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Self-Regulation of Priming Effects on Behavior

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Abstract

In three experiments, we tested whether people can protect their ongoing goal pursuits from antagonistic priming effects by using if-then plans (i.e., *implementation intentions*). In Experiment 1, concept priming did not influence lexical decision time for a critical stimulus when participants had formed if-then plans to make fast responses to that stimulus. In Experiment 2, participants who were primed with a prosocial goal allowed a confederate who asked for help to interrupt their work on a focal task for a longer time if they had merely formed goal intentions to perform well than if they had also formed implementation intentions for concentrating on the task. In Experiment 3, priming the goal of being fast increased driving speed and errors for participants who had formed mere goal intentions to drive only as fast as safety allowed or who had formed no goal intentions, whereas the driving of participants who had formed such goal intentions as well as implementation intentions showed no such priming effects. Our findings indicate that implementation intentions are an effective self-regulatory tool for shielding actions from disruptive concept- or goal-priming effects.

Keywords

behavior priming, self-regulation, shielding goal pursuits, implementation intentions, action control

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Numerous studies have shown that behaviors can be primed outside of conscious awareness (see reviews by Bargh, 2006; Dijksterhuis & Aarts, 2010). Although it is highly functional that behaviors can be initiated and run to completion without conscious attention and guidance (e.g., people may drive especially carefully when taking their children to school), situational cues sometimes activate behaviors that are at odds with how people consciously intend to behave (e.g., they may drive too quickly after watching the Formula One Grand Prix). In the present research, we examined a self-regulatory tool that may help people control such unwanted behavior-priming effects. Specifically, we tested whether spelling out the when, where, and how of an intended course of action (i.e., forming *implementation intentions*; Gollwitzer, 1993, 1999) protects performance from intrusive behavior priming.

Two routes to behavior priming have been studied (Bargh & Ferguson, 2000; Förster, Liberman, & Friedman, 2009). The first is concept priming (the "perception-behavior expressway"; Dijksterhuis & Bargh, 2001), which causes people to act in line with activated concepts, such as "intelligent" or "warm." Because perception and action overlap, concept priming causes people to act in accordance with the primed concepts (the common-coding hypothesis; Hommel, Müsseler, Aschersleben, & Prinz, 2001). For instance, Bargh, Chen, and Burrows (1996) found that activating the concept "elderly" made participants walk more slowly when they left the laboratory.

The second route to behavior priming involves activating mental representations of goals (Bargh, 1990; Chartrand & Bargh, 1996; Dijksterhuis & Aarts, 2010). Past experiences forge associations between situations, representations of goals, and the behavioral means used to attain those goals. Thus, situational cues can activate goals and subsequent goal striving. For instance, Bargh, Gollwitzer, Lee-Chai, Barndollar, and Trötschel (2001) observed that priming achievement goals improved performance on intellectual tasks, enhanced persistence in the face of obstacles, and promoted resumption rates after interruptions—all indicators of effective goal striving (Lewin, 1935; Oettingen & Gollwitzer, 2001).

Self-regulation of behavior-priming effects is difficult because, by definition, people do not realize when priming affects their behavior (Oettingen, Grant, Smith, Skinner, & Gollwitzer, 2006; Wilson & Brekke, 1984). So, what can

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people do to protect their consciously intended behaviors from antagonistic behavior-priming effects? We hypothesized that spelling out in advance exactly how the intended behavior will be performed can effectively safeguard its execution.

Whereas goal intentions specify what a person wants to achieve ("I intend to pursue goal *X*"), implementation intentions specify how a person will act toward a goal, following an if-then format (i.e., "If I encounter critical situational cue *Y*, then I will perform goal-directed behavior *Z*"). A meta-analysis indicated that, compared with forming mere goal intentions, forming implementation intentions enhances rates of goal attainment ($d = 0.65$; Gollwitzer & Sheeran, 2006) by producing a heightened readiness both to detect the critical cue (e.g., Parks-Stamm, Gollwitzer, & Oettingen, 2007; Webb & Sheeran, 2004, 2007) and to enact the specified behavior when the cue is encountered (e.g., Bayer, Achtziger, Gollwitzer, & Moskowitz, 2009; Brandstätter, Lengfelder, & Gollwitzer, 2001). Recent neurological data (Gilbert, Gollwitzer, Cohen, Oettingen, & Burgess, 2009) indicate that forming implementation intentions switches action control from top-down (control by goals) to bottom-up (control by specified cues). Thus, if-then plans delegate the control of behavior from the self to anticipated situational cues.

