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Kicking the Habit: Why Evidence for Habits in Humans Might Be Overestimated

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Researchers typically classify behavior as habitual if it occurs independently of changes in the value of its outcomes (revaluation test) or the impact it has on those outcomes (contingency degradation test). We argue that these tests are valid only if they (a) are sufficiently sensitive and (b) target the outcomes that might actually control behavior. These criteria resemble the sensitivity and information criteria that are widely adopted in research on learning without awareness. We argue that past and future evidence for habits should be evaluated in light of these criteria and illustrate this approach by applying the information criterion to the studies conducted by de Wit, Niry, Wariyar, Aitken, and Dickinson (2007) and de Wit, Ridderinkhof, Fletcher, and Dickinson (2013). In 3 experiments that were modeled after these studies, we used alternative revaluation and contingency degradation tests that targeted other outcomes than those targeted in the original studies. These alternative tests consistently provided evidence for knowledge of outcomes that could have controlled seemingly habitual behavior. Our results suggest that the revaluation test used by de Wit et al. (2007, 2013) did not meet the information criterion, which questions the validity of their conclusions regarding the habitual nature of the observed behavior.

Keywords: habits, goals, outcome revaluation

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Habits are often thought to be the backbone of human behavior (see Wood & Rünger, 2016, for a review). It has been argued that behavior is by default habitual, with nonhabitual behavior arising only when people have the need and resources to deviate from the habitual response (Evans & Stanovich, 2013). This point of view is definitely acceptable if habits are defined broadly as encompassing all behaviors that are emitted frequently or automatically (e.g., without conscious deliberation). However, habits have also been defined more narrowly as that subset of (frequent and automatic) behavior that is not goal-directed (Heyes & Dickinson, 1990; Wood & Rünger, 2016). These two definitions do not overlap because frequent and automatic behavior can be goal-directed (e.g., Aarts & Dijksterhuis, 2000). The distinction is also not trivial because different strategies must be used to influence behavior that is goal-directed versus habitual in the strict sense. Most importantly, whereas (frequent or automatic) goal-directed behavior can be changed by altering the outcomes it produces (e.g., making an outcome less desirable) or the impact it has on those

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outcomes (e.g., eliminating the impact of the behavior on the outcome), truly habitual behavior cannot. Despite these important differences, many researchers have maintained the claim that habits—even when defined in the strict sense—are the default mode behavior (e.g., Wood & Rünger, 2016).

The validity of that claim hinges upon the ability to determine whether a behavior depends on goals. In line with the idea that true habits should be immune to changes in (the relation to) their outcomes, two tests have been used to identify habits: the outcome revaluation test and the contingency degradation test. In order to pass the outcome revaluation test, it needs to be demonstrated that a particular behavior is unaffected by changes in the value of its outcomes. For instance, rats that received extensive training to obtain a sucrose reward by pressing a lever will continue to vigorously press this lever even after the sucrose was devalued by pairing it with the induction of illness (e.g., Adams, 1982). The contingency degradation test, on the other hand, assesses the result of changes in the impact of the behavior on its outcomes. For instance, lever pressing is considered to be habitual if rats continue to press the lever that produced sucrose even if lever pressing no longer produces sucrose.

Importantly, both the revaluation and the contingency degradation test are known to be problematic in that they require the demonstration of a null effect: the *absence* of an impact of changes in outcome value or behavior–outcome contingency (Heyes & Dickinson, 1990; Moors, Boddez, & De Houwer, in press; Thrailkill & Bouton, 2015). It is well known that null effects can be due not only to the absence of a specific target process (e.g., the absence of a goal-directed process), but also to a host of other factors such as inadequate manipulations or measurements (e.g., Colwill & Rescorla, 1990). Interestingly, the same problem arises when researchers wish to establish learning without awareness. More specifically, learning is considered to be unaware only when awareness tests *fail* to provide evidence for the presence of conscious knowledge of the contingencies that influence behavior. As in research on habits, the demonstration of unconscious learning thus hinges on the demonstration of a null result.

