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Infants' representations of michottean triggering events

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ABSTRACT

The classic Michottean 'launching' event is consistent with a real-world Newtonian elastic collision. Previous research has shown that adult humans distinguish launching events that obey some of the physical constraints on Newtonian elastic collisions from events that do not do so early in visual processing, and that infants do so early in development (< 9 months of age). These include that in a launching event, the speed of the agent can be 3 times faster (or more) than that of the patient but the speed of the patient cannot be detectably greater than the speed of the agent. Experiment 1 shows that 7–8-month-old infants also distinguish canonical launching events from events in which the motion of the patient is rotated 90° from the trajectory of the motion of the agent (another outcome ruled out by the physics of elastic collisions). Violations of both the relative speed and the angle constraints create Michottean 'triggering' events, in which adults describe the motion of the patient as autonomous but not spontaneous, i.e., still initiated by contact with the causal agent. Experiments 2 and 3 begin to explore whether infants of this age construe Michottean triggering events as causal. We find that infants of this age are not sensitive to a reversal of the agent and patient in triggering events, thus failing to exhibit one of the signatures of representing an event as causal. We argue that there are likely several independent events schemas with causal content represented by young infants, and the literature on the origins of causal cognition in infancy would benefit from systematic investigations of event schemas other than launching events.

1. Introduction

Representations of causality are central to human thought and our ability to interact successfully with our environment. Causality is a relation that connects two or more events in the environment through counterfactual licensing (if the cause had not happened, the effect would not have occurred) or mechanistic linkage (the key closed an electrical circuit to the spark plug, causing it to create a spark which ignited the gasoline, starting the engine; Ahn, Kalish, Medin, & Gelman, 1995; Lewis, 1973; Paul & Hall, 2013). Without causal representations, we would experience the world as a series of disconnected events that just happen, with some statistical associations but little coherence or generalizability to new situations.

Given how central causal representations are to human experience, it is perhaps unsurprising that their ontogenetic origins have been a topic of interest in psychology and cognitive science since at least the early 20th century (Michotte, 1946/1963; Olum, 1956; Piaget, 1927/1930), and in philosophy since long before that (Hume, 1748/1902; Maine de Biran, 1834/2016). From decades of empirical work there is clear evidence for causal representations in the first year of life, but perhaps surprisingly, that evidence is restricted to a handful of specific events, and there is little consensus about how exactly these representations emerge (for review see Muentener & Bonawitz, 2017; Saxe & Carey, 2006).

1.1. Identifying early causal representations

There are two quite different components of establishing that infants (or anyone) represent a given event as involving a cause-and-effect relationship. First, one must provide evidence that there is a distinct event schema elicited by causal events, but not by perceptually similar non-causal events. That is, participants must distinguish the causal events from similar events that lack one or more perceptible property of real-world causal interactions. The representation must have a causal event in its extension. Second, one must provide evidence that these schemas are represented as *causal*, in that the structure of the representation distinguishes causal roles, and/or the schema supports causally relevant inferences about the properties of the entities involved in

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the event, and/or are used to provide a causal explanation for what has been observed.

By these criteria, the well-studied Michottean launching event (Michotte, 1946/1963) is represented as causal by 6 months of age. In this event, an object A moves toward an object B until they are adjacent, at which point A stops moving and B immediately starts moving in the same direction with comparable speed. This event is rendered schematically in Fig. 1a and can be found in animated form (along with animations of all of the other events described in this section) at https://www.jfkominsky.com/demos.html. In Fig. 1a, the black square is the situational causal agent and the light grey square is the situational causal patient.

By six months of age, infants who are habituated to a launching event will dishabituate to events in which there is a temporal delay or spatial gap between the end of A's motion and the start of B's motion and vice versa, but if habituated to one non-causal event (e.g. a temporal delay event) they will not dishabituate to the other (e.g. a spatial gap event; Cohen & Oakes, 1993; Cohen & Amsel, 1998). These studies provide unambiguous evidence for a launching event schema: launching events such as billiard ball interactions are Newtonian elastic collisions, and there is no causal connection between the motions of A and B in gap or delay events. The motions of each are independent of each other. Thus, infants are sensitive to perceptible features of causality in representations of launching events: the motion of B must begin immediately upon contact.

The evidence that infants' representations of launching events are *causal* comes from two lines of work. First, by 7 or 8 months of age, if infants see an event in which one object goes behind an occluder and a second object at the opposite edge of the occluder subsequently moves, they expect there to be contact between the two objects when the occluder is removed, and show a violation of expectancy if there is a spatial gap (Ball, 1973; Muentener & Carey, 2006; Spelke & Van de Walle, 1993). One interpretation is that, upon seeing the two successive motions, they infer a causal interaction and expect contact, thus going beyond merely perceptual discrimination of causal from non-causal interactions. Notably, they will suspend this expectation if the objects are replaced with human figures, which infants understand to be capable of self-propelled motion (Kosugi & Fujita, 2002; Woodward, Phillips, & Spelke, 1993). That is, inferring a launching event as an explanation for an object's motion is affected by knowledge of the stable causally relevant features of the participants in the event: If the situational patient is represented as a dispositional causal agent, a person capable of self-generated motion who acts intentionally, infants do not infer that the

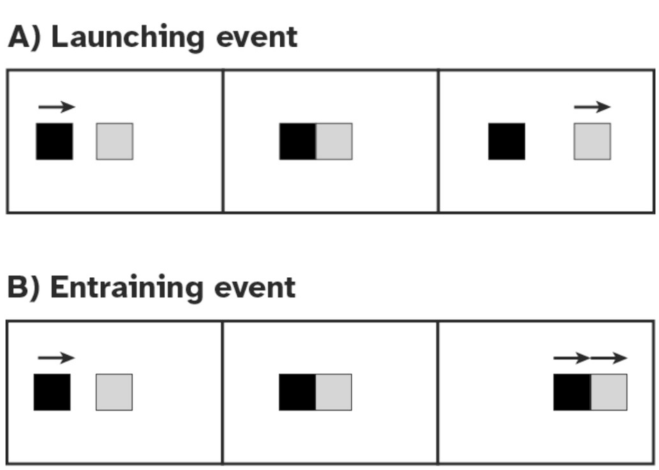

Fig. 1. Schematic depictions of (a) Michottean launching and (b) entraining events. In causal versions of these events, object B begins moving immediately on contact with object A. Non-causal 'delay' events simply add a 500 ms pause at the moment of contact (middle frame). Animated demos of these events can be found at https://www.jfkominsky.com/demos.html.

person's motion was caused by contact with the potential situational agent. Thus, infants' inferences of causality are integrated with other causally relevant features of the event.

Second, 6-month-old infants who are habituated to a launching event in which A launches B will dishabituate to events in which B launches A, but those habituated to a non-causal event will not dishabituate to its reversal (Bélanger & Desrochers, 2001; Leslie & Keeble, 1987). These studies show that infants at this age represent the situational causal roles in these events (i.e., that the motion of A is the *source or cause* of B's motion), and respond to a reversal of the causal relation between the two objects. The fact that their representation of launching is structured in terms of identifying situational agents and situational patients further indicates that it is a causal representation.

Recent evidence suggests that there may be multiple *distinct* and *independent* causal representations in the first year of life. Another basic Michottean event is called 'entraining'. In this event, object A moves until it is adjacent with object B, at which point A and B *remain in contact* and move together in the direction of A's motion (see Fig. 1b; Michotte, 1946/1963). Whereas launching is analogous to a billiard-ball collision event, this event is analogous to picking an object up and moving it around, and indeed infants seem to have a rich causal representation of it in that exact situation. Infants as young as three months of age who are habituated to a hand picking up and moving a doll will dishabituate to an event in which the hand hovers next to the doll and they both move but never contact each other, and vice versa (Leslie, 1982). This provides evidence for an entraining event schema; infants distinguish a causal event from a highly similar one in which both objects are moving independently in synchrony.

With respect to entraining being construed as causal, infants' representations of entraining are influenced by their representations of the dispositional properties of the objects involved. For example, the results described above are not observed if the situational agent is a stick (Leslie, 1982). Starting around seven months of age, infants who see an event in which a hand, a furry puppet, and a ball all move together while in contact with each other will identify the hand as the source of motion, but if the hand does not come into contact with the puppet and ball while they move together, they attribute self-propelled motion to the puppet as the more likely animate agent (Träuble & Pauen, 2011). Furthermore, representations of entraining support inferences about the dispositional properties of the objects involved: Ten-month-old infants show a violation of expectancy effect if a novel fur-covered object that demonstrated self-propelled movement is later shown to be hollow, but no such violation of expectancy if the same movement is generated by a human entraining the object (Kominsky, Li, & Carey, 2022; Setoh, Wu, Baillargeon, & Gelman, 2013). That is, they take the entraining event as an explanation of the object's motion, and therefore do not infer that it has an internal cause for its movement. Thus, representations of entraining events are influenced by representations of properties of dispositional causal agents, and support inferences about dispositional animate agency in ambiguous objects.

Critically, while the evidence reviewed above shows that both launching and entraining are represented as causal in early infancy, there is also evidence that these representations are *initially independent* of one another in that the inferences they support are non-overlapping in the first year of life. While 6-month-old infants are sensitive to reversals of situational causal roles in launching events (Bélanger & Desrochers, 2001; Leslie & Keeble, 1987), they are not sensitive to the reversal of the situational agent and the situational patient of entraining events (Bélanger & Desrochers, 2001). Conversely, even as late as ten months of age, infants fail to make inferences about dispositional animate agency from launching events (Cicchino, Aslin, & Rakison, 2011), even in paradigms where they successfully make these inferences from entraining events (Kominsky et al., 2022). These findings further imply that when infants respond to the reversal of a launching event, they are doing so on the basis of situational causal roles (i.e. A's relation to B in the context of that specific event), not because an object they previously thought to be dispositionally inert (B) turned out to be self-propelled.

The sharpness of this distinction might seem somewhat surprising given that the only difference between a launching display and an entraining display is whether A stops at the moment of contact, but in the real world there is a critical difference in the underlying physics of these two events: canonical launching events are consistent with constraints on real-world Newtonian elastic collisions, while entraining events are not. In particular, an entraining event (especially the ones used by Michotte and most others in which the causal agent does not slow down on contact) require a constant application of force from A in order for the two objects to remain in contact. The constant application of force is naturally true of picking up an object and moving it around as well. In other words, while a launching interaction can occur between any two arbitrary objects, an entraining event actually *requires* an agent with the ability to apply constant force, like a human hand, some other dispositionally animate agent, or a mechanical object with an internal capacity to generate motion.

