipulation should reduce impressions of social causality. On the other hand, if children ignore this distinction, treating all motions without contact, including occluded onset motions, as social, this would fit better with a view that children's impression is an automatic reaction to the perceptual configuration. A reduction in social impressions for occlusion events may, of course, grow with age, as children become more and more capable of integrating inference about the causes of motion with their perception.

Children saw various motions-at-a distance with occluded onsets. In one event, both objects emerged, one after the other, already in motion from the left edge of the screen, and eventually disappeared behind the right edge. This event, compared to the standard reaction in **Figure 1B**, has a much longer period of simultaneous motion-at-a-distance, which in itself might make the event appear more social. Another event therefore had occluders to both sides of the screen, with B and A emerging in motion from the left and disappearing behind the right occluder, as if seen through a window in the screen (**Figure 5**). Occluders were spaced so that the event had the same amount of simultaneous motion-at-a-distance as the standard.

In two further occluder events, B moved faster than A, so that their distance increased over the course of their simultaneous motion. Backwards extrapolation then suggests contact some time before. B and A emerged once in such quick succession that contact was suggested just previously, behind the edge of the occluder. In the other event, contact was suggested behind the middle of the occluder. Although only simultaneous motion-at-a-distance was shown, inference about motion onset would in both cases not just allow for contact, but make this the likely possibility, which should reduce social impressions even further, if children are mainly concerned with motion onset.

The main issue, in sum, is whether events with occluded motion onset elicit weaker social impressions than standard reaction events, as the standard view would predict (Premack, 1990). Such reduction might be due to children making inferences about the possibility of contact motion. If this appears, it will also be important to show that this reduction is not simply due to the addition of occluders or reversal in motion order *per se*. To assess this, we had two control events. One had occluders placed in front of a standard reaction, so that B could still be seen to self-initiate motion. The other had no occluders and visible onsets, but B moved first, so that A ran after B, rather than chasing it away, as in the standard.

MATERIALS AND METHODS

Materials

Events involved the same animated shapes as before, moving at standard speed from left to right, but the amount of visible motion differed. In edge-to-edge motion (row 1 of **Table 3** in the results), B emerged, already in motion from the left edge of the screen, followed by A, both then moved across the whole screen, disappearing at the other side. This stimulus did not show motion onset, but 158 frames of simultaneous motion at a distance when the standard had only 30 frames. In the occluded reaction (row 2), therefore, black occluders were placed on the screen so that only 30 frames of simultaneous motion at a distance were visible, exactly as in the standard unoccluded reaction event. The event differed, however, in that B rather than moving from rest in the middle of the screen, emerged from the left occluder, and A disappeared behind the right occluder rather than stopping in the middle; also B moves first, rather than A. To test the effect of the occluders *per se*, we also had a control stimulus with a standard reaction: a emerged already in motion from behind the left occluder moving toward B in the middle of screen, which started from rest prior to A reaching it and disappeared behind the right occluder (row 5 of **Table 3**). These three stimuli had equal speeds for A and B, as in the standard.

In two further occluder events B moved at twice the speed of A, so over the course of movement their distance increased. In both events A and B moved simultaneously between occluders over 30 frames, but in one event A was only 8 pixels behind B when it first emerged, suggesting contact 2 frames earlier, just behind the right edge of the occluder (row 3). In the other event, A was 60 pixels behind B when it first emerged, suggesting contact 15 frames earlier (row 4). Finally, we had another control event, without occluders and with visible motion onset, but here B moved prior to A, rather than the reverse, as in the occluder events, but unlike the standard (row 6). Shapes had the same locations as in the standard, thus when B moved first, this increased the effective distance between the shapes during the simultaneous motion part to 300 pixels; it was only 60 pixels in the standard reaction when A moved first.

FIGURE 5 | Occluded reaction. B (the black square) emerges already in motion from the left, followed by A (the lighter square); both then disappear on the right; the amount of visible motion at a distance is as in a standard reaction.

Table 3 | Percentage of physically causal, non-causal, and socially causal attributions, in 4 age groups, for 4 events showing motion-at-a-distance without visible motion onset, and for 3 other events in Experiment 3.

Row No.	Occluded onset events	Age											
		3 years			5 years			7 years			Adult		
		Phy	Non	Soc	Phy	Non	Soc	Phy	Non	Soc	Phy	Non	Soc
0	Standard reaction (A and B self-initiated, A moves first, data from Experiment 2; **Figure 1B**)	17	7	**77**	0	20	**80**	20	10	**70**	18	14	**68**
1	No visible onset motion, edge-to-edge (large amount of simultaneous motion, B moves first)	7	23	**70**	3	27	**70**	0	37	**63**	23	36	**41**
2	No visible onset motion, occluders (same amount of simultaneous motion as standard reaction, B moves first; **Figure 5**)	7	17	**77**	10	3	**87**	0	3	**97**	0	5	**95**
3	No visible onset motion, occluders (B moves first, vB = 2vA, distance suggests contact behind edge of occluder)	19	13	**69**	13	13	**75**	25	13	**63**	38	0	**63**
4	No visible onset motion, occluders (B moves first, vB = 2vA, distance suggests contact behind middle of occluder)	0	6	**94**	25	13	**63**	31	0	**69**	6	13	**81**
5	Control: standard reaction with occluders (A moves first, no visible onset, B moves 2nd, self-initiated)	17	**53**	30	20	**50**	30	10	**83**	7	0	23	**77**
6	Control: unoccluded non-standard reaction (A and B self-initiated, B moves first)	19	31	**50**	0	**63**	38	6	**50**	44	0	**56**	44

Modal values in bold.

Subjects and procedure

The same children as in Experiment 2 participated, seeing the three stimuli with equal speeds (1, 2, and 5 in **Table 4**), and other stimuli reported later in Experiment 4, in a 10 min long second session on the afternoon of the day of Experiment 2. Children were briefly reminded of target concepts and questions, then the study proceeded as before.

As with any within subjects experiment, there is a possibility of carry-over and learning effects. In fact, we expect that children will indeed remember the general task set-up, in order to make instruction for the second session simpler. We presented the stimuli in two sessions to reduce noise in the data due to overly long sessions affecting children's concentration. The possibility of introducing artifacts, for instance, due to children communicating about the study in between sessions is slim, e.g., the verbal report studies (Olum, 1956, 1958; Lesser, 1974, 1977) show that children of this age cannot appropriately describe such stimuli. As for stimulus-specific learning effects, we randomized stimulus presentation within each session to control for this, but this does not, of course, preclude learning from the first to the second session. However, the only learning effects reported in the perceptual causality literature are stimulus adaptation effects within a given session for adults (e.g., Gruber et al., 1957; Powesland, 1959; Schlottmann et al., 2006; Woods et al., 2012), such that exposure to many causal stimuli increases sensitivity to and exclusion of stimuli with deviations from the causal category. It has, however, not been shown that such effects last beyond the experimental session. In any event, the first session stimuli here typically appeared half as causal, half as non-causal, so sizable adaptation effects would not be expected.

Another set of participants, 16 per age group, saw the other three stimuli (3, 4, and 6 in **Table 4**), also as part of a larger session, not reported here for the sake of brevity. The same procedure was used, but some stimuli were different. This set also involved children from different London nurseries and primary schools, and adults (mean ages 3 years 8 months, 5 years 9 months, 7 years 10 months, 40 years; range 3 years 3 months to 3 years 11 months, 5 years 1 months to 6 years 2 months, 7 years 0 months to 8 years 11 months, 19 years to 58 years).

Results

The first rows of **Table 3** show that simultaneous motion at a distance elicits impressions of social causality even when motion onset is not visible and both shapes emerge already in motion. The impression is not at all reduced relative to the standard event (repeated as row 0) by the absence of self-initiated motion onset. Comparison of rows 1 and 2 shows that amount/duration of simultaneous motion does not matter: motion from one edge of the screen to the other (row 1) elicited the same proportion of socially causal impressions than when there were only 30 frames of simultaneous motion (row 2). Attempts to suggest the possibility of contact out of sight more strongly, by giving the shapes initial distances and speeds that imply contact behind the occluder (rows 3 and 4) produced at best small reductions in socially causal impressions. The single exception to predominantly social causality choices to occluded onset events is for adults seeing edge-to-edge motion (row 1).

These strongly social impressions are not due to incidental changes in event structure increasing social impressions and compensating for a reduction due to lack of self-initiated onset: the data in rows 5 and 6 show that the presence of occluders *per se* or a change in motion order if anything reduces rather than increases social impressions. The single exception here is again for adults whose social attributions are not reduced when the standard reaction is occluded (row 5).

Two ANOVAs were conducted, one for the stimuli shown to the first group of children (rows 1, 2, 5, as well as 0, the standard reaction data from Experiment 1), one for the stimuli shown to the second group of children (rows 3, 4, and 6), confirming the visual inspection. In the first group of children, a significant effect of event, $F_{(3,\ 324)} = 18.88$, $MSE = 0.36$, $p < 0.01$, $\eta^2_{\text{partial}} = 0.14$, reflected the reduced social impression on the occluder control event 5, and an event × age interaction, $F_{(9,\ 324)} = 4.84$, $MSE = 0.36$, $p < 0.01$, $\eta^2_{\text{partial}} = 0.11$, reflected that adults responded differently from children on this and the edge-to-edge motion event 1. In line with this, there were no significant effects of age or event, with uniformly high negative causal scores, when only the children's data were considered omitting event 5, $F_{(2,\ 174)} < 2.47$, $MSE = 0.36$, but adults differed from children on this event 5, as well as on the edge-to-edge motion event 1, $F_{(3,\ 108)} > 3.09$, $MSE = 0.34$, $p < 0.01$, $\eta^2_{\text{partial}} = 0.20$.

When the three events shown to the second group of children were compared (rows 3, 4, 6), no significant effects appeared, $F_{(2,\ 120)} < 1.69$, $MSE = 0.556$, despite the reduction in social impressions apparent for the reversed-order control motion (row 6). This likely reflects lack of power with the smaller group size ($n = 16$ vs. $n = 30$), because a similar size reduction for the occluder control (row 5) was significant with the larger first group. However, this slight ambiguity in result for the reversed-order stimulus does not affect the overall interpretation. The reduction in control events with visible onset would have been important if events without visible onset had also shown a reduction relative to the standard reaction (row 0), but the events without onset showed no such reduction.

If the equal and different speed occluder events shown to the two groups of children are compared, then the small reduction in social attributions when contact behind the occluder is implied (row 2 vs. row 3 and row 2 vs. row 4) is significant in both cases, $F_{(1,\ 168)} > 6.97$, $MSE = 0.328$, $p < 0.01$, $\eta^2_{\text{partial}} = 0.04$. The age × event interaction is significant as well in the comparison of event 2 and 4, $F_{(3,\ 168)} > 3.99$, $MSE = 0.328$, $p < 0.01$, $\eta^2_{\text{partial}} = 0.06$, with 3-year-olds showing no such reduction in row 4.

Discussion

Events with simultaneous motion-at-a-distance but without visible onset are nevertheless seen as involving social causality, by adults and children. In the equal speed occluder event, there was no reduction in social impressions whatsoever, compared to previous experiments. When B moved faster and extrapolation from speeds and distances implied contact out of sight, there was a small reduction, mainly at the older ages, but even so responses remained predominantly social. If observers see motion-at-a-distance as social because it involves self-initiated motion, then occluding motion onset should reduce or eliminate social impressions, because one can infer a possibility of contact. This is not the case.

Substantially more motion-at-a-distance in the edge-to-edge stimuli neither increased nor decreased strength of the social impression for children, but for adults impressions were much reduced. One possibility is that adults expect contingent motions of animate agents not to be completely smooth and straight, but to show small variations in direction or speed. Our simultaneous motion, however, remained steady and constant over an extended period of time, which might suggest a mechanically rigid connection between the shapes. From this perspective, the event might be seen as a pulling event, as studied by White and Milne (1997). The children here, however, seemed oblivious to this possibility. Note that pulling is equally compatible with the equal speed occluder event (row 2 of **Table 3**), yet no age seemed to entertain that possibility.

While tangential to the main issue, it is also of interest why the control events showed a reduction in social impressions. In particular, addition of the occluder to the standard reaction (row 5), for children at least, eliminated social impressions even though the simultaneous motion was fully visible. The reason may be that the stationary B was initially visible mid-screen, while A's starting position was hidden behind the occluder. Thus, children may have focused initially on B which may have made them miss some of A's motion when it emerged to the left. This would not affect children in any other events, because all other events initially showed both A and B, or neither. Adults do not seem to have had a difficulty backtracking to A, even if only B was shown initially.

The negative effect of changing motion order (row 6) was less clear, not reaching significance, but this may be mainly a power issue. If we accept the reduction for the sake of discussion, then it might be attributed to increased distance between the shapes while moving. However, in an adult psychophysical study (Congiu et al., 2010), distance effects were minor. Moreover, in events 3 and 4 here distances ranged between 8 and 180 pixels, with no detrimental effects. A more likely account is that the changed motion order afforded a slight change in interpretation that did not fit our instructions well: in the standard reaction (**Figure 1B**), when A moves first, it chases B, and this in turn causes B to run away. Our instruction emphasized this view, but when B moves first from rest (row 6 of **Table 3**), B causes A to run after it. Both views are of action and reaction, and B moving first from behind the occluder fits equally with both, but event 6 does not quite fit the first interpretation. This mismatch may have reduced the social responses at all ages, in other words, we think this reduction, if reliable, is an artifact of the particular way social causality was instantiated here. Again, this is a side issue.

The most important finding here was that the occlusion events themselves showed little to no reduction in social causality compared to the data from previous experiments. Observers do not seem concerned with the onset of motion, but rather they react to the motion configuration *per se*.

EXPERIMENT 4

As a second test of the view that children are mainly concerned with the onset of motion, we considered how children react to potential conflicts of features typical of social and physical events, i.e., involving both simultaneous motion-at-a-distance and contiguous contact motion, in sequences of either reaction + launch or launch + reaction events. In addition, this experiment also allows an assessment of the relative strength of children's reaction and launch percepts.

To achieve reaction + launch and launch + reaction sequences, B moved either faster or slower than A. The reaction + launch sequence began as a reaction event, with A moving toward B, then A and B moved simultaneously. However, B moved slower, so A eventually caught up, made contact and stopped, while B moved on, as in a launch event (**Figure 6A**). Rationally, with concern for the onset of motion, this sequence should appear as social causality, because ultimately B's motion is self-initiated.

To achieve a launch + reaction event, in contrast, B moved faster than A (**Figure 6B**): initially A set B in motion, but after contact both continue to move simultaneously, as in a reaction, though at ever increasing distance. Rationally, this should appear as physical causality, because B does not self-initiate motion. A similar sequence might be observed in the real world in a collision of a much heavier A with a much lighter B.

A second reaction + launch event differed from the one already described in that B (rather than A) was the first to move, but A caught up and contacted B nevertheless (**Figure 6C**). We did this because we worried that if children do not show concern for the onset of motion in standard reactions or in the event of **Figure 6A**, this might be because they might initially be drawn to the first (A) motion, missing the onset of the second (B) motion which provides the crucial evidence for its self-initiated motion. If B moves first, this should draw more attention to the self-initiation of B's movement. Again, the self-initiated motion theory predicts more social responses for reaction + launch than for launch + reaction sequences.

What, in contrast, is expected if children react to the perceptual configuration *per se*, without inference about onset? Event order should not matter then, but, because all sequences contain both the social and physical configurations, they might appear ambiguous. Alternatively, one percept might be stronger and could dominate. For adults, launch is stronger than reaction causality (Schlottmann et al., 2006). For children, we do not know this yet; we only know they identify launch and reaction causality equally well. These conflict event sequences therefore also address if launch and reaction causality have similar strength. If so, conflict events might lead to ambiguous impressions. If, in contrast, one interpretation dominates, then this percept may be stronger.

Reaction + launch vs. launch + reaction sequences not only differ in causal order, but also in the shapes' speeds, so we needed controls for how these speed differences affected the impression. Accordingly, we also had identical motion configurations to **Figures 6A–C**, except that the shapes were further apart so never made contact.

MATERIALS AND METHODS

Materials

Events involved the same animated shapes as before moving from left to right, and with A moving at the same speed reported previously, however, B moved either at half or double the speed, and the initial distances between A and B were adjusted as needed.

In the reaction + launch event of **Figure 6A**, B moved at half speed, so took 120 frames to cover the 240 pixels distance that A covered in 60 frames. First, A moved for 30 frames toward B, then both moved simultaneously for 30 frames with decreasing distance due to the slower B. After 60 frames of motion, A had caught up, made contact with B and stopped, while B continued for the remaining 90 frames. In the launch + reaction event of **Figure 6B**, B was twice as fast, so took only 30 frames to cover the distance, when A took 60. First, A moved for 30 frames, then contacted B, which began to move upon contact. Then both moved simultaneously at increasing distance for due to the faster B, and both stopped after 30 frames. In the second reaction + launch event of **Figure 6C**, B again moved at half speed, but this time B moved first in the reaction, not A. Initially, B moved alone for 15 frames, then both moved simultaneously with decreasing distance between them. After 60 frames of simultaneous motion, the shapes made contact, A stopped and B continued for another 45 frames. Corresponding animations without contact had identical temporal patterning, but the shapes were 60 pixels further apart initially, so never made contact.

Subjects and procedure

The same children participated as in Experiment 2 and 3. The first set of children saw launch + reaction and reaction + launch sequences of **Figures 6A** and **B** and corresponding non-contact control stimuli, interspersed in the same session as the occlusion stimuli of Experiment 3, in individually randomized presentation.

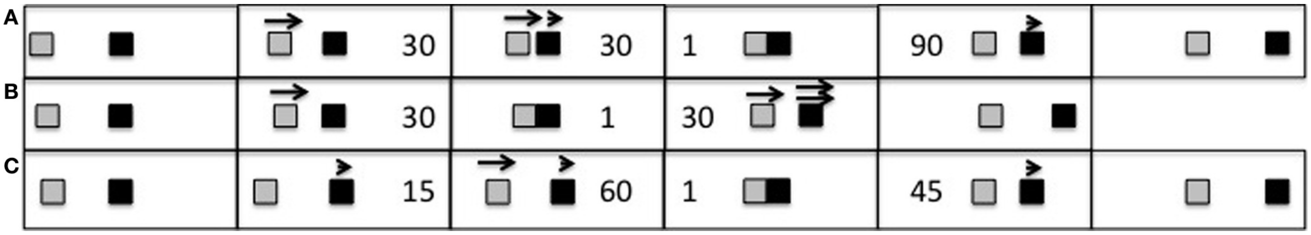

FIGURE 6 | Conflict events involving both contact and motion-at-a-distance. The top **(A)** shows motion at a distance followed by contact when A catches up with a slower B. **(B)** middle shows contact followed by motion at increasing distance due to B moving at double speed. **(C)** bottom, again shows motion at a distance followed by contact with a slower B, but here, in contrast to **(A)**, the first shape to move is B. (Short and double arrows indicate halved and double speed relative to the standard; numbers indicate the duration in frames of each motion component).

Table 4 | Percentage of physically causal, non-causal, and socially causal attributions, in 4 age groups, for 3 events combining contact motion and motion-at-a-distance, and for 3 control events with the same temporal pattern, but without contact in Experiment 4.

Row No.	Conflict event	Age											
		3 years			5 years			7 years			Adult		
		Phy	Non	Soc	Phy	Non	Soc	Phy	Non	Soc	Phy	Non	Soc
1	Launch + reaction (A first, vB = 2 vA) (**Figure 6B**)	**90**	0	10	**87**	3	10	**97**	3	0	**73**	0	27
2	Reaction + launch (A first, vB = 0.5 vA) (**Figure 6A**)	**90**	3	7	**87**	10	3	**77**	3	20	**45**	36	18
3	Reaction + launch (B first, vB = 0.5 vA) (**Figure 6C**)	**88**	13	0	**81**	0	19	**69**	6	25	**69**	0	31
4	No contact control (A first, vB = 2 vA) for row 1	10	20	**70**	0	13	**67**	13	10	**77**	9	18	**73**
5	No contact control (A first, vB = 0.5 vA) for row 2	13	37	**50**	20	40	**40**	13	43	**43**	9	45	**45**
6	No contact control (B first, vB = 0.5 vA) for row 3	6	**63**	31	38	38	25	25	19	**56**	25	38	38

Modal values in bold.

The second set of children saw the second, non-standard reaction + launch stimulus in which B moved first (**Figure 6C**) and its control without contact, again as part of a larger session.

Results

Table 4 show that contact motion strongly dominates the impression: all conflict stimuli were seen as depicting physical causality, even if B self-initiated motion-at-a-distance before A made contact with it (row 2) and even if B was the very first shape to move in the sequence (row 3). Data for the control stimuli without contact (rows 4–6) show that the impression for these conflict stimuli is clearly not a function of the speed parameters used to create them, because these control stimuli elicit far more social impressions, in particular when B moves faster than A (row 4).

Adults, and possibly 7-year-olds, may give physical reports to conflict events slightly less often than younger children. This reduction was more pronounced when motion-at-a-distance preceded contact (row 1 vs. row 2), as expected under a rational evaluation that therefore B self-initiated motion. However, in the event of row 3, B was the very first shape to move, which should have made the self-initiated nature of B's motion even more salient, but for adults at least this did not produce less physical attributions (69%) than the launch + reaction sequence (73%). The data pattern here is not entirely clear.

Statistical analysis of the events given to the first group of children (1, 2, 4, and 5), found no difference between responses given to conflict events, whether contact preceded or followed motion at a distance (row 1 and 2), $F_{(1,\ 108)} = 2.23$, $MSE = 0.36$, $p = 0.14$, $\eta^2_{\text{partial}} = 0.02$, in line with the main point that responses to both stimuli were uniformly physical. The age pattern was not clear in the statistical analysis either: the slightly reduced level of physical attribution at the older ages produced a small age main effect, $F_{(1,\ 108)} = 4.98$, $MSE = 0.44$, $p < 0.01$, $\eta^2_{\text{partial}} = 0.12$, but this did not differ between the two events, $F_{(3,\ 108)} = 1.95$, $MSE = 0.36$, $p = 0.13$, $\eta^2_{\text{partial}} = 0.05$, in contrast to the impression from **Table 4**. The patterns was not due to the speed differences: for the control events without contact (rows 4 and 5), there was no age difference; the only effect was that social attributions appeared more frequently the event of row 4, $F_{(1,\ 108)} = 17.00$, $MSE = 0.46$, $p < 0.01$, $\eta^2_{\text{partial}} = 0.14$. When conflict and control events were compared, these differential patterns produced corresponding effects of contact, speed, an interaction, as well as an overall effect of age, the smallest of these effects, with $F_{(3,\ 108)} = 2.98$, $MSE = 0.38$, $p = 0.04$, $\eta^2_{\text{partial}} = 0.08$.

When the reaction + launch stimulus shown to the second group of children (row 3) was compared to its no contact control (row 6), the only effect was an effect of contact, $F_{(1,\ 60)} = 23.41$, $MSE = 0.71$, $p < 0.01$, $\eta^2_{\text{partial}} = 0.28$, again reflecting more physical attributions to the conflict event. When the two events showing motion at a distance preceding contact motion, but differing in whether A or B moved first (rows 2 and 3) were compared between the two groups of children, no difference appeared, $F < 1$.

Discussion

There are two main points to these results: first, children gave the same response to stimuli containing both contact motion and motion-at-a-distance, regardless of whether the event began as a launch in which B's motion appeared ultimately initiated by contact (**Figure 6B**), or as a reaction in which B ultimately self-initiated motion (**Figures 6A,C**). This result converges with the finding from Experiment 3 that children do not seem concerned with the onset of motion.

Second, the response children gave to these stimuli was strongly physical, even though the stimuli were ambiguous, involving a cue conflict. When the motion sequence includes an element of contact motion, this apparently dominates the impression. This was not a primacy or recency effect, because it appeared regardless of whether the launch element came first or last. This was also not a function of the differing speeds used to create the stimuli, because control stimuli with the same speed characteristics, but without contact at all, elicited more social than physical impressions, as expected. Adults may have somewhat weaker responses to the conflict stimuli, but the age effect was not entirely clear in the data pattern. Most importantly, however, adults, like children, typically had a physical impression.

GENERAL DISCUSSION

Four experiments considered how children distinguish domains of perceptual causality in schematic motion events. The first two experiments showed that, contrary to prevailing opinion, even for pre-schoolers identification of physical and social causality does not only depend on spatial information. In Experiment 1, when temporal processing requirements were low, children's causal impressions were affected by whether or not the agents move like animals. In Experiment 2, a brief period of simultaneous motion was shown to be crucial for impressions of social causality in motion-at-a-distance events. Experiment 3 and 4 went on to show that simultaneous motion-at-a-distance is not important for social causality because it signals self-initiated motion: strong impressions of social causality arose even in occluder events that did not show self-initiated motion, but social impressions were eliminated in self-initiated motion events, if contact motion followed. Our results thus suggest that simultaneous motion-at-a-distance is an important cue for social causality independent of concern with motion onset, but also that contact motion is a stronger cue for physical causality.

Below we discuss the implications of these findings. First we evaluate the theory that children infer domain-specific motion onset from the perceptual configuration, then move on to the alternative view that domain-specific impressions are automatic reactions to specific perceptual configurations, including the issue of why contact causality appears stronger than social causality. Finally we consider the processing implications of temporal information in perceptual causality, including whether contact causality is processed faster than social causality, whether temporal delays between cause and effect affect perception in different ways than learning/inference, and whether different perceptual informers differ in processing demands.

ARE CHILDREN MAINLY CONCERNED WITH WHETHER MOTION IS SELF-INITIATED OR NOT?

A classic view since Piaget (1969) holds that children attribute intentionality to objects that self-initiate movement without contact, which makes these object potential social agents. Premack (1990) updated this in an influential paper arguing strongly for parallel perceptions of causality and intentionality, such that from infancy self-movers are automatically seen as possessing agency, while (physical) causality is perceived when objects are propelled after contact. Gelman (1990; Gelman and Spelke, 1981; Gelman et al., 1995) further argued that humans of all ages are concerned with the causes of motion, with animates having internal sources of motion while inanimate motion is externally caused. In the modal view, therefore, contact motion signals from infancy that an event belongs to the physical domain, while motion without contact belongs to the social domain (see Mandler, 1992, 2004; Baron-Cohen, 1994; Leslie, 1994, 1995; Baron-Cohen and Ring, 1995; Carey, 2009). Typically this is discussed as rudimentary thinking about the causes of motion, with early inferences supported by innate biases to attend to perceptual correlates of each domain.

Importantly, under these views, contact or lack of contact is not important *per se*, but because it indicates externally vs. internally caused motion and thus potential for social agency. The debate as to where perception ends and thinking begins in infancy is perhaps unresolvable and certainly beyond this paper, but the prediction from this view would have to be that early perception/thinking should be consistent with rational expectations about motion onset, and that motion onset and other agency cues should be more important than spatial or temporal parameters *per se* in causal attributions.

In our study of domain-specific perceptual causality, however, neither children nor adults were concerned with whether the motion was self-initiated. Primary concern with motion onset would have predicted in Experiment 2 that contiguous motion-at-a-distance in gap events should appear socially causal, not just simultaneous motion-at- a-distance, and similarly we should have seen social attributions for conflict sequences in which motion-at-a-distance was self-initiated and preceded contact in Experiment 4. In Experiment 3, in contrast, we should have seen fewer social attributions when onset was occluded and contact out of sight possible/likely.

Our results were quite different. Despite self-initiated motion, gap events elicited at best ambiguous choices in Experiment 2, and reaction + launch events elicited physical choices in Experiment 4. Thus, self-initiated motion is not sufficient for perception of social causality. Self-initiated motion is not necessary for this either, because in Experiment 3, social attributions showed no sign of a reduction when motion onset was occluded. This pattern does not fit the view that domain-specific impressions depend on motion onset.

We should add that motion onset was not completely ignored: although social choices were less frequent in gap than reaction events, they were more frequent than in delay events, also slightly more frequent in simultaneous non-contact than contact motion in Experiment 2. When contact was strongly implied in the occluder Experiment 3, this led at least to a small reduction in social responses, and similar in Experiment 4 when self-initiated motion preceded contact, this possibly led to a small reduction in otherwise strongly physical responses, at least in older observers. But while concern with motion onset may have played a small role, it was clearly not observers main concern, at any age.

Our youngest subjects were 3-years-old, while the motion-onset theory was formulated with infants in mind (Premack, 1990; Mandler, 1992, 2004; Baron-Cohen, 1994; Leslie, 1994, 1995; Baron-Cohen and Ring, 1995; Carey, 2009), so it is possible that concern with self-initiated motion is predominant early on, but gets overridden with age. However, the theory is meant to align infant skills with later more rational expectations, and if older children and adults do not have these rational expectations for the present events, it undermines the theory, especially since we saw no developmental trends.

There is surprisingly little direct support for spatial information as perceptual basis for a domain distinction in infancy either. Demonstrations that infants perceive causality in launch events with and reaction events without contact are consistent with this view, but it is not clear at this point that preverbal infants even distinguish two domains of perceptual causality (Schlottmann et al., 2009, 2012). Infants' expectations of contact differ for animate agents and inert objects (Spelke et al., 1995), but this is the converse of the claim that contact/non-contact signals whether the action involves agents or inert objects. Only when self-motion is repeated and amplified, by second-long pauses and reversals of direction it appears to serve as an agency cue (Luo and Baillargeon, 2005; Luo, 2011).

Counter to the claim, on the other hand, are multiple findings that simple, non-repeated self-initiated motion is not sufficient or necessary for attributions of (social) causality or goal-directed agency in infants. (Gergely et al., 1995; Csibra et al., 1999; Schlottmann and Surian, 1999; Movellan and Watson, 2002; Shimizu and Johnson, 2004; Johnson et al., 2007; Schlottmann et al., 2009, 2012). This set of findings has not received much attention, because the data are usually from control conditions not of primary interest, but the studies converge on the idea that the simple contrast of contact/inert object and self-motion/animate agent typically assumed in standard theories of infants' initial ontology is too simple. Data from all ages thus seem to argue against Premack's (1990) claim that self-initiated motion is automatically perceived as intentional motion.

This is not to deny that infants differentiate self-initiated from externally caused motion, or that older observers can see such motion as intentional. Luo et al. (2009) showed that 5-months-olds had different expectations about the kind of activities a box they had seen to engage in self-initiated motion or not can undergo. They were not surprised if the self-moving box reversed direction, stayed still when hit, remained in mid-air when released, seemed to move behind an occluder. They were surprised when the same box, presented as inert, only propelled by a hand, engaged in the same actions. Infants were also surprised when either object appeared to vanish from behind an occluder or passed through an obstacle, so this not an expectation that self-propelled objects can do anything. Luo et al. (2009) argue that infants have a concept of self-propelled object as possessing internal energy. This enables a wider range of actions than seen in inert objects, but is not the same as that of an agent with potential to engage in social interaction. In Leslie's (1994, 1995) terms, one is a mechanical agent, the other an intentional agent. Self-initiated motion should be of primary concern as a cue to social causality only if it is directly linked to intentional agency. These views thus converge with the present position.

With older children as well, the results do not fit Premack's (1990) claim. It is easy for pre-schoolers to infer that animals move themselves (Gelman et al., 1994; Massey and Gelman, 1988), but there are age differences in the inverse ability to use self-movement as a cue to animacy/intentionality: Richards and Siegler (1986) showed that from age 7 children saw spontaneous motion as the most important cue to whether a novel object was alive, but younger children considered limbed motion instead. In Montgomery (1996), older 3-year-olds saw self-initiated motion of a human as more intentional than pushed motion, but younger 3 year-olds, even with extra help, did not do this to the same extent. Thus, there is little evidence that self-initiated motion is automatically interpreted as the motion of an animate agent, but humans clearly develop an inclination to make such inferences—when asked about animacy and when shapes differ only in that one self-initiates motion while the other is pushed, adults consider the former more animate (Gelman et al., 1995). However, animacy/intentionality does not come up in spontaneous descriptions of such stimuli, nor is self-motion always used to infer animacy when stronger cues are available (Schlottmann et al., 2006).

In line with this, we argue here that seeing self-initiated motion as intentional and thus potentially belonging to the social world is a possible interpretation, not a necessary perception, and also that observers do not consider this interpretation in domain-specific causal impressions of Michotte-type motion events.

THE SOCIAL CAUSALITY CONFIGURATION: IS MINIMALLY CONTINGENT MOTION-AT-A-DISTANCE OPTIMAL?

If an inference of self-initiated or mechanically initiated motion onset is not crucial for perception of social or physical causality in children, then one alternative is that children's causal impressions are automatic reactions to particular perceptual configurations. In case of social causality, the effective spatio-temporal cue configuration may be the shapes' simultaneous, overlapping motion-at-a-distance. This cue is not just temporal, because simultaneous motion with contact appears entirely physical, as in the entraining events of Experiment 2, rather it is spatio-temporal, involving separated, but correlated motion paths, discussed also by Mandler (2004).

Does the reaction event provide the optimal instantiation of such correlated motion path configurations? Kanizsa and Vicario's (1968) reaction event was designed for a minimal contrast to launch events, allowing for demonstrations that minimal differences between events can lead to a switch in the perceived causal domain, so we know now that impressions of social causality do not appear only in events exceeding a certain level of complexity. However, in the conflict Experiment 4, the impression was not determined by the minimally correlated motion paths configuration but by the contact configuration. Moreover, in adults at least, social reaction impressions are weaker than those of physical launching (Schlottmann et al., 2006). This could be because the minimally contingent motion-at-a-distance used here is a passable, but suboptimal configuration.

One could speculate that stronger impressions of a social relation might be achieved with more extensive contingencies-at-a-distance, as in Heider and Simmel (1944), or Gao et al. (2009). Judging from adults' reaction to the edge-to-edge stimulus of Experiment 3, such extended contingencies should not be rigid and monotonous, but involve variations in paths, so as to not be mistaken for mechanical linkages, perhaps pulling with a rope or towbar between shapes (White and Milne, 1997).

However, the findings from children's perception of the edge-to-edge stimuli in Experiment 3 argue against this view: in contrast to adults, children did not have reduced impressions of social causality for these. This was the only sizable developmental difference in the present studies, which rather intriguingly suggests that impressions of a mechanical connection-at-a-distance might develop later, with experience. On the other hand, children did not have stronger social impressions either with the edge-to-edge stimuli than with more minimal path correlations. The present studies thus provide no clear evidence that extensive correlated motion provides a better cue for social causality than minimal motion-at-a-distance—and alternative views of asymmetries in performance as in Experiment 4 are discussed below.

THE PHYSICAL CAUSALITY CONFIGURATION: FASTER TO PROCESS OR STRONGER THAN SOCIAL CAUSALITY?

Our studies also provided data on physical causality, confirming that the configuration for this includes both contiguous and simultaneous contact motion, launching and entraining (Michotte, 1963), so physical and social perceptual causality are not entirely parallel in this respect as well.

The main new finding here came from Experiment 4: physical contact causality dominated social causality-at-a-distance: observers reported physical causality regardless of whether the social configuration was also present, regardless of motion order, and regardless of concern with self-initiated motion. Contact causality thus seems to automatically draw attention and dictate the interpretation, while other aspects of the event are ignored. Strikingly, contact causality interfered with processing of preceding contingent motion-at-a-distance that would otherwise lead to a social interpretation. One might speculate that this ability of contact causality to override the usual reaction to a precedent contingent motion-at-a-distance depends on how temporally close the two perceptual configurations are. In the present study, contact occurred after 0.6–1.3 s of simultaneous motion. If this is insufficient time to complete processing, interference from the contact configuration might preclude that a social impression is ever achieved. Presenting more extensively correlated motion over a longer period may then well help boost the social interpretation of conflict stimuli, not necessarily because more extensively correlated motion is a better cue to social causality, but because it would provide extra time to complete processing. This account is not to deny the intrinsic advantage of the contact configuration: while launch causality interfered with preceding reaction causality, the reverse clearly did not appear, so either processing of launch causality is completed far more quickly, becoming resistant to interference earlier, or it is intrinsically stronger, as would appear from the adult data (Schlottmann et al., 2006).

A different reason previously considered for why social impressions tend to be weaker than physical impressions in adults was that even with similar strength perceptions, the schematic motion events are further from social reality involving real animate agents than from physical reality involving inert objects (Schlottmann et al., 2006). That imbalance in impression strength previously appeared only in adults, but not in children's choices, or infants' looking time data fit with the view that children are closer to merely perceiving the events, while adults interpret them. Our finding that all ages tend to see conflict events as physical does not fit this view, but the processing time account outlined above might help resolve the discrepancy. Further work on why asymmetries between physical and social causality occur is clearly necessary.

TEMPORAL DELAYS IN CAUSAL PERCEPTION AND CAUSAL INFERENCE

Temporal delay has received extensive attention, not just as a cue to (non)causality but also as a processing factor. In perceptual causality it is typically seen as a cue. Delays from 100 ms or so reduce the causal impression and by 200 ms or so it is replaced by the perception of two independent movements (e.g., Michotte, 1963; Kanizsa and Vicario, 1968; Schlottmann and Anderson, 1993). However, it is also clear that naïve observers seeing launch/reaction events for the first time tolerate far greater delays of a second or more (e.g., Michotte, 1963; Schlottmann et al., 2006). Formal studies of such adaptation effects (Gruber et al., 1957; Powesland, 1959; Woods et al., 2012) found that with more exposure to causal events, observers become sensitive to smaller delays. Short-term experience thus demonstrably affects causal perception. Such stimulus adaptation effects are common throughout perception, and can originate at the neural level (Helson, 1964; Clifford et al., 2007), posing little difficulty for a view that causality is perceived in a bottom–up way.

Children are generally more tolerant of delays than adults, frequently treating delayed launch and reaction events as causal when they rarely treat events without delay as non-causal. As argued earlier, this could reflect that delay events, in contrast to causal events, have no intrinsic meaning, so children need to make considered judgments, which improve slowly with age (Schlottmann et al., 2002). In infant looking time studies, no explicit judgments are required, and 6-month-olds have no difficulty separating causal from non-causal events based on the delay (Leslie and Keeble, 1987; Oakes, 1994; Schlottmann et al., 2009, 2012), but the shortest delay used with infants are about 600 ms. We can thus not entirely rule out that children have higher delay thresholds in causal perception, or that delays are less effective at degrading perceptual causality, perhaps as more long-term adaptation.