Unlike to what is the case in habit research, however, the problems associated with demonstrating a null effect have received a lot of attention in research on unconscious learning. Most importantly, it is now generally accepted that awareness tests should be evaluated with regard to the information and sensitivity criteria (Shanks & St. John, 1994; see Lovibond & Shanks, 2002, for a third criterion). First, the information criterion specifies that the information that an awareness test is designed to capture should be the information that is responsible for learning. For instance, awareness tests should assess not only conscious knowledge about the rule that the experimenter implemented in a certain task (e.g., an artificial grammar that generates grammatical letter strings) but also other conscious knowledge that is correlated with this rule (e.g., the fact that some pairs of letters are more likely to occur in grammatical than in ungrammatical letter strings; see Shanks & St. John, 1994). Second, an awareness test needs to meet the sensitivity criterion. This second criterion implies that a test is sensitive enough to capture relevant conscious knowledge. More specifically, an awareness test can be regarded as insensitive when it suggests that a specific piece of conscious knowledge is absent even when participants do possess such knowledge. Insensitivity could result from the fact that the awareness test does not create the optimal conditions for people to express their conscious knowledge. For instance, they might not be motivated to express their knowledge, or the awareness test might differ from the learning context in ways that hamper the use of conscious knowledge (see Lovibond & Shanks, 2002, and Shanks & St. John, 1994, for more details).

The introduction of the information and sensitivity criteria constituted a major step forward in research on learning without awareness because it set a new quality benchmark. After Shanks and St. John (1994) introduced these criteria (see Dulany, 1961, for a precursor of the information criterion), the quality of evidence for unconscious learning was evaluated on the basis of the extent to which researchers could argue that their awareness test was sensitive enough and probed for all relevant conscious knowledge. In this paper, we put forward the idea that research on habits would benefit in the same way from the introduction of quality criteria for revaluation and contingency degradation tests. More specifically, the quality of

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evidence for habits (in the strict sense) should be evaluated on the basis of the extent to which it can be argued that the revaluation or contingency degradation tests (a) target the outcomes that might actually control behavior and (b) create the optimal conditions for observing an impact of those outcomes. In line with the literature on unconscious learning, we refer to these two criteria as the information criterion and sensitivity criterion for habit research.

In the present paper, we illustrate the usefulness of the information criterion by applying it to a series of studies on habit learning that was conducted by de Wit and colleagues (de Wit, Niry, Wariyar, Aitken, & Dickinson, 2007; de Wit, Ridderinkhof, Fletcher, & Dickinson, 2013). In their studies, participants first completed an instrumental discrimination learning phase. On each trial of that phase, a box was presented with a piece of fruit (stimulus; S) pictured on the front. Participants pressed a left or right key (response; R) in order to open the box. Depending on whether their response was correct or incorrect, they either gained points and saw another piece of fruit inside the box (outcome; O) or they did not gain points and saw an empty box. There were three types of trials: congruent trials, in which the S and O were the same fruit; biconditional trials, in which S and O were different and did not have any other function within the experiment (i.e., Ss only functioned as Ss and Os only functioned as Os); and incongruent trials, in which the S and the O were different fruits that had different functions (S or O) within the experiment (see Table 1 for an example of the different possible trial types).

Crucially, on incongruent trials, the same piece of fruit was related to different responses depending on whether it served as a stimulus or as an outcome. For instance, S1 (e.g., grapes) would lead to O2 (e.g., pineapple) after a left key press and S2 (e.g., pineapple) led to O1 (e.g., grapes) after a right key press. de Wit et al. (2007, 2013) reasoned that if a goal-directed system would operate during incongruent trials, it would result in conflicting associations between fruits and responses (i.e., the same fruit would be linked with both left and right responses), thus leading to response conflicts and poor performance. A habit system, on the other hand, would link each fruit with only one response (i.e., the response it was linked to when functioning as an S), thus eliminating ambiguity and facilitating performance on incongruent trials. Although learning might initially be based on a goal-directed system, the habit system would therefore quickly take over, thus leading to the formation of habits (also see de Wit et al., 2013, pp. 781–782). On other types of trials, a goal-directed system would not produce response conflicts and would thus be more likely to continue to play a prominent role.

