Abolishing the effect of reinforcement delay on human causal learning

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Associative learning theory postulates two main determinants for human causal learning: contingency and contiguity. In line with such an account, participants in Shanks, Pearson, and Dickinson (1989) failed to discover causal relations involving delays of more than two seconds. More recent research has shown that the impact of contiguity and delay is mediated by prior knowledge about the timeframe of the causal relation in question. Buehner and May (2002, 2003) demonstrated that the detrimental effect of delay can be significantly reduced if reasoners are aware of potential delays. Here we demonstrate for the first time that the negative influence of delay can be abolished completely by a subtle change in the experimental instructions. Temporal contiguity is thus not essential for human causal learning.

An associative learning analysis of human causal learning postulates two main determinants of judged causal strength: the contingency and the contiguity between the potential cause (cue) and the effect (outcome) (e.g., see Shanks & Dickinson, 1987). Empirical research in the last decades has largely focused on the congruency between cue–outcome contingency and judged causal strength. Early reports suggested that human causal judgements closely track variations in cue–outcome contingency (e.g., Jenkins & Ward, 1965), while more recent studies revealed a more complex picture (e.g., Chapman & Robbins, 1990). In fact, the theoretical and empirical relations between contingency and judged causality are still the subject of a hot debate (e.g., Buehner & Cheng, 1997; Cheng, 1997; Dickinson, 2001; Lober & Shanks, 2000; for an overview, see Shanks, Holyoak, & Medin, 1996). In comparison to contingency, contiguity has received relatively little attention. In a seminal paper, Shanks, Pearson, and Dickinson (1989) established the importance of temporal contiguity for human causal induction: They reported that participants could no longer distinguish between causal and noncausal actions when actions and outcomes were separated by more than two seconds. A few follow-up studies employing a similar paradigm (Reed, 1992, 1999) replicated the effect: Reinforcement delays always impaired causal learning. The notion that temporal contiguity is important, or even essential, for human causal learning has been widely accepted in the literature (for the classic...
An alternative approach to human causal learning is causal power theory (Ahn, Kalish, Medin, & Gelman, 1995; Bullock, Gelman, & Baillargeon, 1982; and Cheng, 1997, for a computational causal power account), according to which human reasoners postulate that (often unobservable) causal mechanisms with the power to produce or prevent events exist in the environment, and that these powers may manifest themselves through observable contingencies. Knowledge (or inference about the properties) of a plausible causal mechanism plays a key role in such theories, and therefore these theories predict that the impact of both contingency and contiguity should be mediated by prior knowledge or assumptions about the mechanism in question. Empirical research has shown that the degree to which variations in cause–effect contingency influence causal judgments indeed depends on such assumptions about the causal mechanism (see e.g., White, 1995; or Ahn et al., 1995). Whether the same is true for contiguity has been less clear, however. Knowledge about the mechanism by which the cause produces the effect automatically entails assumptions about the timeframe over which the relation should unfold. Consequently, the role of temporal contiguity should crucially depend on prior assumptions about the causal mechanism in question. If the reasoner assumes a mechanism that brings about the effect immediately, degradations of cause–effect contiguity should prove to be detrimental to causal learning; if, however, the reasoner assumes a delayed mechanism, such as between pressing a button at a pedestrian crossing and the light changing, there is no reason to expect a detrimental influence of delay.

According to associative learning theory, regularities in the environment, such as the repeated simultaneous presence of a cue and outcome, lead to the formation of mental associations between the constituent events. The strength of such an association depends on the stimulus properties (i.e., their saliencies) and the learning history (i.e., how often the pairing is experienced and how close in time it was). Following Hume’s (1739/1888) empiricist argument, abstract knowledge in such an account is nothing else but (a network of) associations. Causal power theory is not limited to association weights, however, and has room for rich representations of mechanism and structure (Waldmann, 1996; Waldmann & Holyoak, 1992). Consequently, preexisting mental concepts can determine how sensory experience will be parsed and processed. This is not possible within a standard associative perspective, however. Whereas causal power theory allows and even predicts that identical sensory experiences should be parsed differently, depending on the reasoner’s assumptions about the properties of the task, such flexibility is not granted in standard associationism: Abstract ideas cannot influence how sensory experience will be parsed. In other words, according to an associative learning perspective, the same covariational evidence should always give rise to the same causal impression, whereas according to causal power theory, the same evidence may be interpreted differently, if the assumptions vary. For the impact of contiguity and delay on causal induction this implies that associationism predicts a robust and stable detrimental influence of cause–effect delays on causal learning, whereas causal power theory predicts that the impact of delay depends on prior knowledge about the timeframe of the relation in question.