In the present study, in any event, children's non-causal responses improved relative to prior work, for events with extra-long 2+ s delays (Experiment 2). Performance was not quite at adult level, but the small age differences were not significant anymore. An alternative to this reflecting improved discriminability is that only at such longer delays it becomes noticeably more difficult to learn a causal relation, with the default response shifting from causal to non-causal.

To evaluate this possibility, consider that temporal contiguity effects appear not just in perceptual causality, but causal inference more widely, under conditions not conducive to causal perception, and that different processes could underlie contiguity effects in these other paradigms. For example, at a rational level, contiguity/delay effects can reflect concern with the time course of assumed causal mechanisms, for adults (Buehner and May, 2002, 2003, 2004) and children (Schlottmann, 1999). If a known mechanism requires a delayed effect, e.g., an energy-saving light bulb requires time to warm up (Buehner and May, 2004), or a ball has to reach a bell via a slow runway (Schlottmann, 1999), subjects choose delayed over contiguous causes. Thus, top–down effects mediated by pre-existing causal knowledge can reverse the usual cue relation between contiguity and causality.

Contiguity effects also appear in causal judgments of the link between subjects' own actions and their outcomes, when subjects may not be reasoning about mechanism, paralleling contiguity effects in instrumental learning in humans and animals (Shanks et al., 1989). In instrumental causal inferences, contiguous contingent sequences are judged more causal than non-contiguous contingent sequences, e.g., in Shanks et al. (1989, Experiment 3), 2 s delays reduced causal ratings slightly, 4 s delays reduced them more and as much as 8 s delays, but even then judgments remained well above those in a non-contingent control condition, so adults can learn causal links over extended delays. Occasionally, contiguity effects even appear in the absence of contingency (Anderson and Sheu, 1995, Experiment 4, delays between 250 and 8000 ms). We are not aware of any parametric studies of instrumental causal learning and contiguity effects in children, but for adults it would seem that contiguity effects in instrumental causality operate over a more extended time frame than in perceptual causality. This could simply reflect differential sensitivity of the tasks in disparate areas, but a domain difference appears even with identical tasks (Huber et al., 2004).

The implication is that while brief delays may disrupt perceptual causality and access to an automatic causal meaning for launch/reaction configurations, they may not yet disrupt a tendency to infer a causal link between events. This may only be reduced with much longer delays, and only then may young children begin to give reliably non-causal responses. In the gray zone of briefly delayed events, performance then depends on age, perhaps inhibition skills, knowledge, external scaffolding and other factors that might shift the response away from the causal default. This speculative account, separating the role of temporal information in causal perception from its role in causal inference, awaits further test, of course, and yet another possibility is discussed below.

DO DELAY AND MOTION-STYLE INFORMATION DRAW ON COMMON PROCESSING RESOURCES?

The above discussion implied that in causal inference as envisaged by instrumental theories, temporal information is not so much seen as providing cues toward causality, i.e., as information that points to/away from a causal relation, but as a processing factor. Rather than temporal information being represented explicitly, it constrains the computation, affecting the speed of learning/processing or the rates/probabilities of outcomes used to derive causal strength (see Buehner, 2005, for review). Another aspect of timing as a processing factor may be how it affects resource distribution. Causal inference (as opposed to causal perception) is typically seen as a domain-general process drawing on central resources, so if the inference is easier, more resources are left for other aspects of the task. If delay processing requires more resources than contiguity processing for children, this might then affect ability to consider further cues to causality. Such a resource account was considered here for Experiment 1.

In previous work, adults had shown strong effects of motion-style, with animate motion reducing physical impressions of launching, but enhancing social impressions of reactions, while children and pre-verbal infants seeing identical stimuli showed no effects (Schlottmann et al., 2002, 2009, 2012), despite all ages recognizing the motions as animate and inanimate. In Experiment 1 here, in contrast, children from age 3 showed clear reduction/enhancement effects as seen for adults previously, which we attribute to more available processing resources when the need to attend to non-causality was abolished. This result was crucial in showing that previously reported age differences in perceptual causality do not reflect a difference in perception, but merely task difficulty.

The need to share central resources may also help explain the improvement on delay events in Experiment 2 relative to prior work. Not only were delays longer than in previous work, as discussed above, but also the task did not involve non-rigid motion stimuli. Without need to attend to animacy information, children may have had more resources to cope with delays. Thus, Experiments 1 and 2 had complementary results.

To slightly modify the arguments from Experiment 1 in view of subsequent findings, the data all fit with the view of a processing hierarchy, such that non-delayed contact motion and simultaneous motion-at-a-distance are processed automatically and from early on, while events with alternative spatio-temporal configurations, e.g., involving temporal delays, or motion-at-distance without simultaneous motion, or events containing additional cues, e.g., about animate motion-style, require extra processing resources. Whether a task shows developmental differences then depends on the extent to which it draws on these more processing intensive elements. Michotte (1963) argued, for instance, that the shapes' speeds are also crucial for perceptual causality, but this has not been studied with children. It is controversial whether speed effects are perceptual or reflect rational physical inference (Sanborn et al., 2013), so developmental data showing whether speed is processed automatically and early, or whether, akin to animacy and delay, this requires resources and shows age effects, will be of much interest.

CONCLUSIONS

These experiments clarify development of perceptual causality. Prior work showed that temporal information is important for distinguishing perceptually causal from non-causal events. Here we showed that temporal information also contributes to distinguishing domains of perceptual causality. First, contrary to prevailing belief, absence of contact is not the crucial cue for social reaction causality. The important cue is spatio-temporal in nature, correlated motion-at-a-distance. Equally contrary to prevailing belief, use of such perceptual information does not reflect concern with motion onset. Second, the temporal structure of the event is also important because it may affect ease of processing, as suggested by a trade-offs between attention to delay and motion style information, or by the dominance of physical causality in conflict sequences. We know little of the processes underlying perceptual causality, but consideration of the dual role of perceptual informers, as cues to causality and processing factors, might help move the debate beyond the long-ranging controversy on whether perceptual causality is modular or not (Scholl and Tremoulet, 2000; Schlottmann, 2000).

It is difficult to pin down in our study where causal perception ends and causal thinking begins. In our view, relatively pure causal perception might exist in young infants, but by the age children talk they have had much relevant causal experience affecting perception. Nevertheless, the absence of major age differences between 3-year-olds and adults, here and in previous work, shows that this experience does not slowly and gradually create a meaning for what before learning were meaningless artificial motions. Rather, in line with Michotte's views, launch and reaction events, even to young children and infants, have intrinsic causal meaning accessible from minimal information that experience merely modulates, not generates.

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Context modulates the contribution of time and space in causal inference

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Humans use kinematic temporal and spatial information from the environment to infer the causal dynamics (e.g., force) of an event. We hypothesize that the basis for these inferences are malleable and modulated by contextual temporal and spatial information. Specifically, the present research investigates whether the extent of a person's ongoing experience with direct causal events (e.g., temporally contiguous and spatially continuous) alters their use of time and space in judgments of causality. Participants made inferences of causality on animated launching events depicting a blue ball colliding with and then "launching" a red ball. We parametrically manipulated temporal contiguity and spatial continuity by varying the duration of contact between the balls and the angle of the second ball's movement. We manipulated participants' level of exposure to direct causal events (i.e., events with no delay or angle change) between experiments (Experiment 1: 2%, Experiment 2: 25%, Experiment 3: 75%). We found that participants adjust the temporal and spatial parameters they use to judge causality to accommodate the context in which they apprehended launching events. Participants became more conservative in their use of temporal and spatial parameters to judge causality as their exposure to direct causal events increased. People use time and space flexibly to infer causality based on their ongoing experiences. Such flexibility in making causal inferences may have adaptive significance.

Keywords: causality, causal inference and perception, contextual information, decision-making, time, space, temporal contiguity, spatial continuity

INTRODUCTION

The ability to infer causal structure in events is a central feature of human cognition (e.g., Hume, 1740/1960, 1748/1977; Michotte, 1946/1963). Many researchers argue that the ability to infer causal relationships in physical and social events is an innate facet of human cognitive systems (e.g., Michotte, 1946/1963; Leslie, 1982, 1984; Leslie and Keeble, 1987; Oakes and Cohen, 1990; Scholl and Tremoulet, 2000; Blakemore et al., 2001; Wolpert, 2003, 2006, 2009). This ability allows us to understand relationships in our environment, predict future outcomes, and plan goal-directed actions. Wolpert (2003, 2009) argues that causal inferences set humans apart from animals and was critical in the evolution of *Homo sapiens*.

We use kinematic information, like time and space, to infer the dynamic properties of an event. In other words, we use visible parameters to make inferences about invisible forces (i.e., dynamics). Wolff (2007, 2008) suggests that we make causality judgments based on such inferences of invisible forces. The notion of force is of course derived from the apprehension of acceleration ($f = ma$; $f =$ force, $m =$ mass) that itself is dependent on how an object changes in time and space ($a = \Delta v/\Delta t$; $v = \Delta d/\Delta t$; $a =$ acceleration, $v =$ velocity, $d =$ displacement in space, $t =$ interval of time). The kinematic properties of objects in time and space fundamentally contribute to our judgments of causality in mechanical events (Schlottmann and Shanks, 1992; Scholl and Tremoulet, 2000; Blakemore et al., 2003; Guski and Troje, 2003; Roser et al., 2005; Schlottmann et al., 2006; Wolff, 2007, 2008; Buehner and Humphreys, 2010).

The present research investigates the mapping of time and space on to causal judgments. Specifically, we examine the role of the context in which participants apprehend kinematic temporal and spatial information. Contextual information strongly modulates human decision-making (e.g., Rohrbaugh and Shanteau, 1999; De Martino et al., 2006; Dror et al., 2006) by allowing us to integrate relevant proximate information. It plays an important role in how we interpret events and plan appropriate responses. For example, imagine a person standing in a room with a dangerous animal. What would you infer to be the person's next action? What if the person standing in the room is a zookeeper or the animal is inside a cage? Contextual information alters our interpretation of the relationship between objects in an event, as well as our predictions of the actions and subsequent reactions of the objects. The same appears to be true for inferences of causality in events (e.g., Gruber et al., 1957; Powesland, 1959; Shanks, 1985; Schlottmann, 1999; Buehner and May, 2002, 2003).

Previous research suggests that contextual information provided by foreknowledge about the temporal characteristics of an event can influence how we interpret the relationship between time and causality (e.g., Schlottmann, 1999; Buehner and May, 2002). Schlottmann (1999) and Buehner and May (2002, 2003) demonstrated that the role of temporal information in causal inferences is mediated by people's assumptions about the timeframe of events.

For example, when people expect a delay in events, they expand the temporal delays they are willing to incorporate in their causal inferences. That is to say, people are willing to bridge the temporal gap in the event and infer a causal relationship.

Gruber et al. (1957) and Powesland (1959) also found that prior experience with clearly causal or non-causal events alters people's representation of the relationship between time and causality. Gruber et al. (1957) found that providing participants with prior experience on practice trials demonstrating large violations of temporal contiguity (i.e., time delays) in a bridge collapse event relaxed subsequent temporal criteria for causal judgments. Powesland (1959) found that previous experience with practice trials demonstrating causal events without violations of kinematic temporal information (i.e., no time delay) made subsequent temporal criteria for causal judgments more conservative. Furthermore, Powesland demonstrated that inserting a series of example trials between blocks of events also influenced temporal criteria used to make causal judgments. Collectively, these data suggest that contextual information influences our interpretation of the relationship between time and causality.

While the use of temporal information to infer causality appears susceptible to context, the susceptibility of spatial information to context remains unknown. Furthermore, although prior experience and foreknowledge influence inferences of causality, it remains unclear whether ongoing exposure to contextual information modulates the contributions of time and space to causal inferences. In the present study, we focus on the role of contextual information in causal inferences using depictions of simple mechanical events (e.g., two balls colliding). We hypothesize that contextual information modulates the use of kinematic temporal *and* spatial information when inferring causality. The present study focuses on two aspects of temporal contiguity and spatial continuity: temporal delay and linearity of movement. In the present study, we specifically investigate whether contextual information provided by recent and ongoing experience with direct causal events (a mechanical event depicting a linear collision without any delay) influences participants' judgments of how time and space contributes to causality. Consistent with Powesland's (1959) findings, we propose that the proportion of recent experience with direct causal events will lead participants to interpret the relationship between time, *space*, and causality more conservatively. That is to say, people with increased exposure to direct causal events will only accept smaller violations of time and space as causal. Such a finding would suggest malleability of the use of time and space in judgments of causality, specifically in response to ongoing changing dynamics in a sequence of events.

To test our hypotheses, we varied the proportion of direct causal events in three experiments (Experiment 1: 2%, Experiment 2: 25%, Experiment 3: 75%). Unlike previous research, we manipulated the probability of exposure to direct causal events during the actual experiment, rather than with previous experience (e.g., practice trials; Powesland, 1959) or foreknowledge of underlying mechanisms (e.g., Schlottmann, 1999; Buehner and May, 2002, 2003). Thus, changes in the contribution of kinematic temporal and spatial information to causal inferences would reflect response to recent and ongoing experience with clearly causal events. Furthermore, the present research extends our understanding of the role of spatial information in causality, whereas previous research only investigated the influence of temporal contextual information. Experiment 1 provided a baseline for comparison of the context manipulation and evaluated participants' general representation of the relationship between time, space, and causality. Experiment 2 investigated whether an increase in proportion of exposure to direct causal events modulates the contribution of time and space to causal inferences (i.e., causal context). Finally, Experiment 3 sought to extend findings from Experiment 2 by exposing participants to trials predominantly composed of direct causal events.

EXPERIMENT 1

We presented participants with *launching events* containing parametric manipulations of time and space relevant to causality. Launching events have a long history in the study of causality (e.g., Hume, 1740/1960, 1748/1977; Michotte, 1946/1963; Scholl and Tremoulet, 2000, etc.). These simple mechanical events portray one ball moving toward, contacting, and *launching* a second ball into motion. An increase in the time between initial contact of the first object and the initial movement of the second object or an increase in the deviation of the angle of egress for the second object decreases the likelihood of causal perception (e.g., see for examples Schlottmann and Anderson, 1993; Straube and Chatterjee, 2010).

Participants in Experiment 1 made inferences of causality on 98 launching events. Only two of the 98 launching events depicted direct causal launches (no time delay, no change in angle). The remaining 96 trials depicted a combination of parametric variations in temporal and spatial continuity.

MATERIALS AND METHODS

Participants

Sixteen right-handed, native English speaking college students at the University of Pennsylvania participated in Experiment 1. All participants had normal or corrected to normal vision and were naïve to the purposes of the experiment. All participants gave written informed consent prior to participation in the study. The University of Pennsylvania's Institutional Review Board approved the study.

Materials

Stimuli were 2 s animated video clips, generated in Strata 3D, depicting a blue ball colliding with a red ball (i.e., a launching event). Contact of the blue ball then "launched" the red ball (**Figure 1**). Temporal contiguity was parametrically varied between the contact of the blue ball and initial movement of the red ball (seven time delays: 0, 33, 67, 100, 133, 200, 267 ms). Spatial continuity was parametrically varied by changing the angle of egress of the red ball (seven angles: 0°, 7.5°, 15°, 22.5°, 30°, 45°, and 60°). The speed (9 cm/s), distance traveled (4.5 cm), and size (1.5 cm diameter) of each ball were constant. Videos were presented using Presentation experimentation software on a Windows XP computer.

Experimental design

Stimuli from Experiment 1 presented launching events where the blue ball approached the red ball along the horizontal axis (see

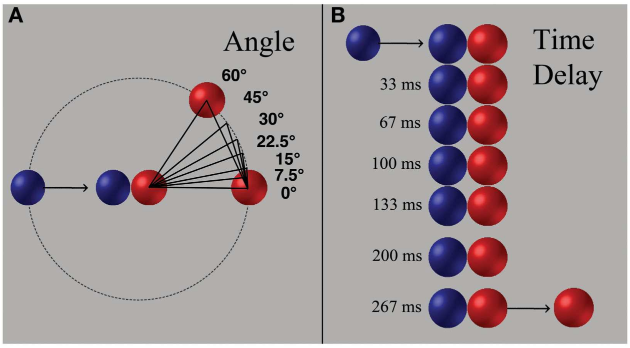

FIGURE 1 | (A) Parametric manipulations of spatial continuity and **(B)** temporal contiguity common to all experiments. Balls on the left were blue ($r = 14$, $g = 5$, $b = 223$), balls on the right were red ($r = 255$, $g = 0$, $b = 0$), and the background was gray ($r = 192$, $g = 192$, $b = 192$).

Figure 1A). Upon contact of the blue ball, the time delay of initial movement of the red ball varied (**Figure 1B**) followed by variation in its angle of egress (**Figure 1A**). All possible combinations of time delays and angle changes resulted in 49 different stimulus conditions. Each stimulus was presented twice (total trials = 98). Only two of the 98 events demonstrated direct causal events (i.e., no time delay or change in angle). Videos were presented in random order. Each video was followed by a fixation cross with a variable duration of 2–8 s (average 5 s). Testing time in Experiment 1 was ~12 min.

Procedures

Participants judged the causal relationship between the balls using a two-alternative forced choice design (Instructions: In every video, you will see a blue object and a red object move across the screen. You will be asked to judge whether the blue object caused the red object to move. We are interested only in your perception. There are no right or wrong answers. Please respond as quickly as possible to each video. Press "index finger" if you believe the blue object caused the red object to move. Press "middle finger" if you do not). Participants responded with the dominant hand and were asked to push a button with the index finger (Yes/causal) or middle finger (No/non-causal). Participants were first exposed to six representative practice trials before proceeding with the test trials. Practice trials were the same for all three experiments in the present manuscript and only one of the six trials demonstrated a direct causal launch. All participants were tested in a quiet testing room with the door closed to prevent distraction.

Analyses

In all experiments, generalized linear mixed models (GLMM) were applied to evaluate the contribution of changes in time delay and in angle to the odds of making a causal judgment, which explicitly model within-subject correlation by using subject-specific random effects. GLMM were calculated using the Proc NLMIXED procedure in SAS and variables were non-centered. Time delay and angle changes were coded in milliseconds and degrees, respectively. Trial number was included in GLMM to evaluate effects of experience over time. This factor is particularly important because each of the three present experiments contain different numbers of total trials. Parameter estimates from GLMM were evaluated for both sign and significance as an indication of sensitivity to variation in violations of time and space and significant use of either time or space for the judgment of causality, respectively. Significant Trial × Time or Trial × Space interactions would suggest that experience with launching events over the duration of the experiment influenced participants' inferences of causality.

RESULTS

Generalized linear mixed models analysis demonstrated that participants used kinematic temporal and spatial information to infer causality (see **Table 1** for parameter estimates). There was no main effect of Trial, nor any significant interactions (e.g., Trial × Time, Trial × Space, Time × Space; **Table 1**). Negative parameter estimates in main effects of Time and Space demonstrate that participants were more likely to judge an event as causal with smaller time delays and angle deviations (see top panel of **Figure 2**; see **Table 2** for mean values).

DISCUSSION

Results from Experiment 1 demonstrate that participants use kinematic temporal and spatial information to infer causal relationships in launching events. The absence of Trial × Time or Trial × Space interactions suggests that over the course of the experiment participants did not alter their use of time and space to infer causality. These results provide us with a baseline to examine context effects. If contextual information provided by recent and ongoing exposure to direct causal events plays a significant role in how kinematic temporal and spatial information contribute to causality, increasing participants' exposure to direct causal events should alter how participants' use time and space to infer causal relationships in launching events. In contrast, if contextual information provided by increased exposure to direct causal events does not influence our use of time and space to causality, there should be no effect of increased exposure to "causal context."

EXPERIMENT 2

In Experiment 2, participants were presented with more direct causal launches (25%) than in Experiment 1 (2%). If the contextual experience provided by proportionate exposure to direct causal events modulates participants' inferences of causality, the temporal, and spatial intervals associated with causal inferences should change with increased presentation of launches depicting direct causal events. Unlike Powesland (1959), increased exposure to causal context was not presented in a series of practice trials before the actual test trials or in a block of example trials between blocks of test trials. Instead, more direct causal events were randomly inserted into the test trials. If Powesland's (1959) contextual findings generalize to ongoing exposures, participants would use smaller kinematic temporal parameters to infer causal relationship with increased exposure to direct causal launches. Additionally, previous research has yet to demonstrate an influence of contextual information on people's use of kinematic spatial parameters to infer causal relationships. We predict that space will also be affected by the contextual information of increased exposure to direct causal events. However, if participants are insensitive to the

Table 1 | Parameter estimates for generalized linear mixed models.

	Experiment 1 (DF = 15)			Experiment 2 (DF = 15)			Experiment 1 vs. 2 (DF = 31)			Experiment 3 (DF = 15)			All experiments (DF = 47)		
	Est. ± SE	***t***	***p***	**Est. ± SE**	***t***	***p***	**Est. ± SE**	***t***	***p***	**Est. ± SE**	***t***	***p***	**Est. ± SE**	***t***	***p***
Intercept	2.0 ± 0.42	4.8	<0.001*	2.4 ± 0.34	7.3	<0.001*	0.66 ± 0.91	0.72	0.475	2.6 ± 0.21	12.6	<0.001*	2.1 ± 0.56	3.7	<0.001*
Context	–	–	–	–	–	–	0.98 ± 0.53	1.8	0.078	–	–	–	0.17 ± 0.22	0.78	0.441
Trial	0.002 ± 0.006	0.36	0.725	−0.002 ± 0.003	−0.72	0.480	0.01 ± 0.01	1.3	0.195	0.002 ± 6E−4	4.7	<0.001*	−0.005 ± 0.007	−0.79	0.433
Time	−0.009 ± 0.002	−3.8	0.001*	−0.01 ± 0.002	−6.4	<0.001*	−3E−4 ± 0.005	−0.06	0.955	−0.02 ± 0.002	−9.0	<0.001*	−0.006 ± 0.003	−1.6	0.100
Space	−0.02 ± 0.01	−2.3	0.036*	−0.05 ± 0.009	−5.5	<0.001*	0.02 ± 0.02	1.0	0.327	−0.09 ± 0.008	−10.1	<0.001*	−0.002 ± 0.01	−0.14	0.885
Context × trial	–	–	–	–	–	–	−0.01 ± 0.007	−1.4	0.156	–	–	–	0.002 ± 0.002	1.1	0.250
Context × time	–	–	–	–	–	–	−0.008 ± 0.003	−2.2	0.032*	–	–	–	−0.004 ± 0.002	−2.8	0.006*
Context × space	–	–	–	–	–	–	−0.03 ± 0.01	−2.7	0.011*	–	–	–	−0.02 ± 0.006	−4.1	<0.001*
Trial × time	−3E−5 ± 4E−5	−0.64	0.533	−5E−5 ± 3E−5	−1.5	0.142	−7E−5 ± 9E−5	−0.72	0.477	−5E−6 ± 1E−5	−5.2	<0.001*	−1E−6 ± 6E−5	−0.02	0.981
Trial × space	−9E−5 ± 1E−4	−0.45	0.656	−5E−5 ± 1E−4	−0.42	0.681	−4E−4 ± 4E−4	−0.94	0.352	−1E−4 ± 4E−5	−3.4	0.003*	1E−4 ± 2E−4	0.73	0.469
Time × space	−4E−5 ± 6E−5	−0.45	0.656	−9E−5 ± 1E−4	−0.92	0.371	−8E−5 ± 2E−4	−0.44	0.667	3E−4 ± 7E−5	4.3	<0.001*	−2E−4 ± 1E−4	−1.4	0.168
Context × trial × time	–	–	–	–	–	–	1E−5 ± 5E−5	0.23	0.821	–	–	–	−2E−5 ± 2E−5	−0.81	0.421
Context × trial × space	–	–	–	–	–	–	2E−4 ± 2E−4	0.78	0.441	–	–	–	−1E−4 ± 8E−5	−1.4	0.159
Context × time × space	–	–	–	–	–	–	4E−6 ± 1E−4	0.03	0.976	–	–	–	1E−4 ± 6E−5	2.4	0.017*
Trial × time × space	−3E−7 ± 1E−6	−0.21	0.839	2E−6 ± 1E−6	1.9	0.069	−2E−6 ± 3E−6	−0.51	0.612	1E−6 ± 4E−7	4.1	<0.001*	−2E−6 ± 2E−6	−0.68	0.500
Context × trial × time × space	–	–	–	–	–	–	2E−6 ± 2E−6	1.0	0.311	–	–	–	1E−6 ± 8E−7	1.4	0.1433

**Significance at $p < 0.05$; Est., parameter estimate; SE, standard error.*

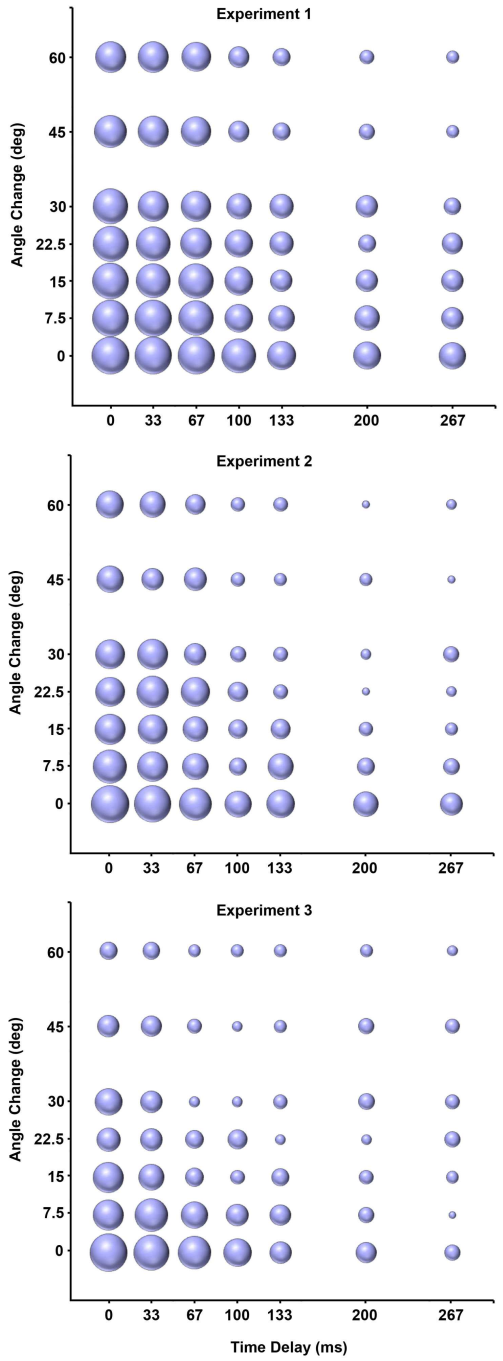

FIGURE 2 | Change in the percent of temporal and spatial parameter combinations judged causal across experiments (Top: Experiment 1, Middle: Experiment 2; Bottom: Experiment 3). Bubble area represents the percentage of a unique parameter combination (e.g., 33 ms delay × 15° angle change) judged causal. Increased bubble sizes for shorter delays and smaller angle changes, relative to longer delays, and angle changes, demonstrates that participants were more likely to judge shorter delays and angles as causal. Decreased size of bubbles across experiments demonstrates the effects of increased exposure to direct causal launches across experiments. Means for direct launches were calculated based on different numbers of trials in each experiment (Experiment 1 = 2; Experiment 2 = 32; Experiment 3 = 288 trials).

causal context of events, temporal, and spatial parameters associated with causal inferences would not change. Alternatively, if time, but not space, is susceptible to influence from contextual information, increased exposure to direct causal events will only influence participants' use of kinematic temporal information to infer causal relationships.

In addition to the manipulation of causal context, participants in Experiment 2 were presented with 25% more trials than participants in Experiment 1. Thus, if experience over time plays an important role in the contribution of time and space to causal inferences, participants' use of this information should change over the course of the experiment and be significantly different between experiments.

MATERIALS AND METHODS

Participants, materials, design, procedures, and analyses

A new group of sixteen participants meeting the same criteria as in Experiment 1 participated in Experiment 2. The design, procedures, and analyses were similar to Experiment 1 except for two modifications. Participants in Experiment 2 viewed the same 98 launches as in Experiment 1, with an additional 30 clearly causal launches. Thus, 25% of 128 trials contained unambiguously causal events. Context condition (i.e., 2 vs. 25% clearly causal events) was included in a separate GLMM, in addition to trial number, angle change, and time delay, to evaluate between group differences in the influence of increased causal context on participants' use of time and space in inferences of causality. Significant Context × Time and Context × Space interactions would suggest that exposure to different degrees of causal context influenced participants use of time and space when making causal judgments. Significant Trial × Time or Trial × Space interactions would suggest that participants were adjusting their use of time or space through the duration of the experiment when making causal inferences.

RESULTS

Generalized linear mixed models analysis demonstrated that participants in Experiment 2 used kinematic temporal and spatial information when inferring causality (see **Table 1** for parameter estimates, see middle panel of **Figure 2**). There was no main effect of trial, nor any significant interactions in the model (e.g., Trial × Time, Trial × Space, Time × Space; **Table 1**). A GLMM evaluating the influence of causal context conditions between Experiment 1 and Experiment 2 demonstrated significant Context × Time and Context × Space interactions (**Table 1**). All other main effects and interactions in the model were non-significant (**Table 1**). Negative parameter estimates from significant Context × Time and Context × Space interactions suggest that participants exposed to more direct causal events (25%) were more conservative in accepting time delays and angle deviations in judging causal launching events (see top vs. middle panel of **Figure 2**; see **Table 2** for mean values).

DISCUSSION

During increased exposure to direct causal events, participants used smaller kinematic temporal *and* spatial parameters to infer causal relationships (i.e., more conservative use of time and space),

Table 2 | Percent of parameter combinations judged causal by experiment.

Angle (°)	Delay (ms)	Experiment 1 Percent ± SE	Experiment 2 Percent ± SE	Experiment 3 Percent ± SE	Angle (°)	Delay (ms)	Experiment 1 Percent ± SE	Experiment 2 Percent ± SE	Experiment 3 Percent ± SE
0	0	94 ± 4	96 ± 1	96 ± 0.3	22.5	133	38 ± 9	13 ± 6	6 ± 4
0	33	91 ± 5	91 ± 5	84 ± 7	22.5	200	19 ± 7	3 ± 3	6 ± 4
0	67	91 ± 5	71 ± 8	72 ± 8	22.5	267	28 ± 8	6 ± 4	16 ± 7
0	100	78 ± 7	47 ± 9	52 ± 9	30	0	81 ± 7	56 ± 9	48 ± 9
0	133	53 ± 9	52 ± 9	31 ± 8	30	33	63 ± 9	63 ± 9	31 ± 8
0	200	50 ± 9	41 ± 9	28 ± 8	30	67	59 ± 9	31 ± 8	6 ± 4
0	267	47 ± 9	32 ± 9	16 ± 7	30	100	41 ± 9	16 ± 7	6 ± 4
7.5	0	88 ± 6	75 ± 8	63 ± 9	30	133	38 ± 9	13 ± 6	13 ± 6
7.5	33	91 ± 5	61 ± 9	75 ± 8	30	200	31 ± 8	6 ± 4	17 ± 7
7.5	67	81 ± 7	47 ± 9	47 ± 9	30	267	19 ± 7	16 ± 7	13 ± 6
7.5	100	53 ± 9	19 ± 7	32 ± 9	45	0	69 ± 8	47 ± 9	31 ± 8
7.5	133	44 ± 9	44 ± 9	28 ± 8	45	33	63 ± 9	31 ± 8	28 ± 8
7.5	200	41 ± 9	19 ± 7	16 ± 7	45	67	59 ± 9	34 ± 9	13 ± 6
7.5	267	31 ± 8	16 ± 7	3 ± 3	45	100	28 ± 8	13 ± 6	6 ± 4
15	0	88 ± 6	61 ± 9	63 ± 9	45	133	19 ± 7	10 ± 5	9 ± 5
15	33	78 ± 7	56 ± 9	44 ± 9	45	200	16 ± 7	9 ± 5	16 ± 7
15	67	81 ± 7	41 ± 9	22 ± 7	45	267	9 ± 5	3 ± 3	13 ± 6
15	100	53 ± 9	22 ± 7	13 ± 6	60	0	63 ± 9	50 ± 9	20 ± 7
15	133	31 ± 8	25 ± 8	19 ± 7	60	33	63 ± 9	44 ± 9	19 ± 7
15	200	31 ± 8	13 ± 6	13 ± 6	60	67	56 ± 9	25 ± 8	9 ± 5
15	267	31 ± 8	9 ± 5	9 ± 5	60	100	28 ± 8	13 ± 6	9 ± 5
22.5	0	84 ± 7	56 ± 9	38 ± 9	60	133	19 ± 7	13 ± 6	9 ± 5
22.5	33	78 ± 7	69 ± 8	32 ± 9	60	200	13 ± 6	3 ± 3	9 ± 5
22.5	67	63 ± 9	56 ± 9	22 ± 7	60	267	9 ± 5	6 ± 5	6 ± 4
22.5	100	50 ± 9	25 ± 8	25 ± 8	–	–	–	–	–

SE, standard error; means for direct launches were calculated based on different numbers of trials in each. Experiment (Experiment 1 = 2; Experiment 2 = 32; Experiment 3 = 288 trials).

compared to Experiment 1. These results demonstrate that the use of space, like the use of time, is susceptible to contextual influence in causal inferences. Participants update their use of time and space in judging causality based on recent and ongoing experience with events. The lack of Trial × Time or Trial × Space interactions suggests that this updating was evident across the duration of this experiment in a straightforward manner.

EXPERIMENT 3

Participants in Experiment 3 were presented with more direct causal events (75%) than in the previous two experiments. If participants flexibly use time and space to infer causality and the degree of causal context experienced plays a role in this process, further increasing participants exposure to causal structure should amplify effects of the previous experiment.

PARTICIPANTS, MATERIALS, DESIGN, PROCEDURES, AND ANALYSES

A new group of sixteen participants meeting the same criteria as the previous two experiments participated in Experiment 3. The design, procedures, and analyses were similar to Experiment 1 and 2 with two exceptions. Participants in Experiment 3 viewed a block of trials containing 75% clearly causal launches ($n = 288$) and 25% with varying temporal and spatial parametric combinations ($n = 96$; total trials = 384). Context condition (i.e., 2, 25, 75% direct causal events) was included in a separate GLMM, in addition to trial number, angle change, and time delay, to evaluate between group differences in the influence of increased causal context on participants' use of time and space to make inferences of causality. Significant Context × Time and Context × Space interactions would suggest that exposure to different degrees of causal context influenced participants use of time and space when making causal judgments. Significant Trial × Time or Trial × Space interactions would suggest that differences between total trial numbers influenced the contribution of time and space to causal inferences.

RESULTS

Generalized linear mixed models analysis demonstrated that participants in Experiment 3 used kinematic temporal and spatial information to infer causality. There was also a main effect of Trial and both the Trial × Time and Trial × Space interactions were significant. Furthermore, the Time × Space and Trial × Time × Space interactions were significant (see **Table 1** for parameter estimates). A main effect of Trial suggests that participants' judgments of causality changed over the duration of the experiment. The significant negative parameter estimates for the Trial × Time and Trial × Space interactions suggest that as participants were exposed to more trials, they became more likely to reject smaller intervals of time and space as contributing to causality than they were earlier in the experiment (i.e., more conservative). Two distinct trial-based effects are evident when direct and indirect launches are plotted separately (**Figure 3**). Participants' consistently judged direct launches as causal throughout the experiment, with a slight increase in the rate of this judgment over time (positive slope in top of **Figure 3**). In contrast, participants' judgments of indirect launches became more conservative over time (i.e., more likely to reject smaller intervals of time and space; negative slope in bottom of **Figure 3**). Thus, effects of trial in Experiment 3 appear to be driving changes in causal judgments on indirect launching events. However, a GLMM analysis containing only indirect launches failed to demonstrate a significant effect of Trial [Parameter Estimate (Est.) = -0.0003, SE = 0.001, $t = -0.18$, $p = 0.85$], Trial × Time (Est. = $-2.5\text{E}-5$, SE = $1.4\text{E}-5$, $t = -1.7$, $p = 0.08$), Trial × Space (Est. = $-3\text{E}-5$, SE = $5.8\text{E}-5$, $t = -0.56$, $p = 0.57$), or a Trial × Time × Space interaction (Est. = $5.7\text{E}-7$, SE = $4.7\text{E}-7$, $t = 1.2$, $p = 0.22$), making clear interpretation of these data less than straightforward. In terms of the Time × Space interaction, participants in Experiment 3 generally demonstrated a decreased likelihood of causal judgment with increasing time delays. However, for angle deviations between 15° and 45°, participants also demonstrated a slight increase in the likelihood of causal judgment on longer time delay parameters (see **Table 2**). Thus, certain combinations of temporal and spatial parameters altered participants' likelihood of making a causal judgment. This pattern likely resulted in the Time × Space interaction in Experiment 3. The positive parameter for the Trial × Time × Space interaction suggests that this pattern was more pervasive as trial number increased in Experiment 3.

A separate GLMM evaluating the influence of causal context conditions between Experiment 1, 2, and 3 demonstrated significant Context × Time and Context × Space interactions (**Table 1**). There was also a significant Context × Time × Space interaction. All other main effects and interactions in the model were non-significant (see **Table 1**). Negative parameter estimates from Context × Time and Context × Space interactions suggest that as participants were exposed to more direct causal events, they were more likely to use smaller intervals of time and space to make causal inferences on launching events (**Figures 2** and **4**; **Table 2**). Consistent with the graphic depiction of the data in **Figure 4**, participants exposed to more direct causal launches typically only accepted smaller time delays and spatial angles as causal. The Context × Time × Space interaction was likely driven by the Time × Space interaction in Experiment 3, but not in Experiments 1 or 2. However, this interaction could mean that interactions between time and space were stronger across experiments with more direct causal events (see **Table 2**; **Figure 2**). The absence of significant Time × Space interactions in or between Experiments 1 and 2 (**Table 1**) make the latter hypothesis less likely.