1.2. Newtonian constraints and 'triggering' events

The conclusion that infants distinguish launching events from causal events that involve an agent entraining an object raises questions about the causal representation of other event types that are very similar to launching but also do not obey the constraints on Newtonian elastic collisions. One Michottean event that has gained some attention in recent years is 'triggering' (Fig. 2a). In this event, object A moves until it is in contact with object B, at which point A stops and B immediately begins moving at a much faster speed. Michotte (1946/1963, Experiment 40) found that adults described events with A:B speed ratios of roughly 1:2 or greater as "triggering" or "releasing", alternative translations of Michotte's term "déclenchement" that have been used in the English experimental literature. Later work found convergent evidence that the launching impression is replaced by the releasing/triggering impression at speed ratios of roughly 1:2 (Natsoulas, 1961). Michotte described triggering/releasing as an event in which B's motion was *autonomous* but not *spontaneous*, as it was *released/triggered* by contact. In other words, while the motion of B was initiated by contact with A, B's subsequent movement could not be explained by the force of the collision alone.

This distinction between launching and triggering corresponds to a Newtonian constraint on elastic collisions: B can never move at more than double the speed of A from the force of the collision alone. To be clear, this is not just an issue of relative mass; this is an absolute limit that can be mathematically proven from Newton's third law. We shall work through a simple example here. Consider a case in which an object A contacts a stationary object B, as in Fig. 2a. The following Eq. (1) captures the relations between velocity and mass in a Newtonian elastic collision in an idealized frictionless environment. In the equation below, the velocity of A and B immediately prior to contact are v_{A1} and v_{B1} respectively, while m_A and m_B represent the masses of the two objects, and v_{B2} is the velocity of B immediately following contact.

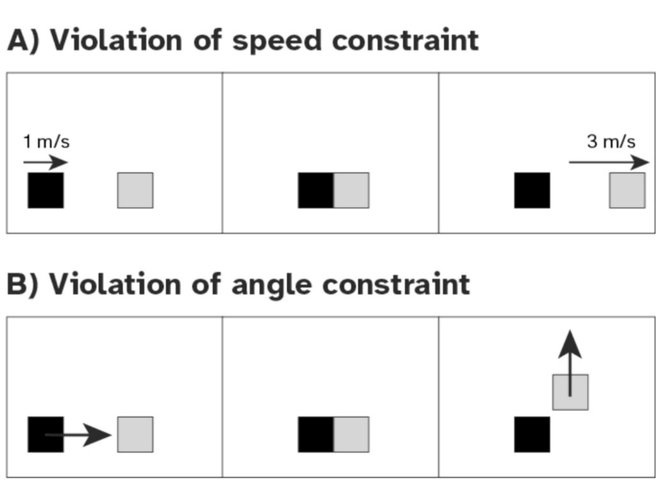

Fig. 2. Collision events that violate Newtonian constraints on elastic collisions.

$$v_{B2} = \frac{v_{B1}(m_A - m_B) + 2(m_A v_{A1})}{m_A + m_B} \tag{1}$$

In the event shown in Fig. 1a, $v_{B1} = 0$, since B starts at rest, so the first term in the numerator can therefore be removed. To find the absolute theoretical limit of B's velocity, we can assume that B has zero mass (i.e., $m_B = 0$), signifying that, in relative terms, A is infinitely more massive than B. This simplifies the equation to the following (2):

$$v_{B2} = \frac{2m_A v_{A1}}{m_A} \tag{2}$$

We can then cancel out the mass of A, and we are left with the absolute theoretical limit of B's speed following a perfect frictionless elastic collision (3):

$$v_{B2} = 2v_{A1} \tag{3}$$

Thus, any launching-like event in which B moves at more than double the speed of A, the motion of B cannot be explained by the collision alone. This limit is absolute, and common additional forces in of real-world collisions, such as like friction, inelasticity, or air resistance are not able to counteract it. Rather, for B to move more than twice the speed of A, some other force entirely must be present. Note that the inflexible nature of this constraint applies only to the motion of B following contact. Of course, Newtonian laws also constrain the motion of A after contact. The canonical launching event in which A stops upon contact with B is an exception to what would happen in most elastic collisions, but it is not ruled out by absolute constraints (i.e., it would happen if the two were equally massive, or if there were enough friction between A and the surface it is moving on). We return to the question of how rich the Newtonian representations of launching events might be in the general discussion.

Importantly, there is no corresponding constraint on how much faster the situational agent (A) in a Newtonian elastic collision event can move, relative to the speed of the situational patient (B). If B is massive enough B won't move at all. Thus, whereas the patient in a launching event cannot move >2 times as fast as the agent (a v_A:v_B speed ratio of 1:2), the agent can move 3 times as fast as the patient (a 3:1 speed ratio) or 5 times as fast, or even infinitely faster (if B does not move at all). Thus, real-world collision events satisfy an asymmetric A:B speed ratio constraint; there are limits on how much faster B can move than A, but not vice-versa.

No such constraint applies to entraining events or triggering events. In entraining, an additional force is applied by the entraining agent; in triggering, the force is most often construed as internal to the patient (Michotte, 1946/1963). In both cases, there is a force that is independent of the collision itself, and thus no constraints from the physics of the interaction on the relative speeds of motion between A before contact and B after contact.

Visual search experiments establish that adults perceptually distinguish 1:3 triggering events from asymmetric 3:1 launching events automatically. Kominsky et al. (2017) found that when told to find one 'asymmetric' event in an array of symmetric (1:1) launching events, adults were faster to find the 1:3 event than the 3:1 event. When asked to find a symmetric (1:1) launching event in an array of asymmetric events, they were marginally faster but significantly more accurate if the asymmetric distractor events had a 1:3 speed ratio rather than a 3:1 ratio. In both cases it was easier for adults to distinguish the 1:3 event than the 3:1 from event symmetric (1:1) launching events, despite both events being equally asymmetric in objective terms. This indicates a boundary in perceptual processing between 1:3 collision events and symmetric 1:1 collision events that does not exist between 3:1 and symmetric 1:1 collision events. Adults are sensitive to the asymmetric A:B speed ratio constraint in launching events. Critically, these search advantages are *not* found for a variety of perceptually similar non-causal events with spatial gaps or temporal delays. Non-causal events are not bound by these Newtonian constraints since the movement of each object is independent of any forces imparted by the other.

Infants also are sensitive to the asymmetric A:B speed ratio constraint in launching events; they too distinguish launching from triggering. Seven- to 9-month-olds habituated to a 1:1 launching event look longer to a motion-on-contact event with a 1:3 speed ratio (triggering) than one with a 3:1 speed ratio (launching). In contrast, if habituated to a 1:1 delay event, they look equally to delay events with both speed ratios (1:3 and 3:1). That is, in delay events, which are seen as non-causal in 1:1 events, infants notice the change from 1:1 to 3:1 equally to the change from 1:1 to 1:3, but when it is in the context of movement upon contact, the two ratios 1:1 and 3:1, both launching, are treated the same, but 1:1 (launching) is distinguished from 1:3 (triggering; Kominsky et al., 2017).

Relative speed is not the only constraint on the relative motions in elastic collisions: There is also a constraint on the *direction* of B's motion, relative to the direction of A's motion. In particular, regardless of how glancing the collision, B can never move at an angle of 90° or greater relative to the direction of A's motion (Fig. 2b). This constraint is somewhat easier to grasp intuitively than is the relative speed constraint: Some component of the vector of A's velocity (shown by the arrow in the first panel of Fig. 2b) must be present in B's motion following the collision. If B moves orthogonally to A, then by simple geometric definition, this is not the case, and B's movement is once again autonomous but not spontaneous if it is triggered by contact from A.

There is some evidence that adults are also sensitive to constraints on relative direction, though interestingly all the previous investigations we know of have not asked for a distinction between "launching" and "triggering" but between "causal" and "non-causal." Michotte himself reported (1946/1963) that increasing the angle of deflection diminished the causal impression (Experiments 33–35), and later more systematic work found convergent evidence that causal judgments decreased by as much as 40% as the relative angle of B's motion increased (Straube & Chatterjee, 2010), even in the range of 0°-60°, angles in which the collision might account for some of the motion of the situational patient. However, there have been no investigations using less explicit behavioral measures with adults. With infants, there has been no study directly contrasting launching with angle-deflection events.[1]

The fact that investigations of angle-based triggering are posed as "causal/non-causal" judgments rather than "launching/triggering" judgments raises an interesting set of questions. First and foremost, what exactly distinguishes a triggering event from a non-causal event, such as a gap or delay event? Both are readily distinguished from launching, even in infancy. A temporal or spatial gap event is also one in which the movement of object B cannot be explained by a Newtonian elastic interaction with object A. The primary difference, according to Michotte, is that the movement of object B in a triggering event is not *spontaneous*, that is, participants see contact by A as having *initiated* the motion; the motion would not have occurred without this contact. However, even with adults, the only evidence that any form of triggering is understood as causal comes from the explicit reports of Michotte's participants, some studies that used ratings of "force" (Vicovaro, 2018; White, 2006, 2009), and from a forced-choice task in which "non-causal" was not an option (Natsoulas, 1961). That the launching/triggering distinction is made only in the context of immediate motion on contact, i.e., not in delay or gap events, may depend upon the representation of causality within launching events alone. There is to date no evidence whatsoever concerning the integration of triggering events with other causally relevant information; the present experiments begin to explore this question.

1.3. The current experiments

As reviewed above, by 7 to 10 months of age, infants' causal representations of launching are consistent with at least *some* Newtonian principles: the contact and simultaneity constraints, (i.e., motion of the situational patient immediately upon contact), and the asymmetric A:B speed ratio constraint. In Experiment 1 we test whether triggering is distinguished from launching on a basis other than relative speeds—on the basis of the relative angle between A's motion and B's motion. Experiments 2 and 3 begin to explore whether triggering is represented as *causal*.