Accordingly, we hypothesized that behavior primes will have no effect on behaviors that are specified in if-then plans. Because such behaviors are directly controlled by the specified situational cues, they should no longer be susceptible to priming effects. Thus, it may be possible for people to self-regulate the unwanted influence of behavior primes despite the fact that people do not realize when they have been primed. To test this hypothesis, we conducted three experiments, using different priming procedures, conscious goal intentions, and if-then plans.

Experiment 1: Not Being Sluggish

In our first experiment, we used a trait-priming paradigm developed by Aarts and Dijksterhuis (2002). Participants read a fictitious scientific article that emphasized the similarity between animals and humans. The animals referred to in this article were exemplars of either extreme speed (e.g., cheetah) or extreme slowness (e.g., slug). Aarts and Dijksterhuis found that emphasizing the similarities between humans and animals made these two categories seem comparable and resulted in a contrast effect: Participants who had been primed with fast animals walked more slowly when they left the laboratory than did participants who had been primed with slow animals. We used this priming procedure because the behavioral impact is counterintuitive (the primed concept and the direction of the behavior are not aligned) and thus should be particularly difficult to control.

Our dependent variable was speed of responses in a lexical decision task. All participants were assigned the goal of classifying letter strings as words and nonwords as quickly and accurately as possible. In addition, participants formed an implementation intention with respect to one of the nonword stimuli (i.e., "avenda"); the if-component of the implementation intention specified this critical stimulus and the then-component specified an especially fast classification response. We hypothesized that the priming procedure would affect response times for the noncritical stimuli but not for the critical stimulus.

Method

Participants and design. Participants were 50 undergraduates (9 men, 41 women) who took part in return for experimental credits. The experiment had a 2 (concept prime: fast vs. slow; between subjects) × 2 (classification-task stimuli: critical vs. noncritical; within subjects) design. The dependent variable was speed of lexical decisions.

Procedure. Participants were tested individually. They were told that they had to perform two unrelated tasks. First, participants were asked to study a fictitious scientific article titled "The Genetic Comparability of Humans and Animals." The article emphasized how similar animals and human beings are in their genetic makeup. The concept prime was manipulated by referring to five animal exemplars of either extreme slowness (slug, tortoise, hedgehog, caterpillar, and turtle; fast prime) or extreme speed (cheetah, puma, hare, horse, and greyhound; slow prime).

Next, participants started the classification task, for which they had to indicate as quickly and accurately as possible whether each presented stimulus was a word; responses were made by pressing keys labeled "yes" and "no." Participants completed 10 practice trials before forming the implementation intention, "And if the nonword 'avenda' appears, then I respond especially quickly!" Participants then completed 100 trials of the classification task. Each trial comprised the presentation of a fixation point for 500 ms, followed by the presentation of the stimulus, which remained on-screen until the participant responded. Classification response times were measured in milliseconds from the onset of the stimulus to the time participants responded. There was a 2-s interval between trials. Fifty of the 100 stimuli were words, and 50 were nonwords; the order of presentation was random. The critical stimulus, "avenda," was presented six times.

Debriefing indicated that participants did not notice that the animals mentioned in the article were exemplars of speed or slowness, and did not believe that reading the article could have influenced their performance on the classification task. Thus, participants were not consciously aware of any impact of the concept priming on their behavior.

Results and discussion

Classification response times were submitted to a 2 (concept prime: fast vs. slow) × 2 (classification-task stimuli: critical vs. noncritical) mixed-model analysis of variance (ANOVA).

Table 1. Mean Classification Response Time in Experiment 1 as a Function of Concept Prime and Classification-Task Stimuli

Concept prime	Noncritical stimuli	Critical stimuli
Slow	1,171 ms (498)	680 ms (158)
Fast	951 ms (228)	689 ms (147)

Note: Standard deviations are given in parentheses.