A revaluation test was used to examine whether behavior on incongruent discrimination trials qualified as habitual. On each trial of the revaluation test, participants saw two open boxes, each with a different type of fruit presented inside. One of the two fruits was presented with a red cross superimposed on it, signifying it had lost its value. The other fruit had not lost its value and would still lead to points. Participants were asked to select the response that caused the fruit that was not devalued. The logic behind this test was that if participants still chose the response that produced the devalued outcome, then the behavior cannot qualify as goal-directed (i.e., directed at the goal of obtaining the valuable fruit). Therefore, chance performance during the revaluation test would indicate the absence of goal-relevant knowledge and thus that above-chance performance during the discrimination learning phase was habitual. In several studies, de Wit et al. (2007, 2013) observed chance or below-chance performance on incongruent revaluation trials and above-chance performance on congruent and biconditional revaluation trials. Based on these findings, they concluded that performance on the incongruent discrimination trials was habitual whereas behavior on the congruent and biconditional discrimination trials was goal-directed.

Table 1
Example of Possible Trial Types in the Study of de Wit and Colleagues (2007)

Stimulus	Response	Outcome	Trial type
Cherry	D	Cherry	Congruent
Pear	K	Pear	Congruent
Grapes	D	Pineapple	Incongruent
Pineapple	K	Grapes	Incongruent
Coconut	D	Orange	Biconditional
Banana	K	Apple	Biconditional

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In three studies, we examined whether the revaluation test of de Wit et al. (2007) might have been suboptimal in terms of the information criterion. It is important to note that within the discrimination learning phase of de Wit et al. (2007) there were actually two outcomes on each trial in which participants gave a correct response. First, there was the piece of fruit that was presented inside the box. Second, participants received 5, 4, 3, 2, or 1 point(s) for each correct response, depending on the speed of their reactions. The importance of both outcomes was stressed in the instructions, but arguably the points were more salient than the fruits, because participants were explicitly asked to gain as many points as possible. It is therefore plausible that participants primarily focused on learning which response resulted in points when a certain stimulus fruit was presented on the outside of box. Hence, it is possible that during the discrimination learning phase, participants never exhibited habitual behavior but always pressed the key that they thought would deliver points. The fact that the revaluation test revealed a lack of knowledge about the outcome fruits of the incongruent trials might thus be irrelevant for determining the habitual nature of the behavior during the discrimination phase, simply because that behavior did not depend on knowledge about the outcome fruits but on knowledge about the points that a response would deliver. Because the revaluation test might not have targeted the outcomes that actually drove behavior during the discrimination phase, it did not meet the information criterion and cannot be used to determine whether behavior was habitual. The fact that participants learned less about the outcome fruits of incongruent trials than those of other trials indicates that it is more difficult to learn about outcome fruits when the same fruits are related to multiple responses. But this difference would arise also if participants always selected responses with the aim to gain as many points as possible, that is, if their behavior was goal-directed.

In our studies, the discrimination learning phase was similar to that in the studies of de Wit et al. (2007, 2013), but we used a different revaluation test (Experiment 1) or a contingency degradation test (Experiments 2 & 3) that targeted knowledge about the point outcomes. In Experiment 1, participants were told before the start of the revaluation phase that, from now on, every point earned would be deducted from the total number of points. In Experiments 2 and 3, after the discrimination learning phase, participants were told that response-point relations would be reversed (i.e., responses that previously led to points would no longer result in points, whereas responses that previously did not lead to points would result in points).[1] If behavior is driven by the goal to maximize the total number of points, participants should reverse their responses during these tests, also on incongruent trials.

Experiment 1

Method

Participants. We tested 24 participants at Ghent University who were paid 5 euro for taking part in the experiment. All were native Dutch-speakers.

Stimuli and materials. We used the same stimuli as de Wit et al. (2007): eight colored pictures of different types of fruit (apple, bananas, cherries, coconut, grapes, pear, pineapple, and orange). The size of the fruit stimuli was 220 by 220 pixels. These stimuli were presented on a colored picture of a closed box (when they functioned as an S) or an open box (when they functioned as an O). The size of the box pictures was 362 by 378 pixels. There were also three practice stimuli (colored pictures of a cup of coffee, a glass of beer, and a bottle with a glass of wine next to it, of the same size as the fruit stimuli). All stimuli were presented on a black background and presented in the center of the screen. Above the boxes, a counter with the total number of points was presented. On correct trials, a yellow text box appeared indicating the number of points that were earned on the trial. All text was presented in 18-point Courier New font.

