



# Measuring causal perception: Connections to representational momentum?

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Received 27 October 2005; received in revised form 1 June 2006; accepted 6 June 2006  
Available online 14 August 2006

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## Abstract

In a collision between two objects, we can perceive not only low-level properties, such as color and motion, but also the seemingly high-level property of causality. It has proven difficult, however, to measure causal perception in a quantitatively rigorous way which goes beyond perceptual reports. Here we focus on the possibility of measuring perceived causality using the phenomenon of representational momentum (RM). Recent studies suggest a relationship between causal perception and RM, based on the fact that RM appears to be attenuated for causally ‘launched’ objects. This is explained by appeal to the visual expectation that a ‘launched’ object is inert and thus should eventually cease its movement after a collision, without a source of self-propulsion. We first replicated these demonstrations, and then evaluated this alleged connection by exploring RM for different types of displays, including the contrast between causal launching and non-causal ‘passing’. These experiments suggest that the RM-attenuation effect is not a pure measure of causal perception, but rather may reflect lower-level spatiotemporal correlates of only some causal displays. We conclude by discussing the strengths and pitfalls of various methods of measuring causal perception.

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*PsycINFO classification:* 2323

*Keywords:* Causal perception; Representational momentum; Event perception; Michotte

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## 1. Introduction

In the study of physics, billiard-ball collisions are often considered to be among the simplest possible examples of certain laws of Newtonian mechanics in practice. In the study of psychology, however, even these simple events raise interesting problems. Imagine viewing such an event: a cue ball strikes a target ball, causing it to move toward a pocket. There are several ways in which even this description seems too simple. First, it is just as valid from a physical point of view to say (or to see!) that the target ball caused the cue ball to stop moving (or to slow down) as it is to say that the cue ball caused the target ball to move – yet our natural apprehension of this event seems much more driven by the latter aspect of this symmetric connection (White, 2006a, 2006b). A more troubling problem, perhaps, is that one may argue that we are unjustified in perceiving any causal connection at all between these two events. We might instead think, following Hume (1748/1977), that: “Motion in the second Billiard-ball is a quite distinct event from motion in the first; nor is there any thing in the one to suggest the smallest hint of the other” (p. 18).

It seems, however, that our visual systems do not traffic in such abstract problems, since our actual percepts of such events are typically unambiguous and even irresistible: we simply see the cue ball cause the target ball to move. This observation turns out to have deep implications for the nature of visual perception. Vision scientists typically think of perception in terms of the recovery of stimulus features such as color, shape, and motion, which seem much more concrete than causality. The insight that perception may also traffic in abstract categories such as causation is due to the work of Albert Michotte, as summarized in his seminal book, *The Perception of Causality* (1946/1963). As noted in this same issue by Wagemans, van Lier, and Scholl (2006), Michotte had a formidable talent for taking straightforward and somewhat obvious observations (such as that we see partially occluded objects to *be* partially occluded wholes) and both (1) recognizing the often-deep psychological implications of such observations, and (2) figuring out how to study them experimentally.

Nowhere is this more true than in the study of causal perception, where Michotte’s original work reported more than 100 studies of this phenomenon, and was judged at the time to have “an epoch-making flavour” (Oldfield, 1949, p. 104). Through this work Michotte determined several of the principles that give rise to causal perception, and perhaps his primary conclusion was simply that such principles exist: our causal impressions from even very simple visual events are largely automatic, nearly irresistible, and driven by highly constrained and stimulus-driven visual cues. This conclusion continues to be supported in many ways today (see Scholl & Tremoulet, 2000), and is also consistent with other evidence concerning the universality of causal perception across cultures (e.g., Morris & Peng, 1994), the observation that even young infants perceive causality (e.g., Leslie & Keeble, 1987; see Saxe & Carey, 2006 for discussion), and the possibility that causal perception may recruit specialized neurophysiological circuits (e.g., Blakemore et al., 2001; Fugelsang, Roser, Corballis, Gazzaniga, & Dunbar, 2005).

### 1.1. Recent work on causal perception

Michotte’s early studies of the rules underlying the perception of causality focused on the role of various lower-level visual factors involved directly in the putative causal event, such as the absolute and relative speeds of the objects in a collision, the distance and direction

in which they traveled both before and after impact, the influence of spatial and temporal gaps in the trajectories, etc. Many recent studies have continued this project, exploring how the perception of causality is affected by different types of motion (e.g., apparent motion; Gordon, Day, & Stecher, 1990), spatiotemporal gaps (e.g., Schlottmann & Anderson, 1993; Schlottmann, Ray, Mitchell, & Demetriou, 2006), and influences from both other modalities (e.g., Guski & Troje, 2003) and from higher-level expectations (e.g., White, 2005). Other recent studies have begun to show both how causal perception in individual events is influenced by other contextual properties of scenes, and also how causal perception is related to other types of visual processing such as Gestalt grouping and attention (Choi & Scholl, 2004, 2006a; Scholl & Nakayama, 2002, 2004).

### 1.2. *Measuring causal perception*

One reason why causal perception is so fascinating is its phenomenal appeal: you can simply see various effects on the perception of causality in the relevant displays without requiring a complex statistical analysis to be convinced. (This appeal was also characteristic of many of Michotte's discoveries, involving phenomena such as amodal completion and the 'tunnel effect' – as is illustrated and discussed in many of the other contributions to this special issue – e.g., Bertamini & Hulleman, 2006; Fulvio & Singh, 2006; Kawachi & Gyoba, 2006; Van Lier, de Wit, & Koning, 2006.) This phenomenal character can also be a downside, however, especially when it is the *only* source of data. Most studies of causal perception, including those of Michotte, have depended on explicit verbal reports of perceptual experience, in the form of either protocols (“Tell me what you saw”) or ratings (“How causal did the event look, on a scale from 1 to 10?”). This is perhaps the most direct way to assess perceptual experience, and it can yield robust results (as illustrated, for example, by Schlottmann et al., 2006). However, this method also has several problems.

First, explicit reports are always sensitive in principle to extra-perceptual factors, and one of the most serious concerns is that verbal reports reflect not only what subjects are *seeing* but also their higher-level interpretations and judgments. This is an especially acute issue for perceptual studies, perhaps, due to the ambiguity and flexibility of words such as “see” and “perceive” (e.g., Dretske, 1995). In the context of causal perception, the worry is that perceptual reports may be contaminated by subjects' intuitions and reasoned judgments about whether an event *should* be interpreted as causal. This problem becomes all the more salient when observers must simply make forced-choice judgments (causal vs. non-causal) on repeatedly displayed stimuli which differ only along a few obvious dimensions. This problem has been salient ever since the publication of Michotte's book, with critics accusing him of failing “to guard against the possible effects of prior knowledge and suggestion” (Joynson, 1971, p. 295).