2. Experiment 1

Kominsky et al. (2017) demonstrated that infants habituated to a 1:1 launching event detect a three-fold increase in B's speed relative to A's . Past work has found that 6-month-old infants habituated to a single object moving at a given speed will only dishabituate to another object moving at a different speed if the ratios of the two speeds are 1:2 or more (Möhring, Libertus, & Bertin, 2012). It just so happens that minimum difference in object speed that infants can detect in *any* context (not just launching events) aligns with the Newtonian limit on B's speed in an elastic collision event, so the Kominsky et al. (2017) finding could give the impression of sensitivity to Newtonian constraints but in fact has nothing to do with them. That infants in the motion-on-contact condition distinguish 1:1 A:B ratios from 1:3 ratios but not 3:1 ratios shows that contact does not *merely* lead to an increase in sensitivity to relative speeds. However, these experiments cannot establish whether this constraint reflects sensitivity to the precise Newtonian 1:2 limit, either for adults or infants. The ratios used were chosen to clearly exceed participants sensitivity to speed differences even within a single object's motion. Because the Newtonian 1:2 limit is close to both adults' and infants' threshold for detecting differences in speed, the constraint respected in representations may simply be an asymmetric relative speed constraint rather than the Newtonian absolute 1:2 speed ratio constraint. However, this asymmetric speed ratio constraint nonetheless reflects the asymmetry in Newtonian mechanics that can be framed in more approximate terms: that B can only move so much faster than A without some additional force, while A can move faster than B to an unlimited degree, depending upon relative mass, friction between the objects and the surface they are on, and other common physical variables.

If the asymmetric speed ratio constraint reflects innate or early learned intuitive physics, we might expect that as soon as infants distinguish launching events from gap events, delay events, and 1:3 triggering events (i.e., 6-to-8 months; see above), they might be sensitive to other Newtonian constraints on launching. Here we test whether infants this young distinguish launching from triggering when the event specifies triggering on the basis of the relative angle of motion constraint. Using the same methods as Kominsky et al. (2017), Experiment 1 tests whether infants are sensitive to an additional Newtonian constraint on relative motion of the two objects in launching events. We ask whether infants perceive a triggering event in which the patient moves at a 90° angle from that of the situational agent (impossible in elastic collisions in which the motion of the patient is entirely caused by the motion of the situational agent) to be different from a launching

[1] One study with neonates (<1 month old) suggested that they prefer to look at 90° perpendicular events with a delay compared to 90° perpendicular events without a delay, but show a preference for causal launching over linear temporal gap events (Mascalzoni, Regolin, Vallortigara, & Simion, 2013). These preference studies are difficult to interpret given that they found not simply chance looking but an actual *preference* for the delay event in the 90° relative motion case, and the age group is far younger than has been used in any prior work.

event.

The current experiment, like Experiment 3 in Kominsky et al. (2017), uses a classic habituation/dishabituation design (Colombo & Mitchell, 2009). Infants in the motion-on-contact condition were habituated to a launching event (Fig. 3a), and were then shown one of two test events: 1) an event which violates the angle constraint on launching events, such that the motion of B was perpendicular to that of A (Fig. 3b, top), or 2) a new launching event in which *the whole launching event* is rotated 90° from the habituation event (Fig. 3b, bottom). There were also two conditions where infants were habituated to non-causal delay events, and then tested on one of two test events: 1) a delay event in which the second moving object moved at a 90° angle relative to the first or 2) a fully rotated delay event in which the two objects moved in the same direction (now vertical), with a delay upon contact. The delay condition was included to ensure that infants were not merely responding on the basis of the changes in the features of the individual objects' independent motion trajectories.

The prediction is straightforward: If infants are sensitive to the angle constraint on relative directions of motion in elastic collisions, then their attention should be drawn to the violation when the situational patient (B) moves off on a trajectory at a 90° angle (i.e., perpendicularly) from the motion of the situational agent in the motion-on-contact condition, more than to the perpendicular motion in the delay condition. Notably, there is a clear and contrary alternative hypothesis based on low-level visual features: the fully rotated events are actually more different from the habituation events in terms of the motion characteristics of each object individually, as well as the area of space occupied by the whole event, even though the *relative* angle between the objects is unchanged. This is true both in the motion-on-contact condition and in the delay condition. Thus, if infants are habituating not to the *relation* between the movements of A and B but to each individual objects' movements, they should look as long or longer to the rotated test event, both in the motion-on-contact and delay conditions. Alternatively, if the change from linear motion trajectories to perpendicular ones is a salient perceptual feature of these displays, independently of representations of launching, attention should be drawn to the perpendicular events both in the motion-on-contact (launching) condition and the delay condition.

2.1. Methods

Participants. Sixty-four infants ages (months; days) 7;15–9;15 (mean age 8;16, 30 female, 34 male) from the greater Boston metropolitan area participated in the experiment at the Harvard Lab for Developmental Studies. This sample size was set to half that of Kominsky et al. (2017) based on the expectation that perpendicular angle changes would be more detectable than speed changes. An additional 19 infants (8 female, 11 male) were recruited but excluded from the final analysis for fussing out (2), moving off-camera during the experiment (5), parental interference (3), experimenter error (2), or an above-threshold discrepancy during offline re-coding (7, see Appendix A for a description of all preset exclusion criteria). One additional (female) participant was replaced in the final sample due to having a test trial looking time > 3 standard deviations from the average for their condition (a predetermined exclusion criterion, though this experiment was not preregistered; in this case the actual magnitude of difference was 3.67 standard deviations from the condition average). Appendix A provides a full description of the exclusion criteria for all experiments.

Apparatus. Stimuli were controlled using PyHab (Kominsky, 2019), an add-on for PsychoPy (Peirce et al., 2019). PyHab automatically controls the timing and content of stimulus presentation according to the experimenter's real-time coding of looking times, including calculations of when to end habituation and proceed to the test trial.

Stimuli were presented on a 63.5 cm wide by 39.4 cm high Apple Cinema Display operating at 1280 × 800 pixel resolution and 60 frames per second. The edges of the display were hidden behind a black foamcore frame, with black fabric around the frame running floor to ceiling and about a foot on either side.

Beige curtains obscured the rest of the room from the infants' view. Infants sat on their parent's lap about 142 cm from the display screen. A hidden camera located directly under the center of the display monitor recorded infants' looking behavior and displayed a live feed to the experimenter. Light was provided by four overhead dimmable compact fluorescent track lights set at approximately 10% brightness.

Stimuli and procedure. After providing informed consent, parents were instructed to sit in a chair facing the display screen with their infant in their lap. Parents were asked to close their eyes and avoid interacting socially with their infant for the duration of the experiment (they were shown the stimuli afterward). They were also asked to try to prevent their infant from standing up on their lap, in order to keep the infant's face in view of the camera.

Fig. 3 portrays the events in Experiment 1, which had a between-subjects 2 (event type; motion-on-contact vs. delay events) x 2 (test event; fully rotated vs. perpendicular movement test) design, 16 infants per condition. The habituation events were always horizontal; in the motion-on-contact condition these were standard 1:1 launching events and in the delay condition these were 1:1 non-causal delay events with a 500 ms pause at contact. The test events always began with a vertical motion starting at the bottom of the display; in the fully rotated motion-on-contact condition these were still standard 1:1 causal launching events, whereas in the perpendicular motion-on-contact condition these were triggering events. In the delay condition, all habituation and test events were non-causal. Condition assignment was randomized, and looking-time coding (including reliability coding) was always done with the experimenter unaware of the participant's assigned condition.

In all four conditions, the basic parameters of a trial were the same: At the start of the trial, the experimenter pressed a key to play an attention-getter, consisting of a rapidly looming and spinning yellow rectangle and a rapid rising series of notes, taking exactly 1.1 s. Immediately following this, two squares appeared on the screen, one red and one green, each one 80 pixels on a side with a 240 pixel gap between them. One square was always adjacent to the center of the screen, and the other to its left or right (counterbalanced between subjects). The two squares appeared static on the screen for a minimum of 200 ms. The experimenter could play the attention-getter again if the infant failed to look at the screen initially, and the trial did not start until the infant looked at the screen. Each trial started after the attention-getter when the infant initially looked at the screen, and lasted until the infant looked

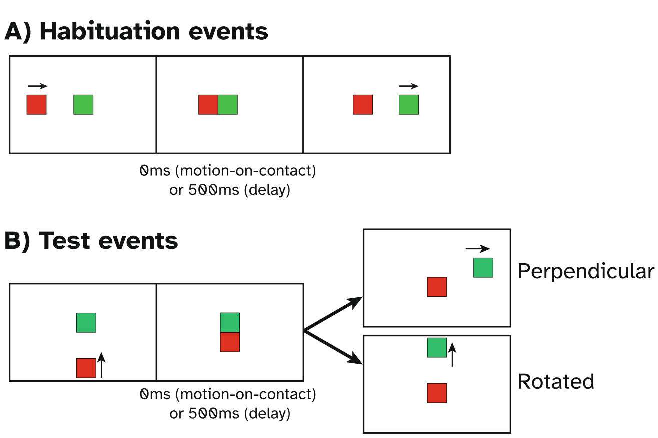

Fig. 3. Events shown to infants in Experiment 1. (a) During habituation, participants saw either a motion-on-contact 1:1 horizontal event, or the same event with a 500 ms delay at contact, between-subjects. (b) At test, they then saw another motion-on-contact or delay event (whichever they saw during habituation) in which the first object started below the second, and the second object moved either vertically (a rotated event) or horizontally (a perpendicular event), between-subjects. In the motion-on-contact condition, the perpendicular event is a violation of Newtonian constraints.

away for 2 *consecutive* seconds or 60 s had passed.

First, infants saw up to 14 habituation trials according to their assigned condition. Each object moved for one second at 4 pixels/frame, covering 240 pixels. Within each habituation trial, on the last frame of object B's movement, both squares vanished for 333 ms, and then the animation repeated from the beginning.

The habituation criterion was calculated as the sum of the infants' looking time over the first three trials, divided by two. The experiment moved on to the test trial when the sum of the infant's looking time across three (subsequent) consecutive trials was less than the criterion. The experimenter was not informed when the criterion was met.

Infants then saw a single test trial (see Fig. 3b). In the test trial, the arrangement of the squares was different. Object B was now in the center of the screen, and object A 240 pixels directly below it. In both test events, object A moved up toward object B until they were adjacent, at which point object B began moving either immediately (in the motion-on-contact condition) or after a 500 ms delay (in the delay condition). In the fully rotated test trials, object B moved up as well, preserving the linearity of the habituation trials and remaining a canonical launching event in the motion-on-contact condition. In the perpendicular motion test trials, object B moved identically to how it had moved in the habituation trials, i.e. to the left or right. Thus, in the motion-on-contact perpendicular motion test event condition, this event violated the angle constraint on Newtonian elastic collision events.