Recent research from our laboratory (Buehner & May, 2002, 2003) has tried to disentangle the predictions regarding cause–effect delays from associative learning and causal power theories. As Shanks et al. (1989) pointed out: “Subjects in judgment studies such as ours assume that the word ‘causes’ in the experimental instructions means ‘causes
immediately’. After all, they presumably have considerable experience of the immediacy of cause–effect relations in such electrical devices as computers” (Shanks et al., 1989, p. 155). Consequently, participants in Shanks et al.’s study may have simply refrained from attributing causality to delayed cue–outcome pairings because they always expected immediate consequences. They might have performed more accurately if they had known that the apparatus might involve a delay. In order to test this idea, Buehner and May (2002, 2003) replicated Shanks et al.’s design with one important addition: They manipulated whether or not a cause–effect delay was plausible either by explicitly mentioning the delay to participants in the instructions (Buehner & May, 2003), or by implicitly inducing different delay assumptions via different cover stories (Buehner & May, 2002). Both studies showed that the extent to which cue–outcome delays impaired causal learning was significantly reduced when participants were led to believe that the causal relation in question involved a delay.

While Buehner and May’s (2002, 2003) results support the idea that knowledge mediates the timeframe of covariation assessment in human causal induction (cf. Einhorn & Hogarth, 1986), they still suggest that the impact of temporal contiguity is superordinate to the influence of abstract knowledge: After all, cause–effect delays always impaired causal judgments, albeit to a lesser extent when participants were made aware of potential delays. If abstract knowledge about the properties of the causal relation is as important as the causal power account postulates, it should be possible to abolish any detrimental effects of delay completely. Alternatively, it could be that there is a strong contiguity bias, which will always override the influence of abstract knowledge.

**EXPERIMENT**

Buehner and May’s (2002, 2003) manipulations regarding the knowledge about the timeframe of the relation in question may not have been strong and convincing enough to completely abolish the detrimental effect of delay. The scenario and instructions intended to make a delay plausible asked participants to imagine that they are firing off grenades and observing explosions several miles away. This scenario was probably not very familiar to the student sample, none of whom were likely to have experienced such a situation in real life. Consequently, the assumptions about the timeframe Buehner and May tried to induce via this scenario may not have been stable and strong enough to completely abolish the detrimental effect of delay. In this experiment we adopted a similar paradigm to that of Buehner and May (2003), but we employed a scenario that should be very familiar to our student sample: judging whether a light switch makes a bulb light up or not. To induce different assumptions about the timeframe of the causal relation, one group of participants were told that the bulb was an ordinary light bulb that should light up right away, while another group of participants was instructed that the bulb was an energy-saving bulb that takes 4s to light up. As in the earlier studies, we crossed the factors *time(delay)* and *cover story* in a between-subject design, and we adapted the free-operant procedure used by Shanks et al. (1989) with a few modifications to improve ecological validity and to avoid a confound of delay with lower objective contingency (see Buehner & May, 2003).
Method

Participants

A total of 51 adult humans (42 female, 9 male, median age: 19 years) volunteered to participate; for 38 of them the experiment was part of an open day tour around the Department of Psychology, University of Sheffield, and the remaining 13 participants were students enrolled in undergraduate courses at the University of Sheffield and participated either to fulfil a course requirement or in exchange of £1.

Design

The factors cover story and time were combined in a 2 × 2 between–subjects design. Cover story had the levels “delay: plausible” and “delay: implausible”, and time was set to either 0 s or 4 s of action–outcome delay. In each of the four conditions resulting from the design, the apparatus was programmed to schedule outcomes according to a free-operant procedure set to a fixed ratio of .75. Depending on the level of time, the reinforcement latency was either 0 s or 4 s. Two important modifications from standard free-operant procedures commonly employed in delay or trace conditioning paradigms were that (1) every response, including those made during the delay period (if any) were recorded and subjected to the reinforcement schedule, and (2) the sampling period was not divided into experimenter–defined learning trials, thus resulting in a truly continuous paradigm. In other words, every response had a 75% chance to produce the outcome, and it could do so either immediately (time: 0 s) or after a delay (time: 4 s). Expressed in terms of conditional probabilities the probability of an outcome given a response $P(e|c)$ was .75, and the outcome never occurred unless there was a response, $P(e|\neg c) = 0$.