A related problem is that perceptual reports are not quantitatively sensitive, and are typically unable to discriminate fine differences in the strength of perceived causality that may still be informative if captured in other ways. For example, certain contextual effects on causal perception are acutely sensitive to the precise strength of the underlying grouping cues in both space (Choi & Scholl, 2004; Scholl & Nakayama, 2002) and time (Choi & Scholl, 2006a), but these differences are not readily captured in individual perceptual reports. Rather, this work must typically employ ambiguous baseline events, with subtle differences then revealed in the likelihood across many trials of a given event being seen as causal or non-causal – even if each distinct percept is itself fully one or the other.

These problems illustrate the need for more rigorous, quantitative, and indirect methods for measuring causal perception. Though the use of the perceptual reports can be optimized and used rigorously, it would be helpful to have converging evidence from other types of performance measures – as has been the case with other phenomena such as visual completion (where perceptual results have often later been verified and quantified using measures such as masking and visual search; e.g., Rauschenberger & Yantis, 2001; see also Fulvio & Singh, 2006, for an example in this issue). Ideally, such methods should be indirect – so as to measure causal perception without asking directly about causation at all – and also capable of discriminating the strength of causal percepts. And, of course, such methods must measure causal perception, *per se*, rather than other indirectly correlated aspects of some such displays. In this paper we evaluate the possibility of satisfying these goals by measuring causal perception in terms of its effects on representational momentum.

### *1.3. Representational momentum and causal perception*

Representational momentum (RM) is a phenomenon wherein memory for the final position of a moving target (that has suddenly disappeared) is displaced in the direction of implied motion – as if the internal representation had continued to move for a small distance over an extrapolated path. The phenomenon has proven controversial in some ways; for example, there is an ongoing debate about the degree to which RM sometimes reflects (only) differential eye movements (e.g., Hubbard, 2006; Kerzel, 2006). Nevertheless, since Freyd and Finke's initial discovery of this phenomenon (1984), many other studies have verified the existence and generality of such illusory displacement (for recent reviews, see Hubbard, 2005; Thornton & Hubbard, 2002). Initial studies of RM were far removed from causal events since they typically involved only a single object. More recent studies of RM, however, have begun to explore the effect of additional objects or movements on illusory displacement (e.g., Hayes & Freyd, 2002; Hubbard, 1993; Hubbard & Ruppel, 1999; Kerzel, 2003; Whitney & Cavanagh, 2002).

One recent study, in particular, examined RM in the context of Michotte's launching effect, which is similar to the collision of two billiard balls (Hubbard, Blessum, & Ruppel, 2001). In this event a single disc (A) moves toward a stationary disc (B) until they are adjacent, at which point A stops and B starts moving along the same path (see Fig. 1a). We perceive this type of event as a 'launch' or a collision: A smashes into B, causing its motion. (Dynamic animations of many of the conditions employed in this paper can be viewed online via links from the website <http://www.yale.edu/perception/>.) Hubbard and colleagues measured the amount of illusory displacement in this event, when B was launched by A, and then later disappeared suddenly. In this situation, the illusory displacement (based on subjects' estimates of the location at which B disappeared) was attenuated, compared to several cases when B simply traversed the display in isolation (either appearing suddenly, or moving across the entire screen in one of several speeds). This relationship is depicted in Fig. 2. This attenuation of RM for causal launching was explained by appeal to the hardwired visual expectation that without a continuous supply of energy to sustain its motion, a 'launched' object should eventually stop because of its inert nature. In other words, subjects were more accurate at identifying the location at which B disappeared because they "believed impetus from the launcher [i.e., A] was imparted to the target [i.e., B] and then dissipated" (p. 294). A moving object in isolation, in contrast, is seen as self-propelled rather than as inert (see also Hubbard & Ruppel, 2002).

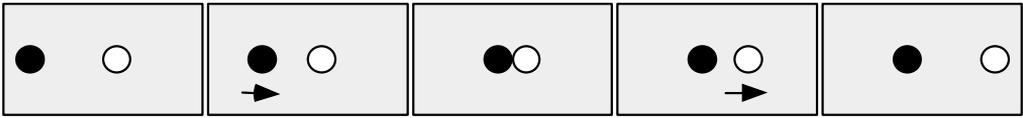
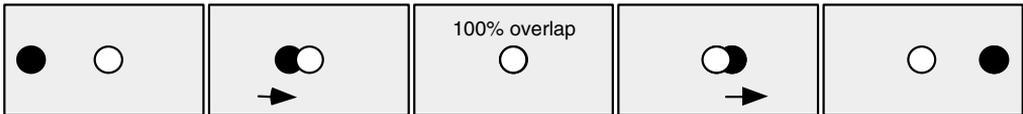
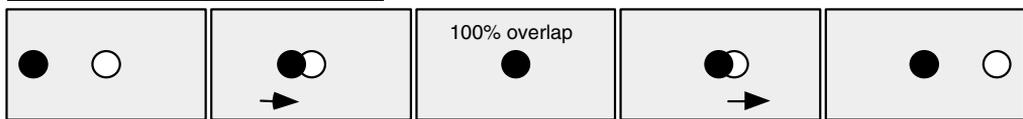
**(a) Causal launching****(b) Non-causal Passing****(c) A bistable Full-overlap event**

Fig. 1. Three standard visual events employed in studies of causal perception. These events are presented as a sequence of static frames, with time increasing toward the right. Arrows indicate motion. (a) A causal *Launching* event, wherein one disc is seen to collide with a second disc, causing it to move. (b) A non-causal *Passing* event, wherein one moving object passes under another stationary disc. (c) A bistable *Full-overlap* event, which observers often perceive as non-causal passing, despite the color change. See text for details.

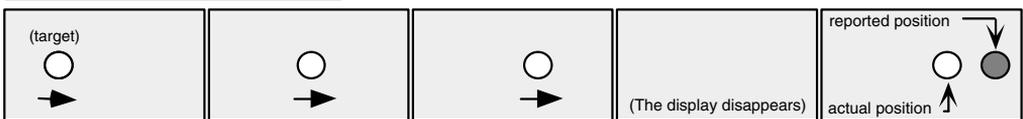
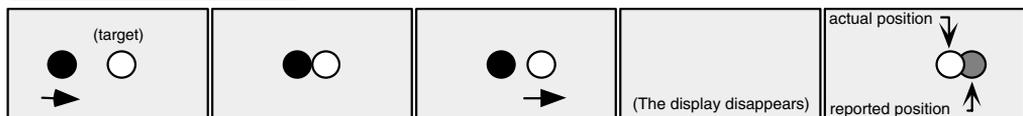
**(a) RM in the typical display****(b) RM in a causal event**

Fig. 2. Depiction of a basic assumption about the relationship between representational momentum and causal perception, as derived from Hubbard et al. (2001). The magnitude of RM is expected to be attenuated (b) when the target is launched by another object relative to (a) when a single moving target is presented.