2.2. Results

Habituation. Five infants never reached criterion but saw the test trial after 14 habituation trials, and their data were included in the analysis. As can be seen from Table 1, the motion on contact and the delay conditions did not differ in looking times to the habituation trials. Two 2 × 2 ANOVAs confirmed there were no effects or interactions involving condition (motion on contact vs. delay or test events (perpendicular vs rotated) on the average looking times over the first three habituation trials (all $ps \geq 0.26$), nor on the average looking times over the last three habituation trials (all $ps \geq 0.18$). The lack of any effects of test event is a sanity check; within condition, the habituation events were identical; the events differed from each other (fully rotated or perpendicular) only at test, after habituation. Infants had become equivalently bored with both delay events and contact events before proceeding to the test trial.

Test Trials. Average test trial looking time by condition can be found in Fig. 4. Eyeballing this figure, one can see a clear difference in looking times in the motion-on-contact condition such that infants looked longer at the perpendicular motion test event than the fully rotated test event, whereas there was no such selective looking in the delay condition.

We confirmed these impressions with the following analyses: A 2 × 2 ANOVA examined the effects of condition (motion-on-contact vs. delay) and of test event (rotated vs. perpendicular) on test trial looking times. There was no main effect of condition, $F(1, 60) = 1.07$, $p = .3$, $\eta_p^2 = 0.02$, and no main effect of test event, $F(1, 60) = 0.72$, $p = .4$, $\eta_p^2 = 0.01$, but a significant interaction, $F(1, 60) = 4.50$, $p = .038$, $\eta_p^2 = 0.07$.

Table 1
Mean looking times (in seconds) in each condition for habituation and test trials. Numbers in parentheses are standard deviations. Dishabituation was calculated as the test trial looking time minus the average looking time of the last three habituation trials.

Condition	Motion-on-contact		Delay	
Test Event	Perpendicular	Rotated	Perpendicular	Rotated
First 3 hab (avg)	19.51 (7.98)	23.03 (12.64)	20.66 (10.62)	18.55 (6.87)
Last 3 hab (avg)	8.06 (3.23)	7.60 (4.75)	8.79 (3.95)	6.74 (2.18)
Test trial	16.09 (10.63)	9.38 (3.95)	13.63 (7.14)	16.52 (12.15)
Dishabituation	8.03 (9.63)	1.78 (5.34)	4.84 (7.88)	9.78 (11.61)

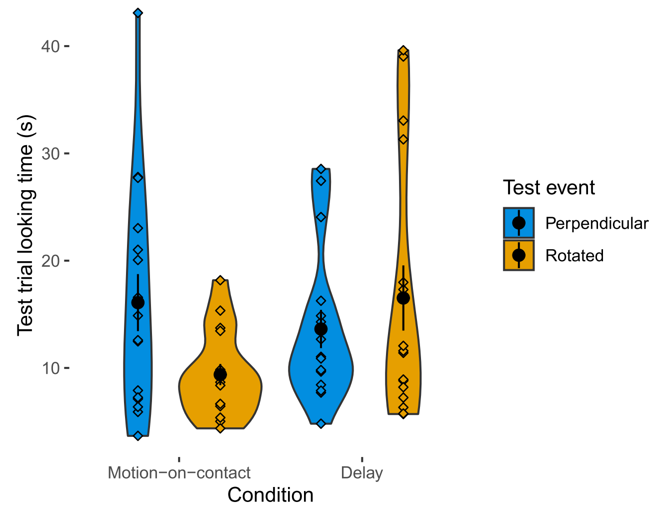

Fig. 4. Results of Exp. 1. Filled dots represent means, error bars represent ±1 SEM.

Table 1 shows the means and standard deviations for each condition. Planned comparisons of the effect of test event in each causal condition found that infants in the motion-on-contact condition looked significantly longer at the perpendicular motion test event ($M = 16.09$ s, $SD = 10.63$) than the rotated test event ($M = 9.38$ s, $SD = 3.95$), $t(31) = 2.37$, $p = .029$, $d = 0.84$. In the delay condition, there was no significant difference between the angle violation ($M = 13.63$ s, $SD = 7.14$) and rotated test events ($M = 16.52$ s, $SD = 12.15$), $t(31) = 0.82$, $p = .4$, $d = -0.29$.[2]

Table 1 also displays the amount of dishabituation in each condition (test trial looking time minus the average looking time of the last three habituation trials). Exploratory analyses analyzed whether infants in each condition dishabituated to the test trial. All *t*-tests report *p* values Bonferroni-corrected for 4 independent tests. Infants in the motion-on-contact launching condition generalized habituation to the fully-rotated launching test trial (average dishabituation 1.78 s, *t*-test against 0: $t(15) = 1.34$, $p = .8$, $d = 0.33$), whereas those in the launching condition strongly dishabituated to the perpendicular test trials (8.03 s, *t*-test against 0: $t(15) = 3.34$, $p = .016$, $d = 0.83$). This pattern reversed in the delay condition; dishabituation to the fully rotated test trials (9.78 s) was significantly >0, $t(15) = 3.37$, $p = .016$, $d = 0.84$, but dishabituation to the perpendicular test trials (4.84 s) was not significantly >0 (with correction), $t(15) = 2.46$, $p = .108$, $d = 0.61$. However, the difference between the two delay test trials was also not significant, $t(30) = 1.41$, uncorrected $p = .17$, $d = -0.50$. In short, in the delay condition there is some evidence that infants dishabituated to the fact that the movement of each individual object changed, whereas in the motion-on-contact condition, they were sensitive only to the *relative* motion of the two objects.

[2] A visual inspection of Fig. 4 suggests that our outlier exclusion criterion (>3 standard deviations from group mean) may have been too conservative, as there are still a few extreme individual looking times. Therefore, we conducted a set of exploratory analyses with a stricter outlier exclusion criterion of 2.5 standard deviations. This removed only one additional participant from the causal/violation condition (motion-on-contact, perpendicular). With this restricted sample, the interaction between condition and test event no longer reaches significance ($p = .07$), but the pairwise comparison between the angle violation and the rotated event in the motion-on-contact condition remains significant, $t(29) = 2.13$ $p = .046$, $d = 0.78$. No other results were meaningfully affected by this exclusion.

2.3. Discussion

Previous work had shown that 6- to 8-months old infants' representations of launching event are consistent with several constraints on Newtonian elastic collisions: contact and simultaneity constraints (i.e., motion of the situational patient immediately upon contact) and the asymmetric A:B speed ratio constraint, in which 3:1 events are seen as similar to 1:1 launching events where as 1:3 triggering events are sharply distinguished from 1:1 launching. Experiment 1 adds another Newtonian constraint to this list. Habituated to launching, 8-month-old infants dishabituated to triggering, when triggering was specified by relative angle of motion, but when habituated to a delay event, they dishabituated to the changes in the individual objects' movements. Thus, infants of this age are sensitive to the relative angle of motion constraint in launching events.

3. Experiment 2

Infants clearly distinguish launching events from triggering events, whether evidence for triggering derives from the situational patient B moving too fast, given the situational agent A's speed (Kominsky et al., 2017), or because B moves in a direction perpendicular to that of A's motion (Experiment 1). We now turn to the question of whether infants represent *both* triggering and launching as causal interactions (though they distinguish the two), or whether perhaps they represent triggering as a non-causal interaction, akin to gap and delay events, in which the motions of A and B are simply independent.

We begin an investigation of this question by exploring whether infants dishabituate more to the reversal of a triggering event (where the motion of the patient begins immediately upon contact) than to the reversal of an otherwise identical delay event. This is, of course, what is observed for launching events (Bélanger & Desrochers, 2001; Leslie & Keeble, 1987). In launching events 6-month-old infants are sensitive to the reversal of causal roles (situational agent during habituation becomes situational patient at test).

In Experiments 2 and 3, as in previous experiments on triggering with infants (Kominsky et al., 2017), the ratio of speeds that specified triggering was 1:3. This ratio was chosen because previous work has found that at 6 months of age, the speeds of a single object's motion relative to another's can be discriminated only in a ratio of 1:2 (or 2:1; Möhring et al., 2012). The speeds of an object going 3 times as fast as another are clearly discriminable at the ages in these studies.

Importantly, a reversed triggering event is more different, in terms of the motions of the individual objects in the events, than a reversed 1:1 launching event. Just as in the reversed symmetric (1:1) events, reversed 1:3 triggering event change the direction of motion of the two objects and the order of their movements, but in reversed triggering events, the speeds of individual objects also change. We thus sought, in Experiment 2, to test whether an infant habituated to asymmetric 1:3 and 3:1 events would dishabituate to a reversed 1:1 event *regardless of whether there was motion on contact or a delay* (see Fig. 5). If we find such an effect in both motion-on-contact and delay cases, then it indicates that changes in the mere speed and directions of the individual objects' motions is sufficient for dishabituation regardless of causal status and, consequently, this method cannot be used to address whether triggering is construed as causal.

On the other hand, finding a greater effect in the motion-on-contact condition than in the delay condition for 3:1 habituation (launching) and reversed 1:1 test (launching) would be an extension of the Leslie and Keeble finding that infants are sensitive to reversal of agent and patient causal roles in symmetric launching events, showing sensitivity to the reversal of causal roles across asymmetric launching events (3:1) and symmetric launching events (1:1). Finding this effect in the case of 1:3 habituation (triggering) to reversed 1:1 launching would have two possible interpretations. First, it may simply be another case of distinguishing launching from triggering (Kominsky et al., 2017). Second,

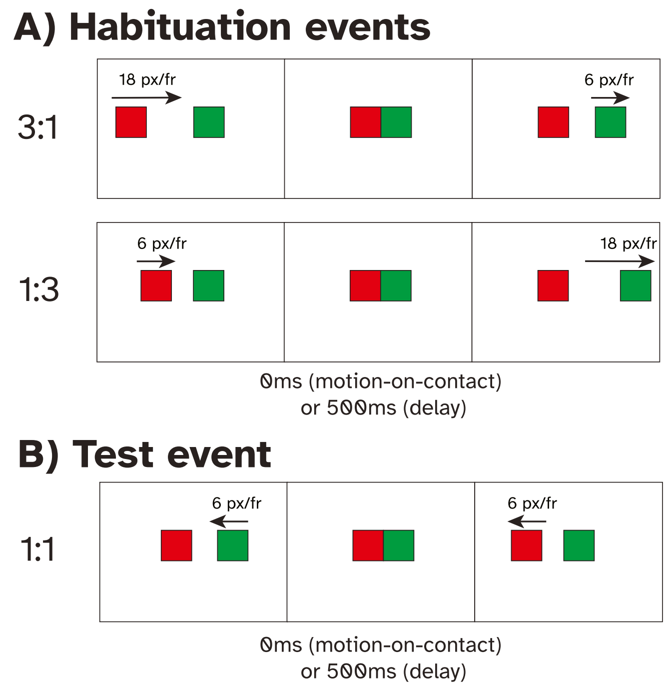

Fig. 5. Stimuli in Experiment 2.

it may extend the sensitivity to reversal of causal roles (A agent/ B patient to B agent/A patient) across causal schemas (triggering /launching). This latter interpretation presupposes that triggering is seen as a causal event. Experiment 3 adjudicates between these two possibilities.