Participants were tested in a spacious room in which four computers were set up, separated by partitions. One, two, three, or four participants

[1] Note that this test involves a reversal of contingencies rather than a degradation (i.e., weakening) of contingencies. We will, however, continue to use the expression "contingency degradation test" because this term is commonly used in habit research to refer to procedures in which response-outcome contingencies are changed.

were tested during each session. They were seated in front of a laptop PC with a 17-in. color monitor at a distance of approximately 45 cm. For stimulus presentation and response registration, we used the E-Prime software package (Schneider, Eschman, & Zuccolotto, 2002a, 2002b). The program was started after participants gave their informed consent.

Procedure. The experiment consisted of a practice block and an experimental block, both of which involved a discrimination learning phase and a revaluation phase. First, participants were presented with instructions for the discrimination learning phase, which stated that they could earn points by collecting objects from inside a box. They could open the box by pressing the left (D) or the right (K) key on the keyboard. A correct key press would reveal the object inside the box, whereas an incorrect key press would result in an empty box. Finally, they were told that the picture on the front of the box would provide a clue regarding the correct response.

A discrimination learning trial started with the presentation of a closed box with a picture of an object (i.e., a drink in the practice block; a piece of fruit in the experimental block) superimposed. It remained on the screen until participants pressed the D (left) or the K (right) key. Immediately after this, an open box was presented for 1,000 ms, with either an object present inside (on correct trials) or nothing present inside (on incorrect trials). On correct trials, a yellow text box appeared with the text "+1". After this, the empty box was presented for 1,000 ms, followed by a 1,500-ms blank screen.

After the discrimination learning phase, participants were instructed that points that would be earned from that moment onward would no longer be added but rather deducted from their total score. They were further instructed to avoid losing points and thus to keep their total score as high as possible. Furthermore, they would no longer receive feedback, but they would learn their total score at the end of the phase. Revaluation trials were similar to discrimination trials except that after a response was recorded, the trial ended and no feedback was given. The inter-trial interval was 1,000 ms.

The practice phase consisted of 10 discrimination learning trials, of which the first four were explicitly cued (i.e., participants were told which key to press; see de Wit et al., 2007). The experimental discrimination learning phase consisted of four blocks of 12 discrimination learning trials (yielding 48 trials in total). There were 36 revaluation trials.[2]

For each participant, the different types of fruit were randomly assigned to a function (S or O) and to a trial type (congruent, incongruent, or biconditional). As can be seen in Table 1, there were two trials for each trial type, one requiring a left (D) response and another requiring a right (K) response.

Results

Discrimination phase. We performed a repeated measures ANOVA with two within-subjects factors: block (1, 2, 3, and 4) and trial

[2] For exploratory reasons, at the very end of Experiments 1 and 2, we assessed knowledge about stimulus–outcome relations by asking participants to indicate which stimulus fruit was paired with which outcome fruit (i.e., "If this fruit was inside the box, which of the fruits below was presented on the front of the box?"). In Experiment 1, performance was above chance level for the biconditional trials (M = .69, SD = .36), $t(23) = 2.58$, $p < .05$, but not for the incongruent trials (M = .63, SD = .47), and the congruent trials (M = .63, SD = .47) ts < 1. Likewise, in Experiment 2, performance was above chance for biconditional trials (M = .71, SD = .33), $t(21) = 2.88$, $p < .01$, and congruent trials (M = .73, SD = .43), $t(21) = 2.49$, $p < .05$, but not for incongruent trials (M = .52, SD = .42), $t < 1$. Finally, in Experiment 3, performance was also above chance for the biconditional trials (M = .70, SD = .39), $t(33) = 2.87$, $p < .01$, and for the incongruent trials, (M = .77, SD = .36), $t(33) = 4.41$, $p < .001$, but not for the congruent trials, trials (M = .62, SD = .45), $t = 1.54$. Experiments 2 and 3 also included an assessment of knowledge about response-outcome relations (i.e., "Which key did you need to press in order to find this piece of fruit inside the box"?") that was administrated immediately before the assessment of stimulus–outcome knowledge. In Experiment 2, performance was above chance for biconditional trials (M = .75, SD = .30), $t(21) = 3.92$, $p < .01$, and congruent trials, (M = .89, SD = .26), $t(21) = 6.86$, $p < .001$, but below chance for incongruent trials M = .27, SD = .37), $t < 2.89$, $p < .01$. In Experiment 3, performance was above chance level for biconditional trials, (M = .80, SD = .28), $t(33) = 6.27$, $p < .001$, and for congruent trials, (M = .92, SD = .22), $t(33) = 11.04$, $p < .001$. Again, it was below chance level for incongruent trials, (M = .35, SD = .42), $t(33) = 2.06$, $p < .05$. Below chance performance on incongruent trials suggests that participants used stimulus–response knowledge rather than response–outcome knowledge to answer this question. Note, however, that knowledge about stimulus–outcome and response–outcome relations was assessed only after the revaluation or contingency degradation test in which no outcomes were presented. This might have interfered with memory for the relations in which the outcomes were involved. Hence, we refrain from drawing strong conclusions on the basis of the knowledge assessments.