This previous study explored the relationship between RM and causal perception, but it was not intended primarily to develop a way of measuring perceived causality; rather, the point of this study was to better understand the nature of RM. However, the clear implication of this result is that RM may serve as an indirect and quantitatively precise measure of causal perception: in such an experiment one can ask subjects simply about where an object disappeared (without ever mentioning causality), and the dependent measure is continuous in nature (in terms of the distance between the perceived and actual offset locations).

#### 1.4. The current experiments

In the experiments reported below we critically evaluate the possibility that RM could be used to measure causal perception. In particular, we focus on the possibility that the observed connection between these phenomena may in fact be due not to the perception of causality, *per se*, but rather to lower-level spatiotemporal correlates of (only) some causal displays.

Hubbard et al. (2001; see also Hubbard and Ruppel, 2002) contrasted causal launching with several other non-causal control displays: (1) a *Target Only* condition, in which neither A nor B was initially presented, but B suddenly appeared at the moment it would have begun moving due to the collision, and then proceeded to move in the same manner; (2) a *Slow Total* condition in which a single item traversed the entire distance at the speed of the target (B) in the launching effect; (3) a *Fast Total* condition in which a single item traversed the entire distance at the speed of the launcher (A); and (4) a *Deceleration* condition in which a single object traversed the display, slowing when it reached the center. In addition, a later study by Hubbard and Favretto (2003) used a *Spatial Gap* control, which was identical to the launching effect except that B began moving (and A stopped) before A reached B's initially stationary position. All of these controls yielded more RM than the launching effect, which Hubbard and colleagues interpret as an effect due to perceived causality.

All of these control displays, however, differ from the launching effect in a lower-level manner which is not purely constitutive of causal relationships: the launching effect is the only one of these displays which involves *two distinct objects but a single spatiotemporally continuous motion*. In this paper we report three experiments which explore and support the idea that it is this indirect – and only occasional – correlate of causal perception which is responsible for the differing degrees of RM in these contrasts. As a result, the presence of causal perception is neither necessary nor sufficient to produce this attenuated RM.

In Experiment 1 we first successfully replicated the contrast between causal and non-causal events in terms of their effects on RM, using the same parameters and contrasts as in Hubbard et al. (2001). In Experiment 2 we contrasted the influence on RM of causal vs. non-causal displays when these cases both included two objects and a single spatiotemporally continuous motion. We did so by employing the contrast between launching and non-causal *passing* that has been used in several other recent papers (Choi & Scholl, 2004, 2006a; Scholl & Nakayama, 2002, 2004). This contrast failed to produce any reliable difference in RM, both when using the same stimulus parameters as in previous studies with such displays (Experiment 2a) and when using the same stimulus parameters as in Hubbard et al.'s (2001) original study (Experiment 2b). In Experiment 3 we then eliminated all visual differences when contrasting causal vs. non-causal displays by using an auditory cue to promote causal perception, but again we failed to find any resulting effects on RM. We conclude that some causal displays may influence RM for other reasons, but that RM cannot be used as a general measure of causal perception.

## 2. Experiment 1: replicating an effect of causality on RM

We first attempted to replicate the study of Hubbard et al. (2001) using that subset of their control conditions which most clearly indicated a role for causal perception. We measured the effects on RM of five different conditions, as described above: *Launching*, *Target*

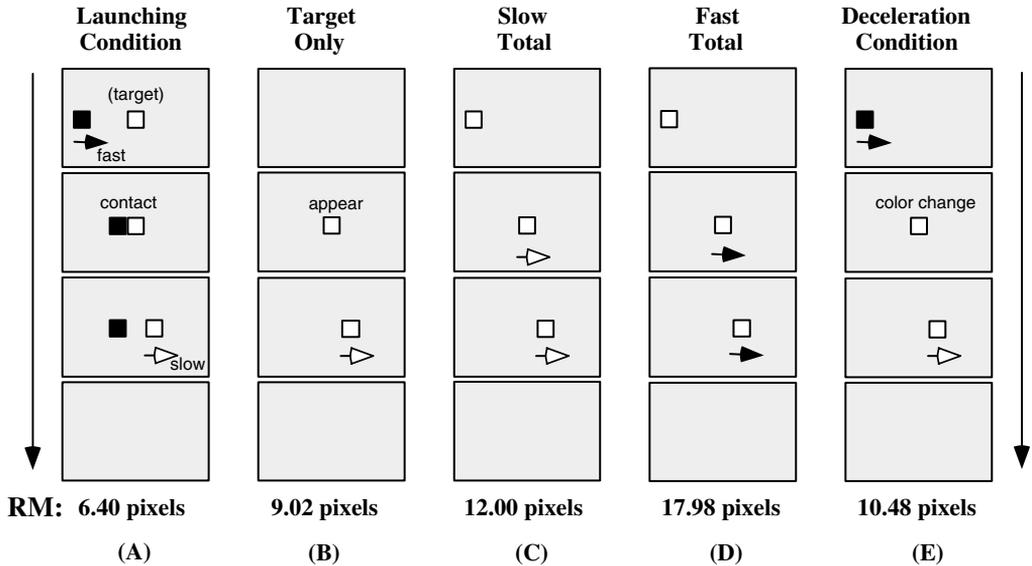


Fig. 3. Depiction of the five conditions used in Experiment 1 (in a partial replication of Hubbard et al., 2001) and the resulting magnitudes of representational momentum. Arrows indicate motion; the black arrow indicates the fast velocity, and the white arrow indicates the slow velocity (see text for details).

*Only*, *Slow Total*, *Fast Total*, and *Deceleration* (see Fig. 3). Following the study of Hubbard and colleagues, we predict that the amount of RM obtained for the final motion in the Causal Launch condition will be significantly smaller than that obtained in any of the other control conditions.

## 2.1. Method

We designed this study to be as similar as possible to the methods used by Hubbard et al. (2001).

### 2.1.1. Participants

Twelve naïve Yale students participated for course credit or a small monetary payment. All observers had normal or corrected-to-normal acuity and normal color vision.

### 2.1.2. Apparatus

All displays were presented on a Macintosh iMac computer. Observers were positioned approximately 60 cm from the monitor, without head restraint, so that the display subtended 27 by 20 deg of visual angle. The display refreshed at 117 Hz, and motion was always perceptually smooth. All displays were constructed using custom software written with the use of the VisionShell graphics libraries (Comtois, 2005). All experiments were conducted in a dimly lit room.

### 2.1.3. Stimuli

Each trial involved a single visual event, consisting of either one object (the ‘target’), or two objects (a ‘launcher’ and the target). Both the launcher and target shapes were small

0.84 deg squares, centered vertically on a white background. The launcher was a solid black square and the target was a outlined black square with a stroke of 1 pixel. Motion was always on the horizontal plane, since the perception of causality is weaker in other orientations (Michotte, 1946/1963). The motion proceeded from the left to the right in half of the trials, and the right to the left in the other half of the trials.