There was a significant main effect of classification-task stimuli, $F(1, 48) = 53.15$, $p < .05$, $d = 2.10$, and a marginally significant main effect of concept prime, $F(1, 48) = 3.07$, $p < .09$, $d = 0.51$. These main effects were qualified by the predicted interaction between the two factors, $F(1, 48) = 4.17$, $p < .05$, $d = 0.59$ (see Table 1).

Priming affected the speed of responses to the noncritical stimuli, $F(1, 48) = 4.03$, $p < .05$, $d = 0.60$. Participants in the fast-prime condition responded more quickly (M = 951 ms) than did participants in the slow-prime condition (M = 1,171 ms); this finding replicated the results of Aarts and Dijksterhuis (2002). As expected, however, concept priming had no impact on latencies for classification responses to the critical stimulus (slow prime: M = 680 ms; fast prime: M = 689 ms), $F(1, 48) = 0.04$, n.s., $d = 0.00$. Indeed, participants were considerably faster in responding to the critical stimulus compared with the noncritical stimuli irrespective of the priming condition—fast prime: $F(1, 24) = 27.81$, $p < .001$, $d = 2.16$; slow prime: $F(1, 24) = 31.76$, $p < .001$, $d = 2.30$. This increased speed did not come at the cost of increased classification errors; participants were marginally less likely to make errors in classifying the critical stimulus (M = .08) than in classifying the noncritical stimuli (M = .13), $F(1, 48) = 3.00$, $p = .09$, $d = 0.50$. This pattern of findings suggests that concept priming does not affect behavioral responses when these responses are regulated by if-then plans.

Experiment 2: Curtailing Disruption

In Experiment 2, we explored whether implementation intentions can prevent behavior-priming effects when goals rather than concepts are primed and when if-then plans are formed before, rather than after, the priming occurs. We used a between-participants design in which one group received a prosocial-goal prime and another group (control) did not. All participants worked on a concentration test, during which they were disrupted by a confederate who asked them for help. The dependent variable was the amount of time taken up by the disruption. We hypothesized that the prosocial-goal prime would increase disruption time for participants who had formed the mere goal intention to concentrate on the test, but that participants who had formed an implementation intention for how to deal effectively with distractions would keep disruption time to a minimum, regardless of whether or not they had been primed with a prosocial goal.

Method

Participants and design. Sixty-two female university students participated in this experiment in return for payment ($5). Participants were randomly assigned to conditions of a 2 (goal prime: prosocial vs. control; between participants) × 2 (instruction: goal intention vs. implementation intention; between participants) factorial design.

Procedure. Participants arrived individually at the office of the experimenter, who took them downstairs to an experimental cubicle where the study was conducted. There, participants were told that they would take part in two unrelated experiments, one on memory for biographical information and the other on concentration. First, participants received the instructions for the concentration test (Düker, 1949), which involved solving a series of simple but tedious arithmetic problems presented on a computer. In order to familiarize themselves with this test, participants solved five initial problems. Next, all participants were instructed to form and write down the goal intention, "I will try to find as many correct solutions as possible!" Half of the participants were also instructed to form and write down an implementation intention: "If I get distracted, then I will concentrate on the test even more!"

Before participants continued working on the concentration test, they undertook the biographical memory task (priming manipulation). Participants in the prosocial-goal-prime condition studied a biography of Mother Teresa that detailed her many prosocial activities; control participants studied the biography of Margaret Thatcher, a woman who had the same initials but whose activities were decidedly not prosocial. Participants then received a slightly modified version of the biography they had studied and were instructed to mark where changes had been made in the text.

Next, participants returned to the concentration test, which they were told would last 10 min. The experimenter explained that she needed to leave but would be in her office to ask some final questions when participants had completed the test. The experimenter checked that participants could remember the route to her office before they started on the test. Two minutes later, a female confederate playing the role of another participant entered the cubicle. She asked a series of scripted questions: "Sorry for disturbing you, but have you seen the experimenter?" "Do you know when she will come back?" and "Could you describe how I can get to the office and where I can find her?" The amount of time participants were distracted by the confederate's disruption (measured in seconds from the moment the confederate entered the room to the moment she left) served as the dependent variable.