3.1. Methods

Participants. We recruited 64 infants ages 6;15–7;15 (mean age 6;25, 28 female, 34 male, 2 not reported) from the greater Boston metropolitan area. The age was chosen to be roughly comparable to that in Leslie and Keeble (1987). This sample size per condition (16) was based on the sample size used by Leslie and Keeble (1987) of ~17 infants per condition. An additional 5 infants (4 male 1 female) were run and excluded after reliability coding under the predetermined exclusion criteria described in Appendix A. In addition, 3 infants were excluded prior to analysis due to fussiness (1), software crashes (1) and parental interference (1). No infants were excluded for having a test trial looking time >3 standard deviations from their condition mean.

Apparatus. The testing apparatus was identical to the one described in Experiment 1.

Stimuli and procedure. After providing informed consent, parents were instructed to sit in a chair facing the display screen with their infant in their lap, and asked to close their eyes and avoid interacting socially with their infant for the duration of the experiment (they were shown the stimuli afterward). They were also asked to try to prevent their infant from standing up on their lap, in order to keep the infant's face in view of the camera.

This experiment used a 2 (motion-on-contact vs. delay) x 2 (habituation speed ratio, 3:1 vs. 1:3) between-subjects design. Condition assignment was randomized. In all four conditions, the basic components of the display were the same: Infants saw an event involving a red square and a green square, each one 80 pixels by 80 pixels, one with its center at the center of the screen, and the other with its center 440 pixels to the left or right (such that the closest edges of the two squares were 360 pixels apart). Each trial started with the experimenter pressing a key to play the attention-getter, which consisted of spinning and looming yellow rectangle accompanied by a rising musical scale. Following this, both objects appeared on the screen, but the animation did not start until the experimenter pressed down a key indicating that the infant was looking at the screen. The experimenter then held down the key while the infant looked at the screen, and released the key when they looked away, until the trial ended. Trials ended after 60 s had passed since the start of the animation or the infant looked away for 2 consecutive seconds after looking at the display for at least 1 cumulative second, whichever happened first.

During habituation, infants saw the object that started on the side of the screen, A, moving toward the object in the center of the screen, B. In the 3:1 habituation speed ratio conditions, object A moved at 18 pixels/frame for 333 ms, while in the 1:3 conditions object A moved at 6 pixels/frame for 1 s. At the end of this movement, A and B were directly adjacent. In the motion-on-contact condition, object B immediately began moving away from object A. In the 3:1 condition, object B moved at 6 pixels/frame for 1 s, and in the 1:3 condition object B moved for 18 pixels/frame for 333 ms. The delay conditions were identical except that there was a 500 ms delay between when object A stopped and when object B started moving, the same delay used by Leslie and Keeble (1987) in their delay condition and in our own Experiment 1. At the end of object B's movement, both objects disappeared for 333 ms, and then the display reset and the event was repeated until the end of the trial. Whether the movement was left-to-right or right-to-left, and which object was red or green, was counterbalanced between infants in each condition. A schematic version of these events can be found in Fig. 5.

The habituation criteria were identical to those used in Experiment 1, and once habituated, infants saw a single test event. Following Leslie and Keeble (1987), the test trial was a 'reversed' version of the habituation event, but with a 1:1 speed ratio (see Fig. 5). In other words, object B started from the position where it ended in a habituation trial and moved toward object A, which was in the center of the screen. B stopped when it made contact with A, at which point A began moving away from B either immediately (in the motion-on-contact conditions) or after 500 ms (in the delay conditions). In the test event, both objects moved at 6 pixels/frame for 1 s. We used a 1:1 test event to ensure that infants would not simply dishabituate to the fact that we were changing the speeds of *both* objects in the event. Rather, in all conditions, we were only changing the speed of one object, whichever one was moving at the faster speed (A in the 3:1 conditions and B in the 1:3 conditions). As with the habituation events, the test event displayed in a loop until the infant looked away for 2 consecutive seconds after looking at the display for 1 cumulative second, or 60 s had passed, whichever came first.

Given that 1:1 launching and 3:1 launching are seen as the same kind of event at these ages (Kominsky et al., 2017), we expected infants in the 3:1 motion-on-contact condition to dishabituate to the reversal of situational roles more than the infants in the 3:1 delay condition dishabituate to the changes in the speeds, directions, and order of the two individual motions. This also being the case in the 1:3 conditions would be consistent with the interpretation that triggering is also seen as causal, with situational causal roles that generalize to those in launching. Alternatively it might simply be an extension of the previous finding that infants habituated to launching dishabituate to triggering, extending the finding to the other direction (habituate to triggering, dishabituate to launching).

3.2. Results and discussion

Habituation. Four infants never reached the habituation criterion but saw the test event after 14 habituation trials, and their data were included in the analyses. Mean looking times can be found in Table 2. Two 2 (event type: motion-on-contact vs. delay) x 2 (habituation speed ratio: 3:1 vs. 1:3) ANOVAs found no effects or interactions on the average looking time to the first three habituation trials ($ps \geq 0.14$) or the average looking time to the last three habituation trials ($ps \geq 0.24$). Infants found all four habituation events equally interesting and were equally bored by the end of habituation.

Test trial looking time. The primary DV was test trial looking time, and

Table 2
Mean looking times (in seconds) and standard deviations (in parentheses) in each condition for habituation and test trials in Experiment 2.

Cond, hab + test	Motion-on-contact		Delay	
Hab speed ratio	1:3	3:1	1:3	3:1
First 3 hab (avg)	24.27 (11.41)	21.78 (11.04)	22.90 (9.90)	17.72 (7.74)
Last 3 hab (avg)	8.26 (3.45)	9.76 (10.03)	8.78 (4.07)	6.75 (3.16)
Test trial (1:1)	22.06 (18.64)	15.05 (13.28)	11.29 (8.72)	9.87 (5.77)
Dishabituation	13.80 (16.92)	5.29 (13.32)	2.51 (9.04)	3.12 (4.84)

results can be found in Fig. 6. We conducted a 2 (event type: motion-on-contact vs. delay) x 2 (habituation speed ratio: 3:1 vs. 1:3) fully between-subjects ANOVA. This analysis found a significant main effect of event type, such that test trial looking times in the motion-on-contact conditions ($M = 18.55$, $SD = 16.32$) were significantly higher than those in the delay conditions ($M = 10.58$, $SD = 7.31$), $F(1, 60) = 6.42$, $p = .014$, $\eta_p^2 = 0.097$. There was no significant effect of habituation speed ratio, $F(1, 60) = 1.79$, $p = .186$, $\eta_p^2 = 0.029$, and no significant interaction, $F(1, 60) = 0.79$, $p = .379$, $\eta_p^2 = 0.013$. Statistically, infants in both motion-on-contact conditions looked longer at the test trials than those in the delay condition, to the same extent.[3]

Dishabituation Analyses. A post-hoc analysis of dishabituation magnitudes found qualitatively identical effects to the analysis of test trial looking times. That is, an ANOVA with dishabituation magnitudes as the DV found a significant effect of event type, such that there was greater dishabituation in the motion-on-contact conditions ($M = 9.55$, $SD = 15.59$) than in the delay conditions ($M = 2.81$, $SD = 7.14$), $F(1, 60) = 5.10$, $p = .028$, $\eta_p^2 = 0.078$. However, the main effect of habituation speed ratio was not significant, $F(1, 60) = 1.75$, $p = .19$, $\eta_p^2 = 0.028$, nor was the interaction between event type and speed ratio, $F(1, 60) = 2.34$, $p = .13$, $\eta_p^2 = 0.037$. In addition, the magnitude of dishabituation in the 1:3 motion-on-contact condition was significantly >0, $t(15) = 3.26$, $p = .02$, $d = 0.82$ (Bonferroni-corrected). None of the other three

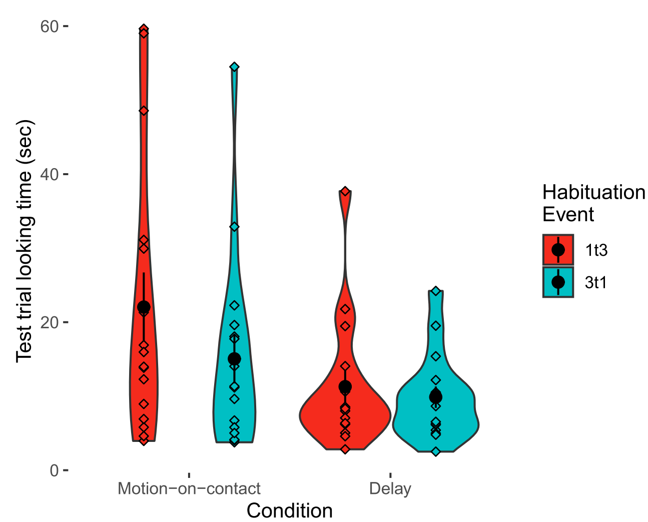

Fig. 6. Test trial looking times in experiment 2. Filled-in dots represent means, error bars represent ±1 SEM.

[3] As in Experiment 1, there are visible outliers left by the >3 standard deviation exclusion criterion, so we conducted an exploratory analysis using a more conservative criterion of 2.5 standard deviations. This removed only two participants, one participant in the causal 3:1 condition and one participant in the delay 1:3 condition. However, removing these participants did not change the significance of any of the reported analyses.

dishabituation magnitudes were significantly >0, when corrected for multiple comparisons (all *ps* > 0.4).

The first important conclusion from this study is that the differences between habituation and test in direction of motion, together with differences in as the speeds of the individual objects, were not sufficient to drive dishabituation, as seen by the lack of dishabituation in the delay conditions.

In the motion-on-contact condition, the 3:1 event type is an asymmetric launching event. Thus, the failure to find significant dishabituation to a reversed 1:1 launching event is a non-replication of Leslie and Keeble (1987)'s finding that infants habituated to a launching event dishabituate when the identity of the situational agent and the situational patient is reversed. Importantly, the degree of dishabituation in the two motion-on-contact conditions (habituation to 3:1 launching events and 1:3 triggering events, dishabituation to reversed 1:1 launching event), did not differ from each other statistically (i.e., there were no significant main effects or interactions with speed ratio), nor did the degree to which that dishabituation was greater than that in the corresponding delay condition (i.e., no significant interaction between event type and speed ratio).