We tested five conditions, as depicted in Fig. 3. In the initial display of the *Launching* condition, the target was presented near the center of the display for 1 s, with its left edge 15 deg (when the motion was rightward) from the rightmost display border or 11 deg (when the motion was leftward) from the leftmost display border. The launcher then emerged gradually from either the right or left display border and proceeded to move at 15 deg/s toward the target. At the moment at which the launcher and target became adjacent, the launcher ceased moving, and the target began moving at 5 deg/s in the same direction as the launcher had been moving. After moving for 250 ms in this fashion (traversing 1.26 deg), the entire display was extinguished. The *Target Only* condition was identical to the *Launching* condition except that the launcher was not displayed: the display began with neither object on the display, and the target appeared suddenly as soon it began moving (i.e., as soon as the target would have arrived adjacent to it, had it been drawn). In both the *Slow Total* and *Fast Total* conditions, a single target smoothly traversed the entire distance of the combined path of the launcher and the target from the *Launching* condition at either the same velocity as the target (in the *Slow Total* condition) or the launcher (in the *Fast Total* condition). Finally, in the *Deceleration* condition, a single target identical to the launcher emerged from either the left or right edge of the screen and traveled across the entire combined paths of the objects in the *Launching* condition, moving initially at the velocity of the launcher in the *Launching* condition, then suddenly slowing to the velocity of the target in the *Launching* condition when it reached the corresponding point near the center of the display (at which point it changed to look like the target).

#### 2.1.4. Procedure

Observers began each trial by pressing a key, after which the animation for the relevant condition was presented. As soon as the target disappeared (when the entire display was extinguished), a cursor (drawn as a small plus sign, with a stroke of 1 pixel) appeared in the center of the display and was allowed to move horizontally. Observers used the mouse to position the cursor at the exact horizontal location where they had seen the target disappear. (We used a plus sign as the cursor following the original study of Hubbard and colleagues, and simply instructed subjects to center the cursor on the position where the target square had disappeared.) Once satisfied with the cursor placement, observers clicked the mouse button to record their response, and then pressed a key to advance to the next trial.

Observers first completed a small number of practice trials, the data from which were not recorded. During these practice trials, observers were familiarized with each of the conditions employed in this experiment. Observers then completed 20 trials in each of the five conditions, for a total of 100 trials presented in a random order, unblocked.

## 2.2. Results and discussion

RM was measured as the difference (in pixels) between the actual and perceived locations where the target disappeared. The degree of RM obtained for each condition is presented in Fig. 3, where a positive score indicates the perceived disappearance location

was extrapolated beyond the actual location, in the direct of the target motion. As is clear from this figure, less RM was obtained for the Launching condition, relative to the other control conditions. These impressions were verified with a single-factor repeated measures ANOVA, which revealed a significant effect of condition ( $F(4,44) = 28.21, p < .001$ ). Planned comparisons indicated that the magnitude of RM in the Launching condition was significantly less than in each other condition (vs. Target Only:  $t(11) = 4.15, p = .002$ ; vs. Slow Total:  $t(11) = 4.39, p = .001$ ; vs. Fast Total:  $t(11) = 8.95, p < .001$ ; vs. Deceleration:  $t(11) = 5.10, p < .001$ ). These results are consistent with those of Hubbard et al. (2001), and with the hypothesis that objects seen to be causally ‘launched’ will give rise to less RM.

### 3. Experiment 2a: Launching vs. Passing

Attenuated RM was observed in Experiment 1 for only that display which was perceived in terms of causal ‘launching’. Nevertheless, it remains possible that perceived causality, per se, had nothing to do with this effect. Instead, attenuated RM could have been due to a confounded factor: of all of the conditions tested in Experiment 1 (and of some additional controls also used in Hubbard et al., 2001), the Launching condition was the only one which involved two distinct objects but a single spatiotemporally continuous motion. In this experiment we thus directly unconfounded this factor from causal perception in order to determine which was responsible for the attenuated RM in the Launching condition. (In addition, as explored below in Section 6, there are some hints in the existing RM literature which suggest ways in which RM may be affected by both the number of objects and the spatiotemporal continuity of a trajectory.)

Conveniently, other recent work on perceived causality has employed a comparison – between *causal launching* and *non-causal passing* – which readily allows these factors to be unconfounded (Choi & Scholl, 2004, 2006a; Scholl & Nakayama, 2002, 2004). Launching events in this contrast proceeded exactly as described above, with object A moving toward an initially stationary object B, and with A stopping and B beginning to move along the same trajectory at the moment they become adjacent. Such displays, as usual, are perceived in unambiguously causal terms. The non-causal *Passing* event began with the same display, but now A simply traversed the entire display, momentarily passing under an always-stationary B (Fig. 1b). This display is not perceived in causal terms, and indeed is not seen to involve any interaction at all. Critically, however, both the Launching and Passing events involved two objects and a spatiotemporally continuous motion.

In this experiment, we first tested the influence on RM of these conditions when they employed objects of similar sizes and speeds to the many other studies of such displays which have yielded robust differences in causal perception (Choi & Scholl, 2004, 2006a; Scholl & Nakayama, 2002, 2004). If RM is influenced by causal perception per se, then there should be less forward displacement in the Launching condition than in the Passing condition.

#### 3.1. Method

This experiment was identical to Experiment 1 except as noted here. Twelve naïve Yale students participated, none of whom had participated in Experiment 1. Each event involved two colored discs, each subtending 2.27 deg, drawn in either bright red (disc A) or green (disc B) on a black background. The Launching event (see Fig. 1a) proceeded as

follows. Disc A was initially presented near either the right or the left edge of the display, so that its most extreme edge was 1.26 deg from the border. Disc B appeared near the center of the screen, with its left edge 10.5 deg from the leftmost display border when the motion was rightward. After 500 ms, A began to move at 17 deg/s toward B. When A and B were adjacent, A stopped and B started moving at the same velocity as A. The entire display was then extinguished after a variable delay, after B had moved either 70, 140, 210, or 280 pixels (173, 346, 519, or 692 ms, respectively). (We varied this factor not for any theoretically motivated reason, but only to maximize the likelihood of observing RM, since the latency to vanish has been found to affect the magnitude of RM in some other situations; e.g., Jordan, Stork, Knuf, Kerzel, & Müssele, 2002. In addition, the distances employed in the original study by Hubbard and colleagues struck us as unusually short, and certainly much shorter than most causal perception displays.) The Passing event was identical except that B never moved; instead, A simply traversed the total path distance from the Launching condition, passing under B for a moment during its trajectory. The final moving object (now object A) then stopped moving when the entire display was extinguished after the same durations and delays as tested in the Launching condition, now measured from the moment of complete overlap of A and B, rather than from the moment at which they became adjacent.