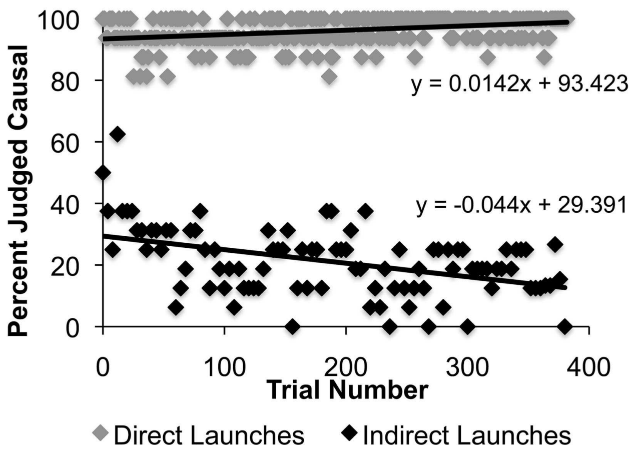

FIGURE 3 | Percent of trials judged causal as a function of trial number for direct and indirect trial types in Experiment 3. The positive trend line (slope = 0.014) for direct launches demonstrates that participants were slightly more likely to judge direct launching events as causal over the duration of Experiment 3. In contrast, the negative trend line (slope = −0.04) for indirect launches demonstrates that participants were less likely to judge indirect launching events as causal over the duration of Experiment 3.

DISCUSSION

A further increase in exposure to direct causal launches resulted in participants being more conservative in their use of kinematic temporal and spatial information to infer causality (**Figures 2** and **4**). The significant effects of Trial in Experiment 3, and not the earlier experiments, may have been because of the greater number of trials in this experiment. **Figure 3** demonstrates that trial effects were related in part to changes in judgments on indirect launches, with participants less likely to call events "causal" as the experiment proceeded. In contrast, direct launches were more consistently judged as causal than indirect launches. A greater number of trials than were present in Experiments 1 and 2 may have been needed to detect these subtle effects. The trial effect in Experiment 3 is also consistent with the idea that as participants were exposed to more direct launches they judged a lower percentage of other trial types as causal (**Figure 4**). The Context × Time and Context × Space interactions in the GLMM of the three causal context conditions demonstrates that the context of apprehending events plays a strong role in modulating how kinematic temporal and spatial information contribute to inferences of causality in launching events. These data suggest that participants flexibly update their representation of how kinematic temporal and spatial information relate to causal relationships in events.

GENERAL DISCUSSION

Contextual information plays an important role in how we interpret the relationship between time, space, and causality. The ability to infer causality from kinematic temporal and spatial information is central to our understanding of events in the environment, as well as our ability to predict future outcomes and plan goal-directed actions (Wolff, 2007, 2008). People adjust the temporal and spatial parameters they associate with causality to accommodate the context in which they apprehend launching events (see **Table 2**; **Figures 2** and **4**). Others have demonstrated that context influences the contribution of time to inferences of causality (e.g., Powesland, 1959; Buehner and May, 2002, 2003). Our data demonstrate that context effects also extend to the use of spatial information in causal inferences.

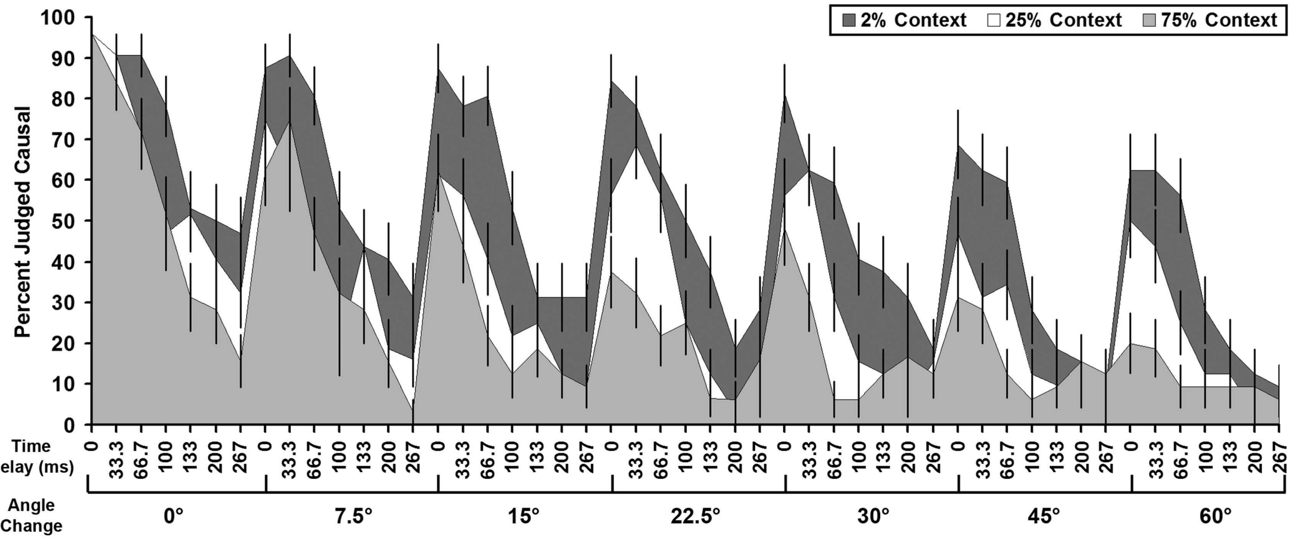

FIGURE 4 | Difference in the percent of trials judged as causal (*y*-axis) for all combinations of temporal and spatial parameters (*x*-axis) in the 2, 25, and 75% causal context conditions. Standard error bars are included for each combination of time and space. The smaller area coverage for the 75% context condition (dark gray) demonstrates more conservative use of spatial and temporal parameters vs. the 25% (white) and 2% (light gray) context conditions. Means for direct launches were calculated based on different numbers of trials in each experiment (Experiment 1 = 2; Experiment 2 = 32; Experiment 3 = 288 trials).

Prior work on the effects of foreknowledge (e.g., Schlottmann, 1999; Buehner and May, 2002, 2003) on causal inferences demonstrates that participants flexibly use kinematic information to infer causality. That is to say, people adjust their judgments of the relationship between time and causality based on contextual information, such as how events are framed before they are encountered. Our findings along with those of Gruber et al. (1957) and Powesland (1959) suggest that flexibility in causal inferences also occurs in an ongoing way during the unfolding of events. These effects may arise from top-down knowledge obtained through ongoing observation of changes in the events occurring in our environment. Alternatively, these effects may arise from perceptual anchoring, a form of perceptual adaptation (Helson, 1964). In this view, changes in the use of time to make causal judgments may reflect perceptual adaptive processes responding to prototypical causal or non-causal trials presented during practice. Whereas knowledge-based manipulations are not attributable to perceptual adaptation, experience-based effects could arise from such an effect. As discussed by Hecht (1996), distinguishing between perceptual adaptation and a top-down process for such experience-based effects is exceedingly difficult, if not impossible.

Regardless of the underlying mechanisms, our results suggest that studies using a narrow range of temporal and spatial violations (e.g., the present experiments; Michotte, 1946/1963) produce a rapid decline in the likelihood of making causal judgments for launches with increasing time delays, spatial gaps, or angle deviations. In contrast, studies using a broader range of temporal and spatial violations appear to produce a more gradual decline in the likelihood of endorsing causality in events as violations of time and space increase (e.g., Young et al., 2005). For example, Young et al. (2005) presented participants with launching events with a range of temporal delays between 0 and 2 s, vs. the 0–267 ms range in the present study. Across both studies, a similar pattern of causal judgment was found across the overall range of temporal parameters. That is to say, participants in Young et al. (2005) responded to the 2 s delay much the same as our participants responded to a 267 ms delay. In contrast, a 500 ms delay in Young et al's. (2005) study was more often judged as causal (∼65%) than our 267 ms delay. Collectively, these results suggest that participants respond to contextual information provided by the overall range of temporal and spatial violations they experience in launching events. The present research also suggests that people are sensitive to contextual information provided by their recent experience with direct causal events. Although the three experiments from the present work contained the same range of temporal and spatial violations, participants were less likely to judge events with longer delays and greater angle violations as causal when they experienced more direct launching events.

Contextually driven changes in people's use of time and space to infer causality suggest the categories "causal" and "non-causal" are applied flexibly to events. Schwarz (1999) demonstrated that when categories, like "frequent," "important," or "successful," lack clear boundaries, they are malleable and susceptible to contextual influence. Our category of causality might similarly lack clear boundaries and thus lend itself to flexibility in its application to events.

The flexibility of criteria used to make causal inferences may also have implications for its expression in disease. For example, people with paranoid schizophrenia or obsessive-compulsive disorder (OCD) often infer causal relationships where none exists (e.g., Tschacher and Kupper, 2006; Dettore, 2011). In contrast, children with autism can fail to comprehend causal relationships in events in their social environment (e.g., Ray and Schlottmann, 2007). Difficulty integrating contextual information into one's judgment of causality may play a role in disorders with impaired comprehension of causal relationships in physical and social events. Future studies investigating patients' ability to flexibly update their representation of the relationship between time, space, and causality in response to changing contextual information will test this hypothesis.

CONCLUSION

Our findings show that even in simple mechanical launching events, recent and ongoing contextual information modulates the way that kinematic temporal and spatial information contribute to causal inferences. Situations we encounter in our environment vary considerably. Accounting for contextual information in our representation of causality allows integrations of novel, varied, and relevant information with a person's own experiences and expectations when making causal inferences. The ability to integrate contextual information into our inferences of causality is likely of adaptive significance.

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The influence of perceived causation on judgments of time: an integrative review and implications for decision-making

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Recent research has shown that the perception of causality affects the judgment of elapsed time: an interval between an action and a subsequent event seems to be shorter when people believe that action has caused the event. This article reviews past work on the phenomenon and integrates the findings from the different settings in which it has been observed. The effect is found for actions people have personally taken, as well as for those they have simply read or heard about. It occurs for very short intervals (e.g., milliseconds) as well as longer periods (e.g., months or years). Beliefs and expectations about different types of causal forces and their trajectories over time can affect the degree of time compression in some settings. But the tendency toward compression of time is the default and dominant response: It persists when people think of generic causal relations and is enhanced when people opt for the quickest interpretation of causal relations. This robust influence of causality on time judgment appears to be linked to the basic tendency to rely on temporal proximity in processing causal relations and to people's early experience with the physical-mechanical world. Past work has focused primarily on the implications of time compression for the sense of agency, but this phenomenon has implications also for decisions that depend on time judgment. The compression of subjective time elapsed between actions and outcomes makes people more optimistically plan the timing of a focal action in the future, experience its effect earlier in the future, and be less likely to switch to an alternative course of action. The tendency toward compression can thus endow an action with a sort of privileged status or advantage.

Keywords: time perception, causality, intentional binding, placebos, intertemporal choice, planning fallacy, judgment and decision making, agency

Time plays an important role in causal inference. People generally expect a short time to have elapsed between causes and their effects and so rely on time as a cue to causation, judging an event that occurred closer in time to an effect as a more likely cause than one that occurred at a greater temporal distance (Hume, 1938; Michotte, 1963; Einhorn and Hogarth, 1986; Shanks et al., 1989; Lagnado and Sloman, 2006). Recent work has demonstrated, however, that this relationship between time and causality is bidirectional—the perception of causality can affect people's judgments of time in such a way that they perceive events that they know to be causally related to have occurred closer in time to each other.

For example, one set of studies focused on intentionality and showed that perceived time between a voluntary movement (e.g., pressing a key) and its effect (e.g., an auditory tone) was shorter compared to a baseline condition in which the action and its effect occurred within the same amount of time but without any causal link (the *intentional binding* effect; Haggard et al., 2002; see also Eagleman and Holcombe, 2002; Wohlschlager et al., 2003; Engbert and Wohlschläger, 2007; Moore et al., 2009; Ebert and Wegner, 2010). Other researchers focused on causality more generally and contrasted, for instance, estimates of time elapsed between pairs of historical events that were causally related (e.g., the launch of Sputnik by the USSR and the landing of man on the moon with Apollo 11) with estimates of time between historical events that were not causally related (e.g., the launch of Sputnik and the Woodstock music festival; Faro et al., 2005; see also Buehner and Humphreys, 2009; Faro, 2010; Faro et al., 2010; Buehner, 2012). Here, too, perceived causality shortened estimates of elapsed time.

In this article we review some of the findings from the different settings in which an influence of causality on time judgments has been observed.[1] We have two primary goals. The various lines of work examining this phenomenon have proceeded mostly independently, but there are links among the findings that may shed light on the general tendency and why it may occur. We draw attention to these. Our second goal is to divert some of the focus from agency to time perception. Most of the work in this area has focused on the implications of the effect for the sense of agency. In particular, because compression of time was not observed for involuntary movements, researchers suggested

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[1] Some studies showing an effect of causality on time judgments have used the term "intentional binding," referring to the shifts in the perceived times of occurrence of voluntary movements and their outcomes. Other work focused on judgments of the time interval and has used the term "compression."

the effect may be an implicit marker of agency (Haggard et al., 2002; see Moore and Obhi, 2012, for a recent review on the link between this phenomenon and agency). In this article we focus primarily on the implications that the effect has for decisions that depend on the judgment of time. The compression of subjective time elapsed between actions and outcomes makes people more optimistically plan the timing of a focal action in the future, experience its effect earlier in the future, and be less likely to switch to an alternative course of action. The tendency toward time compression can thus endow an action with a sort of privileged status or advantage (see also Engbert and Wohlschläger, 2007).

Our review proceeds as follows. First, we examine the robustness of the effect in different settings. We then review some process evidence, focusing particularly on several points of linkage between those findings that may help explain why the effect occurs and why it seems to be so general. Finally, we review the implications of the phenomenon.

THE GENERALITY OF THE EFFECT

Early studies have shown an effect of perceived causation on time judgment by varying the intentionality of motor movements (Haggard et al., 2002; see also Wohlschlager et al., 2003; Engbert and Wohlschläger, 2007; Moore et al., 2009; Ebert and Wegner, 2010). Most of these studies used the Libet clock method, in which participants watch a rotating clock and report the position of the clock hand to indicate the onset of certain events they experience (Libet et al., 1983). In the baseline conditions of a study by Haggard et al. (2002), some participants voluntarily pressed a key while others heard an auditory tone. They indicated the timing of these events on the rotating clock. In the experimental conditions, participants indicated the timing of pressing the key or hearing the tone, but here the tone followed their action. The authors found that in the experimental conditions the perceived time of action was later than the baseline condition and the perceived time of the outcome was earlier than in the baseline condition. Thus, intentionality compressed the time interval between actions and their outcomes. In another set of conditions that tested whether intentionality indeed drives the compression of time, involuntary movements were induced in participants via transcranial magnetic stimulation. Haggard et al. found that in these conditions the compression effect was reversed such that the interval between involuntary action and effect was longer relative to the baseline.

While this initial set of studies focused on intentionality of action, later studies manipulated causality per se. For instance, some studies manipulated perceptions of causation apparent in historical events participants read about (Faro et al., 2005). Other studies have shown the effect of causation on time judgment through perceptual methods using Michotte's launching paradigm (Cravo et al., 2009), by manipulating the experienced covariation or probability of yielding the outcome (Engbert and Wohlschläger, 2007; Moore et al., 2009), and by providing study participants an alternative cause that discounts the role of a focal cause post experience (Faro, 2010). These different demonstrations have shown that the phenomenon is robust to different conceptualizations of causality. They also illustrate that the effect can occur irrespective of when causality is manipulated. Time between actions and outcomes seems shorter when causality is "sensed" before the time interval and the outcome, when it is learned during the repeated experience of the action-outcome sequences, and when casual beliefs are acquired after the experience of the events and the interval.

Various operationalizations of the dependent measure (elapsed time) have been employed, including a derived interval from perceived times of occurrence using the Libet clock method (Haggard et al., 2002; Wohlschlager et al., 2003; Engbert and Wohlschläger, 2007; Moore et al., 2009), a direct estimate of elapsed time on a unit scale (Faro et al., 2005; Engbert et al., 2008; Humphreys and Buehner, 2009; Moore et al., 2009), and reproduction of the experienced interval (Faro, 2010; Humphreys and Buehner, 2010). Finally, the effect has been demonstrated using a variety of timeframes and action-outcome sequences including motor movements and outcomes separated by milliseconds (e.g., Haggard et al., 2002), actions and outcomes separated by longer intervals of up to 4 s (e.g., Humphreys and Buehner, 2009), consumption of a product and the experience of its effect minutes later (Faro, 2010), and pairs of historical events that are years apart (e.g., Faro et al., 2005). **Table 1** provides information about different manifestations of this effect.

The generality and robustness of the effect of causality on time judgments is noteworthy because time perception phenomena are known to be context dependent; effects that were found in one setting were often not observed in others (Block and Zakay, 1997; Tourangeau et al., 2000). Further, the effect occurs both in settings in which people are likely unaware of any notions of causality per se, as well as in settings in which causality and judgments of time are more explicit in the experimental setting. Irrespective of the setting, the tendency for compression is the common default, and it is the standard finding in studies examining causality and time. These consistent findings suggest that the effect is strong, robust, and may reflect a basic tendency in the way people treat causality and time. In the following sections we review some research that may suggest why this may be the case.

EXPLANATIONS AND INTERPRETATIONS

The initial findings of Haggard et al. (2002) on intentional binding were seen as evidence for a predictive motor-control process in the brain that adjusts the perceived timing of voluntary actions and their effects and provides a coherent experience of agency. The findings were also interpreted from a Bayesian perspective as the inverse of the Humean notion of temporal proximity's being a cue to infer causality. In particular, if people tend to attribute causal relations to events that are close to each other in time, then, under uncertainty about time, people may shift their estimates of time for causes and effects toward each other (Eagleman and Holcombe, 2002; see also Buehner and Humphreys, 2009). This argument is related to the notion of attribute substitution, whereby a variable that is hard to judge (e.g., time) may be replaced by a correlated variable (e.g., causality) to which a person may have easier access (Kahneman and Frederick, 2002). This Bayesian interpretation of the phenomenon as an automatic response that relies on a relationship between two correlated

Table 1 | Summary of some studies showing an effect of causality on time judgments.

References	Experimental task (operationalization of cause and effect)	Method of interval assessment	Range of studied time intervals	Proposed process/ explanation	Proposed consequences/ implications
Buehner and Humphreys, 2009; Humphreys and Buehner, 2009	Press key—hear auditory tone	Numeric estimates, event synchronization	150 ms–4 s	Priming of general causality-time relationship	Anticipated action timing
Ebert and Wegner, 2010	Pull/push joystick—see object move on	Numeric estimates	100–700 ms	Retrospective inference	Binding associated with explicit sense of authorship
Engbert and Wohlschläger, 2007	Press key—hear auditory tone	Libet clock method	250 ms	Predictive motor process based on expectations and perceptual associative process	Priviledged representation of intentional actions
Faro, 2010	Take energy product—feel enhanced alertness	Numeric estimates, reproduction	38 s–6.5 min	Retrospective inference based on general causality-time relationship	Delayed consumption, early experience of effect, reluctance to switch to alternative actions
Faro et al., 2005, 2010	Sputnik launch—Apollo 11 landing (historical events)	Numeric estimates	3–184 years	Retrospective inference based on physical-mechanical causality	Evaluation of actions undertaken by others
Haggard et al., 2002	Press key—hear auditory tone	Libet clock method	250–600 ms	Predictive motor control process linking intentional actions and their outcomes	Coherent experience of agency, early experience of effect
Moore and Haggard, 2008; Moore et al., 2009	Press key—hear auditory tone	Libet clock method, numeric estimates	100–700 ms	Predictive motor control process and retrospective inference	Coherent experience of agency, early experience of effect

variables suggests that factors that promote reliance on correlated cues and shortcuts may moderate the effect. In line with this, cognitive load, for instance, resulted in greater compression of subjective time between causally related historical events (Faro, 2010; Faro et al., 2010). Thus, with motor movements as well as with more-conceptual action-outcome sequences, the compression effect was seen as an automatic brain response or judgment that relies on the general relationship between time and causality (**Table 1**).

Later work on this phenomenon has suggested that it can also be driven by inferential processes. In one study, Moore and Haggard (2008) manipulated the probability (50% vs. 75%) that the action (press of a key) resulted in the outcome (the tone). The authors found that when the action was unreliable in causing the outcome (in the 50% condition), there was time compression only in the trials in which the outcome occurred. In contrast, the occurrence of the outcome did not have a significant effect on time compression when the action was a more reliable predictor of the outcome (in the 75% condition). This was interpreted as evidence that time compression can occur through a retrospective inference process, because whether the outcome occurred was known only after the fact (after the action and the time interval). In a parallel finding, though in a very different setting, retrospective expert information that two historical events were causally related compressed time estimates, but only when the causal relationship between the two events was ambiguous (Faro et al., 2005). This suggests that predictive (pre-experience) as well as retrospective cues to causality can result in time compression and that they can substitute for each other (Moore et al., 2009).

The findings that retrospective cues can result in the compression of time suggests that top-down processes, such as explicit beliefs and expectations about causal relations, may affect the prevalence of this phenomenon. In particular, the cognitive system may take into account the causal structure of the environment, and this may moderate the extent to which causality compresses perceived time (Moore and Obhi, 2012). Support for this notion comes from studies that manipulate the salience of different types of causal mechanisms and examine the effect it has on time compression. In one study, participants first elaborated on various physical versus biological phenomena in order to prime causal forces that typically dissipate or build up over time (Faro et al., 2010, Study 2). For instance, to prime physical forces that dissipate over time, participants wrote about how a rock that is thrown into water can capsize a toy boat. To prime biological forces that build up over time, they wrote about how a person who smokes can contract a lung disease. As part of another task, participants then made elapsed time judgments for pairs of historical events. Time compression between causally related historical events was attenuated when participants had considered biological causal mechanisms before the focal task. The priming of different causal mechanisms did not have an effect for time estimates for events that were not causally related, ruling out potential anchoring on short versus long time intervals that could be evoked by the priming.

In another study, participants considered emotions (e.g., pride, anger) vs. traits (e.g., courage, arrogance) of the actors involved in the causally related historical events before making elapsed time judgments (Faro et al., 2010, Study 3). Those considering emotions—a type of causal force that is typically seen as dissipating over time—made shorter time estimates, and time compression was greater relative to the baseline condition. Ratings of whether the emotions involved in the events dissipated (vs. built up) over time were correlated with time estimates. These findings on the role of emotions in the compression effect for historical events are consistent with research showing that motivations and desires play a role in intentional binding studies that employ much shorter time intervals (Engbert and Wohlschläger, 2007).

These recent findings show that although compression of time may be driven by an automatic process, it can also be modulated by higher-level beliefs about causation. The extent to which one or the other plays a role can vary by the task. The role that different types of causal relations play in moderating the effect is also consistent with the mechanism-based view of causal reasoning (Ahn et al., 1995; Ahn and Kalish, 2000). This view proposes that when people say that A causes B, they believe that there is a process that takes place between A and B in which a force or causal power is transmitted. This approach, and the concept of force in particular, can suggest a more-specific accounting for time compression than reliance on the general relationship between time and causality (see **Table 1**). At least in some settings, compression may occur because people believe that many causal forces tend to dissipate, and so, for a cause to have impact, it "needs" to occur close to the effect in time (McCloskey, 1983). Thus, people might compress the time between causes and effects because they believe predominantly in dissipative causal forces, which dominate our early experiences with causality and with physical objects (Faro et al., 2010; see also White, 1998, 1999). This may be especially plausible with conceptual action-outcome sequences like historical events. It would be instructive to examine whether such variations in expectations about types of causal relations would modulate the effect in other settings.

What can we make of these findings from the different settings the effect of causality on time judgments have been observed? Our aim here is not to propose a specific process account for the phenomenon, as its various manifestations may be driven by different specific processes (see **Table 1** for a summary of different explanations proposed). However, the evidence reviewed here does entail some linkages and consistent findings. The phenomenon seems to reflect a possibly automatic, unconscious response: The implicit manipulation of intentions, and the role of cognitive resources in enhancing the effect, point in this direction. That the effect persists in more-conceptual settings, with longer time intervals and with manipulations of causality that take place after the experience of the interval, suggests that the compression tendency can be abstracted and generalized to settings in which people reason more explicitly about causality. That is, people may compress time through an automatic judgment process or the brain's motor function but can draw on causal information to compress time also in more-deliberate settings. The salience of non-default, less familiar types of causal relations (e.g., with causal forces that build up over time) mutes this tendency in some settings. But the default and predominant effect of perceiving a causal relation between two events is to subjectively compress the time between them.

There is a noteworthy parallel here to the inverse and more-familiar relationship—the role that temporal contiguity plays in judgments of causality. When exposed to Michotte's phenomenological causality animations, infants as young as three months show signs of causal processing and "rely" on temporal contiguity between cause and effect. As people mature, these early, partly innate and automatic responses are generalized and play a role as cues to causality in inference (White, 1988). Cognitive development and ability make people more sensitive to temporally distant causes, and here, too, beliefs and expectations about more-complex causal mechanisms reduce the tendency to rely on temporal proximity to infer causality (Fletcher et al., 1986; Schlottmann, 1999; Hagmayer and Waldmann, 2002; Buehner and May, 2003). It is thus possible that the two responses—the effect of time on causality, and of causality on time—are rooted in a common underlying source.

IMPLICATIONS

Most of the work examining the effect of intentionality or causality on time judgments has focused on its implications for the sense of agency. The adjustment of time for intentional actions and their outcomes was seen as evidence for a prereflective sense of agency, as an implicit marker of agency (see Moore and Obhi, 2012). The compression of time that resulted from the shifts in the times of occurrence of actions and outcomes was seen as an index of agency rather than as a variable of interest per se. We next discuss the implications of this work for decisions that are dependent on time judgments.

COMPRESSION AND PLANNING OF ACTION

The subjective compression of elapsed time between actions and their effects may affect people's plans for when a given action would need to be taken to produce timely impact in the future. In a study that tests this hypothesis, participants first consumed chewing gum and then received bogus feedback that their performance on an alertness task showed improvement. Participants then learned that the chewing gum was (or was not) causally associated with improving performance on alertness tasks (Faro, 2010). As dependent measures, participants estimated how long it may have taken for the chewing gum to have an effect on their performance (if any). This was the measure by which time compression was assessed. Then they reported the latest point they would feel comfortable using the gum again before a similar task, for an assessment whether compressed estimates of elapsed time-to-onset affected future consumption plans. Then they indicated when they were ready to begin working on the task after consuming the gum the second time. First, there was a compression of time: Participants in the strong-causal-belief condition thought the product took a shorter time to have an effect on their performance. Second, they reported that they would consume the gum closer to the time of the task on subsequent consumption. Third, they waited a shorter period before starting to work on the task after they consumed the gum again. And, finally, participants' time-to-onset estimates for the initial consumption, which were compressed through the manipulation of causality, predicted future planning and consumption decisions.

Compression of time between causes and effects can therefore make people delay their future actions. People also tend to underestimate the time between causes and effects relative to the *actual* interval (Faro et al., 2005). The combination of these factors (delaying actions because of time compression and underestimation of time relative to the actual interval) implies that people may be unrealistically optimistic in initiating actions. They would end up taking previously efficacious (i.e., causal) actions too late to be effective. This pattern is similar to the "planning fallacy," the tendency to underestimate task completion times (Buehler et al., 1994). We believe the two phenomena are related for several reasons. First, plans may be seen as a series of cause-effect scenarios (Schank and Abelson, 1977). It is thus possible that one reason people underestimate overall task completion times, committing the planning fallacy, is because they underestimate the time between cause-effect pairs making up a plan (see also Roy et al., 2005). Second, one of the main explanations for the planning fallacy involves people's taking an "internal view" of the situation. Constructing a causal scenario represents one favorable story of how the future project is likely to unfold (Kahneman and Lovallo, 1993). In a similar vein, providing a causal scenario of how one event led to another results in time compression (Faro et al., 2005). Finally, for both phenomena there appears to be a self-other difference: Underestimation of task compression time is prominent for tasks undertaken by oneself, not by others (Buehler et al., 1994). Similarly, in some studies, compression of time was found to be stronger when an action was taken by oneself rather than by another person (Desantis et al., 2011; but see Wohlschlager et al., 2003).

Hence, the perceived compression of time between causally related events may affect the timing of subsequent actions—leading to good actions being undertaken too late. This may also be related to the tendency to underestimate task completion times. As we discuss below, compression of perceived elapsed time can also affect expectations regarding the onset of the effect in the future—with people expecting it will occur earlier than they should.

COMPRESSION, EFFECT ONSET, AND PLACEBOS

People sometimes report feeling the effect of product consumption (e.g., caffeine) almost instantaneously—within an unrealistically short time after consumption (e.g., Reid, 2005). Such placebo-like effects may be driven by conditioning (Stewart-Williams and Podd, 2004) or expectations of future performance (Shiv et al., 2005). The compression of time between actions and outcomes suggests a more-specific reason for expectations of unrealistically rapid consumption outcomes. The studies we reviewed showed that perceived time of voluntary actions shifts forward in time, but at the same time, their effects subjectively shift backward in time (thus, resulting in compression of time). This may be one way in which the effect of actions, including consumption of drugs or other products, may be experienced earlier in time, especially if people believe in their causal efficacy.

To our knowledge, previous work on compression of time between actions and outcomes has not shown this tendency to expect that effects of consumption will occur prematurely. Recent work did document a related consequence—how compression of time for a previous consumption episode may make people experience the effect earlier upon future consumption. In particular, the compression phenomenon implies that people may be prone to underestimate the time it took for a product to show its effect when they used it in the past if they believe in its causal efficacy. These recollections of too short a time-to-onset can alter people's subsequent consumption experiences, leading them to report prematurely rapid effects from subsequent consumption.

Participants in one study consumed chewing gum and then performed an alertness task (Faro, 2010). Those in the strong-cause condition were then led to believe that the gum was responsible for the improved performance they allegedly showed on the previous task. Those in the weak-cause condition were made aware of an additional possible influence on their performance (practice with the task). Replicating a compression effect, those in the strong-cause condition thought the product had been faster to have an effect on their previous performance. More importantly, upon second consumption and performance on a similar task, participants indicated they had experienced the effect of the product earlier. Time-to-onset for previous consumption predicted the timing of subsequent-effect onset.

These results of perceived earlier onset of the effect of an external substance link the compression phenomenon to placebo effects. The study reported above manipulated causal efficacy post experience. Work on placebos has shown that various factors can affect people's expectations of the causal efficacy of a treatment before it is administered and that this can affect the extent of the placebo effect. For instance, a given treatment is more effective when it is administered by a clinician than when it is administered by a computer (Colloca et al., 2004). Accordingly, future studies can examine whether manipulations that alter the perceived causal efficacy of external agents before the experience can affect the extent of temporal compression and result in effects that are experienced sooner in time.

COMPRESSION AND INTERTEMPORAL CHOICE

Past work has shown compression for causally related events that were experienced by participants themselves (Haggard et al., 2002; Engbert and Wohlschläger, 2007; Buehner and Humphreys, 2009; Moore et al., 2009; Ebert and Wegner, 2010) or by others in the past (Wohlschlager et al., 2003; Faro et al., 2005). Might a similar effect occur for events people anticipate in the future? Recent work suggests that time compression between causes and their outcomes can extend to anticipated events, and for events that participants expect to produce rather than actually produce

(Engbert and Wohlschläger, 2007; Buehner and Humphreys, 2009; Buehner, 2012). Based on these findings, might a given time period expected to elapse between two events (e.g., "a government initiating public works" and "increased economic growth") be viewed as shorter if a person believes the first event will cause the second? If the anticipated interval seems subjectively shorter, might people be more willing to wait if waiting entails a benefit?

One experimental paradigm through which such intertemporal preferences are examined is that of time discounting (see Frederick et al., 2009, for a review). In a typical study, a participant may be asked whether he or she would prefer to receive, say, £1000 now or £1500 one year from now. The tendency to choose the smaller-sooner amount instead of the larger-later amount is one way to assess the extent to which people discount future outcomes. Discounting studies typically employ two points in time that are void of a semantic link. If causality affects perception of anticipated time, imbuing the two points in time with meanings of cause and effect can affect discounting (see also Zauberman et al., 2009). Thus, a person might be more willing to wait one year and receive £1500 (when "increased economic growth" occurs) instead of £1000 now (when "public works begin") if he or she believes the first event will cause the second. Similarly, and again because of compression in anticipated time, mentally simulating how the first event ("public works") would cause the second ("increased economic growth") may result in greater patience. Using language (e.g., causal verbs; see Talmy, 1988) that increases the perceived causal link between the events could have a similar effect on perceived time and discounting.

The notion that a causal relationship between the two future points in time can affect perceived duration and, as a result, time discounting, is linked to recent conjectures on discounting and another important semantic relationship—perceived similarity. Consider the common finding of hyperbolic discounting that for the same interval *t*, people are more patient when *t* is farther in the future than when it is near. Rubinstein (2003) suggested that this occurs because the similarity between two points in time separated by a common interval increases with the onset of the interval. Thus, 12 months is more similar to 11 months than 2 months is to 1 month. Similarity may also explain the recent findings that discount rates that are imputed when time is described using calendar dates are lower than those revealed when time is described as a delay (Read et al., 2005; see also LeBoeuf, 2006). In one study, respondents evaluated two delayed options framed either as 3 vs. 16 months or as August 29, 2003, vs. September 24, 2004. The authors suggested that 3 and 16 months are less similar to each other than the corresponding dates and thus result in greater discounting. Thus, there is evidence that similarity between two points in time might affect subjective duration judgments and time discounting. This recent evidence lends credence to the possibility that perceived causality can future duration assessments and time discounting in turn.

Recent work has also shown that an additional and potentially related variable, spatial distance, can affect the subjective judgments of duration and, in turn, time discounting. For instance, an individual in Philadelphia may perceive the same three-month duration from today to be longer when he or she is expecting to be in Los Angeles three months later than when he or she is expecting to be in New York (Kim et al., 2012). The person may thus be less patient and discount the same outcome more heavily when it is to be received in Los Angeles rather than in New York. The relationship between space and time discounting is noteworthy because something akin to causal time compression also occurs for spatial judgments: Perceived causation reduces spatial distance judgments (Buehner and Humphreys, 2010).

In summary, we conjectured that causality may affect judgment of time for action-outcome sequences that are anticipated in the future and that this may affect patience and decisions based on perceived time. We base this on recent work showing other conceptual, semantic relationships (similarity, spatial distance) affecting subjective time and patience as a result. The aforementioned variables are related. Time, similarity, and spatial distance are cues to causality (Einhorn and Hogarth, 1986; see also Trope and Liberman, 2010). Thus, various cues to causality may be influenced by causality, may affect each other, and in turn influence decisions that depend on the judged focal variable (e.g., time discounting).

COMPRESSION, AGENCY, AND CAUSAL INFERENCE

The early findings that showed voluntary actions subjectively bind to their effects in time were interpreted as an implicit marker of agency. In particular, it was proposed that the "brain contains a specific cognitive module that binds intentional actions and their effects to construct a coherent conscious experience of agency" (Haggard et al., 2002, p. 384) and thus provides a feeling of fluent flow from actions to their effects (Haggard and Tsakiris, 2009). The effect was seen as a factor that enhances the sense of agency. But it may have implications for causal inference more generally. Marsh and Ahn (2009) noted that people sometimes may need or want to link ambiguous events as causes and effects. The authors suggested that time compression may be one mechanism that enables this to occur. Long elapsed time between cause and effect is typically a limiting factor for the emergence of causal beliefs. Binding related events in time allows people to form and hold causal beliefs that might otherwise conflict with the temporal proximity cue for causality (Einhorn and Hogarth, 1986). This may make people stick with certain courses of action and be less likely to switch to alternatives that have not "benefited" from compression (Faro, 2010). Thus, compression may reinforce the already advantageous role that temporal proximate causes enjoy in causal inference. Even when causes and effects are not very near in time, we may experience them as if they are or remember them as if they were.

CONCLUSION

People subjectively compress the time that has elapsed between causes and effects. This phenomenon appears to be linked to a basic / primitive manner in which people process causality and time and to early experiences with the physical-mechanical environment. The tendency is robust and has been documented in a wide range of settings. The phenomenon has initially attracted attention as an implicit marker of agency, but it also has implications for the planning of action, intertemporal choice, and placebo effects. By shortening the perceived time elapsed between a focal action and an outcome, compression endows a focal course of action or cause with apparent advantage or privileged status. This in turn links the phenomenon back to causal inference: The effect of perceived causality on time perception may reinforce the tendency to attribute causality to temporally proximate causes.

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Attribution of intentional causation influences the perception of observed movements: behavioral evidence and neural correlates

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Recent research on human agency suggests that intentional causation is associated with a subjective compression in the temporal interval between actions and their effects. That is, intentional movements and their causal effects are perceived as closer together in time than equivalent unintentional movements and their causal effects. This so-called intentional binding effect is consistently found for one's own self-generated actions. It has also been suggested that intentional binding occurs when observing intentional movements of others. However, this evidence is undermined by limitations of the paradigm used. In the current study we aimed to overcome these limitations using a more rigorous design in combination with functional Magnetic Resonance Imaging (fMRI) to explore the neural underpinnings of intentional binding of observed movements. In particular, we aimed to identify brain areas sensitive to the interaction between intentionality and causality attributed to the observed action. Our behavioral results confirmed the occurrence of intentional binding for observed movements using this more rigorous paradigm. Our fMRI results highlighted a collection of brain regions whose activity was sensitive to the interaction between intentionality and causation. Intriguingly, these brain regions have previously been implicated in the sense of agency over one's own movements. We discuss the implications of these results for intentional binding specifically, and the sense of agency more generally.

Keywords: agency, causality, intention, mental-state attribution, intentional binding, consciousness, fMRI, social perception

INTRODUCTION

Hume famously argued that causality cannot be perceived directly but must be inferred based on certain cues such as the temporal contiguity of events (Hume, 1739/1888). According to this view, time provides the bottom-up perceptual input to the formation of higher-level causal representations. Intriguingly, more recent research on human agency implies that the reverse relationship also exists, i.e., a belief about a causal relationship between two events alters the temporal experience of those events by top-down modulation. In particular, it has been demonstrated that, when an agent is (or believes she is) the cause of an event, this causal representation can shape the way in which the timing of actions and outcomes are perceived: intentional actions, such as an active key press, and their effects, such as a tone, are perceived as closer together in time than equivalent unintentional (passive) movements and their effects (Haggard et al., 2002; Moore and Obhi, 2012). The existence of this "intentional binding" effect indicates that intentional causation is associated with the subjective binding together in time of actions and their effects. Although this intentional binding effect has been repeatedly observed in the context of voluntary action, it should also be noted here that there is ongoing debate over whether or not this effect is *specific* to voluntary action, or a property of causation more generally (Buehner, 2012; Moore and Obhi, 2012). Nevertheless, the effect reveals an intriguing reversal of the Human relationship between time and causality.