Once participants had finished the concentration test and returned to the experimenter's office, they answered two final questions: "How committed were you to performing well on the concentration test?" (scale from 0, *not committed*, to 5, *very committed*) and "How important was it for you to show good performance on the concentration test?" (scale from 0,

Table 2. Mean Time Taken Up by the Confederate's Distraction in Experiment 2 as a Function of Goal Prime and Instruction

Goal prime	Goal-intention instruction	Implementation-intention instruction
Control	18.36 s (1.08)	12.84 s (2.15)
Prosocial	24.77 s (8.28)	13.13 s (1.86)

Note: Standard deviations are given in parentheses.

not important, to 5, *very important*). Finally, participants were debriefed; 2 participants expressed suspicion about the confederate's interruption and were excluded from the analysis.

Results and discussion

A 2 (goal prime: prosocial vs. control; between subjects) × 2 (instruction: goal intention vs. implementation intention; between subjects) ANOVA was conducted on the amount of time taken up by the disruption. This analysis revealed significant main effects of instruction, $F(1, 56) = 56.46$, $p < .001$, $d = 2.00$, and goal prime, $F(1, 56) = 8.59$, $p < .005$, $d = 0.77$, which were qualified by the expected interaction, $F(1, 56) = 7.17$, $p < .01$, $d = 0.70$ (see Table 2). Simple-effects analyses indicated that priming a prosocial goal increased disruption time when participants had merely formed a goal intention, $t(56) = 4.09$, $p < .001$, $d = 0.54$, but not when they had also formed an implementation intention, $t(56) = 0.35$, n.s., $d = 0.05$. In the goal-intention condition, participants who were primed with a prosocial goal showed longer disruption times ($M = 24.77$ s) than did those who received the control prime ($M = 18.36$ s). By contrast, participants in the implementation-intention condition showed equally low disruption times regardless of the prime (prosocial-goal prime: $M = 13.13$ s; control prime: $M = 12.84$ s). In fact, the disruption times for participants who had formed implementation intentions were significantly lower than those for participants who had merely formed goal intentions both in the prosocial-goal group, $t(56) = 7.21$, $p < .001$, $d = 0.96$, and in the control group, $t(56) = 3.56$, $p = .001$, $d = 0.47$.

Finally, we explored whether implementation intentions increased people's commitment or motivation to perform well on the test. ANOVAs showed that main and interaction effects were nonsignificant for both variables, all *F*s < 1, *d*s < 0.27. These findings are in line with Webb and Sheeran's (2008) meta-analysis, which indicated that implementation intentions do not benefit performance by increasing goal commitment or goal importance.

Experiment 3: Driving Safely

Whereas Experiment 2 showed that forming implementation intentions controls behavior-priming effects better than forming mere goal intentions does, it did not indicate whether goal intentions on their own might suffice to curb such priming effects. To answer this question, we added a no-goal control condition in Experiment 3 and ensured that both the goal intention and the implementation intention used in the other conditions were geared toward protecting goal-directed actions from unwanted behavior-priming effects. In addition, we used a new priming procedure, embodied goal priming, in which participants had to actually act toward the goal and not just read about it. Participants in the goal-intention condition were instructed to form the goal intention to drive only as fast as safety allowed, and participants in the implementation-intention condition were instructed to form that goal intention plus an implementation intention; control participants received no such instructions. We hypothesized that priming the goal of being fast would increase driving speed and error rates among participants in the control and goal-intention conditions, whereas no significant effects of priming would be observed for participants in the implementation-intention condition.

Method

Participants and design. Sixty-seven male university students participated in this experiment in return for payment ($5). Participants were randomly assigned to the conditions of a 2 (goal prime: fast vs. control; between participants) × 3 (instruction: control vs. goal intention vs. implementation intention; between participants) × 2 (session: baseline vs. follow-up; within participants) design.

Procedure. Participants were informed that they would take part in two experiments, each run by a different research team. They were told that the first experiment concerned the relationship between driving behavior and skin conductance. Participants were seated in a driving simulator that comprised a video monitor, a steering wheel, and two foot pedals, one for braking and the other for acceleration. In addition, electrodes were attached to participants, who were told that the electrodes would record galvanic skin responses.