Subjects judged the position of the final moving object after the display extinguished using the same method as in Experiment 1, except that the cursor was now presented as a bright blue disc (the same size as the moving objects), and which simply had to be positioned at the exact location in which the final moving object had just disappeared. Observers completed 20 trials in each of the eight conditions – two causality conditions (launching vs. passing)  $\times$  4 possible distances – for a total of 160 trials presented in a random order, unblocked.

### 3.2. Results and discussion

The degrees of RM obtained for launching and passing are presented in Fig. 4. As is clear from this graph, roughly equal magnitudes of RM were obtained for each condition. This impression was confirmed with a repeated-measures ANOVA conducted with causality (Launching vs. Passing) and Distance (70, 140, 210, 280) as within-subject factors, which revealed a significant main effect of distance ( $F(3, 33) = 18.69, p < .001$ ), but no main effect of causality ( $F(1, 11) < .01, p = .975$ ), and no reliable interaction ( $F(3, 33) = 2.42, p = .084$ ). These results are inconsistent with those of Experiment 1 and those of Hubbard et al. (2001), assuming that causal perception affects RM. On the other hand, if the difference in Experiment 1 was due to the confounded factors involving spatiotemporal continuity and the number of objects in the display, then no differences should have been found here, despite the differences in causal perception.

## 4. Experiment 2b: Launching vs. Passing with original stimulus parameters

Though we did not observe attenuated RM for causal displays in Experiment 2a, our displays differed in several other respects from those of Experiment 1 (and from Hubbard's original experiments): we used different object sizes, colors, speeds, and durations of motion. These design choices were made primarily to equate the stimulus parameters to those previous studies of causal perception that used these same manipulations. Thus, at a

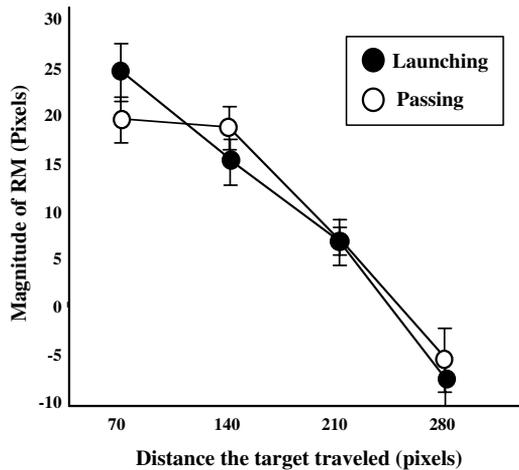


Fig. 4. Depiction of the magnitude of representational momentum in Experiment 2a for the two causality conditions (causal launching vs. non-causal passing) as a function of the four distances which the target traveled. Error bars represent one standard error of the mean.

minimum, the results of Experiment 2a demonstrate that RM attenuation will not always track causal perception, even using robust contrasts between causal vs. non-causal displays (Choi & Scholl, 2004, 2006a). (This already has important implications for our project, of course, since it renders a null effect of RM attenuation as ambiguous for the purpose of measuring causal perception: such a null result could mean that there was no causal perception, or that there was causal perception in a display whose other parameters were not conducive to RM attenuation.) However, it is possible that causal perception, per se, could still influence RM – even controlling for the number of objects and the existence of spatio-temporal continuity – if the stimulus parameters more closely matched those of Experiment 1 and Hubbard et al. (2001). Thus, in this experiment we replicated Experiment 2a using stimulus parameters which were matched as closely as possible to those which yielded robust results in Experiment 1.

#### 4.1. Method

Twelve naïve Yale students participated. In fact, to maximize the utility of comparisons to Experiment 1, these were the same 12 subjects who had participated in Experiment 1, and the order of their participation in the two experiments was counterbalanced. (We present the resulting data in two separate experiments for rhetorical purposes.) This experiment tested the same conditions as Experiment 2a but with the different parameters noted here (chosen to be as matched as possible to Experiment 1). Both objects tested in each trial were drawn with the same speeds, sizes, shapes, and colors (now black and white, instead of red and green) as used in Experiment 1. Both the Launching and Passing conditions employed three possible velocity profiles: (a) a fast initial motion (of the launcher, for the Launching condition, or the target for the Passing condition) and a fast final motion (of the target, in both conditions), (b) a fast initial motion and a slow final motion, and (c) a slow initial motion and a slow final motion. We also now varied when the display was extinguished: after the target had traveled 60 pixels (on half of the trials), or after it had

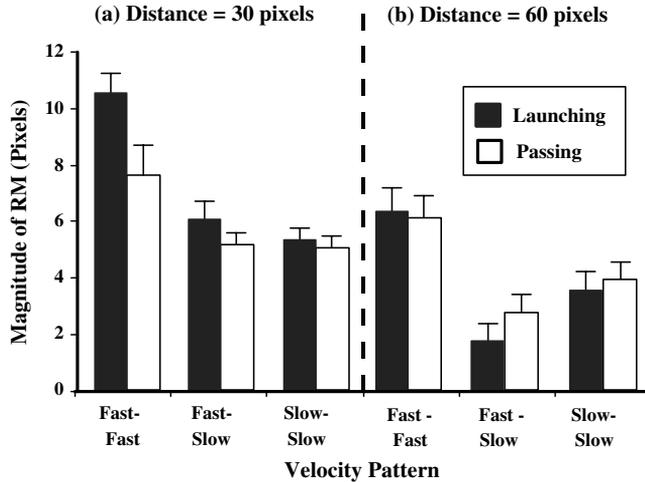


Fig. 5. Depiction of the magnitude of representational momentum in Experiment 2b for the two causality conditions (causal launching vs. non-causal passing) as a function of the three velocity patterns (a) when the target traveled 30 pixels, and (b) when the target traveled 60 pixels. Error bars represent one standard error of the mean.

traveled 30 pixels (on the other half, as in Experiment 1). Observers completed 20 trials of each of the 12 possible conditions – two causal conditions (Launching or Passing)  $\times$  two target distances (30 or 60 pixels)  $\times$  three velocity profiles (fast–fast, fast–slow, or slow–slow) – for a total of 240 trials, presented in a random order, unblocked.

#### 4.2. Results and discussion

The degree of RM obtained for each condition is presented in Fig. 5. As is clear from this graph, roughly equal magnitudes of RM were obtained for launching and passing. This impression was confirmed with a repeated-measures ANOVA conducted with the three factors mentioned above (causality condition, distance traveled, and velocity pattern). This analysis revealed significant main effects of distance ( $F(1, 11) = 9.99, p = .009$ ) and velocity pattern ( $F(2, 22) = 14.50, p < .001$ ), but not of perceived causality ( $F(1, 11) = 1.50, p = .246$ ). All three two-way interactions were also significant: distance  $\times$  perceived causality ( $F(1, 11) = 5.36, p = .041$ ), distance  $\times$  velocity pattern ( $F(2, 22) = 3.44, p = .050$ ), and perceived causality  $\times$  velocity pattern ( $F(2, 22) = 3.87, p = .036$ ). In further planned comparisons which directly contrasted the RM obtained during Launching vs. Passing when matched for distance and velocity pattern, we observed no significant differences (all  $ps > .151$ ), except for trials using a fast/fast velocity profile with a target distance of 30 pixels ( $t(11) = 2.83, p = .016$ ). In this condition, however, the means were actually in the opposite direction than was predicted by Experiment 1: there was slightly more RM for the Launching condition, compared to the Passing condition.<sup>1</sup>

<sup>1</sup> This result continues to strike us as puzzling, and we have no straightforward explanation for it. It deserves follow-up study, however, in experiments focused more directly on the nature and limits of RM.