As noted above, intentional binding is consistently found for one's own self-generated actions. However, it has also been suggested that intentional binding occurs for other people's movements. For example, Wohlschläger et al. (2003) demonstrated that observers perceived the interval between an experimenter's movement and its consequence as bound together in time, whereas there was no intentional binding effect for observed machine-generated movements. Assuming that observers attributed intentionality to the experimenter's but not to the machine's movement, these results suggest that intentional binding may be a property of intentional causation in general rather than being restricted to self-generated movements. This in turn would imply that the high-level conceptualization of an observed movement in terms of the underlying intention and causation shapes the lower-level perceptual processing of this stimulus. Whereas less is known about the role of attributed causation in perception, the notion that the attribution of mental states to a socially relevant stimulus might lead to top-down modulation of perceptual information-processing is consistent with a small but growing body of findings in the social perception literature (e.g., Teufel et al., 2009).

As indicated above, previous studies focusing on first- and third-person intentional binding suggest that, at the perceptual level, there is a distinction between intentional and unintentional causation (but, see Buehner, 2012). The purpose of our study was to add to this literature by exploring brain areas sensitive to the interaction between intentionality and causality when observing other people's movements. In order to be able to address this question, we extended the paradigm used by Wohlschläger et al. (2003) to overcome two limitations. Firstly, the perceptual input used in this study was not matched across the human and machine conditions: in the former, participants saw the movements of the experimenter's gloved hand, while in the latter, they saw a disembodied rubber hand being pulled down by a mechanical device. Such perceptual differences preclude clear distinctions between top-down and bottom-up influences on perception because differences in bottom-up input are confounded with potential top-down effects. In other words, it is impossible to tell whether perceptual differences between conditions rather than the observer's beliefs regarding the intentionality or causality of the movements might be responsible for the differences in perceived duration between an observed movement and its outcome. A second caveat pertaining to the Wohlschläger et al. study is that the key conditions were distinguished not just according to intention but also the presence of an agent: a human hand, unlike a rubber hand operated by a machine, belongs to an agent. In this way, "intentionality" of the stimulus was not systematically and exclusively manipulated.

Our paradigm ensured that (i) sensory stimulation was identical in the different attribution conditions, (ii) with respect to mental-state attribution only intentionality was manipulated, and (iii) we could, on a neuronal level, tease apart the individual and combined effects of attributed intentionality and causation. Participants viewed simple key press movements that caused a tone outcome. Due to an elaborate deception procedure, observers believed that these movements were either intentional or forced upon the finger of the other person, i.e., unintentional. Crucially, the stimuli and thus the bottom-up inputs were perceptually identical across conditions. Using this paradigm, we assessed binding behaviorally with the interval estimation procedure (see Moore and Obhi, 2012, for review). In order to be able to tease apart the individual and combined roles of attributed intentionality and causation using functional Magnetic Resonance Imaging (fMRI), we included two additional non-causal conditions. That is, participants not only viewed (apparent) intentional and (apparent) unintentional causal movements (i.e., key presses that caused tones), they also viewed (apparent) intentional and (apparent) unintentional non-causal movements (i.e., key presses that did *not* cause tones).

Behaviorally, we predicted that, if intentional binding for observed movement does reflect the top-down role of attributed intentionality, binding should be present in intentional but not unintentional causal movements even when perceptual input was identical (as it was in our paradigm). In order to explore the neuronal correlates of the combined effect of attributed intentionality and causation on perception of the finger movements, we chose our ROIs based on two principles. First, in order to assess the extent to which first- and third-person intentional binding are underpinned by at least partly overlapping processes, we selected our ROIs based on previous fMRI investigations of first-person intentional binding (for review, see Sperduti et al., 2011). In particular we focused on the insula, supplementary motor areas, dorsolateral prefrontal cortex, angular gyrus, and superior parietal cortex. In addition, we added brain areas that have been implicated in social perception and social cognition such as medial prefrontal cortex (mPFC), temporo-parietal junction (TPJ), and superior temporal sulcus (STS). This collection of regions has been speculated to be a key network underpinning top-down effects on the perception of socially relevant information (Teufel et al., 2010).

MATERIALS AND METHODS

PARTICIPANTS

Nineteen participants took part in the study (mean age: 22 years; 16 females). Three participants were excluded from the analysis. One participant failed to follow task instructions, one participant did not believe the deception (revealed during the de-brief), and one participant had uncorrected visual impairment (self-reported by participant).

The experiment was approved by the National Research Ethics Service (NRES). All participants gave informed consent prior to the experiment.

BEHAVIORAL TASK

Design

We used a factorial design to systematically explore the effects of intentionality ("intentional," "unintentional") and causality ("causal," "non-causal") on movement perception and neural activity.

Pre-scanning session

Participants attended a pre-scanning session in which they were shown the experimental set-up and given practice with the paradigm. The paradigm depended upon participants believing that they were watching a live webcam video-link of another person, similar to a Skype conference, when in fact they were watching pre-recorded videos (see Teufel et al., 2010, for rationale). We first showed them a phoney video-link set-up in which there was a webcam in one of two adjacent rooms. A confederate was also sat in this room. Participants were told that they would see this person – via the "live" webcam video-link – performing simple manual key press movements on a keyboard (see **Figure 1**). Participants also briefly interacted with the apparatus that the confederate would supposedly be using in the experiment. This apparatus consisted of a keyboard on which a response key could be actively pressed down or a harness attached to the button that could cause the finger passively to move down. Participants made one active key press and were also subjected to one passive key press.

Following this introduction, participants were taken to the adjacent room where they completed practice trials in four different conditions. In two "causal" conditions participants watched videos of the confederate's finger moving down on the response key. This movement caused a beep after one of three delays: 100, 400, 700 ms (following Moore et al., 2009). Participants were told that the movement-beep interval randomly varied between 50 and

FIGURE 1 | A video still showing an example stimulus used in the experiment. The same set of videos was used in all four conditions (intentional causal, intentional non-causal, unintentional causal, unintentional non-causal). In the causal conditions the key press made in the video caused a tone effect after a delay of 100, 400, or 700 ms. The only difference in the two causal conditions was the description of the movement and, consequently, the belief of the observer regarding its intentionality. In these causal conditions participants estimated the duration of this interval in milliseconds. In the non-causal conditions the key press did not cause a tone effect. Again, the only difference in the non-causal conditions was the description of the movement (intentional or unintentional). In these non-causal conditions the participants had to press a response key as soon as possible after they detected an asterisk appearing at a random location on the video over the model's hand (on 20% of trials).

950 ms. We employed an interval estimation paradigm to assess the observers' perception of the duration between the observed movement and the tone. To make their judgment they were initially presented with the default number 500 ms and were told to press a right key to increase this number (in 50 ms increments) or a left key to decrease this number (in 50 ms increments). They continued to increase or decrease the number on the screen representing their interval estimate until they indicated by a button press their final response. These two "causal" conditions differed in terms of intention attribution: in one condition participants were told that the confederate intentionally pressed the button, in the other condition they were told that their finger was passively moved by a motor. Crucially, the videos were identical in both intentional and unintentional conditions. The only difference was the description of the movement, and consequently the belief of the observers regarding the intentionality of the observed button press. Each video lasted 4 s and included a variable delay before movement onset.

In two "non-causal" conditions participants watched videos of the confederate's finger moving but this time the movement did *not* cause a beep outcome. These non-causal conditions were included to allow us to investigate areas of brain activation that were sensitive to the interaction between intentionality and causality. In order to maintain participants' attention to the screen in these conditions and to provide a measure of spatial attention allocation, they were given a behavioral task that required them to respond as quickly as possible to an asterisk appearing at a random location on the video of the confederate's hand on 20% of trials. The only information participants were told about the asterisks was that their appearance was random. These two "non-causal" conditions also differed in terms of intention attribution: in one condition participants were told that the confederate intentionally pressed the button, in the other condition they were told they unintentionally pressed the button. Again, the videos were identical in both intentional and unintentional conditions. The only difference was the description of the movement. Each video lasted 4 s and included a variable delay before movement onset.

Each of the four conditions consisted of 12 trials. In the "causal" conditions (in which the movement caused the beep), there were four trials per interval duration. Conditions were blocked by intentionality. See **Figure 1** for example video stimulus used in the experiment.

Scanning session

When participants arrived for the scanning session they were told that the same live webcam video-link was set-up as they had seen in the pre-scanning session. However, they were not shown it this time. In the scanner they completed the same four conditions they had practiced in the pre-scanning session: intentional non-causal; intentional causal; unintentional non-causal; unintentional causal. Conditions were blocked by intentionality and there were 36 trials per condition. In the "causal" conditions (in which the movement caused the beep), there were 12 trials per interval duration. All conditions were divided into blocks of six trials separated by a period of rest during which participants fixated on a cross on the screen for 12 s.

Prior to each condition the participants were told over the intercom the nature of the movement ("intentional" or "unintentional") and whether or not the movement was causal. They were also reminded of their task, i.e., either interval estimation or detection of an asterisk.

The interval estimates in the causal conditions allowed us to measure intentional binding. We predicted lower interval estimates in the "intentional" condition vs. the "unintentional" condition (following Wohlschläger et al., 2003). The asterisk response task ensured that participants maintained their focus of attention on the moving hand in the non-causal conditions.

FUNCTIONAL MAGNETIC RESONANCE IMAGING

We used a Siemens Trio scanner, operating at 3 T, with a 225 mm field of view in the Wolfson Brain Imaging Centre, Cambridge. In total, 300 volumes were acquired using a T2*-weighted echo-planar imaging sequence with 32 slices, acquired in descending order with an oblique axial orientation, covering the whole brain. Each slice was 3 mm thick with an inter-slice gap of 0.8 mm. A repetition time of 2000 ms was used with echo time; TE = 30 ms, flip angle = 78°, and matrix size 64 × 64.

Data were analyzed using statistical parametric mapping in the SPM5 program (http://www.fil.ion.ucl.ac.uk). Images were realigned then spatially normalized to a standard template and spatially smoothed with an isotropic three dimensional Gaussian filter (8 mm full width at half-maximum). The time series in each session were high-pass filtered (with cut-off frequency 1/120 Hz) and serial autocorrelations were estimated using an AR(1) model.

Four experimental conditions (intentional non-causal, intentional causal, unintentional non-causal, and unintentional causal) were modeled using a box car function convolved with a canonical hemodynamic response. Conditions were specified as covariates in a general linear model and the beta parameter estimated at each voxel for each stimulus type, derived from the mean least-squares fit of the model to the data. The responses to each condition were compared to the fixation baseline, and each of these four contrasts was taken forward to a group analysis treating inter-subject variability as a random effect.

Anatomically defined ROIs were selected based on previous fMRI studies on sense of agency (see Sperduti et al., 2011 for review). Specifically, we included: insula, supplementary motor areas, dorsolateral prefrontal cortex, angular gyrus, and superior parietal cortex. In addition, we added the following ROIs: mPFC, TPJ, and STS. This collection of regions is thought to be a key network underpinning top-down effects on social perception (Teufel et al., 2010). ROIs were specified using PickAtlas toolbox (Maldjian et al., 2003). We report significant interactions, corrected for multiple comparisons (FDR $p < 0.05$ within the mask).

RESULTS

BEHAVIORAL: INTENTIONAL BINDING

The intentional binding effect was measured by comparing mean interval estimates in the "intentional causal" vs. "unintentional causal" conditions (following Moore et al., 2009). A behavioral study ($N = 19$) was conducted outside the scanner, using the same procedure, to examine the effect of intention attribution on intentional binding. The results showed that the mean interval estimate in the intentional causal condition was significantly lower than in the unintentional causal condition, $t(19) = 2.22$, $p = 0.040$ (two-tailed). Based on the results of this initial behavioral result coupled with Wohlschläger et al.'s (2003) study, one-tailed t-tests were used for the analysis of intentional binding data collecting inside the scanner. As predicted, the mean interval estimate in the intentional causal condition (542 ms) was significantly lower than in the unintentional causal condition (560 ms), $t(15) = 1.94$, $p = 0.036$ (one-tailed; see also Appendix). Although this effect is weaker than that found in the prior behavioral study, it nevertheless shows that intentional binding *does* hold for observed movements. This is consistent with Wohlschläger et al.'s (2003) results.

BEHAVIORAL: REACTION TIMES AND ERROR RATES

We compared reaction times (RTs) to the asterisk in the two non-causal conditions as differences in RTs may indicate more general differences in the allocation of attention in the different intention attribution conditions. One participant failed to respond at all to the asterisk in the intentional non-causal and was therefore excluded from this analysis. Although there was a numerical decrease in reaction time in the intentional non-causal condition (779 ms) vs. the unintentional non-causal condition (796 ms), this difference was not statistically significant, $t(14) = 1.73$, $p = 0.105$ (two-tailed). This suggests that differences in the allocation of attention cannot explain our key result.

In order to test further the possible confounding effect of attention, we examined the relationship between RTs and intentional binding. This allowed us to determine whether or not differences in intentional binding were related to (general) differences in attention. We ran a correlation analysis on the difference in mean interval estimates (intentional causal condition vs. unintentional causal condition) and the difference in mean RTs (intentional non-causal condition vs. unintentional non-causal condition). We found no significant correlation, $r(15) = -0.07$, $p = 0.80$ (two-tailed). This suggests that putative general differences in attention (as measured by RTs) are unrelated to the intentional binding effect.

Errors of commission (pressing the response button in the absence of the asterisk) and omission (failing to press the response button in the presence of the asterisk) were also calculated. Excluding the participant who failed to respond at all to the asterisk (see above), there were no errors of commission and only two errors of omission across the entire sample.

Taken together these results suggest that overall task performance was high and that differences in attention and performance are unlikely to explain our results.

FUNCTIONAL MAGNETIC RESONANCE IMAGING

Interaction between "intentionality" and "causality"

Intentional binding reflects a distinction, at the perceptual level, between intentional and unintentional causation. Using fMRI we investigated this distinction at the neural level. Specifically, we explored activations sensitive to the interaction between intentionality and causality. ROI analyses highlighted the involvement of a collection of brain regions reflecting this interaction (see **Table 1**). These activations are shown in **Figure 2** and the associated beta values are shown in **Figure 3**. Superior parietal cortices and motor cortices showed reduced activations for intentional causal vs. intentional non-causal conditions (see **Figures 2B,C,E,F** and **3B,C,E,F**). A more complex picture is found within the insula. Like superior parietal and motor cortices, reduced activation was found in right insula for intentional causal vs. intentional non-causal conditions (see **Figures 2D** and **3D**). On the other hand, activation in left posterior insula was increased in the unintentional causal vs. unintentional non-causal conditions (see **Figures 2G** and **3G**). Finally, the left mid-insular showed the full cross-over interaction, that is, reduced activations for intentional causal vs. intentional non-causal conditions and increased activation in the unintentional causal vs. unintentional non-causal conditions (see **Figures 2A** and **3A**). The specific directions of these effects are scrutinized in the Section "Discussion."

The main effect analysis for "intentionality" revealed no significant activations in our regions of interest. The main effect of

Table 1 | Activations reflecting the interaction between factors of "intentionality" and "causality" from the ROI analysis.

Area	Side	X	Y	Z	Z-score
Mid-insula	L	−38	−5	21	4.37
Anterior insula	R	34	16	14	4.06
Posterior insula	L	−40	−34	22	3.76
Superior parietal	R	14	−41	60	4.29
Superior parietal	L	−18	−38	57	3.81
Primary motor cortex	R	18	−28	53	4.10
Primary motor cortex	L	−16	−23	49	3.84

Talairach co-ordinates are reported.

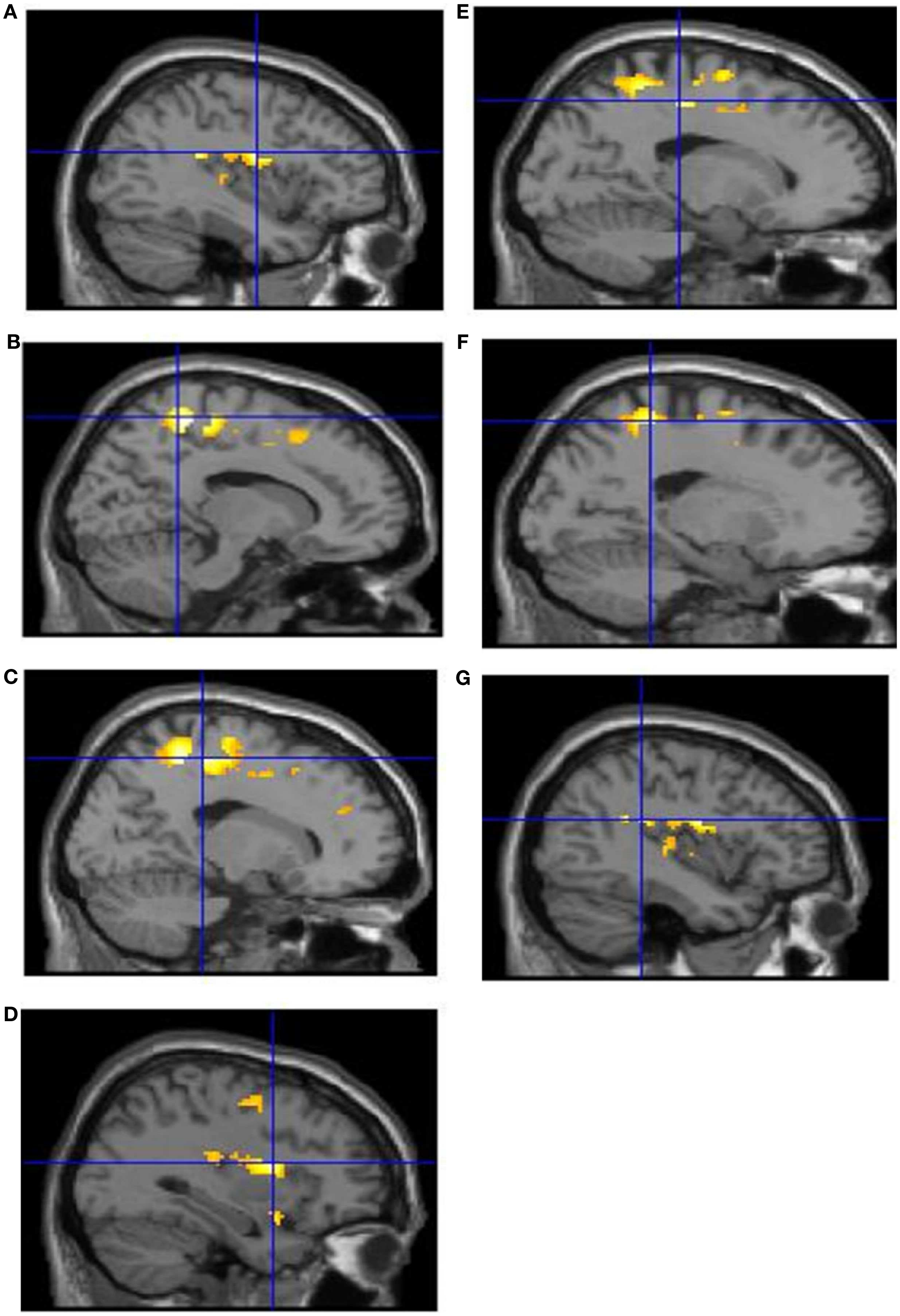

FIGURE 2 | Activations reflecting the interaction between factors of "intentionality" and "causality" from the ROI analysis in: left mid-insula (A); right superior parietal cortex (B); right motor cortex (C); right anterior insula (D); left motor cortex (E); left superior parietal cortex (F); left posterior insula (G).

"causality" was not analyzed owing to confounding task differences between the causal and non-causal conditions.

It should be noted that, based on the neuroimaging literature in social perception, Teufel et al. (2010) speculated about the involvement of mPFC, TPJ, and STS in mediating these top-down effects on action perception. Therefore, our failure to find significant activations in this network for both the interaction and main effect analyses was surprising. The possible reasons for this are considered in the Section "Discussion."

DISCUSSION

Intentional binding refers to the subjective compression of time between intentional movements and their causal consequences. This effect has been most consistently observed for self-generated movements. However, here we report evidence that intentional binding also occurs for observed movements of another person; this third-person intentional binding effect is consistent with previous findings (Wohlschläger et al., 2003). Moreover, using fMRI we explored, at the neural level, the distinction between intentional and unintentional causation that is thought to underlie intentional binding and was hypothesized to be equally important for binding of observed movements. Consistent with our predictions, we found that a collection of brain regions thought to underlie intentional binding of one's own actions was also sensitive to the interaction between attributed intentionality and causality for observed movements. Taken together, the findings indicate (i) that third-person intentional binding is a top-down effect of higher-order areas on lower-level perceptual areas, (ii) that intentional binding relies on intentional causation but is not self-specific, and (iii) that first- and third-person intentional binding are most likely subserved by at least partly shared mechanisms. We discuss these issues in more detail in the following sections.

TOP-DOWN EFFECTS ON SOCIAL PERCEPTION

As mentioned in the Section "Introduction," previous work on intentional binding for observed movements (e.g., Wohlschläger et al., 2003) is undermined by limitations with the paradigm used to study this phenomenon. Most problematic for the question regarding the source of these effects is that perceptual input differed in the two attribution conditions. This is a serious limitation because it makes it impossible to separate bottom-up from top-down effects on this task. In other words, any difference in third-person intentional binding between the two attribution conditions might be due to differences in the perceptual input rather than an effect of top-down modulation by attributed intentionality or agency.

In order to address this issue, we explored intentional binding for observed movements using a more rigorous paradigm (Teufel et al., 2010), in which sensory stimulation was identical in the different attribution conditions; the only difference between them was whether observers believed that the movement they viewed was generated intentionally or was forced upon the finger of the other person. The fact that, even with this design, we found an increase in third-person intentional binding when participants attributed an intention to the movement compared to when they

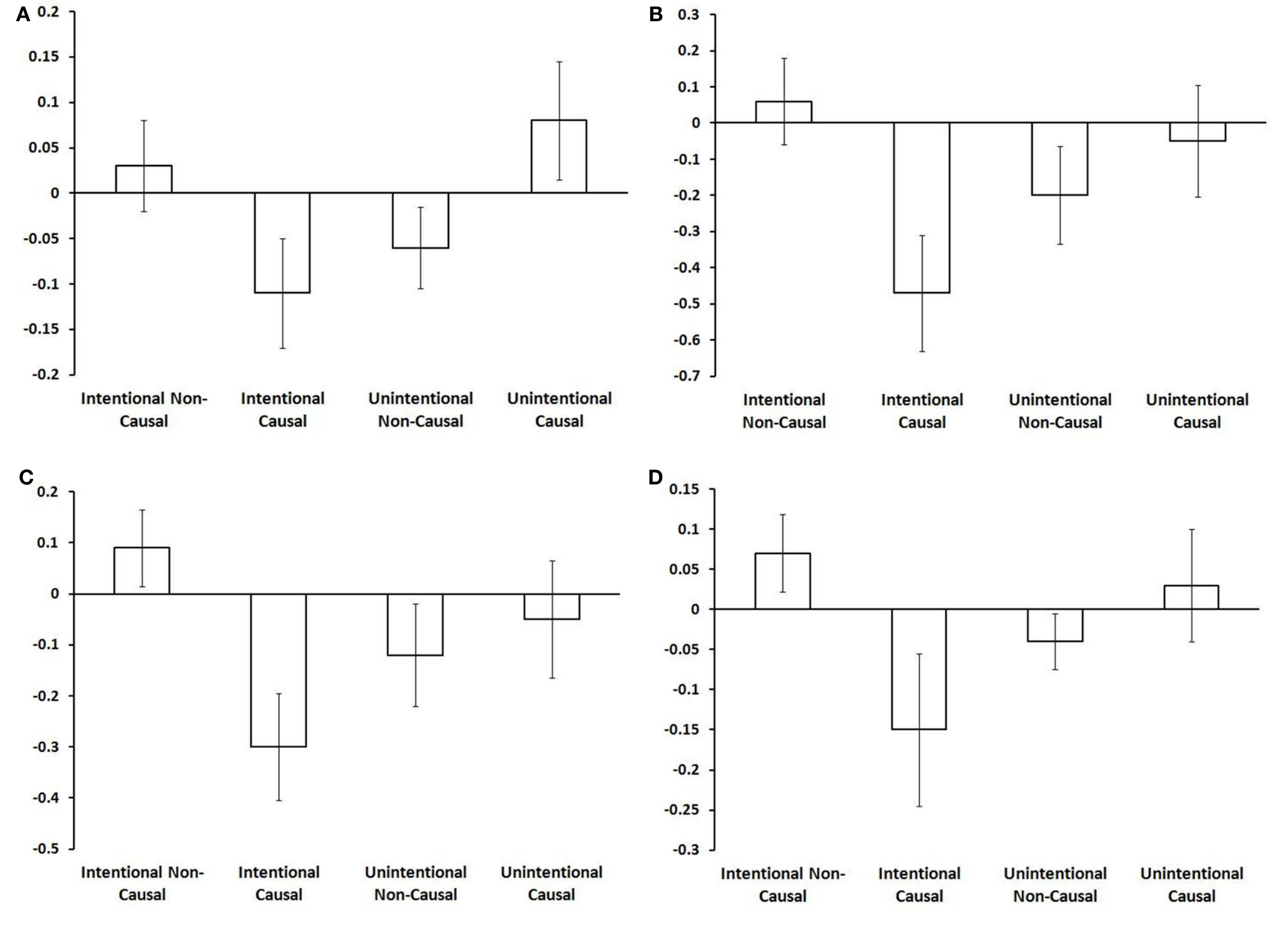

FIGURE 3 | Continued

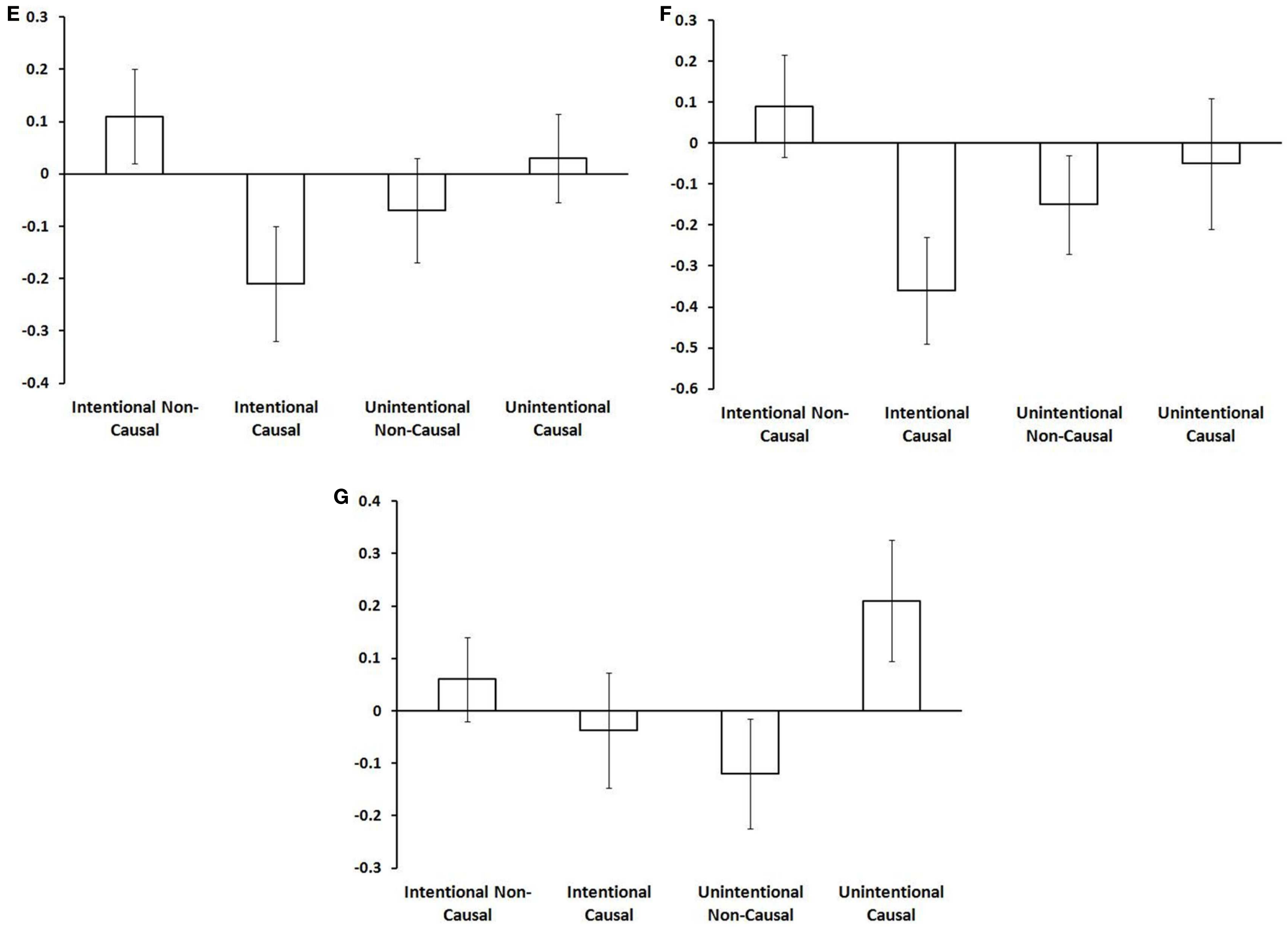

FIGURE 3 | Beta values in each area for each condition (from left to right: intentional non-causal, intentional causal, unintentional non-causal, unintentional causal). Although they are intended for the purposes of guidance only, these plots do suggest differences in the nature of the interaction in these regions. The full cross-over interaction is only found in the left mid-insula **(A)**. An increase in activation for intentional non-causal vs. intentional causal is found in right superior parietal **(B)**, right motor cortex **(C)**, right anterior insula **(D)**, left motor cortex **(E)**, and left superior parietal cortex **(F)**. The only area showing an increase in activation for unintentional causal vs. unintentional non-causal is left posterior insula **(G)**.

believed it was unintentional indicates that this effect cannot be due to differences in bottom-up input. Therefore, it is most likely a result of a top-down modulatory influence of intention attribution on those processes that underlie the perceptual binding of observed actions and their consequences. This finding adds to a number of recent studies indicating that the higher-level conceptualization of a stimulus in terms of the underlying mental states can shape lower-level social information-processing.

In a recent proposal by Teufel et al. (2010), the authors speculated about the neural implementation of such top-down modulation of perceptual processing by the high-level conceptualization of the stimulus in terms of mental states. In particular, they proposed that the high-level component is localized in the theory of mind network, including mPFC (e.g., Fletcher et al., 1995) and TPJ (e.g., Saxe and Kanwisher, 2003), whereas perceptual processing of biological motion and other social stimuli seems to primarily take place in STS (e.g., Puce and Perrett, 2003). In neural terms, top-down modulation of perception by mental-state attribution would thus imply a feed-back loop between mPFC/TPJ and STS. A recent study provided some support for this hypothesis with respect to automatic imitation, the tendency of an observer to automatically mimic the movement of another person (Wang et al., 2011). Given that automatic imitation is closely linked to action perception – in fact, in the proposal by Teufel et al. (2010), the effects on imitation are a knock-on effect of the modulation of perception – it is surprising that we did not detect a similar influence of the ToM network on perceptual processing of another person's movements in the current study. Even more surprising is our failure to detect increased activation of these areas when participants attributed an intention to the observed movements compared to when they believed the movements were unintentional. This lack of activation of crucial parts of the ToM network is inconsistent with many previous reports and necessitates further consideration in future studies.

One possible reason for the lack of activation in this network is that our paradigm was not sensitive enough to produce these activations. However, we think this is unlikely given the fact that (a) we found significant activations in different brain regions for the interaction analysis, and (b) there was a difference in intentional binding in the intentional and unintentional conditions. Both findings suggest that our manipulations were effective. In light of this, we would suggest that this lack of activation may be linked to important differences between previous studies on intention attribution and the current one. First, whereas in previous studies the observed action was typically not followed by any obvious outcome, in our study, both the intentional and the unintentional movements were causal determinants of a tone. While this is largely speculation, it might turn out that the brain processes movements with and without obvious outcomes differently. A second difference relates to the fact that previous studies have used perceptually different stimuli in the different conditions. Conceptually, these stimulus differences are supposed to signal to the observer differences in intentionality of the movements. In our study, the differences in intentionality were not perceptually signaled but were only present in the way in which the observer conceptualized the stimuli (due to our deception procedure). A possible explanation for a lack of activation in ToM related areas in our study might be that attributed intentions that are signaled by the bottom-up input are processed differently than attributed intentions that are purely set-up by the observer's belief system. A more controversial interpretation of previous studies is that because the intention conveyed by a stimulus and the perceptual properties of the stimulus were confounded, it might be that increased activation in mPFC, TPJ, and STS directly reflect differences in perceptual processing rather than reflecting the attribution of intentions.

THE NEUROCOGNITIVE BASIS OF INTENTIONAL BINDING, AGENT CAUSATION, AND LACK OF SELF-SPECIFICITY

Although intentional binding is a widely used implicit measure of sense of agency, there is, nevertheless, an ongoing debate about what processes intentional binding reflects (Moore and Obhi, 2012). For example, some have suggested that intentional binding is not a specific property of agent causation, but is instead a property of any causal relationship. Indeed, a number of studies have demonstrated the importance of causality for intentional binding (Buehner and Humphreys, 2009; Moore et al., 2009; Buehner, 2012). The current findings, although not ruling out the role of causality, do at least suggest that the presence of intentionality augments binding. These results also suggest that whilst binding is likely to be augmented for intentional agent causation, this effect is not *self*-specific. That is, intentional binding is not only found for one's own self-generated movements but instead appears to be a more general property of agent causation. This raises important questions regarding the neurocognitive processes supporting binding and whether they are the same for first-person and third-person binding.

It has been suggested that sensorimotor processes play a central role in intentional binding (Haggard et al., 2002). This is based on observations that intentional binding is most consistently found for voluntary actions (i.e., those which necessarily engage sensorimotor processes; see Moore and Obhi, 2012, for a review). This assumption is potentially undermined by our findings, which show intentional binding occurs when people are *passively* observing another person move. Here, the motor system of the observer is not overtly engaged.

One possible explanation for this finding is that sensorimotor information is not essential for the intentional binding effect. Indeed, this has been demonstrated by a number of studies showing that binding can occur in the absence of voluntary movement. For example, by modifying intentional content prior to a passive movement (Moore et al., 2009) or by *implying* self-causation prior to a passive movement (Dogge et al., 2011), one can modulate the magnitude of binding. This is consistent with a recent theoretical framework highlighting the contribution (and optimal integration) of various cues to sense of agency, of which sensorimotor information is just one (e.g., Moore and Fletcher, 2012).

Another possible explanation is that, although the sensorimotor system is not overtly engaged during action observation, it is nevertheless *covertly* activated. This could generate the binding effect for observed movements. Our fMRI data offer indirect support for this hypothesis. We selected a number of ROIs based on regions commonly involved in the sense of agency of *one's own* overt actions. In the present study we found that a number of these regions were also involved in discriminating between intentional and unintentional causation when *observing* someone else move, including superior parietal cortices, the insula, and primary motor cortices. The involvement of these regions, in particular the primary motor cortices, suggests that sensorimotor processes engaged when performing an action also contribute to agency processing when observing an action. This hypothesis is supported by a large body of research highlighting the tight link between systems involved in action execution and action observation. For example, when observing someone else move there is an automatic tendency to imitate these movements (Brass et al., 2001). Moreover, this tendency is influenced by higher-level mental-state attributions. For example, Wang et al. (2011) found that automatic imitation was enhanced during direct eye contact, and Liepelt et al. (2008) found that it was enhanced when people were led to believe the movements they were seeing were intentional. This latter study is particularly relevant and offers a plausible explanation for our finding of increased binding when people were led to believe the action was intentional: this instruction would have increased covert sensorimotor activity during action observation.

PATTERNS OF ACTIVATION: THE ROLE OF PREDICTION ERROR?

There was an intriguing pattern of activation in those regions reflecting the distinction between intentional and unintentional causation (see **Figure 3**). The interactions revealed a relative increase in activity in both unintentional causal and intentional non-causal conditions. We can only offer a speculative account of what this may mean. One possibility is that these activations are linked to prediction error. Central to this proposal is the notion that action and goal/outcome representations are inextricably linked. According to so-called "response-outcome" (R-O) theories of intentional action (Thorndike, 1931; Dickinson and Balleine, 1993, 1994; de Wit and Dickinson, 2009), once R-O associations have been established, thoughts about actions prior to movement automatically trigger thoughts about associated outcomes. These outcomes are then evaluated with respect to goals and the appropriate response is selected. Based on the assumption of a shared network for action generation and action perception, and in line with R-O theories, we would suggest that when participants were led to believe they were watching an *intentional* action, this would first activate the shared action network, which in turn automatically activates an outcome representation. The higher activity for intentional non-causal action vs. intentional causal action may represent error-related increases in activation linked to the *absence* of an *expected* effect. Regions that appear to be particularly sensitive to this include: superior parietal cortex, the motor cortex, and the right insula. Following this same logic, the representation of *unintentional* action should *not* activate goal/outcome representations. If this were the case then when one is led to believe they are watching an unintentional action, this would fail to activate goal/outcome representations. The higher activity for unintentional causal action vs. unintentional non-causal action may also be error-related activation linked to the *presence* of an *unexpected* effect. The single region that is particularly sensitive to this is the left posterior insula.

Although speculative, this prediction error hypothesis receives support from previous studies which demonstrate the involvement of these regions in outcome prediction and/or the encoding of prediction error. For example, it is well established that the parietal lobe is involved in sensorimotor prediction (Andersen and Buneo, 2002; Blakemore and Sirigu, 2003). Furthermore, it has been shown activity within superior parietal regions is higher during *unpredictable* externally produced tactile stimulation compared with predictable self-produced tactile stimulation (Blakemore et al., 1998). The insula, another core region highlighted by our analyses, is also commonly activated when predictions are violated (Preuschoff et al., 2008; Bossaerts, 2010). Of particular relevance is the suggestion that performance monitoring – detecting mismatches between goals and outcomes – is one of the primary functions of the insula (and in particular, the anterior insula; Ullsperger et al., 2010).