Materials and procedure

The experiment was carried out in a large group setting in the departmental computer laboratory, using Macintosh Power PC 4400/200 computers programmed with Macromedia Director 7.0. Participants read instructions on the computer screen, telling them to imagine that they were working as a quality control officer for a company that manufactures light bulbs. Their specific task would be to find out whether clicking on a light switch would make a bulb light up or not. Participants in the delay: plausible conditions then read that the bulbs were energy-saving bulbs that would take 4 s to light up; participants in the delay: implausible group were told that the bulbs were ordinary bulbs that would light up right away. All participants then read that manufacturing faults may sometimes lead to the bulbs lighting up “on their own” even when the switch was not clicked. Participants were told that they could click the switch as little or as often as they liked, and that after two minutes of sampling evidence they would be asked how strongly clicking the switch causes the bulb to light up. Participants next had the opportunity to ask any clarifying questions they might have, but none did so. After reading the instructions, participants had to click on an OK button to start the 2-min sampling period (for full instructions see Appendix).

The computer then displayed a 5.5-cm high drawing of a light bulb centrally against a grey background. At 3 cm below the light bulb was a 4 × 1-cm white rectangular push-button labelled “Lightswitch”, which inverted its colours when participants clicked on it with the mouse. An outcome was represented as the bulb illuminating in yellow for 500 ms. At the end of the 2-min sampling period participants had to rate how strongly clicking the switch caused the bulb to light up on a scale from 0 to 182

This aspect of our instructions parallels the instructions in the seminal Shanks, Pearson, and Dickinson (1989) paper and subsequent adaptations of it. However, just as in Shanks et al.’s critical experimental conditions, the outcome actually never occurred on its own.
100, where 0 meant that clicking the switch had no influence on whether or not the bulb lit up, 50 meant that clicking the switch moderately caused the bulb to light up, and 100 meant that clicking the switch strongly caused the bulb to light up. A graphical representation of the scale with labels at the extreme and midpoints was displayed on the computer screen in addition to the verbal description of the scale. Participants then entered their rating via the numerical keyboard and clicked an OK button to confirm their rating. Overall, the experiment lasted about 5 to 8 min.

Results and discussion

All statistical analyses adopted a significance level of .05. Figure 1 displays participants’ mean ratings of causal strength in all four conditions. Visual inspection suggests that a 4-s delay of reinforcement selectively impaired causal learning in the delay:implausible group, but did not affect ratings in the delay:plausible group. An analysis of variance (ANOVA) on causal ratings corroborated this qualitative description of the results. There was a main effect of time, $F(1, 47) = 12.16$, and an interaction between time and cover story, $F(1, 47) = 10.77$, but no main effect of cover story, $F(1, 47) = 2.40$. Tukey–Kramer post hoc tests revealed that the main effect of time was significant only in the delay:implausible group, and not in the delay:plausible conditions. Another way to look at the results is to investigate how different cover stories affected causal judgements in conditions with identical delays. Tukey–Kramer post hoc tests revealed that cover story had an effect on causal judgements only in the time:4-s group, but not in the time:0-s group.

These results indicate that temporal contiguity is not essential for human causal induction. Whether or not delays affect causal learning depends on people’s prior assumptions (as manipulated via the instructions) about the timeframe of the causal relation in question. In the current experiment we induced different prior assumptions via two different cover stories. Changing merely a few words in the instructions was sufficient to completely abolish the previously established and robust strong effect of reinforcement delay (e.g., Shanks et al., 1989).