These results are again inconsistent with those of Experiment 1 and those of Hubbard et al. (2001), but they are similar to those of Experiment 2a. Collectively, these results suggest that RM does not always track perceived causality, even when the stimulus parameters are well matched to those in previous successful experiments. Together, Experiments 1, 2a, and 2b are consistent with the idea that RM is not selectively attenuated by perceived causality at all, but rather by the conjunction of other stimulus parameters, namely the existence of spatiotemporally continuous motion in a display with two objects.

### 5. Experiment 3: audiovisual causal perception

In this final experiment we attempted once more to isolate the role of perceived causality in attenuating RM. We again controlled for the number of objects in the display and the existence of spatiotemporal continuity, but in addition we now controlled for every possible visual difference between the displays. We did so by exploiting a multimodal effect on causal perception, whereby certain sounds (presented at certain times) can cause an otherwise ambiguous ‘full-overlap’ event to be perceived as a causal launch.

Audiovisual effects of this type have their origin in more complex ‘bouncing vs. streaming’ displays, wherein (for example) two objects move toward each other from opposite sides of a display until they overlap completely, at which point two objects head away from each other to return to the initial positions. This display is bistable, and can be perceived either as ‘streaming’ (wherein two objects simply stream past each other, each traversing the entire display in opposite directions) or as ‘bouncing’ (wherein two objects meet at the center, then retrace their paths back to their respective starting locations). A single sharp sound presented during this motion can reduce the ambiguity, yielding robust perceived ‘bouncing’ – but only if the sound happens in close temporal proximity to the moment of full overlap (Sekuler, Sekuler, & Lau, 1997; Watanabe & Shimojo, 2001). A similar effect occurs in Full Overlap events (see Fig. 1c), which are identical to Launching events except that A and B fully overlap before A stops and B starts its motion. This display is normally ambiguous between perceived causal launching and non-causal passing (Choi & Scholl, 2004; Scholl & Nakayama, 2002), but a sharp noise played at the moment of full overlap can result in robust perceived launching (cf. Guski & Troje, 2003).

In this experiment we thus employed only full-overlap events (Fig. 1c), but we varied when a sharp noise occurred during the motion: either before, during, or after the moment of full overlap. We measured both (1) the strength of the resulting causal percepts, and (2) the RM resulting from the final moving object when the display was extinguished. Based on previous work, we expect differences in the proportion of perceived causal launching depending on when the noise occurred. By comparing differences in causal perception to the resulting differences in RM, we can thus isolate the impact of perceived causality on RM, divorced from lower-level visual stimulus differences.

#### 5.1. Method

Twelve naïve Yale students participated, none of whom had participated in the previous experiments. Each trial involved two objects which were identical to those from Experiment 2a. Each trial involved a Full-Overlap event that was identical to the Launching display of Experiment 2a except that the initially stationary object (B) started its motion (and

the initially moving object A stopped) after the two objects became fully overlapped (instead of adjacent). All motion occurred at 11 deg/s, and the display was extinguished after the final motion had proceeded 140 pixels away from the moment of full overlap. The exact horizontal position of the initially stationary object (B) was varied so that the final offset position was not constant: the location of B was randomized such that its left edge appeared anywhere from 11.17 to 15.7 deg from the left display edge.

A sudden ‘Pop’ noise – actually a 56-ms digitized recording of two billiard-balls colliding – was played over the computer’s loudspeaker at the moment of full-overlap (on *On-Time* trials), 300 ms before this moment (on *Early* trials) or 300 ms after this moment (on *Late* trials). No sound was presented on additional *Silent* trials. In this experiment, observers made two responses on each trial: in addition to the mouse-click estimate of the location of the final moving object at the time of the display offset (as in Experiments 1, 2a, and 2b), they also reported (via a keypress for each trial) whether they had seen the event as causal launching or as non-causal passing. (In other words, if RM did in fact reliably track differences in causal perception, then this experiment would be measuring causal perception both directly and indirectly.) Observers completed 20 trials of each of the four conditions, for a total of 80 trials, presented in a random order, unblocked.

## 5.2. Results and discussion

Fig. 6 presents both the magnitude of RM and the percentage of trials that were perceived as causal launching, for each of the four conditions. Two results are immediately clear from this graph: (1) as in previous studies with similar displays (e.g., Guski & Troje, 2003), the different sound conditions had a considerable effect on perceived causality, but (2) these robust differences did not have corresponding effects on RM. These impressions were verified via the following analyses. Single-factor repeated measures ANOVAs revealed a significant effect of the auditory condition on the percentage of perceived causality ( $F(3,33) = 5.63$ ,  $p = .003$ ), but not on the magnitude of RM ( $F(3,33) = 1.34$ ,  $p = .279$ ). Further specific comparisons between the two most extreme cases (*On-Time* vs. *Silence*) similarly revealed a considerable effect on perceived causality ( $t(11) = 4.35$ ,

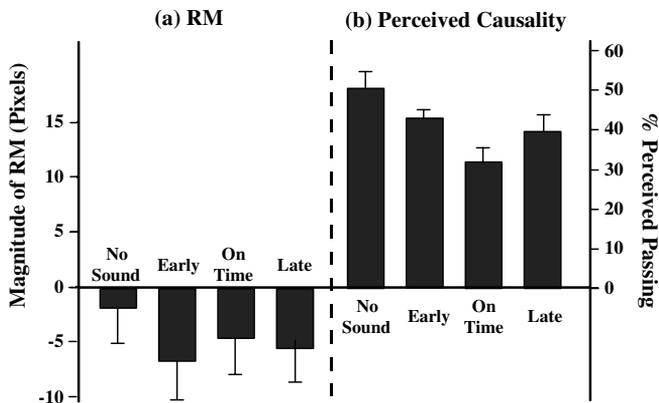


Fig. 6. Depiction of (a) the magnitude of representational momentum and (b) the percentage of perceived non-causal passing in each condition of Experiment 3. Error bars represent one standard error of the mean.

$p = .001$ ), but not on RM ( $t(11) = 1.62$ ,  $p = .134$ ). In fact, there was no reliable effect of RM at all in this context, and the small amounts of displacement observed were actually in the opposite direction (a finding that has been observed elsewhere in complex displays (e.g., Müsseler, Stork, & Kerzel, 2002). These results thus provide another case study wherein perceived causality can differ robustly, but will nevertheless fail to have any corresponding effect on the magnitude of RM. Conversely, this study provides another example of why the existence (or lack thereof) of RM cannot be used to index the presence of causal perception.