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Table 2
Mean Proportion of Correct Responses and Standard Deviations in the Discrimination Learning Phase of Experiment 1

	Block 1		Block 2		Block 3		Block 4	
Trial type	*M*	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Congruent	.69	.24	.82	.21	.95	.10	.96	.10
Incongruent	.52	.25	.80	.21	.89	.18	.91	.19
Biconditional	.64	.33	.82	.17	.91	.16	.96	.12

type (biconditional, congruent, incongruent). Mean proportions of correct responses can be found in Table 2. There was a significant main effect of block, $F(3, 21) = 59.84$, $p < .001$, showing improved performance as participants progressed from block to block. The main effect of trial type approached significance, $F(2, 22) = 3.01$, $p = .06$. Paired-sampled t tests did not reveal a significant difference between performance on biconditional and congruent trials, $t < 1$, nor between biconditional and incongruent trials, $t < 1.52$. However, in line with the findings of de Wit et al. (2007), participants did perform better on congruent, $M = .85$, $SD = .11$, than on incongruent, $M = .78$, $SD = .14$, trials, $t(23) = 2.61$, $p < .05$. The interaction between block and trial type was not significant, $F < 1$.

Revaluation phase. Because all points earned during the revaluation phase were deducted from the total number of points, a response was classified as correct if it did not lead to points. A repeated measures ANOVA with trial type (biconditional, congruent, and incongruent) as a within-subjects factor did not reveal a significant effect of this factor, $F < 1$. Most importantly, performance was significantly above chance level for the biconditional trials, $M = .87$, $SD = .28$, $t(23) = 6.44$, $p < .001$; for the congruent trials, $M = .89$, $SD = .28$, $t(23) = 6.63$, $p < .001$; and for the incongruent trials, $M = .85$, $SD = .29$, $t(23) = 5.92$, $p < .001$. In other words, regardless of the type of trial, participants now selected the response that did not lead to points.

Experiment 2

Method

Participants. Twenty-two students at Doshisha University took part in exchange for 500-yen book coupons. All were native Japanese speakers.

Stimuli, materials, and procedure. Stimuli, materials, and the procedure were the same as in Experiment 1 with two exceptions: Instructions were in Japanese, and after the discrimination learning phase, there was a contingency reversal phase instead of a revaluation phase. Participants were told that in this phase, responses that previously led to points would no longer lead to points, whereas responses that previously did not lead to points would now lead to points. Participants were asked to continue to maximize their total number of points.

Results

Discrimination phase. The repeated measures ANOVA with block (1, 2, 3, and 4) and trial type (biconditional, congruent, incongruent) as within-subjects factors yielded a significant main effect of block, $F(3, 19) = 27.10$, $p < .001$, showing that performance improved over blocks. There was also a significant main effect of trial type, $F(2, 20) = 5.32$, $p < .05$. Paired-samples t tests did not reveal a significant difference between performance on biconditional and congruent trials, $t < 1$, but performance was significantly better on biconditional, $M = .84$, $SD = .14$, compared to incongruent trials, $M = .75$, $SD = .15$, $t(21) = 2.89$, $p < .01$. Performance was also better for congruent, $M = .84$, $SD = .14$, compared to incongruent trials, $t(21) = 3.07$, $p < .01$. The interaction between block and trial type failed to reach significance, $F < 1.07$. The mean proportion of correct responses can be found in Table 3.