CONCLUSION

In summary, our findings support a number of conclusions. First, the fact that intentional binding not only holds for self-generated but also for observed movements suggests that, although it may be a property of *agent* causation, it is not self-specific. Second, we were able to establish the presence of intentional binding for observed movements in the absence of perceptual differences between intentional and unintentional condition. This represents an important methodological advance. Finally, our fMRI data reveal a collection of regions whose activity reflects the interaction between intentionality and causality, something that lies at the heart of the intentional binding effect. These regions have also been implicated in the sense of agency over one's own movements. In light of these observations we have suggested that common mechanisms may underpin the experience of self-agency and the attribution of agency to others.

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APPENDIX

Table A1 | Average interval estimate (ms) for each interval length in the intentional causal and unintentional causal conditions.

	100 ms	400 ms	700 ms
Intentional causal	354 (26)	569 (21)	705 (22)
Unintentional causal	348 (26)	596 (26)	739 (22)

SEM in parentheses.

A 2 (intentionality: intentional causal/unintentional causal) × 3 (interval length: 100/400/700 ms) repeated measures ANOVA conducted on these data (Table A1) shows a near-significant main effect of "intentionality," $F(1, 15) = 3.75$, $p = 0.072$, *a significant main effect of "interval length,"* $F(2, 30) = 137.73$, $p < 0.001$, *and no significant interaction between these factors,* $F(2, 30) = 2.01$, $p = 0.15$.

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To lead and to lag – forward and backward recalibration of perceived visuo-motor simultaneity

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Studies on human recalibration of perceived visuo-motor simultaneity so far have been limited to the study of recalibration to movement-lead temporal discrepancies (visual lags). We studied adaptation to both vision-lead and movement-lead discrepancies, to test for differences between these conditions, as a leading visual stimulus violates the underlying cause-effect structure. To this end, we manipulated the temporal relationship between a motor action (button press) and a visual event (flashed disk) in a training phase. Participants were tested in a temporal order judgment task and perceived simultaneity (PSS) was compared before and after recalibration. A PHANToM©force-feedback device that tracks the finger position in real time was used to display a virtual button. We predicted the timing of full compression of the button from early movement onset in order to time visual stimuli even before the movement event of the full button press. The results show that recalibration of perceived visuo-motor simultaneity is evident in both directions and does not differ in magnitude between the conditions. The strength of recalibration decreases with perceptual accuracy, suggesting the possibility that some participants recalibrate less because they detect the discrepancy. We conclude that the mechanisms of temporal recalibration work in both directions and that there is no evidence that they are asymmetrical around the point of actual simultaneity, despite the underlying asymmetry in the cause-effect relation.

Keywords: time perception, visuo-motor integration, temporal recalibration, multisensory perception, simultaneity perception

INTRODUCTION

When determining the timing of multisensory events, our brains have to compensate for cross-sensory latencies that stem from physical sources (e.g., light travels faster than sound) as well as physiological sources (e.g., differences in sensory transduction or neural transmission times). A growing body of evidence shows that the mechanisms of latency compensation are plastic and that they can be recalibrated by exposing participants for some period of time to a systematic small temporal discrepancy between unimodal events. Temporal recalibration of this kind has been shown, for instance, for the perception of audio-visual, audio-tactile, and visuo-tactile simultaneity (e.g., Fujisaki et al., 1994; Keetels and Vroomen, 2008; Di Luca et al., 2009).

The perceived order of a voluntary movement event and an external sensory event seems to be no exception from this rule. Stetson et al. (2006) have shown that humans recalibrate to partially compensate for a 100 ms lag between a button press and a visual flash. Similar results were reported in experiments with rhythmic finger tapping, including studies of sensory-motor recalibration in other modality pairs (tactile-motor, auditory-motor) and where transfer across modalities was observed (Heron et al., 2009; Sugano et al., 2010; Keetels and Vroomen, 2012; Sugano and Vroomen, 2012). Heron et al. (2009) could show that visuo-motor temporal recalibration weakens with increasing temporal discrepancy. Arnold et al. (2012) have shown that this constraint of temporal proximity is relative to the time of button press, not to the time of movement planning or movement onset. Yet, these kinds of studies have so far been limited to scenarios where the movement event leads the temporal order[1]. It is not clear, however, whether adaptation where an external sensory event precedes a voluntary movement is possible and, if it is, whether it follows the same rules as adaptation to movement-lead discrepancies. This is an interesting question because of the causal relationship that usually is accompanied with such sensory-motor events, i.e., a voluntary button press may trigger a flash but not vice versa. Given this rationale, a possible hypothesis is that it is not possible or more difficult to adapt if a flash precedes the movement event because of a violation of the naturally occurring causal relationship. By contrast, given that mechanisms of sensory-motor recalibration tend to operate symmetrically in space, a different hypothesis would be that recalibration should work symmetrically in time as well. Here we designed an experiment to empirically test these two alternative hypotheses.

Evidence in the literature that supports the asymmetry hypothesis stems from several sources. For instance, differences in processing around the point of actual simultaneity have been found in audio-visual speech perception, where subjects tolerate much larger auditory lags than visual lags, leading to an asymmetric

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[1] A possible exception is a study by Kato et al. (2009) that presumably found evidence for vision-lead adaptation with very small temporal discrepancies (15 ms). To our knowledge, this research has not been published in article format.

temporal window of integration (van Wassenhove et al., 2007). Even though there are also functional explanations for this asymmetry, the authors think it is possible that this asymmetry could arise simply from differences in uni-modal neural processing. Such asymmetries could in principle be found in any modality pair. Also, the above-mentioned possible causal relation between a voluntary movement event and a subsequent sensory event could lead to asymmetry around the actual point of simultaneity. Haggard et al. (2002) have shown that, if a sensory event systematically follows shortly after a voluntary movement, it will be interpreted as sensory feedback (intentional binding) and both will be perceived closer together in time then when movement is not voluntary and intentional (Haggard et al., 2002, cf. also Eagleman and Holcombe, 2002). This intentional binding appears to be a special case of causal binding that occurs whenever humans assume a causal link between two events (Buehner and Humphreys, 2009). Intentional or causal binding thus appears to strengthen the "unity assumption" (Welch and Warren, 1980), i.e., the assumption that events belong together and originate from a common source, which is a key requirement for multisensory integration. If a sensory event precedes the voluntary movement event, however, the underlying cause-effect relationship for intentional binding is violated, which may decrease the unity assumption and slow down or even prevent multisensory recalibration. Thirdly, during growth and development, compensation for longer visual feedback latencies (longer nerve conductance times) may be required, whereas a shortening of neural conductance latencies is, ecologically speaking, not to be expected.

There are, however, also arguments in favour of the symmetry hypothesis. For instance, a shortening of visuo-motor latencies, even if physiologically implausible, can occur in our interaction with digital technology, where the reaction, e.g., the appearance of a letter on the screen, may be delayed with respect to the button press on the keyboard. If we are able to adapt to this kind of delay, we should also be accustomed to a re-adaptation in the reverse direction when stopping interaction with the device, even though there could be absolute limits on this reverse adaptation. Also, simplicity favors delay compensation mechanisms that are general and thus symmetrical, such as the Kalman Filter model suggested by (Burge et al., 2008) or the Smith predictor model of cerebellar visuo-motor control in motor behavior (e.g., Miall et al., 1993), where sensory-motor latency compensation is implemented separately from a plasticity rule that estimates sensory-motor delays to be compensated from experience.

Researchers attempting to empirically settle this question by also studying adaptation to vision-lead temporal discrepancies between voluntary movement and vision will face a technical difficulty. In order to time the presentation of a visual stimulus before a voluntary movement event, the experimenter has to know or to predict when a subject will perform the action. This problem has been elegantly solved by Stetson et al. (2006). The authors kept a running average of participants' reaction times to an external cue event. They were thus able to present visual stimuli from a range of temporal discrepancies that was symmetrical around the point of actual simultaneity of button press and visual event, using the average reaction time in previous trials as a predictor for the timing of the next button press. Recalibration also occurs in the absence of an external cue event, as a second experiment by Stetson et al. (2006) confirmed. In this experiment, participants themselves chose the timing of repeated button presses. The time of a future button press was then predicted from the relative timing of previous button presses. Similarly, Arnold et al. (2012) used a leading button release to time visual stimuli to occur before a second button press at the end of a ballistic reach. These kinds of prediction, however, are likely not accurate enough on a trial-by-trial basis to time a temporal discrepant recalibration stimulus.

For the current study, we developed a new method to test whether participants recalibrate equally to the presence of vision-lead and movement-lead temporal discrepancies. We used a haptic device (PHANToM©force-feedback device, Sensable Inc.) to display a virtual button and tracked participants' finger movement online. Using an adaptive threshold method (cf. Materials and Methods), we predicted the moment of full compression of the virtual button in real time. We were able to predict the button press quite precisely within about 100 ms such that we could reliably present visual-motor stimuli with vision leading by 100 ms with respect to the movement event (the full button press). We could thus compare adaptation to vision-lead and movement-lead temporal discrepancies within a window of ±100 ms.

MATERIALS AND METHODS

EXPERIMENTAL SETUP

Participants were seated in a dark room and placed their head in a chin-rest, looking down into the direction of their hands. The hands were occluded from vision by a mirror (see **Figure 1**). Participants' right lower arm rested on a board and the right index finger was attached to a PHANToM force-feedback device. The device simulated a virtual button (mass $m = 0.1$ kg) with a throw of 8 mm, which contained a 4 mm spring (spring constant $k = 500\ \text{kg/s}^2$) plus a dead-band of 4 mm (see **Figure 2A**). A small restoring force

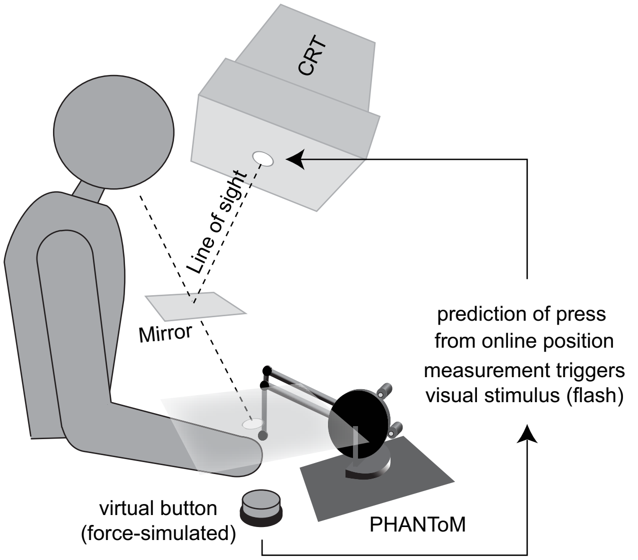

FIGURE 1 | Schematic of the experimental setup.

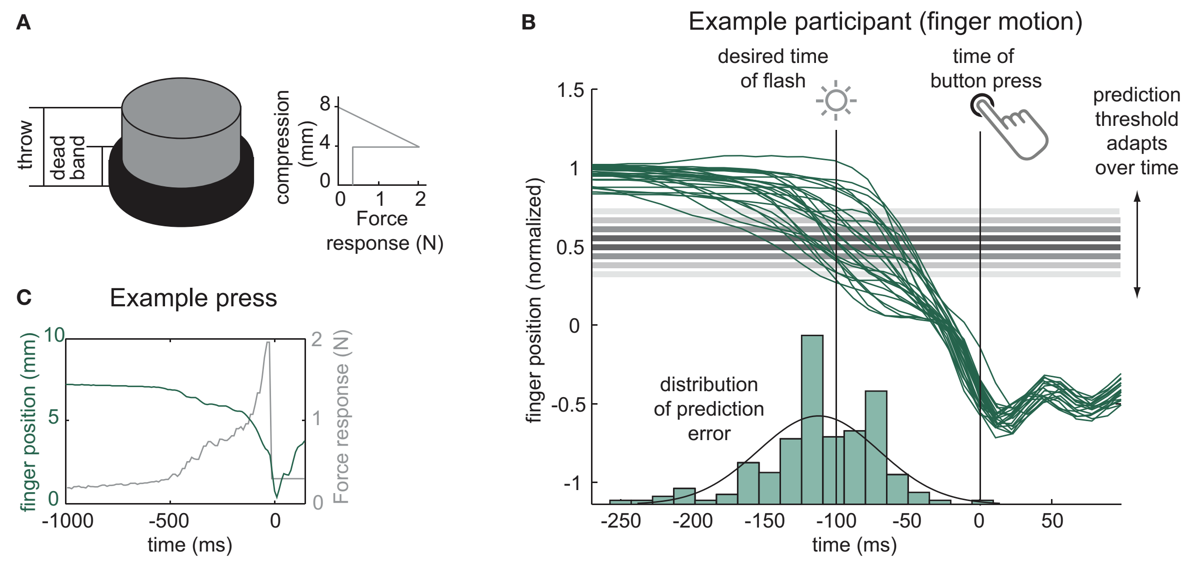

FIGURE 2 | The virtual button and prediction method. (A) Force-response of the virtual button. **(B)** Example finger movement trajectories in the vertical dimension (green solid lines; 1 unit∼4 mm, normalized for initial finger height, cf. main text) and prediction error over 333 trials (green histogram at the bottom) for a $l = -100$ ms (vision-lead) stimulus in an example participant. The adaptive threshold moves up and down according to the sign of the error on a previous trial. **(C)** Example button press (green) and force-response (grey) across time.

(0.3 N) pressed the button back up after full compression (see **Figures 2A,C**). Participants rested with their finger on the button and did not receive visual feedback about the position or compression of the button. Additionally, a haptically displayed virtual object directly above the button blocked participants from raising the right index finger higher than the height of the decompressed button.

The vertical displacement of the participant's finger during the button press was tracked in real time, in order to predict the timing of the full compression of the button from early movement onset (cf. following section). The initial resting height on the button varied slightly from trial to trial as participants started a trial resting with the finger on the button. The top part of the button is compliant and thus small differences in the resting force applied by subjects will lead to slight differences in the resting position. Especially when predicting large negative lags, this variability can lead to early alarms if a participant already compresses the button a bit when resting at the beginning of the trial. The tracked vertical position was therefore normalized for the prediction algorithm to the distance between the initial resting position and the entering of the dead-band (cf. **Figure 2B**, green trajectories), which comprises approximately the 4 mm length of the spring.

Visual probe stimuli were projected into participants' field of view using a CRT monitor mounted upside-down above the mirror. The visual flash stimuli were white disks of 1.5° visual angle on a 50% gray background. At no time during the experiment did the participants receive any visual information beside this flash and instructions printed on the screen (cf. Procedure and **Figure 3**). The flash was projected into the area where participants pressed the button but was not spatially aligned with the finger. The refresh rate of the monitor was 90 Hz and stimuli were flashed for one frame (i.e., ∼11 ms) upon button press.

The setup has an inherent endpoint-to-endpoint delay of 34.5 ± 7 ms interquartile range (IQR) between a button press in the real world and the display of a visual flash on the screen triggered by the button press. This was measured using two photodiodes and a method similar to that described in Di Luca (2010). In the notation used in this paper the system latency is not yet subtracted when computing the stimulus onset asynchronies (SOAs), i.e., a baseline visuo-motor lag $l = 0$ corresponds to a scenario where a button triggers a visual stimulus that then flashes on the screen 34.5 ms later (visual lag l is defined as $l = t_v - t_m$ where t_m is the time of full decompression of the button and t_v the time of the visual flash).

PREDICTION METHOD

The vertical position of the right index finger was tracked with a frequency of 90 Hz to predict the moment of full compression of the button (cf. **Figure 2A**).

Besides being precise, there are a number of requirements the prediction method has to fulfill. It has to be simple, in order to compute in real time; it should be robust because motion profiles for the button presses vary both within and between participants; and it should be unbiased, i.e., it should be more or less equally prone to predicting too early or too late. We found that, by and large, an adaptive threshold method performed well according to all of these criteria. An array of thresholds corresponding to the different SOAs (cf. Procedure) was initialized using the median position of the finger during the 20 practice button presses as starting threshold. Afterward, this threshold

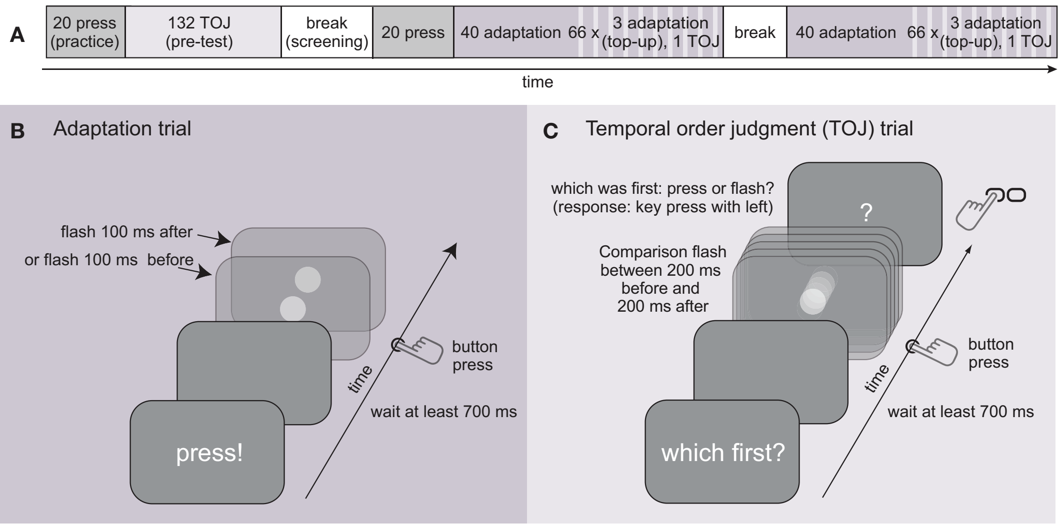

FIGURE 3 | Procedure and task. (A) Timeline of one experimental session. **(B)** In adaptation trials, participants are presented with either a vision-lead or movement-lead 100 ms discrepancy. **(C)** In temporal order judgment (TOJ) trials, participants respond with a key press of their left hand which one they perceived to occur first – button press or flashed disk.

moved up or down with a step size of 0.05 units (~0.2 mm). The direction of the step depended on the sign of the error of the previous prediction, i.e., it moved up if prediction had been too late and it moved down if prediction had been too early. The mean IQR across participants and conditions with which this method could predict the target recalibration discrepancy of $l = -100$ ms was 61 ms (range of IQR of prediction error: 22–122 ms).

PROCEDURE AND TASK

Ten paid volunteers (7 female; Average age 25.7, age range 21–38; all right-handed as by self-report) were tested in two conditions on different days: movement-lead adaptation ($l = 100$ ms) and vision-lead adaptation ($l = -100$ ms). The order of these conditions was counter-balanced across participants. The experiments were approved by the Ethics Committee of the University Clinics Tübingen, Germany. All participants signed informed consent forms and were naïve to the purpose of the experiment. Each session lasted for 60–90 min and consisted of one pre-test block (block 1) and two adaptation/post-test blocks (blocks 2 and 3; cf. **Figure 3A**).

Participants were instructed to wait for at least 700 ms and as long as they wanted after a trial started before pressing the button. This minimum waiting period was introduced for two reasons. Firstly, the predictor had to be given time to generate its prediction. Secondly, we wanted to avoid that button presses are simple reactions to an external trigger (initiation of trial). Self-initiation of an action alters both temporal processing (e.g., Jenkins et al., 2000) and behavior (e.g., Welchman et al., 2010). If the button was pressed too early, the words "too early" were projected into participants' field of view and the trial was repeated. Blocks 1 and 2 started with some training of 20 button presses that triggered a task-unrelated auditory signal, to initiate the predictor and habituate participants to the required minimum waiting time of 700 ms.

In Block 1, participants were exposed to temporal order judgment (TOJ) trials only (cf. **Figure 3C**). A question mark was displayed and participants had to make a forced choice decision about the temporal order (TOJ) whether they had perceived the visual stimulus to have occurred before or after the button press. They were instructed to judge the timing of the visual stimulus compared to the time when they fully compressed it, after entering the dead-band that was haptically clearly perceptible (cf. **Figure 2C**). They gave their response by pressing a response key with their left hand. Participants were tested for 12 repetitions in the TOJ task with visual comparison stimuli aimed at the following visual lags l: $[-200, -150, -100, -67, -33, 0, 33, 67, 100, 150, 200]$ ms. Values from the negative range were predicted from early movement onset (cf. previous section). As mentioned above, the prediction naturally is not always perfect but may contain some prediction error. If, due to these errors, the effective SOA was closer to a different value from the range of target SOAs, planned comparisons for future trials were rearranged online to ensure an overall balanced presentation of SOAs. A psychometric function in form of a cumulative Gaussian was fit to the responses to the TOJ task using the Matlab toolbox *psignifit* (Wichmann and Hill, 2001a,b) to derive the point of subjective simultaneity (PSS) and the just noticeable difference (JND). PSS and JND were the only free parameters. Participants with a JND > 150 ms in their first block were discarded from the experiment, as the narrow range of SOAs around the PSE does not allow for a reliable estimation of the entire psychometric curves for participants with lower perceptual precision.

In Blocks 2 and 3, participants were first exposed to 40 adaptation trials (**Figure 3B**) with the respective lags $l = -100$ (vision-lead) or $l = 100$ (movement-lead), after which they were again tested with the TOJ task (six repetitions per blocks 2 and 3; cf. **Figure 3A**), exposing them to three top-up adaptation trials in between each TOJ trial. The noise on the temporal discrepancy in the vision-lead adaptation condition, due to prediction errors, was mirrored across the $l = 0$ point for the movement-lead condition to assure comparability of the two conditions. If the predictor failed to predict a button press before it occurred (15% of training trials), no visual stimulus was displayed in adaptation trials.

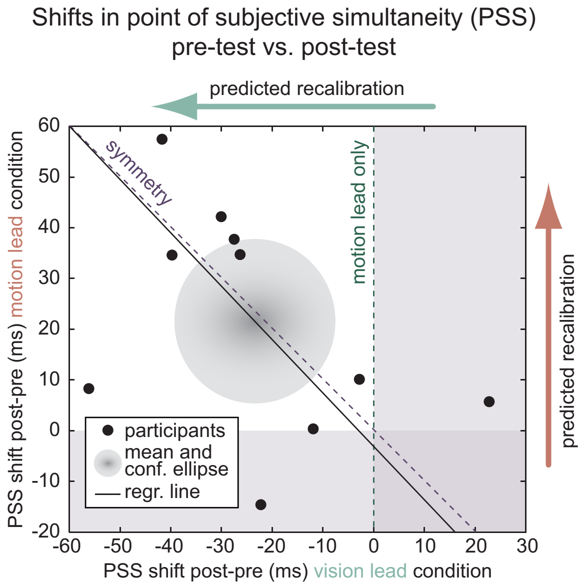

FIGURE 4 | Recalibration results. Individual participants (black dots); group mean and confidence ellipse and regression line (black). Recalibration was observed in both the vision-lead and movement-lead conditions (confidence ellipse in white quadrant) and it was not biased to either side of the inverse diagonal representing symmetry of recalibration mechanisms.

RESULTS

The PSS in the pre-test of the first session was not significantly different from the zero lag SOA at $l = 0$. There was a small non-significant bias of −11 ± 12 ms (SEM) toward the vision-lead discrepancy [t-test: $p = 0.378$, $t(9) = 0.9$], which may reflect the fact that small system latencies are not corrected for (cf. see Materials and Methods). JNDs were on average 70 ± 4 ms (SEM) across all subjects and conditions and there were no significant differences between conditions.

Recalibration was computed by subtracting the pre-test PSS from the post-test PSS. A significant recalibration effect could be found in both the vision-lead condition [recalibration: −24 ± 7 ms (SEM), $p = 0.008$, $t(9) = 3.4$] and in the movement-lead condition [recalibration: 22 ± 7 ms (SEM), $p = 0.015$, $t(9) = 3.0$]. **Figure 4** depicts the recalibration observed for individual participants as well as the group mean, the confidence ellipse, and a regression line. Paired sample t-tests confirmed that there was a significant difference between the two conditions within participants [$p = 0.004$, $t(9) = 3.8$] and that, inverting the sign of recalibration in the vision-lead condition, the magnitude of recalibration did not differ between the two conditions [$p = 0.806$, $t(9) = 0.3$]. This last result supports the hypothesis that recalibration may indeed be symmetrical.

To further assess the symmetry of recalibration in the two conditions, we performed a Deming regression (total least squares fit) of the recalibration in the movement-lead condition to recalibration in the vision-lead condition (see **Figure 4**). This yielded an intercept $a = -3$ ms (confidence interval: −37, 32 ms) and a slope $b = -1.05$ (confidence interval: −2.22, 0.11 ms). The fitted slope is very close to −1 (perfect symmetry). However, given the limited number of participants and the variability of recalibration effect size, the possibility of asymmetry, i.e., that there is stronger recalibration in the case of movement-lead adaptation, cannot be ruled out.

Taken together, these analyses show that there is recalibration in both the vision-lead and the movement-lead condition. They provide no evidence against the symmetry hypothesis.

The size of the recalibration effect we observed was lower than in previous studies, where participants exposed to a 100 ms movement-lead delay recalibrated their PSS between ca. 30 ms (Heron et al., 2009; Sugano et al., 2010) and 44 ms (Stetson et al., 2006). A possible reason for this could be differences in the reliability of the error feedback (i.e., the temporal discrepancy). The prediction method used in our paradigm introduces temporal noise that previous studies on adaptation to movement-lead discrepancies did not have. Burge et al. (2008) found that the rate of adaptation in visuo-motor control decreases with the amount of noise in the error feedback (i.e., the feedback about the temporal discrepancy as measured by the JND) in order to obtain statistically optimal learning of a new sensory-motor mapping. Furthermore, they showed that in a stable world in which the mapping is more predictable the learning rate is reduced. This kind of approach would predict an anti-correlation between the size of the recalibration effect and both the temporal spread of the adaptation signal and the JND. However, there is no significant anti-rank-correlation of recalibration and temporal accuracy of the adaptation signal ($p = 0.302$), even if there is a possible trend in the predicted direction (**Figure 5**, left). Concerning perceptual precision, rather than the predicted anti-correlation, there is a significant rank-correlation of recalibration and JND (Spearman's $\rho = 0.54$, $p = 0.014$), suggesting that precision of one's own estimate impacts negatively on the amount or speed of recalibration (**Figure 5**, right). This suggests a different explanation for the lower recalibration effect size. It is possible that participants with a low JND were better able to detect the temporal discrepancy between the movement event and the visual event, which may have decreased the unity assumption. In this case, the screening for especially precise participants (JND < 150 ms) could explain this weaker recalibration.

Another observation that was not expected was that the recalibration effect in the first session was nearly fully carried over into the pre-test of the second session. **Figure 6** (left) depicts the significant correlation (Pearson's $r = 0.73$; $p = 0.016$) between PSS shift as an effect of training in the first session and the difference between the pre-tests in both sessions. This carry-over effect does not appear to be related to the amount of time

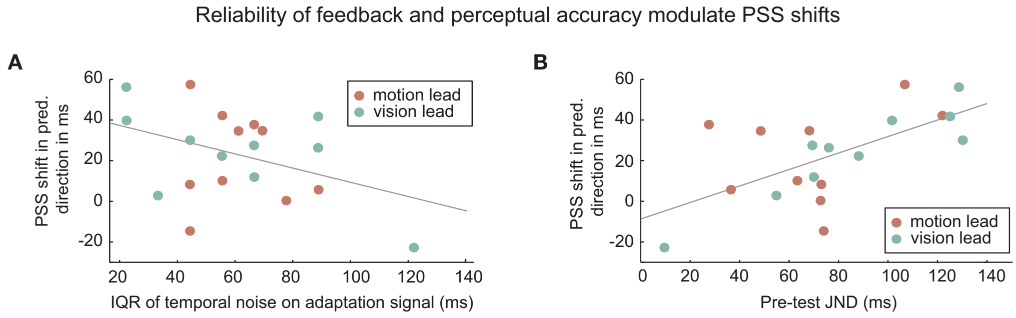

FIGURE 5 | Reliability of feedback and perceptual accuracy modulate PSS shifts. PSS-shifts in the predicted direction (i.e., negative for vision-lead) as a function of (A) temporal accuracy of recalibration signal and (B) perceptual precision (pre-test JND). Grey solid lines are least square regression lines.

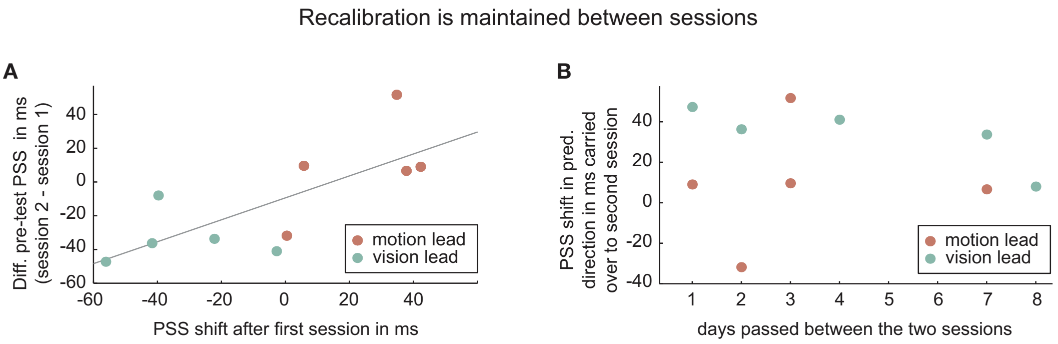

FIGURE 6 | PSS-shifts are maintained across sessions (A). This carry-over of recalibration does not appear to decay over time **(B)**. Grey solid line is least square regression line.

that had passed between the sessions (**Figure 6**, right, rank-correlation $p = 0.528$). The preservation of recalibration across sessions was an unexpected result. We had assumed that, interacting in real time with the real world for at least 24 h, the new temporal relationship learned in the training would be quickly unlearned. However, these data suggest that the learning is highly context-specific (we used the same setup, experimental room, stimuli, etc. in both sessions) and remains present in our setup despite having had plenty of experience for hours and days with sensory-motor stimuli without temporal delay in the natural world.

A possible concern with this carry-over effect is that adaptation to vision-lead occurs only or predominantly if vision-lead adaptation is performed in the second session, simply as a return to the original state, i.e., that there is no real bi-directionality of the recalibration mechanisms. As **Figure 6A** illustrates, this is not the case. The recalibration in the first session is in all cases in the predicted direction (green dots: left of vertical zero-line; red dots: right of vertical zero-line). Indeed, even analyzing recalibration just in the first session (with five subjects), recalibration in the vision-lead condition is already significant [recalibration vision-lead: -33 ± 8 ms (SEM), $p = 0.024$, $t(4) = 3.5$; recalibration movement-lead: 24 ± 8 ms (SEM), $p = 0.024$, $t(4) = 2.8$].

DISCUSSION

We found that humans recalibrate their perception of visuo-motor simultaneity both to vision-lead and to movement-lead discrepancies, despite the causal asymmetry that the voluntary and intentional finger movement introduces into the scenario investigated (i.e., causes precede possible effects). As a consequence of this asymmetry, the temporal recalibration to movement-lead discrepancies found here and reported by other groups (Stetson et al., 2006; Heron et al., 2009; Sugano et al., 2010) has the counter-intuitive implication that a very fast visual feedback event may, after training, be perceived to precede the movement event that caused it (Stetson et al., 2006; Heron et al., 2009). A similar finding on a perceived temporal reversal of cause and effect has been anecdotally reported in a study on adaptation to visual feedback delays in a motor control task (Cunningham et al., 2001). The experiment presented here, by contrast, also studies the inverse scenario, i.e., adaptation to vision-lead temporal discrepancies. By analogy, a visual event that really occurs shortly before the button press, starts off as with said violation of temporal order of cause and effect and may, after adaptation to vision-lead discrepancies, be interpreted as sensory feedback causally linked to and following up to the intentional action. We found that, despite the causally asymmetrical starting conditions, the mechanisms of temporal recalibration operate in both directions. The results do not give any hint that there is an asymmetry of recalibration around the point of actual visuo-motor simultaneity.

An inherent problem in the study of perceived visuo-motor simultaneity with intentional action is the necessity to present stimuli with vision-lead SOAs. In order to generate comparison stimuli or training stimuli that precede an intentional action, the timing of this action has to be predicted (cf. Stetson et al., 2006; Heron et al., 2009; Sugano et al., 2010; Arnold et al., 2012). Here we present a new method for the presentation of visual stimuli before an intentional action: by recording early movement onset and analyzing it online, the time of a button press can be predicted. As evident from the results, this prediction method is sufficiently accurate to provide visual feedback for recalibration studies (cf. **Figure 2**). The method presented here does not involve a perceptible lead event such as an external cue (Stetson et al., 2006) or a previous action (Stetson et al., 2006; Heron et al., 2009; Sugano et al., 2010) that could potentially bias a participant's perceptual judgments. However, even if there is no clearly perceptible lead event in the current experiment, the fact that events happen reliably before a self-initiated action potentially harbors the possibility that participants derive the existence of a non-perceptible lead event that could trigger visual lead stimuli, such as a change in the sensitivity of the button. In the current study, we did not explicitly measure the perceived causal or intentional binding. Therefore, it remains an open question whether the symmetry of recalibration is preceded and catalyzed, accompanied or followed by an analogous change in causal or intentional binding. Further experiments will be necessary to elucidate the link between intentional binding and temporal recalibration of perceived visuo-motor simultaneity.

It should be pointed out that what we and others refer to as visuo-motor temporal recalibration really involves a number of senses. A voluntary movement usually involves at the very least a motor signal (i.e., an efference copy) and proprioceptive feedback. Additionally, given that a button press provides haptic feedback, the tactile sense may play a role in the reported recalibration, given that the visual stimulus is shifted relative to all the mentioned senses. It is unlikely that the recalibration observed in this kind of visuo-motor recalibration paradigm is only due to visuo-tactile or visuo-proprioceptive recalibration. The effect size reported for visuo-motor recalibration (23 ms here; 30–44 ms in Stetson et al., 2006; Heron et al., 2009; Sugano et al., 2010) is much larger than that reported for visuo-tactile only recalibration (12.5 ms; Keetels and Vroomen, 2008) or for visuo-tactile-proprioceptive recalibration (16 ms; Stetson et al., 2006; effect approaching significance). However, it cannot be ruled out that visuo-proprioceptive or visuo-tactile recalibration play a role in the visuo-motor recalibration reported here and it may, therefore, have been more appropriate to use the term "visuo-somatosensory," referring to the whole complex of non-visual senses involved.

The recalibration we found was preserved between measurement sessions (i.e., across several days). This was an unexpected result. We had assumed that participants would quickly readjust their mapping of visual and motor stimuli after our experiment terminates; interacting with the real world in real time should counter the adaptation experienced in the setup. This unexpected finding suggests that the kind of recalibration observed is specific to the task or device and would likely not transfer to other devices. This is not the only case in which recalibration of perception and action appeared to be highly context-specific and long lasting (e.g., Ernst et al., 2000). High context-specificity may be more common than one would assume in this kind of paradigm.

We also found that good perceptual precision (low JND) appears to decrease the strength of recalibration. This observation is inconsistent with predictions of a Kalman filter model of recalibration. For instance, Burge et al. (2008) found that a decrease of precision of a feedback signal (higher measurement noise) slows down adaptation and could model these effects with a statistically optimal Kalman filter. Such a model would predict the opposite effect that we observe here, i.e., that good precision (low JND) would increase recalibration. It is more likely that participants with low JND recalibrated less because they were able to detect the temporal discrepancy between the timing of the visual stimulus and that of the movement event. The screening for participants with good perceptual precision (JND < 150 ms) means that the participant population tested is more reliable in their perception of time than a random sample of the population, which could thus explain why the amount by which participants' PSS shifted as a result of recalibration was lower than that reported in previous studies (Stetson et al., 2006; Heron et al., 2009; Sugano et al., 2010).

In conclusion, we found that humans recalibrate their perception of simultaneity of a voluntary action and vision both if the visual event leads and if it lags. A number of factors (session order, perceptual accuracy) appear to modulate the recalibration process. Surprisingly though, there is no evidence that the direction of the temporal discrepancy (vision-lead or vision-lag) is one of them, despite the causal asymmetry that suggests a weaker intentional or causal binding and thus a weaker unity assumption in the vision-lead condition. The mechanisms of temporal recalibration in visuo-motor simultaneity perception (e.g., Stetson et al., 2006; Heron et al., 2009; Sugano et al., 2010; Keetels and Vroomen, 2012; Sugano and Vroomen, 2012) appear to work both forward and backward in time and the data presented here suggests that recalibration may even be symmetrical around the actual point of simultaneity.

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Motor-sensory recalibration modulates perceived simultaneity of cross-modal events at different distances

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A popular model for the representation of time in the brain posits the existence of a single, central-clock. In that framework, temporal distortions in perception are explained by contracting or expanding time over a given interval. We here present evidence for an alternative account, one which proposes multiple independent timelines coexisting within the brain and stresses the importance of motor predictions and causal inferences in constructing our temporal representation of the world. Participants judged the simultaneity of a beep and flash coming from a single source at different distances. The beep was always presented at a constant delay after a motor action, while the flash occurred at a variable delay. Independent shifts in the implied timing of the auditory stimulus toward the motor action (but not the visual stimulus) provided evidence against a central-clock model. Additionally, the hypothesis that the time between action and delayed effect is compressed (known as intentional binding) seems unable to explain our results: firstly, because actions and effects can perceptually reverse, and secondly because the recalibration of simultaneity remains even after the participant's intentional actions are no longer present. Contrary to previous reports, we also find that participants are unable to use distance cues to compensate for the relatively slower speed of sound when audio-visual events are presented in depth. When a motor act is used to control the distal event, however, adaptation to the delayed auditory signal occurs and subjective cross-sensory synchrony is maintained. These results support the hypothesis that perceptual timing derives from and is calibrated by our motor interactions with the world.

Keywords: time perception, motor-sensory recalibration, intentional binding, simultaneity, temporal order

INTRODUCTION

Brains collect information about the external world through a variety of sensory systems. However, due to differences in transmission velocities, neural architecture, and processing demands, these incoming sensory signals become centrally available to the brain at different points in time (Andreassi and Greco, 1975; Allison et al., 1977; King and Palmer, 1985; Meredith et al., 1987; Spence and Squire, 2003; King, 2005; Eagleman, 2008). The discrepancies in processing times, occurring on the range of tens of milliseconds, have real-world implications. For example, when sprinters line up for the beginning of a race, a gunshot rather than a visual event is used to signal the start of competition. Although sound waves travel much slower than light, auditory information is processed more rapidly in the brain. Sprinters can thus react much faster to a bang than a flash. This behavioral fact has been known for well over a century (Wundt, 1874), and in recent decades has been corroborated by our knowledge of human physiology: the cells in our auditory cortex can change their firing rate more quickly in response to a sound than the visual cortex cells can respond to a light (King and Palmer, 1985).