## 6. General discussion

The central empirical conclusion of this study is that representational momentum appears not to track causal perception per se, but is rather affected by lower-level correlates of only some causal displays. We obtained evidence for this conclusion from four experiments. We first successfully replicated the previous demonstration of attenuated RM for causally launched objects (Hubbard et al., 2001), compared to related non-causal displays (Experiment 1). However, we next discovered that this effect does not hold for other stimulus comparisons (e.g., causal ‘launching’ vs. non-causal ‘passing’) that give rise to equally robust differences in perceived causality (Experiment 2a), even when tested with the same display parameters – and indeed, the same subjects, tested via the same method in the same session (Experiment 2b) – as in the successful replication. Finally, we also failed to find any effect on RM of displays which differed robustly in their perceived causality without any lower-level attendant visual differences, because of an audiovisual interaction (Experiment 3).

### 6.1. Measuring causal perception with RM?

Why did we observe the predicted influence on RM of causal perception in Experiment 1, but not in the later experiments? We suggest that these RM results are all consistent if interpreted not in terms of causal perception, but in terms of a lower-level factor which tracked the causal vs. non-causal difference in previous studies. In Experiment 1 (and in Hubbard et al., 2001), only the Causal Launching condition possessed two objects and a continuous motion; all of the controls involved only a single object, or two objects and a discontinuous motion. In Experiments 2 and 3, however, all of our displays – causal and non-causal alike – employed two objects and a single continuous motion. We thus suggest that RM may be attenuated for such displays in (at least some) situations involving two objects and a single continuous motion, but not otherwise.

This suggestion is consistent with some previous research on the nature of RM. Several studies have demonstrated that the existence and nature of multiple objects in a display can affect the perceived trajectory extrapolation, even when there is no interaction between the objects. In one of the first such demonstrations, RM was altered depending on the behavior of a large frame which surrounded the objects (Hubbard, 1993). When the frame moved in the same direction as the judged target, RM was increased; when it moved in the opposite direction, RM was attenuated – perhaps because this weakened the overall spatiotemporally continuous motion signal. Rather different results were obtained in another study, when an additional object moving in the opposite direction from the judged target actually enhanced RM (Hayes & Freyd, 2002), but other experiments have shown that RM

can be attenuated after the motion by the appearance of a distracting object prior to the response (Kerzel, 2003). Other research more focused on spatiotemporal dynamics per se has shown that RM may sometimes closely track the speed of judged targets (Freyd & Finke, 1985), and adapt constantly to changing speeds (Finke, Freyd, & Shyi, 1986). Future work will be required to disentangle these effects, but they all point to the possibility that the presence of multiple objects and the strength of the motion signal may affect RM, as also suggested in the present experiments.

Another type of context effect suggests a more specific reason why RM might be attenuated by the presence of two objects and a continuous motion signal. In ‘landmark attraction effects’ (e.g., Hubbard & Ruppel, 1999), RM is enhanced when the target is moving toward an additional salient object, but is attenuated when RM moves away from such an object. This type of effect, whatever its explanation, may also explain both (1) the attenuated RM during launching in Hubbard’s original experiments (Hubbard et al., 2001), and (2) the lack of differential RM during launching and passing displays in the present studies. In short, both launching and passing involve a target moving away another salient object (either the launcher, in launching displays, or the additional object in passing displays). Thus, it is possible that alleged connections between causal perception and RM simply reflect a kind of landmark attraction effect.<sup>2</sup>

None of the possibilities discussed so far is necessarily deflationary with regard to the nature of representational momentum itself, and they could still fit comfortably with explanations of RM in terms of implied momentum. In this way, the present results are not directly relevant to the main point of the previous work in this vein (e.g., Hubbard et al., 2001; Hubbard & Favretto, 2003), which was intended to help uncover the nature of RM. However, the present results do have deflationary implications for the possibility that RM could be used to measure causal perception (as has been suggested, for example, by Hubbard, 2005). After all, our proposed explanation for these results – that RM is influenced by the number of objects in a display, combined with the existence of a strong continuous motion signal – does not advert to causality at all. Moreover, as demonstrated in Experiments 2 and 3, this method fails to capture several robust causal vs. non-causal contrasts that have proven important in other recent causal perception experiments. As a result, (1) null effects obtained with RM cannot rule out robust differences in perceived causality, and (2) successful attenuation of RM in such displays can be explained in terms which do not involve perceived causality.

The proposed explanation for our results – adverting to the perception of *two* objects but *one* continuous motion – should sound striking to students of causal perception. In fact, this is exactly the contrast which Michotte thought was essential to causal perception (1946/1963). He thought that such displays give rise to a conflict in the visual system, and that part of the solution to this conflict was to perceive a causal relationship involving the *transfer* of a single motion from one object to another. Thus, this explanation for attenuated RM may not turn out to capture the full range of causal perception as it has been studied in recent work, but it still may involve the same core insight that gave rise to

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<sup>2</sup> Additional experiments should investigate this possibility directly. Though plausible, this view would not necessarily predict other aspects of our results. For example, when a target moves away from another object, ‘landmark’ interpretations might make the prediction that the degree of attenuation of RM would be greater for closer landmarks – which is in fact the opposite of what we observed in Experiment 2.

Michotte's work in the first place.<sup>3</sup> Moreover, it illustrates one way in which this explanation is not arbitrary, but is actually deeply motivated by work on causal perception.

### 6.2. *Implications for other studies of causal perception*

The distinction proposed above – between causal perception per se, vs. the only-sometimes correlated existence of two objects but a single motion – may also prove important in other studies of causal perception. Ever since Michotte, perhaps the most popular type of causal vs. non-causal contrast in work on perceived causality has involved contrasting the launching effect with similar displays involving spatial or temporal gaps: When object B begins moving before A reaches it (i.e., with a spatial gap) or after a long enough pause when A reaches it (i.e., with a temporal gap), causal perception is severely attenuated (Michotte, 1946/1963; see also Schlottmann et al., 2006). However, unlike the contrast between launching and passing (Choi & Scholl, 2004; Scholl & Nakayama, 2002), these contrasts also (by definition!) differ in terms of the existence of a single spatiotemporally continuous trajectory – and this factor could potentially affect responses by itself, independent of (or prior to) causal perception.