Contingency reversal phase. We scored a response as correct if it resulted in an increase in the total number of points during this phase. The ANOVA did not reveal a significant effect

Table 3
Mean Proportion of Correct Responses and Standard Deviations in the Discrimination Learning Phase of Experiment 2

	Block 1		Block 2		Block 3		Block 4	
Trial type	*M*	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Congruent	.66	.28	.88	.17	.92	.16	.92	.19
Incongruent	.60	.18	.70	.26	.84	.18	.86	.23
Biconditional	.64	.23	.86	.20	.92	.14	.93	.11

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of trial type, $F < 1$. Most importantly, performance was significantly above chance level for the biconditional trials, $M = .88$, $SD = .25$, $t(21) = 7.18$, $p < .001$; for the congruent trials, $M = .86$, $SD = .24$, $t(21) = 7.01$, $p < .001$; and for the incongruent trials, $M = .85$, $SD = .24$, $t(21) = 6.91$, $p < .001$.

Experiment 3

An anonymous reviewer correctly pointed out that the discrimination task in our studies differed in one notable manner from that in the original studies of de Wit and colleagues (2007, 2013). Whereas in the original studies, the number of points earned on each discrimination trial depended not only on accuracy but also speed (ranging from 5 points for *very fast responses* and 1 point for *slow responses*), in our studies only accuracy mattered (1 point for each correct response). We had implemented this change because a relation between speed and points during the discrimination phase might result in confusion when changing the value of the points (revaluation test) or the relation between responses and points (contingency reversal). For instance, participants might be uncertain about whether or how speed would influence the points they would earn or loose during the revaluation or contingency reversal test. One could, however, argue that rewarding speed during the discrimination task is crucial in building up habits. Hence, the lack of speed instructions might have prevented the formation of habits in our studies, which in turn might have allowed for more flexible, goal-directed behavior during the revaluation and contingency reversal tests. On the one hand, this alternative explanation does not undermine the potential of our studies to illustrate the merit of the information criterion in habit research: Our studies clearly show that knowledge about relevant outcomes (in this case, points produced by responses) will remain undetected unless revaluation and contingency reversal tests are used that specifically target knowledge about those outcomes. On the other hand, addressing this issue could help us establish whether habits develop during the discrimination learning task that was developed by de Wit and colleagues. We therefore conducted a third study that incorporated the same contingency reversal task as Experiment 2 but did include a relation between speed of responding and number of points.

Method

Participants. Thirty-three students at Doshisha University took part in exchange for 500-yen book coupons. All were native Japanese speakers.

Stimuli, materials, and procedure. Experiment 3 was identical to Experiment 2 except on the following points. During practice and training, participants received 5 points for correct responses that were emitted within 1,000 ms after the appearance of the stimulus fruit on the outside of a box. For each 500 ms that they were slower, they received one point less (i.e., 5 for <1,000; 4 for <1,500; 3 for <2,000; 2 for <2,500; 1 for slower responses). This relation between response speed and points was identical to that used in the original studies of de Wit et al. (2007, 2013). As in Experiment 2, no feedback was given about the points that were earned during the contingency reversal phase. However, the feedback that participants received about their total of points at the end of the experiment did take into account the speed of the responses during the contingency reversal phase.

Results

Discrimination phase. The repeated measures ANOVA with block (1, 2, 3, and 4) and trial type (biconditional, congruent, incongruent) as within-subjects factors again revealed a significant main effect of block, $F(2.05, 65.46) = 30.32$, $p < .001$, showing that performance improved over blocks. The main effect of trial type was also significant, $F(2, 64) = 3.98$, $p < .05$. As was the case in Experiment 2, paired-samples t tests did not reveal a significant difference between performance on biconditional and congruent trials, $t < 1$, but performance was significantly better on biconditional, $M = .83$, $SD = .15$, than on incongruent trials, $M = .76$, $SD = .18$, $t(32) = 2.08$, $p < .05$. Performance was also better on congruent, $M = .83$, $SD = .15$, than on incongruent trials, $t(32) = 2.69$, $p < .05$. The interaction between block and trial type approached significance, $F(6, 192) = 1.89$, $p = .09$ (see Table 4).