But comparing the physiology to perception leads to a paradox. While the sprinter can react at different speeds to the incoming sensory information, perceptually the flash and the bang of the pistol seem to occur at the same time. Even more striking, for the official pulling the trigger, the action itself, and even the decision to act, will also appear synchronous with the sight and sound of the gunshot. The volitional and motor signals, generated far in advance of the sensory effects, are brought into perceptual alignment to produce a unified and coherent temporal experience. This fact is all the more perplexing given that humans are capable of detecting differences in timing as small as 2 ms (Wertheimer, 1912; Hirsh and Sherrick, 1961; Westheimer and McKee, 1977), far below the relevant sensory processing delays. What accounts for the sleight of hand that allows perception to rewrite the timing of its outgoing motor acts and incoming sensory feedback?

In interactions with the world, one of the fundamental challenges animals face, crucial both for learning and survival, is that of determining causality (Michotte, 1963; Waldmann and Holyoak, 1992; Buehner and Cheng, 1997; Pearl, 2000; Scholl and Tremoulet, 2000; Eagleman and Holcombe, 2002; Schulz and Gopnik, 2004; Griffiths and Tenenbaum, 2005; Sloman, 2005; Stetson et al., 2006; Körding et al., 2007). At its most fundamental level, causality requires regularity in temporal order judgments; concluding that event B consistently followed action A. Correctly judging the order of action and sensation, however, is not an easy task for the brain to solve, in part because sensory-motor delays are constantly shifting in relation to one another. For example, as limbs grow throughout development, more time is required for motor commands to travel out and for sensory data to return (Campbell et al., 1981; Alison et al., 1983). Changing lighting conditions, such as entering a dimly lit room, cause signals from the retina to be delayed by up to 100 ms (Matteson, 1971; Purpura et al., 1990). Different acoustic environments can modulate the perceived arrival time of sounds (Kinsler et al., 2000). To account for these changing latencies and ensure proper judgments of causality, the brain must be able to dynamically adjust its expectations about the temporal relationship between motor output and incoming sensations (Stetson et al., 2006; Eagleman, 2008).

While it is clear that it would be useful to calibrate the timing of motor acts and sensory feedback, the mechanism by which this is accomplished is not well understood. How would the nervous system know exactly when to calibrate and under what conditions? One proposal is that organisms calibrate time perception through their motor interactions with the world (Stetson et al., 2006; Eagleman, 2008). This notion has related roots in the literature on spatial vision (Welch, 1978; Bedford, 1999; O'Regan and Noe, 2001), but has only recently been explored in relation to time. In spatial vision, for example, when participants wear left-right inverting prism glasses, their vision is highly distorted and they are unable to interact appropriately with the world. Objects on the left now appear on the right. However, if the participant is allowed to interact with the world (reach out and touch objects), he adapts such that the object on the left now appears to be on the left again (Kohler, 1951; Welch, 1978; Redding et al., 1992; Redding and Wallace, 2002). In other words, the brain can send motor actions out into the world and use the feedback to calibrate perceptual interpretations of the world. This calibration of vision allows the brain to maintain accurate judgments in varied and varying environments.

Analogously, in our framework, an animal can send out a motor action (say, snapping one's fingers) and analyze the returning sensations (the resulting feel, sight, and sound) to calibrate the timing of different modalities. If the animal's brain were to employ the simple prior expectation that sensations should follow actions without delay, then any sensation arriving at a delay could be brought into temporal alignment. For example if finger-snaps were consistently followed immediately by the feel and sight of the fingers, but the "click" sound came 100 ms later, auditory processing could be adjusted until the click was perceived as synchronous with the other modalities. The temporal alignment of modalities can subsequently be useful during passive viewing of the world.

Crucial to this equation will be an animal's ability to recognize which changes in the world it is authoring. A "click" sound unrelated to the fingers, but instead indicative of a separate event, does not need to be brought into temporal alignment. According to several influential theories of motor control, it is our ability to monitor self-generated actions that allows us to distinguish the sensory consequences of our own actions from externally produced sensory stimuli (von Holst, 1954; Jordan and Rumelhart, 1992; Jeannerod, 1997; Wolpert and Ghahramani, 2000). This monitoring is carried out by a predictive forward model that can anticipate and identify the sensory consequences of our own movements. A comparison between predicted and actual sensory feedback, carried out by a central monitor (Frith, 1992), is what then allows us to recognize motor actions as our own. Identifying that the delayed "click" has the anticipated sound of fingers being snapped is what licenses the brain to claim authorship and shift the perceived time of the sound closer to the causally related touch, sight, and action.

As the philosopher David Hume pointed out, making these types of causal inferences relies upon three empirical cues: temporal priority, constant conjunction, and contiguity in space and time (Hume, 1748). Experimental results have suggested that the manipulation of any of these cues can profoundly alter the consequent perceptual experience. For example, precise predictions about the tactile feedback, both in time and space, are what prevent us from having the capacity to tickle ourselves (Weiskrantz et al., 1971). However, if the predictability is manipulated, for example by injecting a temporal delay between the motor action and the end effect, participants can be fooled into thinking that another person is tickling them (Blakemore et al., 1999). Ratings of the intensity of the ticklish sensation vary as a function of the ability of the motor command to predict precisely the spatial and temporal position of the resulting sensory feedback (Blakemore et al., 2000).

Consistent with the hypothesis of recalibration in the time domain, a rich body of experimental work has recently demonstrated that the perceived duration between a repeated voluntary action (pressing a key) and a delayed sensory effect (e.g., seeing a flash or hearing a beep) is contracted (for reviews see Buehner, 2010; Moore and Obhi, 2012). Two competing hypothesis have arisen to explain these results. The first, intentional binding, proposes that the brain "contains a specific cognitive module that binds intentional actions to their effects to construct a coherent conscious experience of our own agency" (Haggard et al., 2002). In this framework, sensory effects are subjectively "pulled" toward intentions, such that all sensations following voluntary action appear closer together in time to the actions (**Figure 1A**). This "binding" putatively results from a compression of the perceived time between action and sensation, typically explained by variations in the rate of pacing signals from an internal clock mechanism (Wenke and Haggard, 2009). The durations between action and sensation appear shorter, on this account, because fewer clock ticks accumulate during a given interval. Crucially, slowing of an internal clock depends on a close association between a participant's intentions and the resulting sensory feedback (Haggard et al., 2002; Haggard and Clark, 2003; Wohlschlager et al., 2003; Engbert and Wohlschläger, 2007; Engbert et al., 2008; Moore and Haggard, 2008).

The second account suggests that because of uncertainty (i.e., measurement noise) associated with temporal judgments, estimates of causally related events are more likely to be judged close in time and space than unrelated events (Eagleman and Holcombe, 2002; Stetson et al., 2006; Buehner and Humphreys, 2009, 2010; Buehner, 2010, 2012). This theory accords with the results from intentional binding [e.g., people are most confident that events caused by themselves are causally related (Stetson et al., 2006)], but also predicts attraction for causally linked events outside of one's own control (Eagleman and Holcombe, 2002). Recent experimental work has supported that hypothesis, showing timing shifts when observing another person perform a causal action (Wohlschlager et al., 2003; Engbert and Wohlschläger, 2007), for joint causal actions (Strother et al., 2010; Obhi and Hall, 2011), for

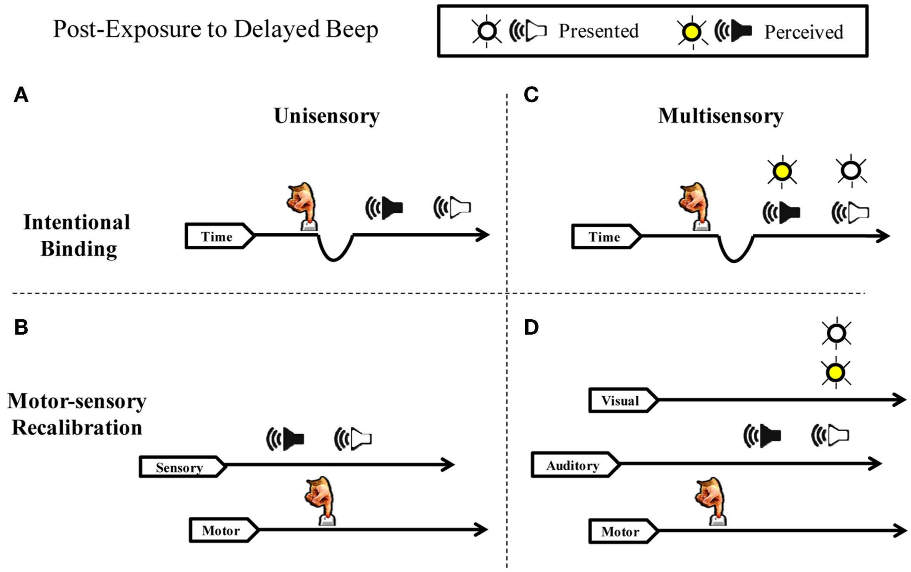

FIGURE 1 | Intentional binding vs. motor-sensory recalibration. Schematic illustration of perceived timing following adaptation to a repeated voluntary action and a delayed auditory effect. **(A)** Intentional Binding suggests sensory consequences are pulled toward intentions and that the phenomenon is explained by the slowing of an internal clock. Delayed auditory effects are drawn closer to actions because subjective time contracts. **(B)** Motor-sensory Recalibration proposes multiple independent timelines and highlights the flexibility and uncertainty inherent in a causal understanding of the world. The theory predicts an illusory reversal of action and effect in the unisensory case. **(C)** If subjective duration contracts between actions and effects, simultaneously presented auditory and visual stimuli should shift in unison toward the action. Therefore, simultaneity judgments between the beep and the flash should not change in relation to one another. **(D)** Contrary to the intentional binding model, motor-sensory recalibration predicts an *independent* auditory temporal shift in multi-sensory timing. The timing of the flash does not shift because it is not presented at a predictable delay, but instead varies in time.

intra-sensory and cross-sensory causally linked events (Haggard and Clark, 2003; Stetson et al., 2006), for non-intentional mechanical causation (Buehner, 2012), and spatial causal binding when no motor planning or intentional action is present (Buehner and Humphreys, 2010; but see Cravo et al., 2009).

Because of the importance of voluntary actions in causal inferences, our hypothesis stresses an active recalibration of the expected timing relationships between outgoing motor acts and resultant sensory signals (Stetson et al., 2006). In this motor-sensory recalibration hypothesis, timing expectations in different modalities (e.g., sensory and motor systems) can shift in relation to one another. In other words, the expectations of how long an action should take to go out, and the expectation of how long sensory feedback should take to return, undergoes dynamic adjustment based on interaction with the world. The injection of a delay violates the expectation that causally related sensory events should occur without delay, and therefore the timing of the system shifts. Stetson et al. (2006) illustrated a striking prediction of this theory: after adaptation to a delay between a button press and flash, a flash presented immediately (and unexpectedly) after a button press will seem to occur *before* the action itself (**Figure 1B**). It is crucial to note this illusory reversal of action and sensation is incompatible with the intentional binding framework: effects "bound" to their intentions would not occur before the intentions themselves; instead, they would merely draw closer together in time. Similarly, it is difficult to see how a clock-rate model could account for a subjective interval turning negative. Despite these concerns, intentional binding and the clock-rate model remain a common interpretation of the phenomenon (see Moore and Obhi, 2012).

In the present study we perform a series of experiments to distinguish between these two hypotheses. Specifically, we test whether recalibration can occur separately and independently along different sensory channels. Studies in this field have generally focused on perceptual timing when a single uni-modal event follows a motor action (Haggard et al., 2002; Haggard and Clark, 2003; Wohlschlager et al., 2003; Stetson et al., 2006; Engbert et al., 2007, 2008; Moore and Haggard, 2008; Heron et al., 2009; Sugano et al., 2010). While there are numerous studies on cross-modal recalibration (Spence and Squire, 2003; Sugita and Suzuki, 2003; Fujisaki et al., 2004; Vroomen et al., 2004; Navarra et al., 2009, 2007; Hanson et al., 2008; Keetels and Vroomen, 2008), only one to our knowledge has examined cross-sensory timing adaptation when a participant's own motor actions are involved (Cravo et al., 2011). We reason that if subjective duration contracts between actions and effects (intentional binding), then simultaneously presented auditory and visual stimuli should shift in unison toward the action (**Figure 1C**), and therefore simultaneity judgments between the beep and the flash should be unchanged in the presence or absence of the motor action. On the other hand, if motor output calibrates timing expectations for vision and audition independently (motor-sensory recalibration model), then these senses will change their perceived timing relationship with each other – but only when a participant's own motor actions trigger the events (**Figure 1D**). To distinguish these outcomes, we had participants judge the simultaneity of audio-visual pairings in active and passive conditions – that is when the participant triggers a beep and flash with a key press, or the computer triggers the events.

Additionally, we had participants make simultaneity judgments at different distances from the stimuli. At distances greater than 30 m, sight and sound appear unsynchronized (when you observe a woodchopper at a distance, the fall of the axe appears to precede the sound) – but an unexplored question is this: if you consistently controlled the distant woodchopper with your own motor action, would that cause the sight and sound to become perceptually synchronized? Note this is a simple but novel paradigm that has no embodiment in the natural world: normally, objects beyond your arms reach (and especially at a distance greater than 30 m) are beyond operant control. In this study we leverage operant interactions with distant objects to unmask how sensory signals are integrated normally; this also allows us to address an unresolved debate concerning how distance cues are utilized in perceptual judgments (Engel and Dougherty, 1971; Stone et al., 2001; Spence and Squire, 2003; Sugita and Suzuki, 2003; Kopinska and Harris, 2004; Lewald and Guski, 2004; Alais and Carlile, 2005; Arnold et al., 2005; Harrar and Harris, 2005; Heron et al., 2007).

MATERIALS AND METHODS

STIMULI

The testing apparatus consisted of a wireless trigger device (transmitter) and a wireless stimulus device (receiver) that triggers an independently timed flash and beep. We call this apparatus the "clapboard," named after the device used in the movie industry to produce simultaneous visual and auditory events for later synchronization. The clapboard's transmitter, which was connected to the testing computer, was responsible for wirelessly sending the stimulus "go" code along with the stimulus parameters on each trial. This was accomplished by a signal sent from the computer to the transmitter (in the *Passive* condition), or by a push-button attached to the transmitter itself (in the *Active* condition).

The clapboard's receiver consisted of a microcontroller capable of wireless transmission. The microcontroller was also connected to a LED light and a speaker (model: Event 20/20BAS, 260 mm × 375 mm × 310 mm), which it was responsible for controlling. Both the microcontroller and LED flash sat atop the speaker. These "real-world" stimuli (i.e., an actual flash and bang at a distance) circumvented a confound inherent in some previous studies (Dixon and Spitz, 1980; Sugita and Suzuki, 2003), in which participants wore headphones while watching a visual display at a distance. That can be a problem because it introduces pollution from a related effect, "spatial ventriloquism" (Spence and Squire, 2003; Zampini et al., 2003).

Each trial consisted of a flash (∼650 cd, 30 ms duration) followed or preceded by a beep (80 dB, 550 Hz, 30 ms duration). Following the trigger signal, the beep arrived at the participant's ears at a fixed delay of ∼210 ms (200 ms + wireless latency, taking into account the speed of sound as a function of stimulus distance). The wireless turnaround transmission latency (from button press to triggering the stimulus) was <15 ms (<8 ms one-way). The flash occurred from 200 ms before (referred to as "−200" ms) to 200 ms after the beep in 50 ms increments (nine possible stimulus combinations; **Figure 2A**).

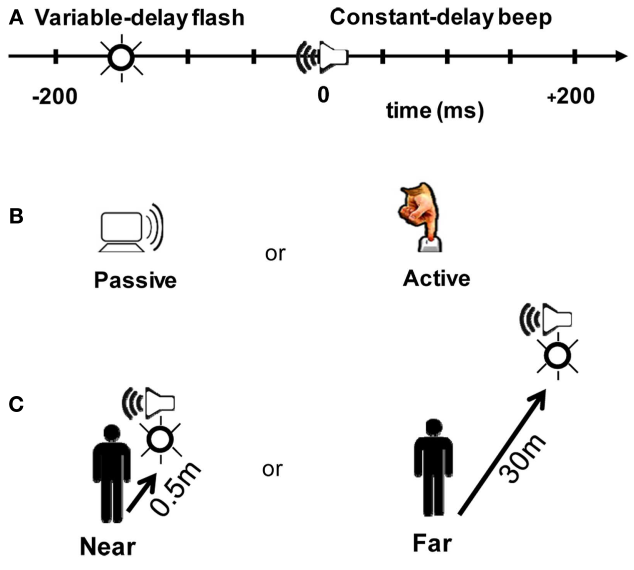

FIGURE 2 | Experimental design. (A) Participants judged whether a beep and a flash were simultaneous or successive. The beep always arrived at the participant ∼210 ms after a trigger signal was received at the "clapboard," a device which generates a beep and a flash. On a given trial, a flash could occur within a 200 ms window around the beep at nine possible offsets (multiples of 50 ms). Zero millisecond corresponds to when the beep and the flash physically arrived at the participant at the same time. **(B)** The beep/flash pair was either presented randomly in a 4 s window after the last response (Passive), or triggered by the participant by a button press (Active). **(C)** The beep and the flash were presented at two different distance conditions: Near (clapboard receiver located 0.5 m from participant) or Far (30 m from participant). Stimulus arrival times at the participant were held constant in the Near and Far conditions (i.e., the slower speed of sound was taken into account so that offsets represent the physical arrival time of the signals at the participant).

PROCEDURE

Fifteen trials were recorded at each offset pairing, yielding a total of 135 stimulus presentations per block. The ordering of trials was randomized for each participant.

Participants ran both a Passive and an Active block (**Figure 2B**). In the Passive block, the beep and the flash occurred randomly within a 4 s window following a participant's answer from the previous trial. In the Active block, participants triggered an event using a push-button that wirelessly transmitted a signal to the clapboard. Immediately following the cross-modal event, participants judged whether the beep and the flash were simultaneous or successive by recording their response on a keypad. The distribution of the relative timing between the flash and the beep were identical in the two blocks – the only difference is that the flash/beep was a direct result of the participant's motor act in the operant (Active) case.

We also tested two distance conditions. In the Near condition (0.5 m, **Figure 2C**), participants were seated in a psychophysical testing room. Light levels were normalized to match the luminance of a lit corridor used in the Far condition (30 m, **Figure 2C**). The corridor in the Far condition afforded abundant visual and auditory cues for estimating distance. The flash luminance was

~650 cd; both the perceived luminance and size of the flash were matched between the two distance conditions. Sound volume was also matched to ~80 dB.

Before running the experiment, participants were required to pass a training version of the Passive task. They then completed a *Passive* block of trials (Block 1), followed by an *Active* block second. We fixed this order of presentation because our initial pilot experiments demonstrated that motor-sensory timing recalibration from an *Active* block can carry over for tens of trials into a subsequent *Passive* block. This aftereffect will be demonstrated and quantified by our experiments below, in which we had a subset of participants complete a third block, this time in the *Passive* condition (**Figure 5**). That third block will allow us to independently investigate the persistence of aftereffects in the absence of action.

PARTICIPANTS

At each distance condition [Near (0.5 m) or Far (30 m)], a set of 18 participants participated in both Passive and Active blocks (**Figure 2**). Six of the participants participated at both distance conditions. Additionally, 10 of the participants from the Near condition completed a third block (Passive) to assess the persistence of adaptation effects (**Figure 5**). Participants were between the ages of 18 and 45 with normal or corrected-to-normal vision and no known hearing loss. All participants consented to the study as approved by the Institutional Review Board at Baylor College of Medicine, and were compensated for taking part in the experiment.

RESULTS

MOTOR-SENSORY RECALIBRATION

Shift in the auditory timeline

Using simultaneity judgments as a measure of cross-modal timing, we found a replication of the phenomenon of motor-sensory recalibration: a beep occurring at a predicable delay of 210 ms after a motor action was perceived as occurring earlier in time (**Figures 3A,B**). Shifts in participants' points of subjective simultaneity (PSS) between the Active and Passive conditions were −18 ms [$t(17) = -3.50, p < 0.01$] in the Near location and −25 ms [$t(17) = -4.04, p < 0.01$] in the Far location. These are comparable to what has been observed in previous experiments with single modality events following motor actions (Haggard et al., 2002; Stetson et al., 2006) and parallel the results obtained in a recent study of motor-triggered cross-sensory timing (Cravo et al., 2011). However, we note that methodological differences between the Cravo et al. (2011) paper and our study, including explicit adaptation to the action event (instead of our implicit method), potential aftereffects induced by mixed ordering of conditions (instead of our fixed ordering), and a longer interval between action and sensory consequences (300 vs. 210 ms, *personal communication*), may have contributed to differences in the size and nature of the effect across the two studies.

Notably, the shifts in our experiment were not accompanied by a significant difference between the number of simultaneity judgments made in the Active and Passive conditions (**Figures 3A,B** *inset*). Simultaneity judgments, while limiting the effects of response bias, are susceptible to changes in criterion for what is classified as simultaneous (Zampini et al., 2005; van Eijk et al., 2008; Spence, 2010). Because the effect in our experiment is primarily a lateral shift in the curve (i.e., total simultaneity judgments did not change), a criterion bias cannot explain these results.

Increased simultaneity immediately following the motor act

Although we have made the argument that a shift of the auditory timeline best explains our findings (in other words, recalibrated expectations of the timing of the beep), we also noted that in the 150 ms immediately following action, changes in simultaneity judgments between the Passive and Active conditions were much larger than those in the corresponding last three offsets (note the larger separation of the *Passive* and *Active* curves on the left side vs. the right side; **Figures 3A,B**). This led us to reason that for equally large offsets between flash and beep (e.g., −150 or +150 ms), proximity to the motor act may have influenced the perception of simultaneity of the two sensory events. We now turn to two possible explanations for this asymmetry.

Many studies of motor-sensory recalibration have shown that the strength of the shift between action and effect dissipates with longer delays between action and feedback (Eagleman and Holcombe, 2002; Haggard et al., 2002; Stetson et al., 2006; Heron et al., 2009; Cravo et al., 2011; Arnold et al., 2012; but see Humphreys and Buehner, 2009). Relatedly, Wenke and Haggard (2009) have provided evidence that participants are more likely to judge two tactile events as simultaneous when they are presented within a 150 ms window after a key press; there is no effect for events occurring later than this window. According to Wenke and Haggard (2009), recalibration models cannot account for this data; instead, it is viewed as evidence in favor of a clock-rate model. In their view, intentional actions "transiently slow down an internal clock" and "two shocks are thus more likely to fall within a single clock period, impairing temporal discrimination" (Wenke and Haggard, 2009). We suggest an alternative interpretation of these results that both accords with the motor-sensory recalibration framework and is supported by our present data in **Figure 3**.

In addition to having a prior expectation that the sensory consequences of actions should occur without delay (Stetson et al., 2006), we hypothesize that the perceptual system also interprets events occurring at short delays after an action as sensory consequences of the action (Hume, 1748; Eagleman and Holcombe, 2002). Moreover, if participants believe that two sensory events originate from a common source, they are more likely to perceive those events as simultaneous with one another (Zampini et al., 2005; van Wassenhove et al., 2007; Stevenson et al., 2012) – in the context of the current study, the common source is their own action. Therefore, we hypothesize that two sensory events closely following a motor act are more likely to be interpreted as (1) caused by the agent, and (2) simultaneous with one another.

Studies of intentional binding have suggested that changes in the timing of sensory events are driven by both a predictive motor component (Haggard et al., 2002; Stetson et al., 2006) and a postdictive inferential mechanism (Moore and Haggard, 2008; Buehner, 2010). These two information sources both contribute to conscious awareness and appear to be weighted in a Bayesian manner according to their reliability. In our experiment, relatively higher rates of simultaneity were observed when the beep and

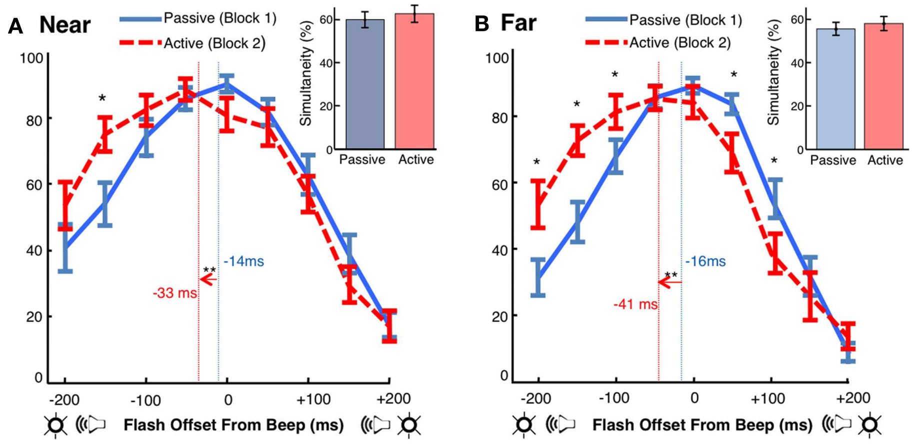

FIGURE 3 | Motor-sensory recalibration. (A) In the Near condition, the point of subjective simultaneity (PSS, calculated as the center of mass of the simultaneity curves) was −14 ms ± 4 (Passive) and −33 ms ± 5 (Active). The auditory stimulus was perceived as occurring earlier in time in the Active condition by approximately 18 ms [$t(17) = -3.50$, $p < 0.01$]. The difference in the number of simultaneity judgments between conditions was not significant [$t(17) = 0.84$, $p = 0.41$] **(B)** In the Far condition, the PSS was −16 ms ± 4 (Passive) and −41 ms ± 7 (Active). The auditory stimulus was perceived as occurring earlier in time in the Active condition by approximately 25 ms [$t(17) = -4.04$, $p < 0.01$]. The difference in the number of simultaneity judgments between conditions was not significant [$t(17) = 1.11$, $p = 0.28$].

the flash occurred in close proximity to the motor act, paralleling the findings of Wenke and Haggard (2009). Due to its unpredictability, the flash was presumably not subject to motor-sensory shifts (Cravo et al., 2011). Rather, we propose that when the flash occurred shortly after the motor act, postdictive inferential mechanisms linked both sensory events to the action, thereby leading to increased simultaneity judgments. The longer the delay between a button press and sensory feedback, the less likely the brain is to claim authorship over the event and judge the two events as simultaneous (Eagleman and Holcombe, 2002).

SIMULTANEITY CONSTANCY

No compensation for distance from the participant

Contrary to previous reports (Sugita and Suzuki, 2003; Kopinska and Harris, 2004; Alais and Carlile, 2005) we find no evidence for compensation of auditory travel time when stimuli are presented at different distances. Expressed as arrival time at the participant's sensory organs, the PSS was −14 ms in the Near Passive condition and −33 ms in the Near Active condition (**Figure 4A**). If participants were able to judge the timing of the events as they are leaving the source, the PSS should have shifted to the left in both of the Far conditions by approximately 87 ms (sound takes ~87 ms to travel 30 m). Thus, compensation for distance-induced auditory delays would have predicted a PSS of −101 ms (−14–87 ms) in the Far Passive condition, and −120 ms in the (−33–87 ms) in the Far Active condition. Instead, we found that stimulus travel times map nearly perfectly onto perceptual time. We will return to this point below, in the Discussion.

Does uncertainty in source localization decrease judgments of simultaneity?

Previous studies have suggested that localization and synchrony judgments are dependent on the spatial and temporal properties of the stimulus (Bertelson and Aschersleben, 2003; Hairston et al., 2003; Alais and Burr, 2004; Bertelson and de Gelder, 2004; Zampini et al., 2005; Körding et al., 2007; Shams and Beierholm, 2010; Heron et al., 2012; Stevenson et al., 2012). In our results a relatively higher (although non-significant) number of overall simultaneity judgments were made when participants were seated close to the stimuli (**Figure 4B**). Although no explicit measures of localization were recorded, we suspect this difference occurred because participants had abundant auditory and visual cues with which to localize the sensory events in the Near condition. They could thus be certain that both the beep and the flash were emanating from the same source (Spence et al., 2003; Gepshtein et al., 2005; Körding et al., 2007; van Wassenhove et al., 2007; Shams and Beierholm, 2010). The greater distance of 30 m may have increased the uncertainty associated with participants' localization judgments. While studies have shown contraction of spatial locations for causally related events (Buehner and Humphreys, 2010), the influence of distance on judgments of causality (and hence simultaneity) is a largely unexplored question and will be investigated in future studies.

PERCEPTUAL AFTEREFFECTS

If brains calibrate time perception primarily through motor interaction with the world (see Introduction) one might expect the effects of adaptation to a fixed delay to persist when that delay is taken away (Cunningham et al., 2001; Kennedy et al., 2009). Indeed, the illusory reversal of action and effect (Stetson et al., 2006) is made possible by just such persistence. However, the way in which temporal judgments are affected when the motor act itself is removed following adaptation in unknown. Studies of adaptation to spatial misalignment (Redding and Wallace, 1993), as well as recalibration with inter-sensory stimuli (Fujisaki et al., 2004), suggest that residual perceptual aftereffects might exist. To address this possibility, we had a subset of our participants ($n = 10$) run

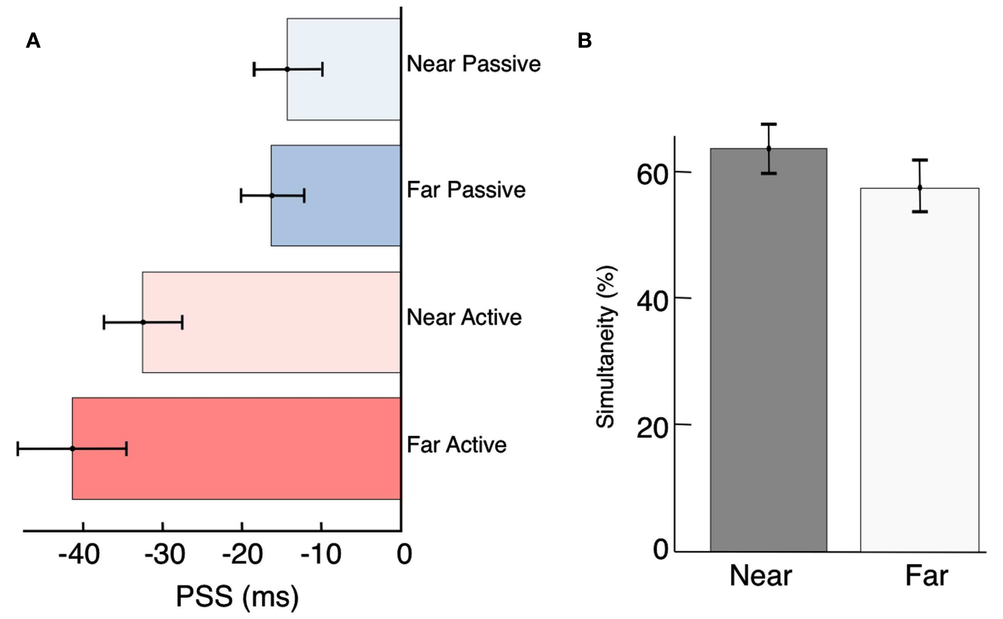

FIGURE 4 | Compensation for source distance? (A) Participant's PSS were not significantly different between the near and far conditions [Passive, $t(34) = -0.36$, $p = 0.72$; and Active, $t(34) = -1.04$, $p = 0.30$]. However, active control over a distal event can compensate for distance-induced auditory delays [$t(17) = -4.04$, $p < 0.01$]. **(B)** Although it was not significant, we did observe a trend toward increased overall simultaneity at the Near location. We speculate that this may be driven by the ease of source localization when the stimuli are presented up close, and could be clarified with a higher sample size in the future.

an additional Passive block (Near condition) following the Active block.

We found that the effects of motor-sensory recalibration remain for ∼35 trials before dissipating (**Figure 5**). The uncoupling from the motor timeline seems to allow the auditory and visual timelines to shift fairly quickly back into alignment. This finding has parallels in the rapid pace at which motor-sensory recalibration establishes itself, reaching full magnitude within ∼20 trials (Stetson et al., 2006). The speeds with which these shifts in timing take place illustrate the central role of causality in our perceptual interpretation of the world. In fact, recent experiments on cross-sensory recalibration have found shifts following exposure to a single presentation of only a few milliseconds (Wozny and Shams, 2011).

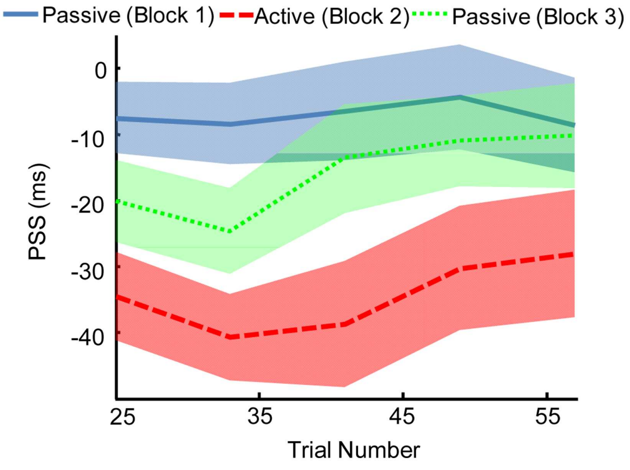

FIGURE 5 | The effect of motor-sensory recalibration persists in a subsequent passive Block. This aftereffect lasts for ∼35–40 trials. Point of subjective simultaneity (PSS) was calculated over a window size of 25 trials, with the window shifting in increments of 8 trials. Width of line indicates SEM.

DISCUSSION

The results of our experiments yield three insights. First, the shift in the timing of the auditory stimulus in relation to the visual stimulus contradicts previous explanations of intentional binding, particularly a clock-rate model, and instead supports the hypothesis of multiple coexisting timelines in the brain. Second, motor-sensory recalibration seems to be driven by both predictive motor signals and postdictive inferential mechanisms. Because of constantly changing neural delays and the critical importance of uncovering causal relationships, the brain utilizes a flexible and adaptive mechanism, rather than simple neural latencies, to construct the timing of events. Third, changes in observer-stimulus distance, resulting in differences in the arrival time of sight and sound, are not taken into account when participants make simultaneity judgments. However, active control over a distal event can result in compensation for the slower speed of sound.

In addition, we tentatively suggest that differences in source localization between the Near and Far conditions may have affected the size of the window that people use to make judgments of synchrony.

TIME TO THROW OUT THE CLOCK?

For over 50 years the dominant paradigm in time perception research has posited the existence of a single, central-clock responsible for constructing a single temporal representation of the outside world (Creelman, 1962; Treisman, 1963; Allan and Kristofferson, 1974; Gibbon et al., 1984). Distortions in duration and timing are accounted for by increasing or decreasing the amount of "ticks" that accumulate during a given interval. Despite an absence of physiological evidence, a majority of findings in the field still rely on this putative clock to explain their results (Wittmann, 1999; Hodinott-Hill et al., 2002; Tse et al., 2004; Morrone et al., 2005; Kanai et al., 2006; Wearden, 2008; New and Scholl, 2009; Wencil et al., 2010). In the intentional binding literature, performing an action is said to "slow down an internal clock, in anticipation of the effect of the action" (Wenke and Haggard, 2009). In other words, because of fewer clock cycles, the interval between action and effects becomes compressed.

Such a model is incapable of explaining the results of our experiment. If a compression of the interval separating action and effect were responsible for the shift in motor-sensory timing, the beep and the flash would both have shifted toward the motor act by the same amount (**Figure 1C**), perhaps resulting in more synchrony in the Active condition. That is, a single slowed clock would affect both sensory consequences equally. Instead, we found that different sensory modalities were able to shift individually in relation to the motor act, and the propensity of participants to make judgments of simultaneity was unaltered between the Passive and Active conditions. Replicating previous studies (Haggard et al., 2002), an auditory stimulus occurring at a predictable delay was perceived as occurring closer in time to the action which caused it. This shift occurred, however, without a concomitant change in the timing of the paired visual stimulus. Rather than time itself being stretched or shrunk, the sensory signals themselves were realigned in subjective time. We also suggest that such a process operates implicitly and below the level of awareness. Participants questioned after the Active block reported being unaware of which stimuli was occurring at a constant delay.

We propose that our results are best explained by an appeal to multiple representations of time that coexist within the brain. Trapped by the assumption of a Cartesian theater in which sensory input is passively recorded (Dennett and Kinsbourne, 1992), modern theories of brain time have largely avoided this framework. Mounting evidence, however, suggests that a single clock-rate model of perceptual time is untenable (Eagleman, 2008). Instead, different aspects of time appear to be underpinned by separate neural mechanisms that sometimes act in concert, but are not required to do so (Eagleman and Pariyadath, 2009).

Previous work has provided compelling evidence for the existence of independent motor and sensory timelines in the brain (Ivry, 1996; Ivry and Richardson, 2002; Stetson et al., 2006; Arnold and Yarrow, 2011). The current experiment extends these findings and shows that individual sensory modalities have their own adjustable timelines. If each sense calibrates against motor acts, this calibrates them in relation to each other as well. Neither the illusory reversal of action and effect (Stetson et al., 2006), nor the sensory specific modulation of cross-modal simultaneity observed here can be explained by a clock-rate model. In light of evidence from other labs (Westheimer, 1999; Yarrow et al., 2004; Morrone et al., 2005; Johnston et al., 2006; Burr et al., 2007; van Wassenhove et al., 2008; Alais and Cass, 2010; Marinovic and Arnold, 2012), we suggest that a paradigm shift is underway within the field of time perception. Discarding the notion of a single central timer allows for novel frameworks and predictions (Westheimer, 1999; Körding et al., 2007; Ivry and Schlerf, 2008; Buhusi and Meck, 2009; Buonomano and Maass, 2009; Eagleman and Pariyadath, 2009; van Wassenhove, 2009; Johnston, 2010; Ahrens and Sahani, 2011; Cai et al., 2012; Liverence and Scholl, 2012; Pacer and Griffiths, 2012) that will force us to think critically about what it means for time to be represented in the brain.