It is possible that the experiment is somewhat underpowered to find an effect in the 3:1 conditions (motion-on-contact vs. delay) alone. The effect size of the result showing sensitivity to reversal of situational causal roles in the 3:1 motion-on-contact conditions (larger differences in looking to the reversed 1:1 event in the motion-on-contact condition than in the delay condition) was Cohen's $d = 0.51$, and a power analysis found that a sample size almost four times as big as the 32 we had here would be necessary to find a significant difference 80% of the time. For the 1:3 motion-on-contact condition, the effect size was larger (Cohen's $d = 0.74$), and a power analysis revealed that we would need 60 participants to find even this larger difference 80% of the time. Accordingly, we increased the sample size from 32 in this comparison to 60 for the comparable comparison exploring the reversal effect within triggering (more dishabituation in the motion-on-contact reversal than in the delay reversal) in Experiment 3.

The situational role reversal effect in the case of habituation to 1:1 launching and test on reversed 1:1 launching is robust. Leslie found it with a sample size of 34 (17 per group) (Cohen's $d = \sim 0.7$) and Bélanger and Desrochers (2001) replicated a difference between reversing launching and reversing a spatial gap event with a sample size of only 16 (8 per group), though they did not find a significant effect contrasting a reversed launching and reversed delay event with this smaller sample size. Further research should explore whether the smaller effect from 3:1 launching to reversed 1:1 launching than from 1:1 launching to reversed 1:1 launching is robust. There are mixed reports about the effect of increasing the relative speed of A in adults' perception of launching. Michotte (1946/1963) reported that the launching impression was actually *stronger* at ratios of 3.6:1 than 1:1 (Experiment 39), while Natsoulas (1961) found that participants asked to classify events as "Launching", "Releasing", or "Braking" were more likely to call 3:1 events "Braking" than "Launching". In short, these asymmetric launching events are understudied, but one way to explore this in the future would be to double the sample sizes, habituate infants to 3:1 launching events and test them on reversed 3:1 launching events and on reversed 1:1 launching events. Importantly, Kominsky et al. (2017) found that infants habituated to 1:1 launching generalized habituation to 3:1 launching (while dishabituating to 1:3 triggering), indicating that infants roughly this age see 3:1 motion-on-contact events as not meaningfully different from launching.

There are two important results from Experiment 2. The first is infants' failure to dishabituate to the reversal in either of the delay events. This result establishes that *in themselves* changing the speed of one of the objects, reversing the direction of motion of each object, and changing the order in which the two objects move are not immediately noticeable to infants. This means that these changes will not, in themselves, explain dishabituation, if observed, in Experiment 3 (1:3 triggering habituation; reversed 1:3 triggering test). This result establishes that this method can be used to study whether triggering events are represented in terms of situational agents and situational patients at the ages at which launching events are.

The second important result was the greater looking to the reversed test events in the motion-on-contact than in the delay conditions. This was expected in the 3:1 (launching) condition because reversed launching changes the identity of a situational agent and a situational patient. That this was found over *both* motion-on-contact conditions is consistent with the possibility that triggering (1:3 motion-on-contact) events are also seen as causal, and so the reversed 1:1 launching event involves a change in situational agent and situational patient relative to both habituation events. If this is the right interpretation, then infants habituated to triggering should dishabituate to a reversed 1:3 triggering event, a hypothesis we test in Experiment 3.

However, Kominsky et al. (2017) found that infants of this age habituated to a launching event dishabituate to a triggering event. This alone may drive the effect seen here, as 1:3 events are triggering events and 1:1 events are launching events, and infants were habituated to the former and tested on the latter, i.e., an inversion of the previous study. Thus, the significantly greater dishabituation in the 1:3 motion-on-contact condition than in the 1:3 delay condition may not provide evidence that 7-month-old infants understand triggering as a causal event in the same way that they understand launching as a causal event. Indeed, it is possible that they do not have a schema for triggering at all, or do not consider triggering to be a causal event at all, in which case these results could also be viewed as a replication of Cohen and Amsel (1998), who found that infants habituated to a non-causal event will dishabituate to a launching event. Experiment 3 is a first step in testing the hypothesis that infants may not see triggering as a causal interaction at all.

4. Experiment 3

Having established that simply changing the speeds of the objects would not create dishabituation effects independent of causal attribution, we now seek to straightforwardly replicate Leslie and Keeble (1987) using triggering events (with a 1:3 speed ratio) instead of launching events, both during habituation and at test. In other words, in the motion-on-contact condition, infants are habituated to A triggering B, and are tested on B triggering A, while in the delay condition they see the same events but with a 500 ms delay between contact and the subsequent motion of the previously stationary object both during habituation and test. If infants represent triggering as a causal event in which A initiates B's motion upon contact, then they should be sensitive to a reversal of the situational causal roles in the motion-on-contact condition, but not in the delay condition.

4.1. Method

Participants. As mentioned above, the sample size of 60 children was chosen on the basis of a power analysis based on the effect size of the motion-on-contact vs. delay contrast in the 1:3 (triggering) condition from Experiment 2, which had a Cohen's *d* of 0.74. This also is roughly the same effect size as in the Leslie and Keeble (1987) study ($d = 0.7$). The power analysis showed that in order to reach 80% power to detect a significant difference ($p < .05$) between looking times to the reversed 1:3 triggering (motion-on contact) event, on the one hand, and to the reversed 1:3 delay event, on the other, we would need to recruit about 30 participants in each of the two conditions. We also widened the age range slightly, from 6;0 to 8;0, in order to allow for more flexible recruitment. The study was preregistered at https://osf.io/sy2bf.

The final sample had 60 infants (mean age 7;3, 30 female, 29 male, 3 not recorded), but due to imperfect randomization there were 31 in the causal condition and 29 in the delay condition. This was partially caused by an unexpected complication during data collection: the COVID-19 pandemic. The data reported here come from three different data collection sites: the Harvard Lab for Developmental Studies in Cambridge, MA, USA; online participants from the US recruited through Children Helping Science (Kominsky et al., 2021; Sheskin et al., 2020); and the Kinder Kognition (KiKo) Lab at Central European University in Vienna, Austria. The procedure was identical other than the setting in which the participants were run (though with some modifications to the apparatus for running on Zoom, see below). Participants are reported broken down by testing location, but to foreshadow our results, there were no detectable differences between these three populations.

The final sample includes 17 participants from the greater Boston metropolitan area at the Harvard Lab for Developmental Studies (11 female, 6 male), 9 in the motion-on-contact condition and 8 in the delay condition. An additional 3 participants (gender not recorded) were excluded during data collection due to fuss-outs, 1 (female) participant was excluded due to technical issues, 7 (2 female, 5 male) participants were excluded after reliability coding (see Appendix A), and one was excluded for having a test trial looking time > 3 standard deviations from the average for their condition (Delay condition, 4.25 standard deviations from condition mean).[4]

The final sample also includes 11 participants (2 female, 7 male, 2 not recorded) who were run online over Zoom (see below), 4 in the causal condition and 7 in the motion-on-contact condition. An additional 3 participants were excluded due to fuss-outs or technical issues, and one was excluded for having a test trial looking time > 3 standard deviations from the average for their condition (Causal condition, 3.78 standard deviations from condition mean).

The final 32 participants in the final sample (17 female, 15 male) were run at the KiKo Lab at Central European University in Vienna, Austria, 18 in the motion-on-contact condition and 14 in the delay condition. An additional 2 infants (both female) were excluded due to fuss-outs, and 14 (9 female and 5 male) were excluded and replaced following reliability coding.

Apparatus. The apparatus at the Harvard Lab for Developmental Studies was identical to the one used in Experiments 1 and 2. The apparatus at the KiKo Lab at Central European University was highly similar, with a slightly smaller screen (54 cm by 31 cm) and the infants sitting slightly closer (~60 cm). However, the screen resolution and PyHab control software (Kominsky, 2019) were exactly the same.

The online sample were collected sitting in their own homes over Zoom using a modified version of PyHab and a website called slides.com, as described in Kominsky et al. (2021). The experimenter's experience and control of the experiment was largely identical to the in-lab version, except that the infant was shown in a Zoom video call. However, the stimulus presentation was controlled through slides.com. Parents opened a web browser window with a link to the study's presentation, which consisted of separate slides for each attention-getter and trial (each one its own movie file), and a blank slide. PyHab then opened a Python-controlled browser on the experimenter's computer and accessed the slides.com presenter interface for this presentation, and then navigated to each slide to present the corresponding stimulus, and the blank slide between trials. The movie files themselves were captured from PyHab's displays and so have all the same properties, but the actual display dimensions and viewing distance varied from participant to participant. These variations were not expected to have a meaningful effect on infants' responses (for further discussion of remote vs. in-person methods with infants, see Chuey et al., 2021).

Stimuli and procedure. The stimuli and procedure were identical to Experiment 2 with the following exceptions: All habituation and test events involved 1:3 speed ratios, and there were only two conditions, "motion-on-contact/triggering" and "delay". The habituation events were exactly as described for the 1:3 habituation conditions in Experiment 2, including the 500 ms delay between contact and the movement of object B in the delay condition. The test event once again involved a reversal, but in this experiment, the test event also had a 1:3 speed ratio such that object B approached object A at 6 pixels/frame, and object A moved away at 18 pixels/frame. Note that this means that the speeds of both objects were different compared to the habituation events, in addition to changing the direction and order of motion.

Reliability coding and exclusion criteria. The reliability coding and exclusion criteria were identical to Experiment 2, and preregistered for this experiment (see Appendix A).

4.2. Results and discussion

Habituation. Three infants in the motion-on-contact condition and one in the delay condition failed to reach the habituation criterion after 14 habituation trials, but they were included in the analysis regardless. Table 3 presents the looking time data during habituation and in the test trials. There were no significant differences between conditions in looking times during the first three or last three habituation trials, *ps* > 0.3. Infants were initially equally interested in the triggering and the delay display, and had become equivalently bored to the two by the end of habituation.

Test events. Because of the pandemic induced move to on-line testing, and a move to a new lab, we added a 2 (motion-on-contact vs. delay) x 3 (data collection site, Harvard vs. online vs. KiKo) ANOVA to our preregistration (prior to any analyses), to determine if there were any differences based on where/how the data were collected. The DV was looking time during test trials. This analysis found no main effects or interactions involving data collection site, all $ps \geq 0.37$. We therefore collapsed the data from all three sources for the main preregistered analyses.