Contingency reversal phase. We again scored a response as correct if it resulted in an

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Table 4
Mean Proportion of Correct Responses and Standard Deviations in the Discrimination Learning Phase of Experiment 3

	Block 1		Block 2		Block 3		Block 4	
Trial type	*M*	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Congruent	.64	.27	.86	.18	.89	.22	.94	.15
Incongruent	.67	.24	.75	.28	.80	.21	.82	.24
Biconditional	.66	.25	.80	.21	.91	.17	.94	.17

increase in the total number of points during this phase. Unlike to what was the case in the previous experiment, the ANOVA revealed a significant effect of trial type, $F(2, 64) = 6.51$ ($p < .01$). Paired-samples t tests showed that performance was significantly superior on congruent trials, $M = .94$, $SD = .09$, compared with biconditional, $M = .87$, $SD = .19$, $t(32) = 2.40$, $p < .05$, and incongruent, $M = .83$, $SD = .21$, $t(32) = 3.57$, $p < .01$, trials. However, performance on biconditional and incongruent trials did not differ significantly, $t(32) = 1.12$, $p = .27$. More importantly, performance was significantly above chance level for each trial type: biconditional trials, $t(32) = 11.36$, $p < .001$; congruent trials, $t(32) = 29.19$, $p < .001$; and incongruent trials, $t(32) = 9.01$, $p < .001$.

Discussion

Despite the emphasis on speed of responding during the discrimination learning phase, participants again flexibly adjusted their behavior to changes in the relation between responses and points at the start of the contingency reversal phase, even for stimuli involved in incongruent contingencies. This finding confirms that participants can acquire relevant knowledge about point outcomes during the discrimination phase and use this knowledge in a flexible manner, even when using the exact same discrimination task as in the original studies of de Wit et al. (2007, 2013). At the general level, the results of Experiment 3 again illustrate the usefulness of the information criterion in habit research by showing that revaluation and contingency reversal tests might miss knowledge about important outcomes unless the tests are specifically targeted at those outcomes. At the level of the nature of the responses during the discrimination task of de Wit et al., the results of Experiment 3 fail to support an alternative explanation of the results of Experiments 1 and 2. The fact that we still found flexible behavior during the contingency reversal test of Experiment 3 argues against the idea that the flexible behavior observed in Experiments 1 and 2 occurred only because of the lack of speed instructions in those first two experiments.

There was, however, one notable difference in the results of Experiments 2 and 3. Whereas performance during the contingency reversal test of Experiment 2 was comparable for all three trial types, in Experiment 3 participants performed significantly better on congruent contingency reversal trials than on both biconditional and incongruent reversal trials. One possible explanation of this discrepancy is that, because of the emphasis on speed, habits exerted some (albeit limited) impact on incongruent trials but not on congruent trials. However, such an account struggles with the observation that performance on biconditional reversal trials did not differ significantly from performance on incongruent reversal trials and was inferior to performance on congruent reversal trials. This is problematic because habit formation should be unlikely on biconditional trials and is typically considered to be a more optimal baseline for comparison with incongruent trials (de Wit et al., 2007, p. 5). A more plausible explanation is that the repetition of stimuli on congruent trials (i.e., the same fruit is presented as stimulus and outcome) facilitates the detection and memorization of the response-point relations for congruent fruits (e.g., the fact that a left response generates points in the presence of a cherry). This advantage on congruent compared to biconditional and incongruent trials (neither of which involves a repetition of fruits) is likely to manifest itself more as the discrimination task becomes more difficult because of time pressure. Regardless of this issue, we can safely conclude that our results illustrate the merits of the information criterion for habit research and raise questions about the claim that the responses observed by de Wit and colleagues (2007, 2013) on incongruent discrimination trials are habitual in nature.