It is important to check whether our manipulation of cover story indeed influenced causal ratings directly (i.e., in affecting how people interpret the same evidence), rather than indirectly (e.g., by way of producing different response patterns in the various groups). It

![Figure 1. Mean causal ratings for immediate and delayed action–outcome pairings split by cover story. Error bars indicate standard errors.](image-url)
could be, for instance, that participants in the delay:plausible group responded less often (i.e., pressed the button and then waited for a delayed outcome to occur). If this were the case, then different response patterns can in principle produce qualitatively different evidence between groups, and it could be that any effects between groups are caused by these differences in evidence. The nature of the free-operant procedure we employed could potentially lead to dramatic differences in observed evidence, depending on participants’ response patterns. Because every response, including those made during the delay period, was subjected to the reinforcement schedule, participants in the 4-s delay group might not experience a 4-s delay if they respond very rapidly. Consider a participant who responds once every second. Such a participant would produce four responses during the delay period, which could be followed more or less contiguously by an outcome, albeit by one that was produced by an earlier response. In other words, high response rates could potentially have eliminated reinforcement delays in the 4-s condition, and if cover story affected response rates in a particular way, the observed difference in causal ratings could simply reflect this change in evidence, rather than showing that people interpret the same evidence differently depending on higher level knowledge.

Table 1 displays the mean response rates for each experimental condition. An ANOVA on response rate revealed a main effect associated with cover story, \(F(1, 47) = 9.11\), and a marginal effect of time, \(F(1, 47) = 3.63\); the time \(\times\) cover story interaction was not significant, \(F(1, 47) = 1.46\). Consistent with earlier findings (Buehner & May, 2003), participants in the delay:plausible group responded less often than participants in the delay:implausible group; likewise, participants exposed to a 4-s delay responded less than participants with 0-s delay. In order to check whether these differences in response rate produced different qualities of evidence between the groups, we additionally analysed the response-outcome timings. Our analysis selected each experienced outcome, during the 2-min sampling period and calculated the time difference between it and the most recent emitted (reinforced or unreinforced) response. We could thus calculate a mean experienced response–outcome interval for each individual participant and investigate whether differences in response rates also produced qualitatively different patterns of evidence.

Figure 2 displays the mean response–outcome intervals for each of the four experimental conditions. Visual inspection reveals that cover story indeed influenced the quality of the evidence that participants experienced in the 4-s delay conditions. An ANOVA revealed main effects of time, \(F(1, 47) = 177.86\), cover story, \(F(1, 47) = 18.63\), and an interaction between them, \(F(1, 47) = 18.64\). Analysis of simple main effects revealed that the effect associated with

### Table 1

<table>
<thead>
<tr>
<th>Cover story</th>
<th>Delay (s)</th>
<th>Response rate</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Mean (M)</td>
</tr>
<tr>
<td>Delay:plausible</td>
<td>0</td>
<td>20.0</td>
</tr>
<tr>
<td></td>
<td>4</td>
<td>9.6</td>
</tr>
<tr>
<td>Delay:implausible</td>
<td>0</td>
<td>26.0</td>
</tr>
<tr>
<td></td>
<td>4</td>
<td>23.7</td>
</tr>
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*In s.*
time was significant in both levels of cover story, but that the effect of cover story was significant only in the 4-s condition, and not the 0-s condition. The different quality of evidence cannot readily explain the results in the causal ratings, however. In fact, if participants had based their causal estimates only on the subjective quality of the evidence, one would have expected exactly the opposite pattern to what we found. Participants in the delay:plausible condition experienced a longer response–outcome interval in the 4-s delay group than did participants in the delay:implausible condition. If differences in causal ratings were solely driven by variations in response-outcome contiguity then the former group should have provided lower ratings than the latter group; in fact, the reverse ordering was obtained. The results in the causal ratings thus cannot be attributed to differences in experienced evidence. Instead, we conclude that expectations of cause–effect delays eliminated the detrimental influence of experienced response-outcome delays.