The results can be found on Table 3 and in Fig. 7. We preregistered a straightforward *t*-test comparing motion-on-contact test trial looking times to delay test trial looking times in each condition, using a student's *t*-test following a Levene test to verify that the variances in the two groups were equal ($p = .38$). There was no significant difference in test trial looking times between the motion-on-contact ($M = 11.39$, $SD = 7.77$) and delay conditions ($M = 10.49$, $SD = 6.15$), $t(58) = 0.49$, $p = .62$, $d = 0.13$. That is, infants showed no sign of selective sensitivity to the reversal of situational causal agent and situational patient in the motion-on contact condition. Reversed triggering, like reversed entraining, does not pattern with reversed launching (Bélanger & Desrochers, 2001; Leslie & Keeble, 1987) .

Dishabituation. Of course, as infants in both conditions were equally habituated at the end of habituation and looked equally long at the test events, the dishabituation measures (test trial looking time - mean looking times for last 3 habituation trials) also did not differ between condition (motion-on-contact = 2.77; delay = 2.09), $t(58) = 0.35$, $p = .72$, $d = 0.09$. This could be because they dishabituated to both test events, being sensitive to the change of the rate of motion of *both* objects. Overall, the magnitude of dishabituation from both conditions combined ($M = 2.44$, $SD = 7.31$) was significantly >0, $t(59) = 2.58$, $p = .012$, $d = 0.33$, indicating that infants did recover interest to some degree due to the changes in the low-level features in both displays. Compared to Experiment 2, aside from having much greater power to detect this difference, the current experiment involved more changes in low-level features from habituation to test. That is, in addition to changing the

Table 3
Results of Experiment 3. Numbers in parentheses represent standard deviations.

Condition	Motion-on Contact	Delay
First 3 hab (avg)	26.09 (12.21)	22.61 (9.73)
Last 3 hab (avg)	8.62 (3.60)	8.41 (4.27)
Test trial	11.39 (7.77)	10.49 (6.15)
Dishabituation	2.77 (7.33)	2.09 (7.41)

[4] In this experiment, changing the exclusion criterion to 2.5 standard deviations did not result in any additional exclusions.

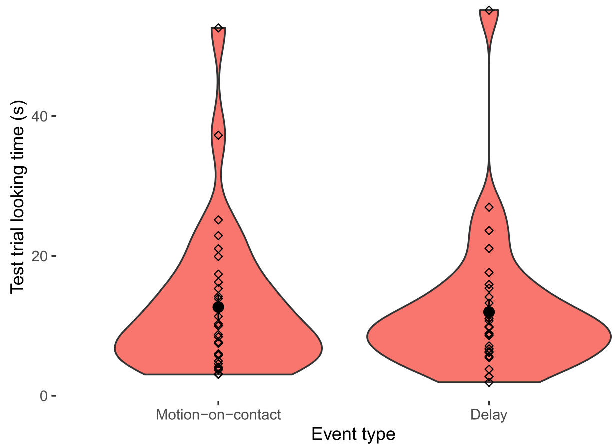

Fig. 7. Results of Experiment 3. Filled-in dots represent means, error bars represent ±1 SEM.

direction and order of motion of both objects, the speeds of *both* objects were individually different.

The important finding is that there was no greater dishabituation to reversed event in the motion-on-contact condition than in the delay condition, unlike in Leslie and Keeble (1987)'s results, and also unlike the finding of Experiment 2, where, when habituated to 1:3 triggering, infants dishabituated very strongly to a reversed 1:1 launching event, and where this difference between habituation and test was significantly greater than in the delay condition. These results support the conclusion that the dishabituation seen in Experiment 2 in the 1:3 motion-on-contact condition was due to the change from a triggering event to a 1:1 launching event, not the reversal of situational causal roles.

One way of testing this hypothesis is to compare the difference in dishabituation magnitude between the motion-on-contact and delay conditions between the current experiment and the 1:3 conditions of Experiment 2. If the dishabituation effect is due to the event category boundary between habituation and test in Experiment 2 that was not present in Experiment 3, there should be a greater difference between the dishabituation magnitude in motion-on-contact and delay 1:3 conditions in Experiment 2 (habituation, 1:3 triggering; test, reversed 1:1 launching) compared to that in Experiment 3 (habituation, 1:3 triggering, test, reversed 1:3 triggering), i.e., an interaction effect. We therefore conducted a post-hoc 2 (motion on contact vs. delay) x 2 (Experiment 2 vs. Experiment 3) between-subjects ANOVA with dishabituation magnitude as the DV. We note in advance that this is an underpowered analysis as our experiments were not designed with it in mind, and the sample size was smaller in Experiment 2. Thus, this analysis should be interpreted with caution. That said, we found significant main effects of motion-on-contact vs. delay, $F(1,88) = 4.29$, $p = .041$, $\eta_p^2 = 0.048$, and experiment, $F(1, 88) = 7.10$, $p = .009$, $\eta_p^2 = 0.075$, and critically, a significant interaction, $F(1,90) = 5.96$, $p = .017$, $\eta_p^2 = 0.063$, indicating that the difference between the motion-on-contact condition and the delay condition was significantly greater in Experiment 2 than in Experiment 3.

In sum, our results suggest that the effect in the 1:3 conditions of Experiment 2 was due to the same effect observed in Experiment 1 and Kominsky et al. (2017): a categorical difference between launching and triggering events, rather than a sensitivity to reversal of situational causal roles in triggering events.

Altogether, the results of this experiment suggest that infants do not represent situational causal roles in triggering events, and thus they either do not represent triggering events as causal at all, or have a completely distinct causal representation of triggering from the representation they have for launching, just as they do for entraining relative to launching (Kominsky et al., 2022).

5. General discussion

Six- to 9 1/2-month-old infants clearly represent Michottean launching events as causal. Here we presented three experiments that addressed whether infants of this age distinguish launching from Michottean triggering events and that began to explore whether they represent triggering events as causal.

Experiment 1 found that 7 1/2- to 9 1/2-month-old infants habituated to horizontal 1:1 launching events generalized habituation to a vertical 1:1 launching event, while strongly dishabituating to a perpendicular motion triggering event. In contrast, when habituated to a 1:1 delay event, the infants dishabituated significantly to the vertical delay event, the event in which the total differences in the motions of each individual object were maximally different between habituation and test. These results provide new evidence that infants have a representational schema for launching. The data show that infants are sensitive to a previously unstudied real-world constraint on Newtonian elastic collisions, namely, the relative angle constraint. This is in addition to the previously demonstrated sensitivity to the motion-on-immediate-contact constraint and the asymmetric A:B speed ratio constraint (1:3 speed ratio, triggering, distinguished from 1:1 launching; 3:1 speed ratio not distinguished from 1:1 launching). They thus converge with the findings of Kominsky et al. (2017): Eight-month-old infants distinguish launching events from triggering events, both when triggering reflects B's moving too fast, relative to A, or moving in the wrong direction relative to the trajectory of A.

Experiments 2 and 3 turned to the question of whether 6–8-month-old infants represent triggering *as causal*, exploring whether infants are sensitive to a change in the causal roles of objects (agent to patient; patient to agent) in triggering events. They are not. The two experiments together confirmed that reversing the direction of the event and changes in the speeds of individual objects between habituation cannot explain all of the dishabituation seen in the change from 1:3 and 3:1 motion-on-contact habituation events to reversed 1:1 test events (Experiment 2), and that infants distinguish triggering from launching even under these conditions. If infants were sensitive to the reversal of situational causal roles from 1:3 triggering to reversed 1:1 launching, they certainly should be sensitive to a reversal of situational causal roles from 1:3 triggering to reversed 1:3 triggering. Contrary to this hypothesis, Experiment 3 showed that infants habituated to a 1:3 triggering (motion on contact condition) showed no greater dishabituation to the test trial than did infants habituated to a 1:3 delay event, in spite of adequate power to detect this effect. Taking the motion-on-contact and delay conditions together, dishabituation was >0, reflecting the power to observe sensitivity to the reversal of direction of motion, a change in which object moved first, and a change in the speeds of each object. But this was equally so for the motion-on-contact (triggering) condition and the delay (non-causal) condition.

Thus, this experiment provides no evidence that 6–8-month-old infants structure their representation of triggering events in terms of a situational causal agent initiating, or causing, the motion of a situational patient. In this regard triggering patterns with entraining; infants of this age are also insensitive to a reversals of situational causal roles when habituated to one object with no features of a dispositional agent entraining the motion of another novel object (Bélanger & Desrochers, 2001). Of course, other work shows that entraining events are articulated in terms of representations of dispositional agents and situational patients; infants of this age explain away the motion of the entrained object and thus do not attribute dispositional agency to that object as a result of its being a situational patient (Kominsky et al., 2022; Träuble & Pauen, 2011). It is currently unknown whether inferences about triggering reflect causal attributions in infancy.

Notably, if subsequent work continues to find that infants do not see triggering as causal, this would differ from the adult representation of triggering. In explicit report studies, adults judge collision events with speed ratios as much as 1:4 as 'causal', and in some cases even *more* causal than 1:1 collision events (Vicovaro, 2018; White, 2006; White, 2009). As such, the current findings provide further evidence that the causal event categories we find in infants, and the inferences they support, may be very different from what we find in adults (see also Kominsky et al., 2022).

5.1. What is triggering for 6- to 9-month-olds?

These results do not definitively establish that triggering is *not* represented as a causal event, they only fail to find evidence that it *is*. However, they highlight several open questions about how a triggering event is represented in infancy that should be explored in future work.

If there is an event schema for triggering in the infant's mind, it is possible that triggering is represented as causal, but the inferences it supports are different from launching, as is the case for entraining. In other words, we failed to find evidence that triggering is represented as causal because we were looking for the wrong kind of evidence. One possibility is that triggering gives insight into the dispositional status of the causal patient. The key feature of triggering is that the movement of object B cannot be explained by the force of the collision with A alone. In the real world, in many cases, triggering occurs because object B is an animate agent with an internal source of motion (e.g., startling a cat). Therefore, we might expect infants would infer that a triggered object has insides (Kominsky et al., 2022; Setoh et al., 2013). However, we would expect the same to be true of an object in a delay or other non-causal event, since by definition objects in those events move independently. Therefore, even finding evidence that infants make dispositional inferences that triggered objects are animate would not necessarily demonstrate that triggering is represented as causal.