Each driving-simulation session comprised two rounds of the same circuit, which included 19 curves. Each incursion onto the road margin was counted as a driving error. The computer registered the time per round (i.e., speed); errors were counted from video recordings of participants' driving sessions. Participants undertook one practice round to familiarize themselves with the task before they completed the baseline driving session. Immediately after this baseline driving session, the instruction factor was manipulated. Participants in the control-instruction condition received no further instructions. Participants in the goal-intention and implementation-intention conditions were asked to form and write down the following goal for the second driving-simulation session: "I will drive only as fast as safety allows!" Participants in the implementation-intention condition were asked to form and write down the additional if-then plan, "If I enter a curve then I will slow down, and if I enter a straight road then I will accelerate!"

Participants were informed that their baseline physiological data needed to be analyzed before they completed the follow-up driving session and that, in the meantime, they should take part in a second experiment. They were led to a different laboratory, where another experimenter explained that her study concerned the motor abilities of children versus adults. Participants were asked either to complete as many join-the-dots tasks (out of 12) as possible in 5 min (fast prime) or to complete only 6 join-the-dots tasks at their own pace (control prime); the latter participants were stopped after 5 min.

Participants then returned to complete the follow-up driving session. At the end of the session, they were questioned by the experimenter to determine if they had any suspicion that the priming task had affected their driving performance in the follow-up session. Finally, they were debriefed.

Results and discussion

We first analyzed the data for driving speed using a 2 (goal prime: fast vs. control) × 3 (instruction: control vs. goal intention vs. implementation intention) × 2 (session: baseline vs. follow-up) ANOVA. The predicted three-way interaction was significant, $F(2, 61) = 4.24$, $p < .02$, $d = 0.75$. We decomposed the interaction by examining the impact of goal prime on the increase in speed from the baseline session to the follow-up separately for each instruction condition (see Table 3). Simple-effects analyses showed that the fast prime caused greater increases in driving speed than did the control prime among both control-instruction participants (control prime: M = −11.35 s; fast prime: M = 68.40 s), $F(1, 61) = 6.07$, $p < .02$, $d = 0.63$, and goal-intention participants (control prime: M = −28.63 s; fast prime: M = 76.86 s), $F(1, 61) = 10.20$, $p < .01$, $d = 0.82$. However, change in driving speed between the baseline and follow-up sessions was not influenced by priming among participants in the implementation-intention condition (control prime: M = 22.44 s; fast prime: M = 0.14 s), $F(1, 61) = 0.46$, n.s., $d = 0.17$.

The number of driving errors was submitted to the same 2 × 3 × 2 ANOVA. Again, the anticipated three-way interaction was significant, $F(2, 61) = 4.09$, $p < .03$, $d = 0.73$. Follow-up analyses revealed that, compared with control priming, fast-goal priming increased the number of driving errors for participants in both the control-instruction condition (fast prime: M = 1.45; control prime: M = −4.41), $F(1, 61) = 4.09$, $p < .05$, $d = 0.52$, and the goal-intention condition (fast prime: M = 1.30; control prime: M = −9.25), $F(1, 61) = 13.58$, $p < .001$, $d = 0.94$. By contrast, the participants in the implementation-intention group showed the same reduction in number of driving errors from the baseline to the follow-up session irrespective of whether the goal of being fast had been primed (control prime: M = −7.00; fast prime: M = −7.90), $F(1, 61) = 0.10$, n.s., $d = 0.08$. In sum, the pattern of findings indicates that forming the goal intention to drive only as fast as safety allows fails to down-regulate the effect of priming speed; to eliminate this priming effect, it is necessary to form an additional plan that specifies how this goal intention will be implemented.

General Discussion

Our findings indicate that implementation intentions qualify as an effective self-regulatory tool for preventing unwanted behavior-priming effects. Three studies that used different primes, priming methods, and measures of behavior each provided evidence to support this claim. In Experiment 1, implementation intentions curbed the contrast effect on speed of lexical decisions among participants who had read about animal exemplars of extreme speed or slowness. Priming had no effect on speed of classifying the critical stimulus among participants who had formed implementation intentions to respond quickly to that stimulus (i.e., they classified the critical stimulus swiftly, regardless of the type of exemplar primed). In Experiment 2, participants formed a mere goal intention or a goal intention plus an implementation intention to perform well on a concentration test; they were then primed with the prosocial goal of helping other people or with a control prime. When the concentration test was interrupted by a confederate who asked for help, goal-intention participants