**GENERAL DISCUSSION**

**Associative learning, stimulus saliency, and prior knowledge**

In this paper we pitched two theoretical approaches to human causal learning against each other: associationism and causal power theory. Our focus was on their respective predictions about how cue–outcome delays affect human causal judgements. According to associationism, temporal contiguity is of utmost importance for causal learning. Although it is acknowledged that considerable time-lags can be tolerated in specific situations such as taste aversion paradigms (cf. Shanks, 1993), such tolerance to delays is usually tied to the specifics of the stimuli employed in the learning paradigm. Garcia, Ervin, and Koelling (1966), for instance, theorized that gustatory and olfactory cues are “biased” to be associated with internal malaise, “even when these stimuli are separated by long time periods” (p. 122). What is not possible under such an interpretation, however, is that the “window of associability” for the same instrumental action (i.e., clicking a button) and stimulus (i.e., bulb flashing) is very narrow in one situation, but considerably wider in another situation.
Causal power theory, on the other hand, views knowledge or inference about a causal mechanism as the essential ingredient to successful causal learning. Such knowledge entails assumptions about the time-frame over which the relation unfolds, and, consequently, whether contiguity is essential or not depends on the assumptions that the learner brings to bear. The same event (e.g., pressing a button) may be tied to an immediate causal mechanism (e.g., an ordinary bulb lighting up), or to a delayed mechanism (e.g., an energy saving bulb lighting up). Causal power theory thus inherently postulates that the manner in which evidence is parsed should be flexible.

Associative learning theory also has some room for flexibility due to its parametric nature. One could, for instance, argue that greater tolerance for delays could be represented by assuming different learning parameters for various situations. Usually such parameters are thought to reflect properties of the stimuli—for example, $\alpha$ and $\beta$ in the Rescorla and Wagner (1972) learning rule, which respectively represent the salience of the conditioned stimulus (CS) and the unconditioned stimulus (US). By assuming higher salience for gustatory or olfactory than for visual or auditory cues, such an account can readily explain the discrepancy in delay tolerance between taste aversion and tone shock conditioning paradigms. An analogous explanation for the current results is not warranted, however. Our experiment employed exactly the same visual stimuli in all four conditions, so assuming greater salience in the delay:plausible conditions is not justifiable. The mere changing of a few words in the instructions was sufficient to abolish any impact of temporal contiguity. We argue that this subtle change of wording resulted in different assumptions about the hypothesized causal mechanism and consequently influenced participants’ tolerance for delays. Such abstract notions of mechanism fall outside the scope of associative learning theory.

Time and conditioning

Another way in which conditioning theory could allow variable influence of cause–effect timings is to overcome the classic one-dimensional view of associative strength. Miller and Barnet (1993), for example, have argued that the temporal relationships between events form an additional dimension. According to their temporal coding hypothesis, information about associative strengths and temporal relationships may be acquired separately, but would both contribute to determine behaviour with respect to (or evaluation of) the cause in question. Our manipulations of the instruction that participants received would then have established different values on the temporal dimension and thus would have produced corresponding expectations about cause–effect delays.

Gallistel and Gibbon’s (2000) theory of conditioning even goes one step further and posits that the learning and representation of conditioned behaviour is exclusively based on temporal relations. This account abolishes the traditional idea of associative strength. According to their rate estimation theory, the organism tracks rates of outcome occurrence, computes changes in rates signalled by cues, and determines whether and, crucially, when to respond based on a decision threshold that compares rates of occurrence in the presence and absence of the cue. Similarly, as with the temporal coding hypothesis, our instructions could be interpreted as having set up different decision thresholds for the various groups.

One problem with applying rate estimation theory to human causal judgement from experiments like ours is that the calculation of rates is not possible when using point events (as
opposed to interval events). In a standard Pavlovian conditioning paradigm, it is easy to compute the rate of US occurrence during (or at the end of) a CS interval and to compare it to the rate of US occurrence in the intertrial interval. Analogous computations are not possible when using point events that do not have extended duration, unless one assumed a window of associability to which rate computations would be relative. This assumption then would of course bring us back to the starting point, and we would have to ask again what determines the size of the window. An alternative position for a rate-based account, which avoids such baroque add-ons, is to represent information in terms of cumulative probability functions (see Gallistel, 2002), but even then there are other problems, such as how to represent events occurring during the delay period. Nonetheless, both the temporal coding hypothesis and the rate estimation theory are significant advances in that they recognize the need to represent sensitivity to temporal relations between events in addition to (or instead of) a one-dimensional associative bond.