The key feature that distinguishes triggering from non-causal delay events, in principle, is that the motion of object B is *initiated by contact* with object A. Therefore, the Ball (1973) paradigm in which infants see object A move behind an occluder, and a partially-occluded object B start moving immediately after, might be a promising approach to pursue. Infants at 6–8 months of age infer that contact has occurred in 1:1 launching events, when object B is not a dispositional agent (Kosugi & Fujita, 2002; Woodward et al., 1993), but this has only been tested in cases where the speed and direction of B is similar to those of A. It is possible that infants infer contact even in cases where B's movement defies Newtonian constraints on speed or angle, which would indicate that they do have a causal representation of triggering that explains the behavior of object B as being initiated by A.

But it is also possible that there is no event schema for triggering at all in infancy. That is, although triggering is distinguished from launching, it may be represented like gap events and delay events – merely two independent motions. There is currently no evidence that motion-on-contact triggering is distinguished from a minimally-different non-causal event. Testing this would be straightforward as well. Following Cohen and Amsel (1998), 6–8-month-old infants could be habituated to 1:3 motion-on-contact events or 1:3 events with a delay or 1:3 events with a spatial gap, and then tested on one of the events they did not see. If infants selectively dishabituate more when going from a motion-on-contact event to a gap or delay event (and vice versa), than from going to a gap to a delay event (and vice versa) it would indicate that there is a triggering event schema. However, if infants do not have a distinct representation for triggering, and it is merely 'not-launching', then they would dishabituate equally regardless of which 1:3 events they saw at habituation and test. These are important questions for future work to address.

5.2. How broad is the infant's category of launching events?

Kominsky et al. (2017) showed that 7–9-month-old infants, habituated to 1:1 launching, generalize habituation to 3:1 launching, while strongly dishabituating to 1:3 triggering. This finding indicates that 3:1 launching falls under the same event schema as 1:1 launching. But in the current Experiment 2, the effect size of dishabituation from 3:1 launching to reversed 1:1 launching was numerically (but non-significantly) less than previous reports of dishabituation from 1:1 launching to reversed 1:1 launching. This finding may be noise due to small sample sizes. Future work should further explore whether 3:1 launching is represented identically to 1:1 launching (i.e., habituate 3:1 launching, dishabituate to reversed 3:1 launching as well as to reversed 1:1 launching) to confirm whether 3:1 launching events are represented in terms of situational agents and patients. Future work should also study 3:1 launching in the Ball (1973) paradigm; habituate to a hidden interaction between an object that goes behind a screen, followed by the motion of a partially hidden object, where the motion of the first object is 3 times the speed of the second, and test whether infants expect contact between the two objects, indicating that they explained the motion of the second object by its being hit by the first. Further, the exact speed ratio could be varied: if the asymmetric speed ratio constraint allows launching events with widely different faster speeds of A to B (e.g., 5:1 events, 4:1 events, 3:1 events, 2:1 events) to fall under the same causal schema as 1:1 launching events, then all of these events should show all of the signatures of infants' representations of launching as causal events.

5.3. How Newtonian is 6- to 8-month-old infants' representations of launching events?

The research so far shows that infants' launching schema is sensitive to several Newtonian constraints on elastic collisions: motion immediately on contact, the relative speed constraint, the relative angle constraints. Such results are consistent with the proposal that perception, and infant core cognition, includes a "physics engine," as do video games, that embodies Newtonian principles (Ullman, Spelke, Battaglia, & Tenenbaum, 2017).

The question, though, is how much of Newtonian mechanics is embodied in this schema. This question has barely begun to be addressed. Does the infant constrain their expectations about the motion of the objects if they have information about their relative mass? We know of one study that suggests that they might: Kotovsky and Baillargeon (1998) habituated 6.5-month-old babies to a launching event where the agent A hit the patient B which travelled a constant distance across trials stopping in full view. They were the shown A hitting a larger B, or a smaller B, and they expected less motion before stopping for the larger B and more motion before stopping for the smaller B. Of course this is a generalization about size. It is unknown whether infants would make similar attributions if given tactile information about relative weights of same size objects, and it is extremely unlikely that generalization is actually in terms of actual mass. It is virtually certain that infants lack perceptual information about gravitational force.

Of course, none of these events match what would happen in frictionless environment; B's speed would be predicted by relative mass between A and B (up the to absolute 1:2 limit), but A would only stop if its mass were *precisely* equal to B's. Perhaps the hypothesized innate physics engine embodies expectations of friction between A and B and the surface it is resting on, and ways of computing the value of the frictional force in operation. In an idealized elastic collision in a frictionless environment, a 3:1 event would involve A rebounding after the collision. Indeed, any case where B has more mass than A should result in this. Conversely, in situations where A has more mass than B, A would not be brought to a complete stop by the collision; it would continue forward at a slower speed. Would infants treat such events as launching? Would infants be sensitive to the properties of glancing collisions, in which A should continue moving following contact and B's movement should not be directly in line with A's? No work that we know of has examined infants' reactions to such events, although adult perception does treat collision events in which A continues at a slower speed as belonging to the same category as launching (Kominsky et al., 2017; Kominsky & Scholl, 2020).

In addition there are at least two other events that have been studied in adult causal perception that also correspond to Newtonian elastic collisions: 'Bursting' (or 'enforced disintegration') events (White & Milne, 1999) in which object B splits into several smaller objects, and 'tool effect' events in which A contacts a stationary object that is in contact with B, causing B to move (like a "Newton's cradle" toy; Hubbard, 2013; Michotte, 1991). To start with, the same constraints on relative speed and angle that apply to launching events also apply to these events. If infants truly have a representation of 'Newtonian elastic collisions' in general, they might be sensitive to situational role reversals in these events as well, and distinguish them from minimally different delay events.

The fullness of the schema bears on the hypothesis of an innate Newtonian physics engine. It is unlikely that infants as young as 6 to 8 months of age have observed enough billiard-ball-type interactions to learn the statistics of these events, but of course this is an empirical question. The data bases from mounted camera studies of what young infants see should be analyzed with this question in mind, but at least one study suggests that infants see many 'entraining'-like events and very few Newtonian elastic collisions (Cicchino et al., 2011).

5.4. Beyond Michottean launching, entraining, and triggering events

Previous research has shown that launching is represented as distinct from entraining, and that both are represented as causal. Here we confirm previous research in showing that launching is distinguished from triggering, while also seeking but failing to find evidence that triggering is represented as causal. Very few other schemas of causal interactions have been studied in early infancy, but there is evidence for at least two others: representations of state changes caused by contact (e.g., an object's changing color or making a sound upon contact from another object, an object collapsing upon contact from another object; Liu, Brooks, & Spelke, 2019; Muentener & Carey, 2010), and representations of expulsion, or throwing (Saxe, Tenenbaum, & Carey, 2005; Saxe, Tzelnic, & Carey, 2007). These event schemas are both represented as causal.

With respect to state changes, Muentener & Carey (2010) showed that 8-month-old infants expect contact between a situational agent and a partially occluded patient that underwent a state change shortly after the agent went behind the occluder (a version of the Ball paradigm). In addition, this inference depended upon the situational agent being a dispositional agent (a hand, or a toy with eyes shown capable of self-generated motion). Liu et al. (2019), found a similar pattern of results at 3 months of age, the youngest age to date for which there is evidence of causal representations in infancy, and showed additionally that the dispositional agent must be acting rationally during habituation, taking the shortest possible path to the patient, for the causal attribution to be made.

With respect to representations of expulsion or throwing, Saxe et al. (2005, 2007) found rich integration of information about the potential situational patient and the potential situational agent in events in which an unfamiliar object came flying into view from off stage. If a known self-moving object comes flying into view, infants do not infer a thrower, but if a dispositionally inert object does, infants expect a dispositional agent to be revealed in a previously hidden place the moving object emerged from.

These latter event schemas merit further research exploring whether they are sharply differentiated from entraining. But in all of this work, including entraining, the evidence for causality derives from inferential integration with representations of dispositional causal or intentional agency, and not from intuitive physics. It may be that contact is the only physical constraint on causal interactions in these events.

6. Conclusion

While we cannot reach firm conclusions about how triggering events are represented, the present studies provide new evidence that they are not represented like launching events. It adds to the evidence that as soon as infants represent launching at all, their representations respect several Newtonian constraints on real-world elastic collisions. Our findings align with recent proposals that there is no single concept of 'causality' in the infant mind, but rather that infants understand multiple different types of causal events independently in the first year of life (Kominsky et al., 2017; Kominsky et al., 2022; Muentener & Bonawitz, 2017). This work presents clear and testable questions for future work seeking to characterize the ontogenetic origins of causal thought.

CRediT authorship contribution statement

Jonathan F. Kominsky: Writing – review & editing, Writing – original draft, Software, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Susan Carey:** Writing – review & editing, Writing – original draft, Supervision, Resources, Funding acquisition, Conceptualization.

Data availability

Data for all three experiments and the preregistration for Experiment 3 can be found at https://osf.io/sy2bf/.

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Appendix A. Preset exclusion criteria (preregistered in Experiment 3)

Participants were excluded and replaced under the following predetermined rules.:

1. Following the collection of a complete sample group, after all of the below exclusions, if a participant's looking time during the test trial was >3 standard deviations from the mean on their group, they were excluded and replaced.
2. Parental interference during the test trial (such as speaking to get the baby's attention, or reorienting them during the trial to point them toward the screen).
3. Fussing out (e.g., crying, fretting, squirming).
4. Moving off-camera during the experiment.
5. Disagreement during (condition-blinded) reliability coding based on the following conditions:
    - 5.1. f the coders disagreed on the infants looking time during the test trial by >10% of the longer of the two coded values.
        - 5.1.1. If the disagreement was <300 ms over this threshold, a third coder recoded the video and if their recoding was in agreement with one of the two earlier coding, that coding was used in the analysis.
    - 5.2. If the second coding indicated that the test trial ended prematurely (i.e., even if the difference was <10%, if the trial was ended during the experiment when the secondary coder's coding showed that the infant did not look away for 2 consecutive seconds).
        - 5.2.1. If the second coder's coding indicated that the trial ended to early but also said the infant looked away for 1.7 s or more (i.e., within 300 ms of a long enough look-away to

end the trial), a third coder recoded the video, and if they agreed with the original coding the data were retained.

5.3. If the second coding indicated that the habituation ended prematurely (i.e., the habituation criterion was not met by the time the test trial was actually displayed according to the second coder's coding).

5.3.1. If the margin by which the infant did not meet the habituation criterion was 300 ms or less in the secondary coding, a third coder recoded the video, and if they agreed with the original coding, the data were retained.

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