We thus think that it will be important for future work to unconfound these factors in many specific situations (perhaps by also turning to the contrast between launching and passing). To take just one example, studies of young infants' causal perception have relied on exactly these contrasts: infants will dishabituate to reversed launching movies, but not (as much) to reversed spatial-gap or temporal-gap movies, or to reversed animations with single objects changing color during their motions (Leslie & Keeble, 1987). Such studies could be replicated using displays such as those used in Experiments 2 and 3 of the present paper, in order to test whether infants are responding categorically to the existence of a causal relationship (and thus to the reversed causal roles involved in these reversed animations), or merely to the existence of a new pattern involving two objects but a single motion. Perhaps, for example, the existence of a single motion in the launching display simply serves to make it more coherent to young infants, thus rendering its reversal more intelligibly different. In contrast, (1) each of the gap displays involves two separate perceived motions, which might render their reversals more complicated or less obviously different at this age; and (2) the color change involved in the reversal of a single moving object might simply not be salient by itself. (Indeed, a general lesson of work on object persistence and event perception is that spatiotemporal features dominate surface features such as color; e.g., Flombaum, Kunder, Santos, & Scholl, 2004; Flombaum & Scholl, in press; see also Kawachi & Gyoba, 2006.)

### 6.3. *Perceptual reports and other dependent measures*

As noted above, we initially began exploring the possibility that RM could be used to measure causal perception because it met two key criteria: (1) it provided a dependent

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<sup>3</sup> One way to investigate this connection further might be to study the effects of different speed ratios more systematically. Notoriously, causal perception is eliminated or greatly attenuated when the second object moves considerably faster than the potential launcher. (In such cases, the percept is of a qualitatively different 'triggering' event.) It would be interesting to see if this same pattern occurs for RM attenuation. Even if it does, however, that would not imply that RM and causal perception are linked – since an analogous bit of internalized physics could be driving the effects in each process independently.

measure which did not directly involve causality in the first place, thus reducing the chance of infection of perceptual reports from higher-level causal judgment; and (2) it provided a continuous dependent measure, perhaps able to discriminate fine differences in the strength of perceived causality, in contrast to dichotomous perceptual reports. Given the conclusions of this study, however, it does not appear that RM can be used in this way to assess causal perception. As such, it will be important for future work to continue to explore other possibilities for indirect and quantitatively precise dependent measures that can tap the perception of causality.

Are there other such dependent measures on the horizon? Here we briefly mention four possibilities:

First, causal perception might still be tapped indirectly via other types of perceptual reports which nevertheless do not advert to causality. For example, whereas most work on causal perception has involved a hunt for the rules which mediate it – i.e., for the *causes* of causal perception – at least one other recent study has explored the downstream *effects* of causal perception. Scholl & Nakayama (2004) introduced the phenomenon of *illusory causal crescents*, wherein the perception of causality affects the perceived spatial relations among two objects involved in a collision event: observers systematically underestimate the amount of overlap between two items in an event which is seen as a causal collision. This occurs even when the causal nature of the event is induced by a surrounding context, such that estimates of the amount of overlap in the very same event are much improved when the event is displayed in isolation, without a ‘causal’ interpretation. Current work is exploring the possibility that the (continuous) magnitude of this underestimation could be used more generally to measure causal perception without directly asking about causality.

Second, it might be possible to tap causal perception not only via other independent perceptual reports, but also by implicit performance measures. Using response times and the object reviewing paradigm of Kahneman and Treisman (1984; Kahneman, Treisman, & Gibbs, 1992) for instance, one previous study suggested that causal events gave rise to different patterns of object-specific priming than non-causal events (Kruschke & Fragassi, 1996). Unfortunately, this preliminary study has not received any published follow-up work, to our knowledge, so it is not clear if it would also generalize beyond highly specific contrasts.

Third, it might be possible to draw inspiration from other recent indirect tests of Michotte’s perceptual demonstrations. Like causal perception, for example, Michotte’s *tunnel effect* (Burke, 1952; Michotte, 1950; Michotte, Thinès, & Crabbé, 1964/1991) involves a perceptual phenomenon – of continued object persistence behind occlusion, despite featural change – that has typically only been studied via perceptual reports. In response, we have recently developed multiple indirect dependent measures which are able to tap and measure the tunnel effect using manual search (Flombaum et al., 2004) or change detection (Flombaum & Scholl, *in press*; see also Kawachi & Gyoba, 2006). We are currently exploring the possibility that these same methods could be adapted to causal perception, by measuring change detection for objects involved in causal events (e.g., Choi & Scholl, 2006b).

Finally, recent neuroimaging work has also begun to isolate the neural circuits which are involved in causal perception (e.g., Blakemore et al., 2001; Fugelsang et al., 2005). Currently this research is at an early state, isolating broad networks (typically involving bilateral V5/MT/MST, superior temporal sulcus, and left intraparietal sulcus) and using either (1) very coarse contrasts between perceived and judged causality (e.g., Fugelsang et al., 2005), and/or (2) contrasts of launching with ‘gap’ displays which may suffer from the same

ambiguities discussed in Section 6.2. Nevertheless, it is possible that this research will succeed in isolating a neural signature (or set of signatures) of causal perception, per se – in which case causal perception could perhaps be assessed in the future without any task given (or questions asked) to subjects at all.

Each of the four possibilities raised above has the potential to meet the two key criteria of (1) not directly asking about causality, and (2) involving a continuous dependent measure. Until these or other indirect tools are further developed, however, it will be important for ongoing studies of causal perception using perceptual reports to cleave to effects and phenomena which have the following three characteristics. First, such studies should attempt to enhance the phenomenological component of the effects. In fact, the concern that higher-level beliefs and judgments about causality could infect reports of causal perception is attenuated for many effects simply because they are so phenomenologically robust: you can simply see them, as with other visual illusions. Second, such studies should be especially careful to distinguish between perception and judgment when asking for perceptual reports, and should report the instructions in detail. This is all the more important given that causal perception and causal judgment can pull in opposite direction (e.g., *Schlottmann & Shanks, 1992*).

Finally, such studies should aim for nuanced (and even counterintuitive) effects which would not likely be influenced by brute causal judgments. In fact, many of Michotte's demonstrations remain convincing today for just this reason: he and his followers have shown that the precise details of the displays can dramatically influence causal perception, even in spite of countervailing beliefs. This logic has long been appreciated. In the words of his students and followers: "If previous knowledge and experience had exercised a decisive influence on the nature and type of judgments made by the subjects, the very close dependence of such judgments on the objective spatiotemporal conditions of the stimulation (such as distance, duration, and speed of movement) would have been extremely difficult to explain" (*Montpellier & Nuttin, 1973, p. 287*). Nevertheless, the renaissance that the study of causal perception has been undergoing in recent years – with ever more varied and nuanced effects – clearly calls for the continued development of additional indirect dependent measures which can tap causal percepts in a compelling fashion. Unfortunately, as explored in the experiments reported here, appeals to representational momentum may not suffice.

## Acknowledgements

For helpful conversation and/or comments on earlier drafts, we thank Tim Hubbard, Steve Mitroff, Rob vanLier, Johan Wagemans, Peter White, Michael Young, and the members of the Scholl and Chun labs at Yale University. B.J.S. was supported by NSF #BCS-0132444. Portions of this work were presented at the 2005 meeting of the Vision Sciences Society.

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