**Artificial stimuli, unnatural procedures, and the apparent sufficiency of contiguity**

Prior work (Buehner & May, 2002, 2003) has already established that abstract prior knowledge mediates the impact of temporal contiguity and delay on judged causality. The evidence, albeit supporting causal power theory, so far has not been strong enough to completely rule out a contiguity bias superordinate to notions of mechanism. Previous research has consistently showed that introducing a delay between cause and effect results in lower causal ratings than those in immediate conditions. Our motivation in this paper was to see whether we could abolish the detrimental effect of delay completely. The reasoning behind the current research was that the scenarios employed by Buehner and May could have been too unfamiliar for the student participants to induce stable and unambiguous assumptions about the causal mechanism. In this paper we employed a highly familiar mechanism to curtail this problem. When participants were presented with an unambiguous, well-known scenario that suggested a delay, the detrimental effect of action–outcome time-lags was completely abolished. The significance of this result, compared to Buehner and May’s earlier findings, is that it seriously questions the notion that an inherent contiguity bias always overrides temporal assumptions about the causal mechanism.

Causal power theory would go even further in its predictions, however. If the scenario employed is one where a delay is necessary (as in, for instance, between sexual intercourse and birth), immediate action–outcome pairings should not give rise to causal attributions, but would instead be explained by (potentially unobserved) alternative causes (Einhorn & Hogarth, 1986). In other words, temporal contiguity would impair causal judgements, and delay would have a beneficial rather than a detrimental or a null effect on causal learning. Our current design did not yield such results. Causal ratings in the time:0-s conditions were high regardless of whether the instructions emphasized an immediate or delayed relation. There was a trend for ratings to be lower in the delay:plausible cell, but it was not significant. Even though we went through great efforts to convince these participants that a delay was plausible, the manipulation may not have been strong enough to make them believe it was necessary. After all, the experiment was carried out on a computer, and anyone remotely familiar with how computer programs or games work would recognize that the computer displayed events
in response to input via mouse or keyboard. Participants in the delay:plausible/time:0-s cell thus had no reason to believe that their clicks did not produce the bulb lighting up immediately. In order for them to conclude that their actions did not cause the bulb to light up they would have to disregard the strong empirical evidence of a high contingency manifested through immediate action–outcome pairings.

Even though our research went beyond Shanks et al.’s (1989) original findings and abolished the detrimental effect of delay in a paradigm using essentially the same apparatus, we could not produce the reverse pattern of results, namely a detrimental effect of contiguity. There is reason to believe that this pattern of results reflects the limitations and artificiality of the apparatus rather than fundamental properties of the learning or reasoning process. Any free-operant procedure is programmed according to some schedule, and human participants usually know that some apparatus controls the reinforcement schedule. If the apparatus is programmed to deliver immediate reinforcements, it would be irrational for participants to disregard those; given the strong empirical support it is much more rational, by comparison, to disregard the cover story, which is not closely tied to the apparatus. After all, the cover story always asks participants to imagine a particular causal mechanism, and the apparatus then simulates such a mechanism on the computer. It would even follow from causal power theory that the instructions and not the evidence should be disregarded: If the reasoner assumes that a causal mechanism indeed underlies the observed regularity, it follows that one draws inferences about the properties of the mechanism. Given that the evidence is stable and not disputable, but the instructions are disconnected to the mechanism, it is no surprise that the instructions are trusted less, if they contradict the inference about the properties of the mechanism.

The picture changes, however, if one employs a less artificial apparatus and procedure. Studies in a more natural setting employing real physical mechanisms rather than mere computer simulations suggest that human subjects do disregard contiguous event pairings if their knowledge of mechanism calls for delays. Schlottmann (1999), for instance, presented participants with a “mystery” box into which balls could be dropped. The effect that participants had to look out for was whether or not a bell rang. There were two different toys that could be placed inside the box—a “fast” see-saw or a “slow” runway; the first made the bell ring immediately when a ball was dropped into the box, while the latter did so only after a few seconds. The box had two holes on its top, and the toys could be positioned in such a way that balls dropped in one, but not the other hole, fell onto them, and made the bell ring after the relevant time period. The crucial phase of the experiment was as follows: First the experimenter put one of the two toys into the box. The participant knew exactly which of the toys (the fast or the slow one) was inside the box, but did not know under which of the two holes it was placed. The experimenter then dropped one ball into a hole, waited a few seconds, and then dropped another ball into the other hole. The events were carefully timed so that the bell rang immediately after the second ball was dropped. Participants then had to indicate which of the two balls made the bell ring. Adult human subjects in Schlottmann’s study uniformly indicated that the second ball (contiguous cause) made the bell ring when the “fast” (see-saw) was in the box. They also uniformly concluded that the first (noncontiguous) ball made the bell ring when the “slow” (runway mechanism) was inside the box. Schlottmann’s results thus show both a detrimental effect of delay and a detrimental effect of contiguity, depending on participants’ knowledge of mechanism: If participants knew that the immediate mechanism
was inside the box, they refrained from attributing causality to a noncontiguous event; if participants knew the delayed mechanism was inside the box, they refrained from attributing causality to a contiguous event.