Table 3. Mean Changes in Driving Speed and Number of Errors From Baseline to Follow-Up in Experiment 3 as a Function of Goal Prime and Instruction

Measure and goal prime	Control instruction	Goal-intention instruction	Implementation-intention instruction
Driving speed (s)			
Control	$-11.35\ (70.31)_a$	$-28.63\ (85.16)_a$	$22.44\ (114.89)_a$
Fast	$68.40\ (34.89)_b$	$76.86\ (85.68)_b$	$0.14\ (41.46)_a$
Driving errors			
Control	$-4.41\ (4.23)_a$	$-9.25\ (7.45)_a$	$-7.00\ (5.90)_a$
Fast	$1.45\ (4.13)_b$	$1.30\ (8.69)_b$	$-7.90\ (8.42)_a$

Note: Positive values indicate that participants drove faster and made more errors in the follow-up session than in the baseline session, whereas negative values indicate that speed and errors were reduced in the follow-up session. Within each measure, means with different subscripts differ significantly, $p < .05$. Standard deviations are given in parentheses.

who had been primed with a prosocial goal allowed the disruption to last longer than did goal-intention participants who had received a control prime. However, participants who had formed implementation intentions regarding how to shield their performance on the task from distractions kept the disruption to a minimum irrespective of priming. Finally, participants in Experiment 3 were assigned to a no-goal, goal-intention, or implementation-intention condition prior to the priming manipulation. The goal of being fast was primed using an embodied goal-priming procedure (i.e., participants completed a set of join-the-dots tasks as quickly as possible). When they were subsequently tested in a driving simulator, participants primed with the goal to be fast drove faster and made more mistakes than did control-prime participants. Implementation intentions geared toward safe driving alleviated this priming effect, whereas mere goal intentions to drive only as fast as safety allowed failed to do so.

Research on the regulation of behavior-priming effects has so far focused on potential moderators. These moderators are either factors that prevent primes from activating mental representations of concepts or goals in the first place or factors that prevent these activated mental representations from influencing behavior. In the former category, particular mind-sets (e.g., "think different"; Sassenberg & Moskowitz, 2005), experimental manipulations (e.g., exemplar vs. category—Dijksterhuis et al., 1998; cued vs. uncued attention—Naccache, Blandin, & Dehaene, 2002), past experiences (manifested as associative strength; Dijksterhuis, Aarts, Bargh, & van Knippenberg, 2000), and individual characteristics (e.g., propensity for self-monitoring; DeMarree & Wheeler, 2005) have been found to prevent or modify the way that priming activates mental representations. With respect to the second category of moderators, studies that have analyzed the effect of primed concepts and goals on behavior have identified the presence of inhibitory cues in the environment (Macrae & Johnson, 1998), independent versus interdependent self-construals (Bry, Follenfant, & Meyer, 2008), and the perceived "usability" of primed material (Croizet & Fiske, 2000) as moderators that influence how much priming affects behavior.

Given that priming effects can be assumed to permeate all aspects of everyday life (Bargh, 1997) and thus can hamper the realization of people's goals, we investigated how people can temper such unwanted influences. Certainly, it might help people to seek or establish situational contexts that contain the moderators we have just listed. However, situating oneself in such a context requires knowledge about effective moderators, the contexts in which they can be found, or how to create such conditions. Our research suggests an alternative self-regulatory approach: People may focus on their goals by spelling out their implementation in advance, using if-then plans. By forming implementation intentions, people submit their subsequent goal striving to the direct control of situational cues, and primed antagonistic concepts or goals can no longer interfere. Prior to our studies, the shielding power of implementation intentions had been observed only for disruptive inner states: People who spelled out a focal goal pursuit by forming implementation intentions found it easier to stay on track even when mood, ego depletion, or incompleteness militated against goal attainment (Bayer, Gollwitzer, & Achtziger, 2010). The finding that implementation intentions can also block the disruptive effects of antagonistic primes in a person's environment suggests that forming implementation intentions is a highly effective self-regulatory strategy for goal shielding.

Declaration of Conflicting Interests

The authors declared that they had no conflicts of interest with respect to their authorship or the publication of this article.

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