ACTIONS CALIBRATE TIME PERCEPTION

Experiments have suggested that our experience of the temporal properties of an event are a result of both predictive (Stetson et al., 2006) and postdictive or inferential mechanisms (Eagleman and Sejnowski, 2000). Retrospective awareness has been reported for both sensory (Choi and Scholl, 2006) and motor (Moore and Haggard, 2008) events and seems to operate over a window 250 ms into the future (Moore et al., 2009). In our experiment, the largest shifts between Passive and Active blocks occurred in the 150 ms immediately following the action. A similar effect, increased simultaneity judgments of two tactile stimuli in a window 150 ms after an action, has been taken as decisive evidence against the motor-sensory recalibration model (Wenke and Haggard, 2009). The authors assume that recalibration only affects when in time events occur, as opposed to affecting the judgment criteria for synchrony. That explanation ignores the causal component that serves as the foundation of our framework. Instead of a slowed clock, we suggest that when a beep and the flash occur in a brief window after the motor action, the brain becomes more likely to claim authorship over the sensory consequences. Because of a prior assumption that sensory consequences of an action should arrive without delay, events causally related to the action are more likely to be judged as simultaneous. Crucially, the influence of the motor signal is limited by its predictive ability, which decays over time.

We have previously suggested that the brain must continually refine its expectations about the normal temporal relationship between outgoing actions and incoming sensations (Stetson et al., 2006; Eagleman, 2008). In this framework, motor interaction with the world calibrates expectations about the timing of feedback from the different sensory channels. These expectations about sensory timing can subsequently be employed when passively interpreting events in the world (i.e., events that were not self-caused). This theoretical framework explicitly predicts that perceptual aftereffects should be observed even in the absence of action, and this is indeed what we found (**Figure 5**). While these temporal aftereffects are not necessarily inconsistent with an intentional binding model, they are an unambiguous prediction of the motor-sensory recalibration model (Stetson et al., 2006; Eagleman, 2008; Cai et al., 2012). Note that the aftereffects we found only lasted ∼35–40 trials into the Passive block; we hypothesize this could be extended by longer training in the Active condition, and our future experiments will test this prediction. Finally, it is interesting to note that our results appear similar, at least on the surface, to reaching aftereffects observed following exposure to spatial misalignment during prism adaptation (Redding et al., 2005). The links between these two research traditions (recalibration to misalignment in time or in space) has only begun to be investigated (Kennedy et al., 2009; Cai et al., 2012) and more studies are needed to elucidate common principles and interactions.

ACTIONS COMPENSATE FOR DISTANCE-INDUCED AUDITORY DELAYS

Our results present a picture in which active control over a distant audio-visual event can modulate its perceived simultaneity. A person observing fireworks at a distance of 30 m (Far condition) would notice a temporal asynchrony between the bang and flash, due to the slower velocity of sound. If that same observer were given a chance to control the onset of the fireworks however, the bang and flash would be more likely to be perceived as a unitary event. Although this appears to support the hypothesis that brains can compensate for delays in auditory travel times, the mechanism is different from that originally proposed.

Beginning with Engel and Dougherty (1971), several studies have suggested that the brain is able to integrate information about distance (whether visual, auditory, or both) to calibrate simultaneity (Sugita and Suzuki, 2003; Kopinska and Harris, 2004; Alais and Carlile, 2005). The temporal location of an integration window is purportedly actively manipulated by the brain depending on the distance of the visible sound source (Spence and Squire, 2003; Sugita and Suzuki, 2003). The window does not widen in size but rather shifts along a timeline. Some authors have interpreted this as reflecting a perceptual mechanism similar to size constancy, a phenomenon wherein the perceived size of an object is maintained despite variations in the retinal information (Gregory, 1963; Kopinska and Harris, 2004; Harris et al., 2010). Such constancies are common for other perceptual attributes including color, brightness, shape, and location (Palmer, 1999).

Our results conflict with these studies and show that the differential velocities of sound and light map nearly directly onto the perceived timing of audio-visual events (**Figure 4**). Several other experiments have questioned the notion of active compensation for source distance and our results concord with these studies (Stone et al., 2001; Lewald and Guski, 2004; Arnold et al., 2005; Heron et al., 2007). As others have pointed out, such a mechanism would require calculations utilizing absolute distance as well as the speed of sound in different environmental settings (Arnold et al., 2005; Heron et al., 2007). In addition to the computational complexity of such a task, it is not apparent why the brain would want to explicitly represent such variables in the first place. Methodological differences between the studies, including the use of a binary forced choice task (Sugita and Suzuki, 2003; Kopinska and Harris, 2004; Arnold et al., 2005), sound presentations through headphones (Sugita and Suzuki, 2003), requirements to use one's imagination (Sugita and Suzuki, 2003), and a lack of physical distance cues (Alais and Carlile, 2005) may have contributed to biased reporting. In line with Arnold et al. (2005), we interpret studies showing active compensation as likely deriving from cognitive strategies tapping into participant's knowledge about the slower speed of sound in the physical world. The use of simultaneity judgments in our experiment limited the effects of response biases and made any attempt to use a cognitive strategy problematic.

While our results did not provide evidence of compensation for distance-induced auditory delays, we did find differences between the Near and Far conditions in participant's proclivity to make simultaneity judgments (**Figure 4B**). Rather than a moveable window shifting along a timeline (Sugita and Suzuki, 2003) our results point toward an integration period that can expand or shrink depending on various spatial and temporal factors of the stimulus. Previous research has suggested that the impression of a plausible unitary event (Guski and Troje, 2003), driven by the temporal synchrony and spatial coincidence of cross-modal stimuli (Körding et al., 2007; van Wassenhove et al., 2007; Shams and Beierholm, 2010; Stevenson et al., 2012), can lead to higher causality ratings and thus increased simultaneity judgments. Our results suggest that differences in source localization caused by changes in distance may also contribute to the perception of a single causal event. Participants were more likely, in both the Passive and Active cases, to judge audio-visual pairings as simultaneous if they were presented directly in front of them (Near condition). Although the brightness of the flash and the loudness of the beep were matched in the Far condition, participants appeared to be less certain that the audio-visual event was emanating from a single location. No other study, to our knowledge, has reported how differences in distance affect the size of the window for cross-modal integration. Future experiments might investigate the flexibility of our causal perception by independently varying the stimulus distances of simultaneously presented auditory and visual stimuli. Such experiments would contribute to a better understanding of the relative roles of perceptual and cognitive factors in our causal judgments.

CONCLUSION

The conventional framework for understanding temporal perception has focused on how the brain passively registers a feed-forward flow of sensory input. We suggest instead that the timing of events is actively constructed by the brain through disparate mechanisms which can be teased apart with experimentation. Crucial to this construction is the brain's ability to distinguish what changes in the environment it is responsible for causing. Because of the difficulty and importance of making such inferences, timing judgments are flexible and dynamically calibrated in order to keep causality assessments accurate. Our motor actions have a special role to play in modulating the expectations associated with sensory feedback and hence perception. While the influence of motor signals on our spatial representation of the world is well established, contributions to temporal perception are still largely unexplored and warrant further investigation.

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"Cutaneous rabbit" hops toward a light: unimodal and cross-modal causality on the skin

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Our somatosensory system deals with not only spatial but also temporal imprecision, resulting in characteristic spatiotemporal illusions. Repeated rapid stimulation at the wrist, then near the elbow, can create the illusion of touch at intervening locations along the arm (as if a rabbit is hopping along the arm). This is known as the "cutaneous rabbit effect" (CRE). Previous studies have suggested that the CRE involves not only an intrinsic somatotopic representation but also the representation of an extended body schema that includes causality or animacy perception upon the skin. On the other hand, unlike other multi-modal causality couplings, it is possible that the CRE is not affected by concurrent auditory temporal information. The present study examined the effect of a simple visual flash on the CRE, which has both temporal and spatial information. Here, stronger cross-modal causality or correspondence could be provided. We presented three successive tactile stimuli on the inside of a participant's left arm. Stimuli were presented on the wrist, elbow, and midway between the two. Results from our five experimental manipulations suggest that a one-shot flash enhances or attenuates the CRE depending on its congruency with cutaneous rabbit saltation. Our results reflect that (1) our brain interprets successive stimuli on the skin as motion in terms of time and space (unimodal causality) and that (2) the concurrent signals from other modalities provide clues for creating unified representations of this external motion (multi-modal causality) as to the extent that "spatiotemporal" synchronicity among modalities is provided.

Keywords: cutaneous rabbit effect, multi-modal integration, vision, tactile, localization

INTRODUCTION

Our daily lives are rich with information from the physical world. While some limits are imposed by sensorineural imprecision (for a review, see Knill and Richards, 1996), the brain has developed strategies to deal with these limitations, including the utilization of prior knowledge and integration among multi-modal information. A percept that misrepresents physical reality (i.e., an illusion) is both a consequence of and a clue as to the brain's expectations regarding the external world (Goldreich, 2007).

The brain takes advantage of prior knowledge to enhance its perceptual resolution. In the case of tactile perception, spatial imprecision due to low receptor density poses a particular challenge (Goldreich, 2007). Even without the benefit of exploratory movements, the fingertips' resolving power – the most discriminating tactile sensor among primates – is on the order of 1 mm (Weinstein, 1968; Johnson and Phillips, 1981). However, the forearm has less acuity: it resolves detail on the order of 1 cm (Weinstein, 1968). This is the case even though the brain contains a representation of the body map in the primary somatosensory cortex (S1; Penfield and Boldrey, 1937) which reflects the locations of physical stimuli on the skin. Furthermore, given the several-ms jitter in the stimulus-evoked – first-spike latencies of somatosensory cortical neurons (Foffani et al., 2004), the somatosensory system has not only spatial but also temporal imprecision; this results in characteristic spatiotemporal illusions. The "cutaneous rabbit effect" (CRE) might be the best-known of these illusions (Goldreich, 2007). The CRE is a subset of a larger class of tactile saltation illusions elicited when a mechanical stimulus is followed by similar stimuli rapidly applied at nearby locations (Geldard and Sherrick, 1972; Warren et al., 2010). For example, repeated, rapid stimulation at the wrist and then near the elbow can create the illusion of touch at intervening locations along the arm, as if a rabbit is hopping along the arm. The apparent location of each stimulus moves from the actual stimulation site toward the other stimulation sites in a predictable manner depending on factors such as stimulus location and frequency (e.g., Geldard and Sherrick, 1972; Kilgard and Merzenich, 1995; Cholewiak, 1999; Flach and Haggard, 2006).

The CRE is apparently related to the classic tau effect (Goldreich, 2007), in which the more rapidly traversed of two equal distances defined by three stimuli is perceived as being shorter (Helson, 1930). When stimulus timing is held constant, the perceived distance between two stimuli both underestimates and grows in proportion with the actual inter-stimulus distance (Marks et al., 1982; Cholewiak, 1999). In contrast, the kappa effect describes the elevated perceived time between stimuli dilations as the distance between stimuli is increased (Suto, 1952). These effects have been explained on the basis of the hypothetical idea that the sensory system imputes uniform motion to discontinuous dynamic displays; therefore, there is an assumption of constant velocity motion (Jones and Huang, 1982). Also, a recent Bayesian perceptual model replicated the CRE by assuming that the brain expects tactile stimuli to move slowly (Goldreich, 2007) since we have evolved to detect the movement of external agents (Leslie, 1995). The inference that signals have a common underlying cause (in this case, movement) enables us to perceive uniform motion; this is an expression of unimodal causality perception in terms of time and space. A similar argument has been proposed to explain visual motion perception. Certain simple visual displays consisting of moving, 2-D, geometric shapes can give rise to percepts with high-level properties, such as causality and animacy. This suggests that just as the visual system works to recover the physical structure of the world by inferring properties such as 3-D shapes, it also works to recover the causal and social structures of the world by inferring properties such as causality and animacy (Scholl and Tremoulet, 2000).

Multi-modal integration can also assist in circumventing the limits imposed by sensorineural imprecision within each modality. Given that many natural events can be perceived via multiple sensory modalities, we typically have access to multiple features of those events across different senses (Vroomen and Keetels, 2010). It is generally assumed that signals that are congruent among modalities create stronger experiences and richer representations of the world than unimodal signals (for a review, see Woods and Newell, 2004). The ability to combine information from multiple sensory modalities into a single, unified percept is a key element of organisms' abilities to interact with the external world (Stevenson et al., 2011). This process of perceptual fusion – the amalgamation of multiple sensory inputs into a perceptual gestalt – is highly dependent on the temporal synchrony of sensory inputs (Meredith et al., 1987; Bishop and Miller, 2009; Stevenson et al., 2011). The inference that signals have a common underlying cause, and hence merit integration, is often called the "correspondence problem" or "causal inference" (Parise et al., 2012). The combination of cross-modal information by humans is highly consistent with an optimal Bayesian model of causal inference; this phenomenon is known as "cross-modal causality" (Goldreich, 2007; Beierholm et al., 2009; Schutz and Kubovy, 2009). For example, while at the movie theater, we hear voices as coming from the mouths of characters on the screen, not from the actual speakers (i.e., spatial ventriloquism; Jack and Thurlow, 1973; Alais and Burr, 2004). This is because we make causal inferences between vision and audition: "I hear the voice because I, see the character speaking." Another example of cross-modal causality can be observed in a simple visual display consisting of moving, 2-D, geometric shapes. Observers usually attribute the launching of one object to another object that abruptly stops in front of a target object (Michotte, 1963; for review, see Scholl and Tremoulet, 2000). Interestingly, a sound marking the onset of the target motion significantly increases the impression of causality. This facilitation is likely due to the observer's intuitive reasoning that audiovisual stimuli comprise parts of a unitary event (i.e., a collision of two objects producing a bouncing sound; Guski and Troje, 2003). It seems that we prefer to perceive just one (or minimal) cause or agent during multi-modal integration to the extent that temporal synchrony among modalities is provided.

Given the idea that we interpret the outer world through our expectations (where prior knowledge and multi-modal integration is helpful), we might assume that unimodal causality perception (like the tau and kappa effects or the CRE) could be modulated under multi-modal presentation; however, this is controversial (e.g., Flach and Haggard, 2006). Indeed, the tactile tau and kappa effects are also susceptible to cross-modal (visual or auditory) influences (Suto, 1952; Russo and Dellantonio, 1989); other combinations are also possible (e.g., the audiovisual tau effect: Kawabe et al., 2008), indicating the incorporation between unimodal and cross-modal causality perception. Conversely, one previous study has suggested that the CRE is not affected by concurrent auditory temporal information (Flach and Haggard, 2006). In that study, three successive taps were presented on a participant's arm, and the participant localized the second tap. Although three, concurrent auditory tones were presented, no cross-modal interaction within their localization was observed, suggesting that the CRE is the spatiotemporal dynamics of an early, "unimodal" sensory map (Flach and Haggard, 2006). Another study also suggested that the illusory somatosensory percepts caused by the CRE can affect the primary somatosensory cortex at a location corresponding to the illusory percept (Blankenburg et al., 2006). However, another recent study suggested that the CRE could be experienced outside of the body, where it lacks a specific receptive field in S1, indicating that the CRE involves not only intrinsic somatotopic representations but also those of the extended body schema that result from body–object interactions (Miyazaki et al., 2010). In other words, these representations impart expectations regarding the movement of the external agent.

The present study attempted to extend this literature. As far as we know, there is no published paper that has thus far indicated a multi-modal influence on the CRE. We assumed that the CRE could be modulated by cross-modal influence only if concurrent information has enough power to create "causal inferences" among modalities (Parise et al., 2012). In particular, we examined the effect of simple visual flashes on the CRE. The auditory tones used in previous studies have only provided temporal information since the tones were presented through headphones (Flach and Haggard, 2006). A visual flash, however, has both temporal and spatial information, which should elicit cross-modal correspondence between tactile and visual senses in terms of time and space. We hypothesized that a simple flash could modulate the CRE depending on its location of presentation, similar to reports of the tau and kappa effects (Suto, 1952; Russo and Dellantonio, 1989). The expected results should be important when we consider the mechanism of the CRE, as well as causality perception in the outer world. Is the CRE truly a phenomenon limited to early unimodal somatosensation (Flach and Haggard, 2006)? The CRE is a good method for demonstrating the relativity or interdependency of space and time in somatosensation; furthermore, the CRE reflects our expectation of the world (Goldreich, 2007; Miyazaki et al., 2010). If this is the case, the CRE should be susceptible to multi-modal presentation in order to create a unified representation of moving stimuli on the skin to the extent that the "spatiotemporal" synchronicity among modalities is provided (as well as other multi-modal couplings).

The results of our five successive experiments actually suggested a visual effect on the CRE, but the results are more complicated than we hypothesized (see also the experiment-specific introductions). The present study has suggested that unimodal causality perception would be enhanced but might not be attenuated by cross-modal causality. This could indicate that our brain is tuned to detect the movement of an external agent on the skin since an essential, evolutionarily stable feature of brain function is the detection of animate entities for survival (Schultz et al., 2005; Pratt et al., 2010). Furthermore, we argue that sensory events at a certain time point are influenced by future sensory events; this is referred to as "postdictive" sensation (Eagleman and Sejnowski, 2000).

MATERIALS AND METHODS

PARTICIPANTS

All participants were right-handed university students, and none participated in more than one experiment. They were recruited randomly from an introductory psychology class, and written informed consent was obtained from all participants before the experiments were conducted. All participants reported normal or corrected-to-normal vision, hearing, and somatosensation and no neurological abnormalities. The experiment was conducted in accordance with the Declaration of Helsinki.

APPARATUS

The experiments took place in a silent, dark room. In order to deliver the visual and tactile stimuli, we used a multi-channel signal processor (UA-101, Roland, Shizuoka, Japan) and an amplifier (QuadMic, RME, Haimhausen, Germany) connected to a PC. The tactile stimuli were presented through vibrators (bone conductors: MGD-701, Golden Dance, Osaka, Japan), and visual stimuli were presented using LEDs (3-mm diameter). Two vibrators (10-mm diameter, used to increase the intensity of tactile stimuli) and one red LED were combined using Velcro fastenings onto a band device (see **Figure 1**). The participants wore three devices on the inside of their left arm: one each at the wrist (Location 1: L1), elbow (L3), and midway (L2) between the two (about 10–13 cm separated each device). The intensities of stimuli (flash and vibrotactile) were set at sufficient levels, and we roughly equalized the subjective intensity of tactile stimuli among the three devices across participants. White noise was presented through a speaker (80-dB SPL) in order to prevent extraneous sounds from influencing the vibrators during the experiment.

STIMULI

Visual and tactile stimuli were controlled by a sound signal (300 Hz sine wave) using MATLAB (MathWorks, Natick, MA, USA). The stimulus duration was 100 ms, and we presented three successive signals with a 100-ms ISI (inter-stimulus interval). For instance, one sequence was as follows: signal (time 1: $t1$) – blank – signal ($t2$) – blank – signal ($t3$). Thus, the three signals for each trial were presented over a 500-ms duration. The CRE is subject to temporal parameters such as stimulus duration and ISI. According to the results of previous studies (e.g., Blankenburg et al., 2006; Warren et al., 2010) and the results of our own preliminary experiments, these temporal parameters were adjusted so that the typical CRE response (L1–L2–L3 tactile feeling under the L1–L1–L3 tactile stimuli condition) would be observed approximately 50% of the time. This was done because it is necessary to have a margin for the multi-modal interaction (i.e., the effects of visual stimuli on the CRE). The $t1$ and $t3$ signals were identical in all conditions: tactile stimuli for the wrist (L1) at $t1$ and for the elbow (L3) at $t3$. At $t2$, tactile and visual stimuli (though one or the other of these was not present under some conditions) were presented somewhere between L1 and L3, including the midpoint (L2) between the wrist and elbow.

PROCEDURE

All participants sat in front of the display, and their left arm was supinated on a table (see **Figure 1**). We instructed participants to relax their left hand during the experiment. Before the experiment began, the participants received a brief training sequence to ensure familiarity with the instruments and experimental requirements. A simple visual and auditory cue signifying the onset of a trial was

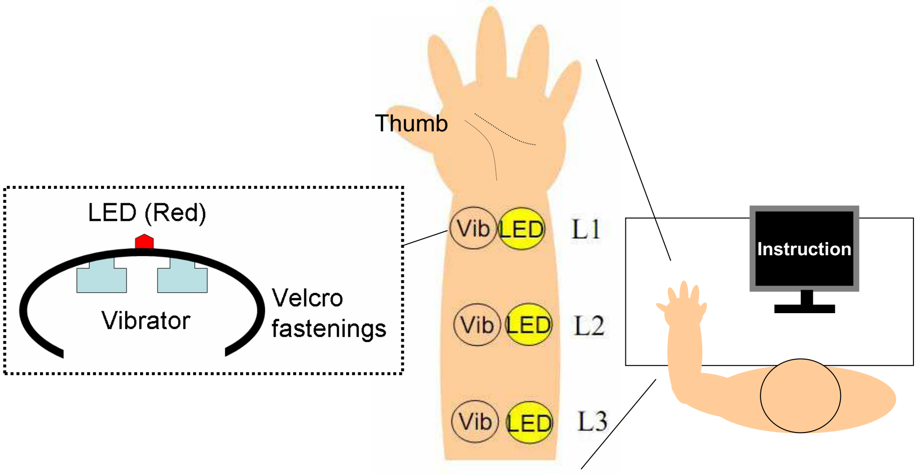

FIGURE 1 | The experimental apparatus.

first presented on the display and through the speaker. Participants then saw their left arm. They were instructed to respond via key press after perceiving a successive visuo-tactile stimulus after a random interval (1000–1500 ms). Although the specific requirements were experiment-dependent, all experiments required participants to report their tactile sensation while ignoring visual stimuli. Participants were informed that they would be presented with three successive stimuli (tactile, flash, or both) per trial, distributed among three devices.

EXPERIMENT 1A

To show that the CRE – an expression of unimodal causality perception – could be modulated by cross-modal influences, we administered a one-shot visual flash accompanied by three successive tactile stimuli. We hypothesized that if the visual flash were presented concurrently with one of the tactile stimuli (and that location were congruent with the CRE saltation), then the CRE would be enhanced. This manipulation would provide causal, spatiotemporal correspondence between the visual (the flash) and tactile (the CRE saltation) senses. We expected that L1–L1–L3 tactile stimuli would be felt as L1–L2–L3 to some extent. Furthermore, a flash on L2 at $t2$ was expected to induce a strengthened L1–L2–L3 tactile sensation in this condition.

METHOD

Twelve university students (four male and eight female, mean age = 18.6 years, range = 18–21) participated in a 3 (patterns of tactile stimuli) × 2 (presence vs. absence of visual stimuli)-factor experiment. For the patterns of tactile stimuli, we presented three successive signals through vibrators: L1–L2–L3, L1–L1–L3, and L1-(blank)-L3. In the visual-stimuli-provided conditions, we presented a one-shot visual signal on the LED located on L2 at $t2$ (**Figure 2**, left panel). These six conditions were randomly repeated 20 times for each participant. Participants were required to respond (via key press) as quickly as possible using their index fingers after the successive multi-modal stimuli were presented. Participants pressed the right (or left) key immediately when they felt the tactile sensation as L1–L2–L3, regardless of visual stimuli. Participants immediately pressed the left (or right) key when they did not feel the tactile sensation as L1–L2–L3. We recorded the response ratios and reaction times (RTs).

RESULTS AND DISCUSSION

The response ratios of L1–L2–L3 tactile sensation and the RTs under each condition were averaged across participants (**Figure 2**, left panel). For the response ratios, a 3 (tactile patterns) × 2 (with vs. without flash) two-way ANOVA revealed a significant interaction between tactile pattern and the presence vs. absence of visual feedback, $F(2, 22) = 6.65$, $p < 0.01$. We also observed a simple main effect of flash under the L1–L1–L3 tactile condition, $F(1, 33) = 17.12$, $p < 0.01$, a simple main effect of tactile pattern under the flash condition, $F(2, 44) = 65.47$, $p < 0.01$, and a simple main effect of tactile pattern in the no-flash condition, $F(2, 44) = 69.79$, $p < 0.01$. These results were analyzed further using Ryan's method of multiple comparisons (i.e., R-E-G-W's F test). Under both flash conditions, each tactile pattern was significantly different from the others ($ps < 0.05$). Conversely, for RT, a similar two-way ANOVA with multiple comparisons conducted using Ryan's method revealed only a significant main effect of tactile patterns, $F(2, 22) = 7.28$, $p < 0.01$. There were significant differences between L1–L2–L3 (average RT = 478.1 ms) and L1–L1–L3 (573.7 ms) and between L1–L2–L3 and L1-blank-L3 (587.2 ms; $ps < 0.05$) in terms of RT, indicating that regardless of the flash, participants more rapidly reacted to L1–L2–L3 stimuli.

Our results suggest that the tactile pattern of L1–L1–L3 could be felt as L1–L2–L3 to some extent. In other words, participants did experience the CRE (e.g., Blankenburg et al., 2006). Furthermore, a visual flash could enhance this illusion. Given that we presented the flash on L2 at $t2$, it seems likely that if the flash conveyed the location of tactile stimuli as L2 at $t2$, participants would feel the L1–L2–L3 tactile sensation instead of L1–L1–L3. However, the results of the L1-blank-L3 tactile condition suggest that the flash itself does not create a tactile sensation. This is because our tactile stimuli are sufficiently intense (cf., McKenzie et al., 2012). In sum, Experiment 1A suggests that as long as a flash is congruent with CRE saltation in terms of time and space, it can apparently relocate tactile stimuli to the location where the LED flashed; however, this multi-modal effect might not be reflected in participants' RTs. The directional movement of a "cutaneous rabbit" indicates that a flash displaces tactile sensation in the direction of forward movement (forward displacement). In the following experiment, we examined whether a flash could also move the tactile location of the cutaneous rabbit backward, against its direction of saltation (backward displacement).

EXPERIMENT 1B

Since the previous experiment suggested a cross-modal impact of forward displacement in the CRE, the current experiment examined the possibility of backward displacement. We predicted that as far as a visual flash is congruent with the CRE saltation (cross-modal correspondence), it should capture tactile location. A flash could also induce backward displacement just as easily as forward displacement. We expected that L1–L3–L3 tactile stimuli would also be felt as L1–L2–L3 to some extent. Furthermore, a flash on L2 at $t2$ was expected to introduce a stronger L1–L2–L3 tactile sensation. Conversely, if the direction of tactile displacement depended on the directional congruency of the whole tactile movement (forward or backward), L1–L3–L3 tactile stimuli with a flash would be felt as L1–L2–L3 to a lesser extent than L1–L1–L3 tactile stimuli with a flash.

METHOD

Thirteen university students (1 male and 12 female, mean age = 19.9 years, range = 18–23) participated in a 3 × 2-factor experiment similar to Experiment 1A. We changed one tactile condition in this experiment. For the patterns of tactile stimuli, we presented three successive signals through vibrators: L1–L2–L3, L1–L1–L3, and L1–L3–L3 (only the last condition was replaced). Along with the same visual stimuli conditions used in Experiment 1A, we formed a sixth condition by presenting a one-shot signal on the LED on L2 at $t2$ (see **Figure 2**, right panel). These six conditions were randomly repeated 20 times for each participant. Participants judged whether the successive tactile sensation was L1–L2–L3, as in Experiment 1A.

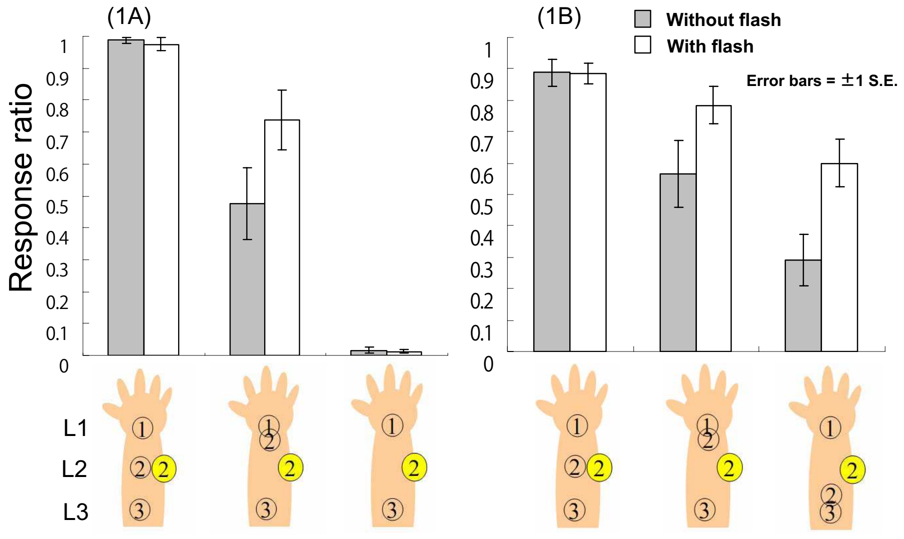

FIGURE 2 | The response ratio (L1–L2–L3 tactile feeling) in Experiment 1A and B. Note: The numbers in circles mean the timings ① corresponds to *t*1. For example, the leftmost illustration means that L1 → L2 → L3 tactile stimuli with flash at *t*2 timing.

RESULTS AND DISCUSSION

For the response ratios, a 3 (tactile patterns) × 2 (with vs. without flash) two-way ANOVA revealed a significant interaction between tactile pattern and the presence vs. absence of visual feedback, $F(2, 24) = 9.69$, $p < 0.01$, a simple main effect of flash under the L1–L1–L3 tactile condition, $F(1, 36) = 9.64$, $p < 0.01$, a simple main effect of flash under the L1–L3–L3 tactile condition, $F(1, 36) = 19.0$, $p < 0.01$, a simple main effect of tactile condition in trials including a one-shot flash, $F(2, 48) = 6.08$, $p < 0.01$, and a simple main effect of tactile condition without a flash, $F(2, 48) = 26.0$, $p < 0.01$ (**Figure 2**, right panel). These results were analyzed further using Ryan's method of multiple comparisons. In the no-flash conditions, each tactile pattern was significantly different from the others (*p*s < 0.05); further, the difference between the L1–L2–L3 and L1–L1–L3 conditions was not significant when flash was presented. For RT, a similar two-way ANOVA with multiple comparisons conducted using Ryan's method revealed similar results to those in Experiment 1A: there was only a significant main effect of tactile pattern, $F(2, 24) = 4.40$, $p < 0.05$. There were significant differences in terms of RT between L1–L2–L3 (average RT = 642.6 ms) and L1–L1–L3 (739.8 ms) and between L1–L2–L3 and L1-blank-L3 (743.3 ms; *p*s < 0.05). This indicated that regardless of whether a flash was presented, participants most rapidly reacted to L1–L2–L3 stimuli.

This experiment replicated the result that the L1–L1–L3 tactile pattern could be felt as L1–L2–L3 and that this illusory sensation could be enhanced by a flash on L2 at *t*2. In this case, the sensation enhancement was almost the same as in the baseline condition (L1–L2–L3 tactile stimuli). It seems that participants made slightly more miss responses (approximately 10%) under the baseline condition in this experiment, while participants in Experiment 1A made hit responses almost perfectly. Since participants felt L1–L2–L3 tactile sensations under all conditions, they seem not to have been sufficiently conservative in their judgments. This might account for why RTs in the current experiment were approximately 150 ms longer than in the previous experiment. Furthermore, the L1–L3–L3 tactile pattern was also felt weakly compared with the L1–L2–L3 pattern. The CRE can occur when the second tactile sensation is subject to forward displacement (e.g., Blankenburg et al., 2006), indicating that stronger tactile displacement should be introduced when the direction of displacement is congruent with the direction of cutaneous rabbit saltation. This is apparently related to the fact that the human brain expects uniform motion and constant velocity of such motion, regardless of modality (Jones and Huang, 1982). This creates causality perception (Scholl and Tremoulet, 2000). Given that forward displacement is congruent with the direction of expected uniform motion, forward displacement in both unimodal (the CRE) and cross-modal (visual influence on the CRE) causality perception would be created more often (see the General Discussion). Nevertheless, the current experiment suggests that if a flash is given at an appropriate location and time, it can cause backward displacement. As a result, L1–L3–L3 tactile sensations enhanced by a flash yielded a similar perceived sensation to L1–L1–L3 tactility without a flash. However, the former sensations did not approach subjective similarity to the tactility of L1–L1–L3 stimuli enhanced by a flash. Experiments 1A and B collectively suggest that the CRE might be enhanced by congruent flashes relatively easily. In the following experiments, we examined whether or not the CRE could be attenuated by a spatially incongruent flash.

EXPERIMENT 2A

The previous experiments suggested that a visual flash congruent with CRE saltation in terms of time and space would enhance the CRE, since this manipulation could lead participants to draw causal associations between visual and tactile sensation. This result might indicate that a visual flash can modulate the CRE; however, another possibility should also be examined. In the previous experiments, even if a flash simply modulated a single tactile location, the same results would be observed. To examine this possibility, we presented a spatially and temporally incongruent flash. This manipulation provides two possibilities. If a flash simply modulates a single tactile location, then the CRE should be attenuated in this setting; in other words, if a flash captures and relocates a tactile location to an incongruent location, then participants should feel less of an L1–L2–L3 sensation. On the contrary, if a flash modulates not a single tactile location but CRE saltation as a whole, we could expect that a spatially incongruent flash might not attenuate the CRE. This is because the cross-modal correspondence between the flash and CRE saltation should not be realized; as a result, the stimuli within each modality should be processed separately. We used the same tactile stimulus patterns as in Experiment 1B, but we delivered a flash on L1 at $t2$ in the current experiment.

METHOD

Thirteen university students (six male and seven female, mean age = 18.1 years, range = 18–19) participated in a 3 × 2-factor experiment similar to Experiments 1A and B. The tactile stimulus patterns were identical to those presented in Experiment 1B, but we changed the location of the flash in the current experiment: the one-shot signal from the LED was located on L1 and presented at $t2$ (see **Figure 3**, left panel). These six conditions were repeated 20 times for each participant, ordered randomly. The participants judged whether the successive tactile sensation was L1–L2–L3, as in Experiments 1A and B.

RESULTS AND DISCUSSION

Using the response ratios as the dependent variable, a 3 (tactile patterns) × 2 (with vs. without flash) two-way ANOVA revealed no significant effects except for a significant main effect of tactile pattern, $F(2, 24) = 20.37$, $p < 0.01$. Multiple comparisons using Ryan's method revealed that each tactile pattern was significantly different from every other pattern in terms of response ratio ($ps < 0.05$; **Figure 3**, left panel). Using RT as the dependent variable, a similar two-way ANOVA and multiple comparisons using Ryan's method yielded the same results as Experiments 1A and B. That ANOVA revealed only a significant main effect of tactile pattern, $F(2, 24) = 5.97$, $p < 0.01$; further, there were also significant differences between L1–L2–L3 (average RT = 513.6 ms) and L1–L1–L3 (620.9 ms) and between L1–L2–L3 and L1-L3-L3 (623.4 ms; $ps < 0.05$) in terms of RT, indicating that regardless of the presence vs. absence of the flash, the participants reacted most rapidly to the L1–L2–L3 stimuli.

These results suggest that visual stimuli might not reduce the CRE though a flash on L1 at $t2$ must be spatially incongruent with the L1–L2–L3 tactile sensation. As for the L1–L2–L3 tactile conditions, if a flash captures and causes backward displacement of L2, participants would feel less of an L1–L2–L3 sensation. The fact that a flash did not reduce the response ratio indicates that it is difficult to observe simple spatial visuo-tactile ventriloquism (see General Discussion). The flash also did not reduce the response ratio in the L1–L1–L3 tactile conditions; even though, the flash was temporally and spatially synchronized with a second tactile stimulus (both on L1 at $t2$). Although the flash indicated the correct location of a second tactile stimulus, participants felt the CRE just as well as when no-flash was present. The effect of the flash also was not observed under the L1–L3–L3 tactile conditions. These results indicate that a spatially incongruent flash did not modulate the CRE. The tactile sensation seems to be processed separately from the visual system in the present experiment, indicating that a flash could modulate not a single tactile location but the CRE as a whole to the extent that the correspondence between the visual and tactile senses is maintained. To further examine the present results, we conducted Experiment 2B, wherein we fixed the tactile pattern as L1–L1–L3 and examined the effects of manipulating the flash location at $t2$.

EXPERIMENT 2B

In order to examine the possibility that a flash could attenuate the CRE, we varied the flash location at $t2$ by fixing the tactile

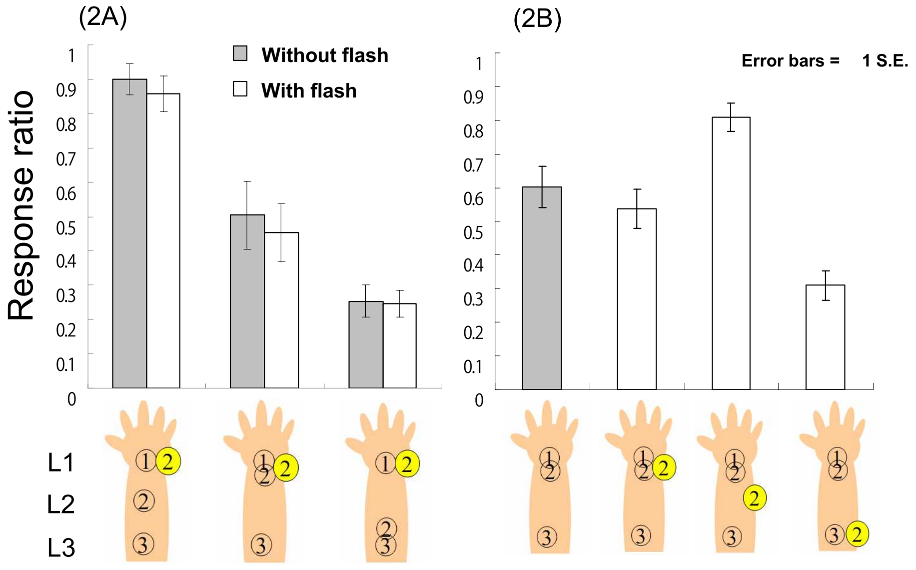

FIGURE 3 | The response ratio (L1–L2–L3 tactile feeling) in Experiment 2A and B.

pattern as L1–L1–L3 (the typical CRE tactile pattern). While a spatially congruent L2 flash enhances the L1–L2–L3 tactile sensation (as we showed in Experiments 1A and B), L1 and L3 flashes are spatially incongruent with the CRE saltation. Although the results of Experiment 2A suggested that the L1 flash did not attenuate the CRE, there is a difference between L1 and L3 flashes: whereas an L1 flash might pull the illusory second tactile location (that is, L2) back to L1, the L3 flash might push it forward to L3. In the present experiment, congruency between displacement of the flash and the direction of CRE saltation was manipulated. As in Experiment 1B, we examined whether a flash at L3 could push the illusory L2 forward to L3 (i.e., the L1–L3–L3 sensation) and attenuate the CRE, where the correspondence between a flash and the CRE saltation might not be realized in this condition.