General Discussion

In this article, we put forward the proposal that the quality of the evidence for habits can be

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evaluated on the basis of the extent to which the revaluation and the contingency degradation tests meet the information and sensitivity criteria. Unless those tests target the right outcomes in a sufficiently sensitive manner, null effects on a revaluation or contingency degradation test do not provide strong evidence for the habitual nature of behavior. We also presented data that strongly suggest that at least one set of data that was previously interpreted as providing evidence for habits in humans (de Wit et al., 2007, 2013) can be criticized on the basis of the information criterion. Although the revaluation test that was deployed in these studies indicated that participants can solve incongruent discrimination problems even when they do not have knowledge of the relation between response and outcome fruits, this observation does not allow for the conclusion that discrimination performance was based on habits. We used revaluation and contingency degradation tests that targeted the points that participants could earn by giving the correct response, thereby showing that participants clearly knew which responses led to points and that they could immediately adjust their behavior when the value of the points (Experiment 1) or the response-point relations (Experiments 2 and 3) changed. These findings strongly suggest that discrimination performance was driven by the goal to earn points and therefore was not habitual.

More generally, the present study illustrates the need to reevaluate also other evidence for habits in light of the information and sensitivity criteria. Doing so may well reveal that the scale and strength of the behavioral evidence for habits in humans is currently overestimated (see Moors et al., in press, for a broader discussion of this issue). Consider the finding that people who often eat popcorn when watching a movie in a cinema theater will eat popcorn even when it is stale (Neal, Wood, Wu, & Kurlander, 2011). Although this observation suggests that, for those people, eating popcorn in a cinema is not driven by the goal to have tasty food, it does not necessarily imply that the behavior is habitual. For instance, it is possible that those people eat popcorn in order to have a complete cinema experience. If eating popcorn is driven by the goal to have a complete cinema experience, people will stop eating popcorn when having a complete cinema experience is no longer valued (revaluation) or eating popcorn no longer serves that goal (contingency degradation). Hence, it is possible that the revaluation test used in the popcorn study (devaluing the taste of popcorn) did not meet the information criterion. Although this alternative explanation is post hoc, it can be tested empirically, for instance, by examining whether adding other elements that increase the cinema experience (e.g., the presence of friends, putting on 3D glasses) decreases the eating of stale popcorn. Adding those other elements reduces the functionality or need of eating popcorn to achieve a complete cinema experience (i.e., it degrades the relation between eating of popcorn and having a cinema experience) and could thus result in eating less popcorn when there are other reasons to not eat popcorn (i.e., when it tastes bad).

We want to emphasize that our arguments are not meant to discredit on an a priori basis all available behavioral evidence for habits in humans. Given the sheer volume and diversity of the evidence (see Wood & Rünger, 2016, for a review), the conclusion that humans can behave in a habitual manner seems unavoidable. Nevertheless, it is essential that researchers use the highest possible standards when studying habitual behavior in humans and rigorously evaluate whether past and future evidence meets these standards. Concluding that behavior is habitual when it is not, not only distorts our view on the relative importance of habitual and goal-directed behavior in humans but also biases our understanding of the conditions that allow for processes that support habitual behavior. Moreover, for applied researchers, it is vital to know whether goals underlie (abnormal) behavior because it determines the ways in which they can influence behavior. Habit research is therefore bound to benefit from a systematic application of the information and sensitivity criteria for revaluation and contingency degradation tests.

Critics may argue that the use of the information and sensitivity criteria sets the bar too high in that it might lead to many misses, that is, instances in which habits are classified as goal-directed behavior. It should be clear, however, that the risk of misses in habit research is probably smaller than the risk of false positives (i.e., instances of goal-directed behavior that are classified as habits). The decision that a behavior is goal-directed can be substantiated by providing evidence for a significant change in behavior as the result of a change in the outcome value or

the response–outcome relation. Although significant effects can be spurious, at least we know the probability of a Type I error. Even from the perspective of nonfrequentist, Bayesian analysis, the absence of an effect is typically less diagnostic than the presence of an effect. Hence, also from this perspective, it seems more likely that researchers will fail to identify the goal-directed nature of behavior than miss classifying a behavior as habitual. We therefore hope that the information and sensitivity criteria will become an important part of the toolbox of habit researchers.

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