Early and seminal work on the role of temporal contiguity in human causal learning suggested that contiguity is necessary for causal induction (Shanks et al., 1989). Our results, in contrast, show that contiguity is not essential for causal induction. Human subjects can learn a causal relation just as well when it is delayed than when it is immediate. At the same time our findings could not rule out the possibility that contiguity is sufficient to create causal impressions. In fact, there is a line of research that showed that contiguity on its own, even in the absence of contingency, can create impressions of causality. Both Wasserman and Neunaber (1986) and Anderson and Sheu (1995) employed a design where pressing a button merely advanced the delivery of the outcome in time, but did not affect its overall probability with respect to a predefined learning trial. In other words, the outcome occurred with a fixed probability at the end of a certain trial period, but if participants emitted a response during this period, the outcome would occur immediately, rather than at the end of the designated period. Participants were more sensitive to this local change of probability than to the lack of change in overall probability and correspondingly indicated that their responses were causally effective. Note again that this behaviour is perfectly rational, in that responses indeed did cause an outcome to occur immediately, which would otherwise have occurred later on. High ratings in Wasserman and Neunaber’s and Anderson and Sheu’s design thus reflect the conviction that pressing the button did result in changes in the environment.

It is easy to see, however, that reinforcement schedules such as those discussed above have very limited ecological validity, in that we hardly ever encounter causal relations where interventions exclusively affect the time of occurrence, but not the probability of an event occurring. We would thus argue that the apparent sufficiency of contiguity is an illusion created by the artificial nature of free-operant procedures commonly employed in causal learning research. Future work employing more realistic procedures like Schlottmann’s (1999) may corroborate our hypothesis.

REFERENCES


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APPENDIX

Instructions for the Delay: Plausible group

You are working in a company that manufactures lamps. Your job is in quality control, and you have to determine whether the produced lamps are functioning properly. In particular, your task is to find out whether clicking a Lightswitch will make a bulb light up.

There are two important things that you need to know to do your job properly:
1.) The lightbulbs your company puts in the lamps are energy saving bulbs. This means that they should light up 4 s after you’ve clicked the switch.
2.) Sometimes lamps have loose connections due to faults in the manufacturing process. This results in the lightbulb lighting up apparently “on its own” i.e., without you clicking the switch.

You can choose at any time whether or not to click the lightswitch. You can click it as often or as little as you like. However, because of the nature of the task it is to your advantage to click it some of the time and not to click it some of the time.

Remember that a properly functioning lamp should light up 4 s after you’ve clicked the switch, and that a faulty lamp may light up independently of your clicking.

Thus, if you observe the bulb lighting up, it may be because you clicked the switch, or because of a loose connection.

You will work on the task for 2 min.

At the end you will be asked how strongly clicking the switch causes the bulb to light up.

If you have no further questions, click “OK” to start the Lightbulb problems.

Instructions for the Delay: Implausible group

You are working in a company that manufactures lamps. Your job is in quality control, and you have to determine whether the produced lamps are functioning properly. In particular, your task is to find out whether clicking a Lightswitch will make a bulb light up.

There are two important things that you need to know to do your job properly:
1.) The lightbulbs your company puts in the lamps are ordinary bulbs. This means that they should light up immediately when you click the switch.
2.) Sometimes lamps have loose connections due to faults in the manufacturing process. This results in the lightbulb lighting up apparently “on its own” i.e., without you clicking the switch.

You can choose at any time whether or not to click the lightswitch. You can click it as often or as little as you like. However, because of the nature of the task it is to your advantage to click it some of the time and not to click it some of the time.

Remember that a properly functioning lamp should light up immediately when you click the switch, and that a faulty lamp may light up independently of your clicking.

Thus, if you observe the bulb lighting up, it may be because you clicked the switch, or because of a loose connection.

You will work on the task for 2 min.

At the end you will be asked how strongly clicking the switch causes the bulb to light up.

If you have no further questions, click “OK” to start the Lightbulb problems.