METHOD

Fourteen university students (4 male and 10 female, mean age = 19.4 years, range = 18–21) participated in the current experiment. The tactile stimulus pattern was fixed as L1–L1–L3, and we varied the flash location at *t*2 (L1, L2, L3, or no-flash; see **Figure 3**, right panel). These four conditions were each repeated 20 times in random order for each participant. The judgment task was the same as in the previous experiments.

RESULTS AND DISCUSSION

A one-way ANOVA using the response ratios as the dependent variable revealed a significant main effect of condition, $F(3, 39) = 21{,}12$, $p < 0.01$, and multiple comparisons (Ryan's method) revealed that each tactile pattern was significantly different from the others ($ps < 0.05$), except for the difference between both with/without flash conditions on L1 (**Figure 3**, right panel). Similar analyses on RTs revealed a significant main effect of condition, $F(3, 39) = 5.48$, $p < 0.01$. Multiple comparisons revealed significant differences between the no-flash (average RT = 615.7 ms) and flash conditions (713.6 ms) on L3 and between the flash on L2 (590.2 ms) and flash-on-L3 conditions ($ps < 0.05$; average RT with flash on L1 = 678.8 ms).

These results essentially replicated the results of the previous experiments. A flash on L2 enhanced the CRE, but a flash on L1 did not attenuate the CRE as compared to the no-flash condition (in this case, L1–L1–L3 tactile stimuli; see the results of Experiments 1A and 2A). However, the newly added condition (a flash-on-L3) reduced the L1–L2–L3 tactile sensation; further, this condition generated the longest RTs among the four conditions. One possible reason for the differences between the L1 and L3 flash conditions (both are spatially incongruent) is the factor of congruency with the direction of CRE saltation. As we suggested in Experiment 1B, forward displacement should be easier than backward displacement in the CRE; however, enhancement of the L1–L2–L3 tactile sensation by a flash could occur even under conditions favorable to backward displacement. However, that type of attenuation was not observed in the current experiment, indicating that the cross-modal correspondence problem and the direction of visual displacement of the tactile location interact. When cross-modal correspondence (spatial and temporal congruency between the flash and CRE saltation) is present, the flash causes forward or backward displacement. As a result, the CRE is enhanced (Experiment 1A and B). Conversely, when cross-modal correspondence is not provided, forward instead of backward displacement is created (Experiment 2A), attenuating the CRE (the current experiment). To investigate why the CRE could not be attenuated under the flash conditions favoring backward displacement, we required participants to report the tactile sensations as they were felt instead of implementing a two forced-choice response task (as in Experiment 2B).

EXPERIMENT 2C

We required participants to report their tactile sensation as it was experienced in order to examine why the attenuation of the L1–L2–L3 tactile sensation by a flash might not be observed in some conditions. We hypothesized that when a flash does not correspond to CRE saltation, it might affect a single concurrent tactile stimulus. Furthermore, since forward displacement is relatively easily realized, as suggested by the results of Experiment 1B, it was expected that only forward displacement would emerge to attenuate the CRE. In the current experiment, the tactile stimuli were identical to those used in Experiment 2A, and a flash was presented on either L1 or L3 (both are spatially incongruent with the CRE saltation). We examined what participants felt when they did not feel the L1–L2–L3 tactile sensation under conditions favoring forward or backward displacement.

METHOD

In the current experiment, because more participants were required in order to achieve stability in the self-reporting of tactile sensation (see below), the number of participants was increased. This was done in order to account for the several potential variations in responses. Sixteen university students (4 male and 12 female, mean age = 19.8 years, range = 18–24) participated in a 3 × 2 factorial experiment: the tactile stimulus patterns were L1–L2–L3, L1–L1–L3, or L1–L3–L3, and a flash was presented at *t*2 on L1 or L3 (see **Figure 4**). These six conditions were repeated 20 times in random order for each participant. Participants were required to report the pattern of tactile stimuli corresponding with their sensation using a key press as they felt it. Since they were informed before the experiment that they would feel three successive tactile stimuli, they reported three locations in order without time constraints (this was not the case in the previous experiments.)

RESULTS AND DISCUSSION

We summarized the participants' raw reports. **Figure 4** shows the tactile patterns reported by the participants after we omitted rare responses (below 4%) and labeled them as "other" responses in each condition. The report ratios of L1–L2–L3, L1–L1–L3, and L1–L3–L3 comprised almost 70% of all responses. A 3 (tactile pattern) × 2 (flash location) two-way ANOVA using the report ratio of L1–L2–L3 as the dependent variable revealed a significant interaction between tactile pattern and flash location $F(2, 30) = 9{,}17$, $p < 0.01$, significant simple main effects of flash condition under the L1–L2–L3 and L1–L3–L3 tactile conditions ($ps < 0.01$), and significant simple main effects of tactile pattern under both flash conditions ($ps < 0.01$). Multiple comparisons using Ryan's

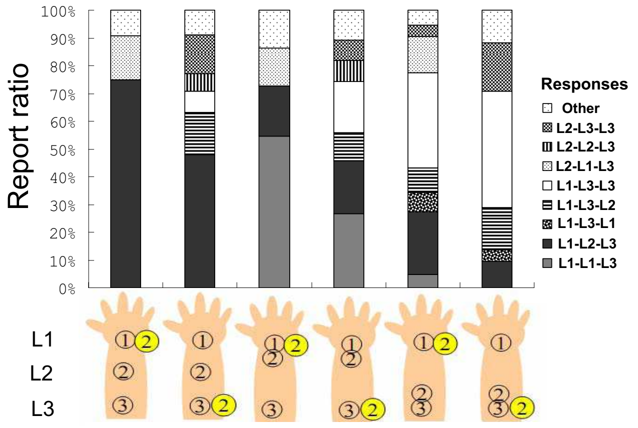

FIGURE 4 | The report ratio in Experiment 2C.

method revealed that the report ratio of the L1–L2–L3 tactile pattern was significantly different from those of the L1–L1–L3 and L1–L3–L3 patterns under both flash conditions ($ps < 0.01$).

First, we need to consider the potential effects of the requirements placed on participants. In the current experiment, participants reported the tactile sensation pattern they felt. Compared with the results of Experiments 2A and B, the report ratios for the L1–L2–L3 sensation were generally reduced. This might be because participants were placed under time pressure during the previous experiments; thus, they might have encoded other potential pattern identifications, like L2–L1–L3 or L1–L3–L2, as L1–L2–L3. That might also explain the lack of significance of flash condition under the L1–L1–L3 tactile conditions. It seems that the feeling of the L2–L1–L3 pattern could be judged as L1–L2–L3 when responses are made quickly (e.g., Experiments 2A and B; see **Figure 4**). The sum of the response ratios of L1–L2–L3 and L2–L1–L3 under the L1 flash condition in the current experiment approximates the response ratio of L1–L2–L3 under the same conditions in Experiments 2A and B. That notwithstanding, why were reports of feeling the L2–L1–L3 or L1–L3–L2 patterns observed (each at a 10–15% rate) in the current experiment? Furthermore, why were these reports encoded as L1–L2–L3 tactile responses in the previous experiments? We discuss these questions in terms of the temporal order judgment between vision and tactile perception in the General Discussion.

Although the presentation of a flash on L1 or L3 is supposed to be spatially incongruent with the tactile feeling of L1–L2–L3, the significant differences between these presentations signify the importance of congruency with the direction of cutaneous rabbit saltation. The attenuation of the CRE might occur only under conditions of forward displacement. Under these conditions, we observe more reports of L1–L3–L3 or L2–L3–L3, which are congruent with the direction of the CRE, instead of L1–L2–L3. The CRE is affected by directional congruency between tactile displacement and the whole tactile movement (i.e., the comparison between the L1–L1–L3 and L1–L3–L3 tactile conditions, as suggested by the results of Experiment 1B). This is also true when the flash modulates the tactile location. The flash caused forward displacement (attenuating the CRE) but not backward displacement (no attenuation of the CRE) in Experiment 2. On the other hand, the flash caused both forward and reverse displacement (both enhancing the CRE) in Experiment 1. Although the reason for this asymmetry in the ease of enhancement vs. attenuation is still unclear, the brain might intrinsically expect consistently moving tactile sensations, such as something hopping along the skin (Goldreich, 2007) or off of the body (Miyazaki et al., 2010). Our somatosensation might be specialized for detecting other species creeping or hopping along our skin; thus, we might prefer false alarms to misses, since the ability to perceive external agents in motion is strongly ingrained (Leslie, 1995; Schultz et al., 2005; Pratt et al., 2010).

GENERAL DISCUSSION

Our present results suggest that a simple flash is able to modulate the CRE. Congruency between the direction of tactile displacement (forward or backward displacement) and CRE saltation and the existence of cross-modal correspondence between visual and tactile cues seem to play key roles in this ability. First, Experiment 1B suggested that the CRE itself is susceptible to modulation by movement direction, indicating that forward displacement would be more acceptable than backward displacement in the CRE. The results of Experiments 1A and B suggest that when a flash is temporally and spatially congruent with the CRE (when a flash is placed at the location of the supposed CRE saltation), and cross-modal correspondence is provided, participants feel the CRE robustly regardless of the direction of tactile displacement. Conversely, the results of Experiments 2A, B, and C suggest that when a flash is spatially incongruent with the CRE saltation, and cross-modal correspondence is not provided, participants experience an attenuated CRE under conditions favoring forward displacement, but not under conditions favoring backward displacement. The results of Experiment 2C also indicate that the combination of these two factors creates various tactile sensations and that temporal order judgment might be modulated under some conditions. Our results provide theoretically important information on unimodal and multi-modal causality perception: the former indicates that our brain needs to detect motion from successive stimuli, and the latter indicates that multi-modal presentation creates a unified, cross-modal representation of motion. Though a previous study suggested that the CRE is the spatiotemporal dynamics of an early, unimodal, sensory map (Flach and Haggard, 2006), the CRE is related not only to a lower level of spatiotemporal perceptual interaction but also a higher level of cognition that includes unimodal causality or animacy perception (Scholl and Tremoulet, 2000). The present study suggests that forward displacement (i.e., constant movement in one direction) of tactile sensation is more acceptable than backward displacement, in accordance with the constant velocity assumption (Jones and Huang, 1982; see the following section). The current study also suggests, for the first time, that unimodal causality perception (the CRE) might be influenced by multi-modal presentation under conditions of cross-modal causality correspondence (Schutz and Kubovy, 2009; Parise et al., 2012).

While one previous study argued that the CRE is not affected by concurrent auditory temporal information

(Flach and Haggard, 2006), the current study suggests that the CRE is affected by a concurrent visual flash, where "spatiotemporal" synchronicity among modalities is provided. The spatiotemporal dynamics of a somatosensory map in the CRE (Flach and Haggard, 2006) might only be modulated by spatiotemporally synchronized stimuli from other modalities. We discuss these specific mechanisms below.

SOMATOSENSORY SYSTEM IN TERMS OF TIME AND SPACE

The somatosensory system, as well as other modalities, faces not only spatial but also temporal imprecision. The most discriminating tactile sensors among primates – the fingertips – house a few hundred sensory nerve fibers per square cm (Johansson and Vallbo, 1979; Darian-Smith and Kenins, 1980), a density four orders of magnitude below the peak density of retinal ganglion cells (Wassle et al., 1990). Nevertheless, the spatial attributes of tactile sensory nerves are likely important. The brain possesses a somatotopic body map within the primary somatosensory cortex (S1; Penfield and Boldrey, 1937) to reflect the locations of physical stimuli on the skin. Furthermore, whereas sound stimuli may provide temporal precision to the perception of spatial attributes (Kubovy and Van Valkenburg, 2001), tactile stimuli may be less temporally defined (Keetels and Vroomen, 2008a,b). Though the CRE might run contrary to the spatial tactile attributes mentioned above, this illusion reflects not only the temporal and spatial imprecision of the somatosensory system but also the brain's expectations regarding the external world (Goldreich, 2007). Previous studies have suggested that the human brain expects uniform motion, regardless of modality (Jones and Huang, 1982). Sensory systems work to recover the causal and social structures of the world by inferring properties such as causality and animacy (Scholl and Tremoulet, 2000). In line with this assumption, the CRE, tau effect, and kappa effect share the same basis: the constant velocity assumption (Jones and Huang, 1982).

A typical tau effect, where the perceived distance between stimuli underestimates, and grows in proportional with, the actual distance when stimulus timing is held constant (Marks et al., 1982; Cholewiak, 1999) and the kappa effect, in which the perceived time between stimuli dilates as the distance between stimuli is increased (Suto, 1952), reflect just two fundamental perceptual distortions: underestimation of inter-stimulus distance (perceptual length contraction) and overestimation of inter-stimulus time interval (perceptual time dilation; Goldreich, 2007). Given that three successive tactile stimuli define two spatial (S1 and S2) and two temporal intervals (T1 and T2), the somatosensory system intuitively imputes motion at a given speed to the tactiles and tries to equalize the ratios S1/S2 and T1/T2; thus, it follows that $S1/T1 = S2/T2$ (modified from Jones and Huang, 1982). In this way, the sensory system – which includes somatosensation, vision, and audition (Cohen et al., 1953; Shore et al., 1998) – attempts to equalize the velocity between the first and second stimuli (S1/T1) and that between the second and third stimuli (S2/T2); this is known as the constant velocity assumption (Goldreich, 2007). Though the CRE is also in line with the tau and kappa effects, the present study also suggests that the direction of tactile displacement is crucial. The L1–L1–L3 tactile stimuli are felt more as L1–L2–L3 than as L1–L3–L3 (Experiment 1B). The constant velocity assumption is rooted in the notion that motion perception is closely related to animacy perception or detection of the movement of external agents (Leslie, 1995; Scholl and Tremoulet, 2000). Thus, it is reasonable that not only the speed but also the direction of motion should be equalized for motion perception. Because forward displacement is congruent with the expected direction of uniform motion, the brain might perceptually relocate illusory sensations in the forward direction, as changes in the motion signal usually create forward displacement of representational momentum. Thus, the general tendency is to displace the judged position of a moving target as being relatively far forward along the path of motion (Tremoulet and Feldman, 2006; Getzmann and Lewald, 2007, 2009).

VISUO-TACTILE INTERACTION IN TERMS OF CROSS-MODAL CORRESPONDENCE

When sensory signals are presented simultaneously across multiple modalities, they tend to be detected more quickly, accurately, and at lower thresholds than if the same signals are presented individually (e.g., Hershenson, 1962; Frassinetti et al., 2002). In addition, if those signals are incongruent, various multi-modal illusions will be observed as far as the temporal synchronicity among modalities is provided. The McGurk effect is a perceptual phenomenon that demonstrates interactions between hearing and vision in speech perception (McGurk and MacDonald, 1976). This effect can be experienced when the visual representation of a phoneme is dubbed with a sound recording of a different phoneme being spoken; in such situations, the perceived phoneme is often a third, intermediate phoneme. Moreover, spatial ventriloquism occurs when the visual locations of stimuli capture and displace their auditory locations (Jack and Thurlow, 1973; Alais and Burr, 2004). Certain visuo-tactile interactions have also been reported. When participants discriminate the locations of vibrotactile stimuli by ignoring distractor lights, such tactile discriminations are slowed when the distractor light is incongruent with the tactile target (Pavani et al., 2000). In addition, the perceived number of tactile stimuli is influenced by the number of flashes presented (and vice versa; Violentyev et al., 2005; Bresciani et al., 2006); there have also been reports of bidirectional attentional blink between vision and touch (Soto-Faraco et al., 2002).

Vision also captures tactile sensations. The rubber hand illusion (RHI) refers to the effect of watching a rubber hand being stroked synchronously with one's own, unseen hand. Viewing this for a short time causes the observer to perceptually assimilate the rubber hand into his or her own body (Botvinick and Cohen, 1998). The RHI might indicate that the visual location of stimuli displaces the tactile one [e.g., a typical subjective feeling for RHI is, "It seemed as if I were feeling the touch of the paintbrush. . . where I saw the rubber hand (being) touched" (Asai et al., 2011; Botvinick and Cohen, 1998)]. In addition, aspects of the sense of body ownership such as body posture (Austen et al., 2004; Ehrsson et al., 2004), visual appearance (Tsakiris and Haggard, 2005), hand identity (Tsakiris et al., 2006), or the self-other representation (Schutz-Bosbach et al., 2006; Asai et al., 2011) also strongly affect the RHI. To our knowledge, so-called simple "visuo-tactile spatial ventriloquism" cannot be observed (see also our results of Experiment 1A): we do not simply feel tactile sensations on locations where the light flashes, except when we are attempting to detect near-threshold signals (i.e., light-evoked false alarms; McKenzie et al., 2012). Though the concept has still not been completely elucidated, the somatotopy of tactile body location in the brain might be responsible for these results (cf., the tonotopy of audition). Thus, temporal ventriloquism has been reported in the domain of visuo-tactile interaction, suggesting that small amounts of latency between vision and touch are reduced and tend to go unnoticed (Spence et al., 2001; Keetels and Vroomen, 2008a; Vroomen and Keetels, 2010).

Although previous studies have not reported such spatial visuo-tactile ventriloquism, our results suggest that we feel the tactile sensation where the light flashes in several specific situations. This suggests that although a flash might not relocate a tactile stimulus, a flash could modulate illusorily located tactile sensation, as visual influences on the tau and kappa effects have been reported (Suto, 1952; Russo and Dellantonio, 1989). Cross-modal causality plays a key role in governing the integration of sensory information, depending on its ecological plausibility (Schutz and Kubovy, 2009). Humans can use the similarities between the temporal structures of sensory signals in different modalities to solve the correspondence problem, ultimately inferring causation from correlation (Parise et al., 2012). Given that people infer which signals have common underlying causes and hence merit integration (Parise et al., 2012), the most common perceived cause in the current study – in which we observed visuo-tactile integration (cross-modal causality) while focusing on the CRE (unimodal causality) – ought to be the external agent in motion (Leslie, 1995; Scholl and Tremoulet, 2000).

TEMPORAL ORDER JUDGMENT AND CROSS-MODAL INFLUENCE

However, there might not be any differences between the actual and illusorily located tactile sensations within the human brain. Illusory sequences activate the contralateral primary somatosensory cortex at somatotopic locations corresponding to the filled-in illusory perceptions on the forearm (Blankenburg et al., 2006); this suggests that this illusion is associated with the early sensory body map represented in S1. Why does the cutaneous rabbit hop toward the light, and why is the CRE attenuated, even though illusorily located tactility should be represented within a corresponding area of S1? One possible reason might be related to temporal visuo-tactile ventriloquism. Small amounts of latency between vision and touch (or sound) tend to be reduced and go unnoticed, because the timing of visual events is flexible and adjusts immediately (for a review, see Vroomen and Keetels, 2010). The results of Experiment 2C indicated that the pattern of tactile feeling could often be predicted by the flash. Participants' report ratios of L2–L1–L3 (under L1 flash conditions, 10–15%) and L1–L3–L2 (under L3 flash conditions, 10–15%) are of interest. The former might be interpreted as reflecting the participants' inclination toward answering L1–L2–L3 (i.e., the CRE), and the L1 flash might rearrange the temporal order into L2–L1–L3. The latter is also as well. The flash might not modulate the extent to which the second tactile is felt in S1 but does modulate the temporal order of the tactile sequence. As a result, a flash could attenuate the CRE, especially when participants have enough time to report their sensation. The CRE and its interaction with vision indicate that sensory events at a given time point are influenced by future sensory events; this is called "postdictive" sensation (Eagleman and Sejnowski, 2000).

If we accept this two-step explanation of the visuo-tactile interaction, other interpretations regarding the lack of CRE attenuation caused by the L1 flash (Experiment 2) might be possible. The current results indicate that a flash captures the tactile location, except when the flash is presented on L1. However, even if a flash on L1 captured the tactile location, it would not change the current results. If we feel the sensation of the flash location, the L1–L2–L3 tactile stimuli with flash on L1 at *t*2 might be felt as L1–L1–L3. This illusory L1–L1–L3 tactile sensation could be re-encoded as L1–L2–L3, according to the CRE. One might expect longer RTs under the flash than the no-flash conditions, but we did not observe this pattern of results (see Experiment 2A). Although no clear conclusions can be drawn regarding the influence of the presentation of a flash on L1, the results indicate that it modulates successive tactile sensations spatially, temporally, or both.

LIMITATIONS OF THE CURRENT STUDY

The current paradigm has some limitations, and further research is needed to expand our knowledge on the effects of visual flash on the CRE. First, visual capture of the CRE might be susceptible to response bias in Experiments 1 and 2. For instance, previous studies examining the CRE required participants to judge whether or not the tactile stimuli were presented on L2 (e.g., Blankenburg et al., 2006), not to judge whether or not the tactile stimuli were felt as L1–L2–L3 (as in the current study). Because we examined multi-modal interaction, the temporally congruent feeling of tactility on L2 does not always equal an L1–L2–L3 sensation. This can be inferred from the results of Experiment 2C, in which participants reported feeling various patterns of tactile stimuli, including tactile sensation on L2. Since the results of Experiments 2A and B suggested that participants did not simply follow the flash, we are optimistic about this possibility. Furthermore, the current study did not consider participants' attention in detail: we simply instructed participants to see their left arm as a whole, since the CRE is not affected by gaze direction (Flach and Haggard, 2006). Nevertheless, potential visual effects of attention on tactility (Pavani et al., 2000; Soto-Faraco et al., 2002) should be controlled in future studies. Finally, it is possible that participants felt more than three tactile sensations because of the flash (Bresciani et al., 2006); however, we could not directly test this possibility, given that we informed participants that they would experience three successive stimuli. Although no participant reported such sensations, this question is worth examining.

CONCLUSION

The CRE is an attractive phenomenon, as postdictive processing is one of the key concepts that characterizes our conscious perception (Miyazaki et al., 2010). However, this mechanism includes temporal and spatial factors that are difficult to ascertain (Flach and Haggard, 2006; Goldreich, 2007). While the CRE might have the spatiotemporal dynamics of an early, unimodal, sensory map (Blankenburg et al., 2006; Flach and Haggard, 2006), other studies have also suggested that attention or expectations (e.g., Kilgard and Merzenich, 1995), body posture (Eimer et al., 2005), and extended body schemas (Miyazaki et al., 2010) would affect the CRE. For the first time, our results suggest that a simple visual flash could modulate the CRE. Experiment 1 suggested that the

CRE itself is susceptible to movement direction and that forward displacement within the CRE would be more acceptable. Furthermore, when a flash is temporally and spatially congruent with CRE saltation (i.e., cross-modal correspondence is provided), participants feel the CRE more robustly regardless of the direction of tactile displacement. Conversely, Experiment 2 suggested that when a flash is spatially incongruent with CRE saltation (i.e., cross-modal correspondence is not provided), participants feel the CRE to a lesser extent under conditions favoring forward than backward displacement. Participants' raw reports also indicated that the combination of these two factors creates various tactile sensations and that temporal order judgment is modulated under some conditions. Our results reflect (1) how the human brain interprets successive stimuli in terms of time and space (i.e., motion or causality perception according to the constant velocity assumption) and (2) that available information from other modalities provides key clues (cross-modal enhancement/attenuation of unimodal causality) about the extent to which the modalities are spatiotemporally synchronized (i.e., cross-modal correspondence). We suggest that the CRE needs to be considered not only as a perceptual phenomenon, but also as a higher cognitive function including spatiotemporal causality or animacy inferences in both unimodal and multi-modal domains.

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Erratum: Cutaneous rabbit "hops toward a light: unimodal and cross-modal causality on the skin"

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A commentary on

"Cutaneous rabbit" hops toward a light: unimodal and cross-modal causality on the skin
by Asai T and Kanayama N (2012). Front. Psychology 3:427. doi: 10.3389/fpsyg.2012.00427

We would like to apply corrections regarding word-by-word citations.

Abstract

The following sentences

Repeated rapid stimulation at the wrist, then near the elbow, can create the illusion of touch at intervening locations along the arm (as if a rabbit is hopping along the arm).

->**should be replaced by**

Under some conditions, successive tactile stimuli at some locations on the forearm can be felt as an illusion of movement along the arm (as if a rabbit is hopping along the arm).

P1, left column

L6

A percept that misrepresents physical reality (i.e., an illusion) is both a consequence of and a clue as to the brain's expectations regarding the external world (Goldreich, 2007).

->

We can know through some perceptual illusions how the brain expects an event that is happening in the external world (Goldreich, 2007).

L9

The brain takes advantage of prior knowledge to enhance its perceptual resolution. In the case of tactile perception, spatial imprecision due to low receptor density poses a particular challenge (Goldreich, 2007).

->

The brain can utilize prior knowledge to overcome its limitation in perceptual resolution. For example, the spatial resolution in tactile perception depends on its low density of receptors (Goldreich, 2007).

L12

Even without the benefit of exploratory movements, the fingertips' resolving power- the most discriminating tactile sensor among primates - is on the order of 1 mm (Weinstein, 1968; Johnson and Phillips, 1981).

->

The spatial tactile resolution at the fingertips, where primates have the most discriminating tactile sensor, is on the order of 1 mm (Weinstein, 1968; Johnson and Phillips, 1981).

L17

This is the case even though the brain contains a representation of the body map in the primary somatosensory cortex (S1; Penfield and Boldrey, 1937) which reflects the locations of physical stimuli on the skin.

->

This is the case even though the brain contains a body representation or so-called body map in the primary somatosensory cortex (S1; Penfield and Boldrey, 1937), which reflects the locations of physical stimuli on the skin.

L20

Furthermore, given the several-ms jitter in the stimulus-evoked - first-spike latencies of somatosensory cortical neurons (Foffani et al., 2004), the somatosensory system has not only spatial but also temporal imprecision.

->

Furthermore, given the stimulus-evoked first-spike latencies of somatosensory cortical neurons (Foffani et al., 2004), the somatosensory system has not only spatial but also temporal imprecision.

P1, right column

L3

The CRE is a subset of a larger class of tactile saltation illusions elicited when a mechanical stimulus is followed by similar stimuli rapidly applied at nearby locations (Geldard and Sherrick, 1972; Warren et al., 2010).

->

The CRE is a "subset of a larger class of tactile saltation illusions" that could be caused when a stimulus is followed by similar stimuli rapidly applied at adjacent locations (Geldard and Sherrick, 1972; Warren et al., 2010).

L6

For example, repeated, rapid stimulation at the wrist and then near the elbow can create the illusion of touch at intervening locations along the arm, as if a rabbit is hopping along the arm.

->

For example, repeated rapid stimulation at a location near the wrist and then at another location near the elbow could be felt as an illusion of movement along the arm, including illusory touch at the intervening location, as if a rabbit is hopping along the arm.

L9

The apparent location of each stimulus moves from the actual stimulation site toward the other stimulation sites in a predictable manner depending on factors such as stimulus location and frequency (e.g., Geldard and Sherrick, 1972; Kilgard and Merzenich, 1995; Cholewiak, 1999; Flach and Haggard, 2006).

->

The apparent location of each stimulus moves from the actually stimulated location toward the other location "in a predictable manner" (e.g., Geldard and Sherrick, 1972; Kilgard and Merzenich, 1995; Cholewiak, 1999; Flach and Haggard, 2006).

L14

The CRE is apparently related to the classic tau effect (Goldreich, 2007), in which the more rapidly traversed of two equal distances defined by three stimuli is perceived as being shorter (Helson, 1930).

->

The tau effect apparently shares its underlying mechanism with the CRE (Goldreich, 2007): when two equal distances by three stimuli are traversed rapidly, the distance could be perceived as being shorter (Helson, 1930).

L17

When stimulus timing is held constant, the perceived distance between two stimuli both underestimates and grows in proportion with the actual inter-stimulus distance (Marks et al., 1982; Cholewiak, 1999).

->

As long as stimulus timing is kept constant, the perceived distance between two stimuli gets longer or shorter depending on the actual inter-stimulus distance (Marks et al., 1982; Cholewiak, 1999).

L22

These effects have been explained on the basis of the hypothetical idea that the sensory system imputes uniform motion to discontinuous dynamic displays; therefore, there is an assumption of constant velocity motion (Jones and Huang, 1982).

->

These effects can be interpreted to mean that the sensory system would understand discontinuous dynamic displays as uniform motion; that is, an assumption of constant velocity motion (Jones and Huang, 1982).

P2, left column

L2

Also, a recent Bayesian perceptual model replicated the CRE by assuming that the brain expects tactile stimuli to move slowly (Goldreich, 2007) since we have evolved to detect the movement of external agents (Leslie, 1995).

->

In addition, a recent study has suggested that the CRE can be simulated by a Beyesian model based on the assumption that the brain expects the slow movements of tactile stimuli (Goldreich, 2007) since we have evolved to detect the movement of external agents (Leslie, 1995).

L10

Certain simple visual displays consisting of moving, 2-D, geometric shapes can give rise to percepts with high level properties, such as causality and animacy. This suggests that just as the visual system works to recover the physical structure of the world by inferring properties such as 3-D shapes, it also works to recover the causal and social structures of the world by inferring properties such as causality and animacy (Scholl and Tremoulet, 2000).

->

Even simple 2-D moving objects can give us some impressions with high-level properties including causality and animacy, suggesting that "just as the visual system works to recover the physical structure of the world by inferring properties such as 3-D shapes, it also works to recover the causal and social structures of the world by inferring properties such as causality and animacy" (Scholl and Tremoulet, 2000).

L20

Given that many natural events can be perceived via multiple sensory modalities, we typically have access to multiple features of those events across different senses (Vroomen and Keetels,2010).

->

Given that many natural events can be perceived via multiple sensory modalities, we can gain access to multiple features of those external events across modalities (Vroomen and Keetels,2010).

L26

The ability to combine information from multiple sensory modalities into a single, unified percept is a key element of organisms' abilities to interact with the external world (Stevenson et al., 2011).

->

We can combine multiple information across different senses into a unified percept, which is fundamentally required in order for us to interact with the external world (Stevenson et al., 2011)

L29

This process of perceptual fusion - the amalgamation of multiple sensory inputs into a perceptual gestalt - is highly dependent on the temporal synchrony of sensory inputs (Meredith et al., 1987; Bishop and Miller, 2009; Stevenson et al., 2011).

->

This perceptual binding or "the amalgamation of multiple sensory inputs into a perceptual gestalt" is highly related to the temporal synchrony among sensory inputs (Meredith et al., 1987; Bishop and Miller, 2009; Stevenson et al., 2011).

L35

The combination of crossmodal information by humans is highly consistent with an optimal Bayesian model of causal inference; this phenomenon is known as "cross-modal causality" (Goldreich, 2007; Beierholm et al., 2009; Schutz and Kubovy,2009).

->

The human's ability to combine cross-modal information could be explained by an optimal Bayesian model of causal inference, a phenomenon is known as "cross-modal causality" (Goldreich, 2007; Beierholm et al., 2009; Schutz and Kubovy,2009).

P2, right column

L6

Indeed, the tactile tau and kappa effects are also susceptible to cross-modal (visual or auditory) influences (Suto, 1952; Russo and Dellantonio, 1989).

->

Indeed, the tactile tau and kappa effects are also modulated under the cross-modal presentation visually or auditorily (Suto, 1952; Russo and Dellantonio, 1989).

L19

Another study also suggested that the illusory somatosensory percepts caused by the CRE can affect the primary somatosensory cortex at a location corresponding to the illusory percept (Blankenburg et al., 2006).

->

Another study also suggested that the illusory somatosensory percepts caused by the CRE can affect the location in primary somatosensory cortex corresponding to the illusory percept (Blankenburg et al., 2006).

P3, left column

L6

This could indicate that our brain is tuned to detect the movement of an external agent on the skin since an essential, evolutionarily stable feature of brain function is the detection of animate entities for survival (Schultz et al.,2005; Pratt et al.,2010).

->

This could indicate that our brain is tuned to detect the movement of an external agent on the skin, since for survival, "an essential evolutionarily stable feature of brain function is the detection of animate entities" (Schultz et al.,2005; Pratt et al.,2010).

L10

Furthermore, we argue that sensory events at a certain time point are influenced by future sensory events; this is referred to as "postdictive" sensation (Eagleman and Sejnowski, 2000).

->

Furthermore, we argue that future sensory events affect past sensory experiences; this is referred to as "postdictive" sensation (Eagleman and Sejnowski, 2000).

L16

They were recruited randomly from an introductory psychology class, and written informed consent was obtained from all participants before the experiments were conducted. All participants reported normal or corrected-to-normal vision, hearing, and somatosensation and no neurological abnormalities.

->

They were recruited randomly from an introductory psychology class, and written informed consent was obtained from all participants before the experiments were conducted. None of participants had abnormalities in vision, hearing, and somatosensation.

P9, left column

L10

The most discriminating tactile sensors among primates - the fingertips - house a few hundred sensory nerve fibers per square cm (Johansson and Vallbo, 1979; Darian-Smith and Kenins, 1980).

->

The fingertips, where in primates the most discriminating tactile sensors are located, have a few hundred sensory nerve fibers per square centimeter (Johansson and Vallbo, 1979; Darian-Smith and Kenins, 1980).

L34

A typical tau effect, where the perceived distance between stimuli underestimates, and grows in proportional with, the actual distance when stimulus timing is held constant (Marks et al.,1982; Cholewiak, 1999) and the kappa effect, in which the perceived time between stimuli dilates as the distance between stimuli is increased (Suto, 1952), reflect just two fundamental perceptual distortions: underestimation of inter-stimulus distance (perceptual length contraction) and overestimation of inter-stimulus time interval (perceptual time dilation; Goldreich, 2007).

->

A typical tau effect, where the perceived distance between two stimuli gets longer or shorter depending on the actual inter-stimulus distance when stimulus timing is held constant (Marks et al.,1982; Cholewiak, 1999), and the kappa effect, in which the perceived time between stimuli increases when the stimuli distance gets longer (Suto, 1952), reflect "just two fundamental perceptual distortions": underestimation of inter-stimulus distance (perceptual length contraction) and overestimation of inter-stimulus time interval (perceptual time dilation; Goldreich, 2007).

L42

Given that three successive tactile stimuli define two spatial (S1 and S2) and two temporal intervals (T1 and T2), the somatosensory system intuitively imputes motion at a given speed to the tactiles and tries to equalize the ratios S1/S2 and T1/T2; thus, it follows that S1/T1 = S2/T2 (modified from Jones and Huang, 1982).

->

Two spatial distances (S1 and S2) and two temporal intervals (T1 and T2) are defined by three successive tactile stimuli. The somatosensory system intuitively interprets these stimuli as motion at a certain speed, equalizing the ratios S1/S2 and T1/T2; thus, S1/T1 = S2/T2 (modified from Jones and Huang, 1982).

L47

In this way, the sensory system - which includes somatosensation, vision, and audition (Cohen et al., 1953; Shore et al., 1998) - attempts to equalize the velocity between the first and second stimuli (S1/T1) and that between the second and third stimuli (S2/T2); this is known as the constant velocity assumption (Goldreich, 2007).

->

In this way, the sensory system - which includes somatosensation, vision, and audition (Cohen et al., 1953; Shore et al., 1998) - tries to equalize the velocity between the ratios S1/T1 and S2/T2; which is the constant velocity assumption (Goldreich, 2007).

P9, right column

L9

Thus, the general tendency is to displace the judged position of a moving target as being relatively far forward along the path of motion (Tremoulet and Feldman,2006; Getzmann and Lewald, 2007, 2009).

->

Thus, the general tendency is that the judged position of a moving object is displaced forward along the direction of motion (Tremoulet and Feldman,2006; Getzmann and Lewald, 2007, 2009).

L31

When participants discriminate the locations of vibrotactile stimuli by ignoring distractor lights, such tactile discriminations are slowed when the distractor light is incongruent with the tactile target (Pavani et al., 2000).

->

When participants discriminate the locations of vibrotactile stimuli by ignoring distractor lights, the reaction time in tactile discriminations is delayed when the distractor light is spatially incongruent with the tactile stimuli (Pavani et al., 2000).

L39

The rubber hand illusion (RHI) refers to the effect of watching a rubber hand being stroked synchronously with one's own, unseen hand. Viewing this for a short time causes the observer to perceptually assimilate the rubber hand into his or her own body (Botvinick and Cohen, 1998).

->

The rubber hand illusion (RHI) is a tactile illusion where watching a rubber hand being stroked synchronously with one's own hand for a short time causes the subjective feeling of illusory body-ownership of the rubber hand or causes the sense of attribution of the rubber hand to participants' own body (Botvinick and Cohen, 1998).

P10, left column

L14

Cross-modal causality plays a key role in governing the integration of sensory information, depending on its ecological plausibility (Schutz and Kubovy, 2009).

->

Causality perception among modalities is fundamental for integrating sensory information, depending on the ecological plausibility (Schutz and Kubovy, 2009).

L17

Humans can use the similarities between the temporal structures of sensory signals in different modalities to solve the correspondence problem, ultimately inferring causation from correlation (Parise et al., 2012).

->

Humans can use the similarities between the temporal structures of sensory signals in different modalities in order to handle the correspondence problem, "ultimately inferring causation from correlation" (Parise et al., 2012).

L30

Illusory sequences activate the contralateral primary somatosensory cortex at somatotopic locations corresponding to the filled-in illusory perceptions on the forearm (Blankenburg et al., 2006); this suggests that this illusion is associated with the early sensory body map represented in S1.

->

Illusory sequences activate the contralateral primary somatosensory cortex (Blankenburg et al., 2006), suggesting an association with the early sensory body map in S1.

L38

Small amounts of latency between vision and touch (or sound) tend to be reduced and go unnoticed, because the timing of visual events is flexible and adjusts immediately (for a review, see Vroomen and Keetels,2010).

->

Small amounts of latency between vision and touch (or sound) tend to be reduced because the timing perception in vision is flexible and adjusts immediately (for a review, see Vroomen and Keetels,2010).

L53

The CRE and its interaction with vision indicate that sensory events at a given time point are influenced by future sensory events; this is called "postdictive" sensation (Eagleman and Sejnowski, 2000).

->

The CRE and its interaction with vision indicate that future sensory events, even if the modalities are crossed, affect past sensory experiences. This is called "postdictive" sensation (Eagleman and Sejnowski, 2000).

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