Behavioural Processes 200 (2022) 104698

Contents lists available at ScienceDirect

Behavioural Processes

journal homepage: www.elsevier.com/locate/behavproc

Instructional control in choice tasks: The relation between type of schedule and relative expected values

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ARTICLE INFO

Keywords:
Decision making
Choice behavior
Instructional control
Matching law
Optimal, behavior
Description-experience gap

ABSTRACT

The present work aims improve our understanding of the boundaries of instructional control. It does so by solving contradictory results obtained on two different fields: Three studies conducted on the description-experience gap field, showing that instructions are neglected when personal experience is available, and several others conducted on the experimental analysis of behavior paradigm getting to the opposite conclusion. Two factors were studied: the type of schedule, and the relative expected values between options. The present work showed that (1) positive evidence of instructional control was found in a choice task with probability schedules and different expected values between options; (2) negative evidence of instructional control was found in a choice task with VI schedules and similar expected values between options; and (3) these results, together with previous research, suggest that relative expected values are a fundamental factor on understanding the presence of instructional control in choice tasks. We conclude that the relevance of this factor relies on its capacity to make participants' decisions easier: all else being equal, adding descriptions enables participants to better discriminate optimal behavior in choice tasks.

1. Introduction

Psychology paradigms differ from each other, not only theoretically but also empirically: depending on the definition of the psychological science itself and the study field, experimental interests will vary greatly among them. However, there are certain overlaps between empirical research fields and effects that can be illuminating, if identified and understood.

The present work aims to improve our understanding of the boundaries of instructional control. It will do so by disentangling complementary results obtained on two different fields; specifically, from three studies conducted with the description-experience gap paradigm, showing that descriptions do not affect human behavior when personal experience is available (Jessup et al., 2008; Lejarraga and Gonzalez, 2011; Viudez et al., 2017), and several others conducted on the experimental analysis of behavior paradigm getting to the opposite conclusion (Ader and Tatum, 1961; Ayllon and Azrin, 1964; Baron and Kaufman, 1966; Baron et al., 1969; Blair, 1958; Catania et al., 1982; Dews and Morse, 1958; Galizio, 1979; Hackenberg and Joker, 1994; Holland, 1958; Lippman and Meyer, 1967; Matthews et al., 1985; 1977; Shimoff et al., 1981; Takahashi and Shimakura, 1998; Weiner, 1962). One of these studies (Takahashi and Shimakura, 1998) used a procedure that made it possible to be experimentally compared with those of the description-experience gap.

1.1. Absence of instructional control in the Description-Experience Gap

When confronted with formally expressed probabilities, we tend to choose as-if we overweight low probabilities and underweight high probabilities, as described by Cumulative Prospect theory (Tversky and Kahneman, 1992). Nonetheless, the opposite result is found when we have to experience the probabilities of the events ourselves rather than reading them, giving its origin to a phenomenon called the description-experience gap (Barron and Erev, 2003; Hertwig et al., 2004).

In tasks involving description-based choices, different gambles are shown to the individuals in a text and/or graphic way (see Weber et al., 2004 for a meta-analysis). Participants have all the information available from the beginning, that is, the outcomes values and probabilities are known, as in the *instructions* groups in studies from Experimental

Abbreviations: VI, Variable Interval; USD, United States Dollars.

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https://doi.org/10.1016/j.beproc.2022.104698

Available online 5 July 2022

Analysis of Behavior.

In tasks involving experience-based choices, two options are shown to the participants in a symbolic way (e.g., doors, bags, buttons, etc.) (for a review of recent research, see Rakow and Newell, 2010). Therefore, they know nothing about the outcomes values and probabilities, being only able to infer them by sampling, with or without real consequences, from both options, as in the *contingencies* groups in studies from Experimental Analysis of Behavior.

Several studies have investigated the description-experience gap, both in basic tasks involving points/money and in applied tasks such as social cooperation (Martin et al., 2014), online product reviews (Wulff et al., 2015), climate change (Dutt and Gonzalez, 2012a, 2012b) or medical decisions (Lejarraga et al., 2016).

Still, little is known about the description-experience gap paradigm when both descriptions and direct experience are available. On that sense, Jessup et al. (2008) divided the participants in two groups and found that the presence of feedback on repeated decisions from description (*mixed group*) altered the choice behavior compared to the group without feedback *(description group*). Subjects on the *mixed group* were shown the possible outcomes and probabilities, at the same time that were allowed to play the same gamble repeatedly so they could learn the outcomes distributions from their experience too. Subsequently, Lejarraga and Gonzalez (2011) conducted an experiment using three groups: *description, experience* and *mixed*. The results showed that choice behavior of the subjects from the *mixed group* was statistically different from those of the *description group* and almost identical to those of the *experience group*. The authors concluded that when both sources of information are available, individuals neglect the descriptions (Lejarraga and Gonzalez, 2011). Our team successfully replicated the results from Lejarraga and Gonzalez (2011), and included two additional *description* and *mixed groups* which read enriched descriptions of the options (i.e., outcomes likelihood was expressed using probabilities and frequencies, and each option expected value was also provided). Similar results were obtained: the *mixed group* exhibited the same behavior as the *experience group* (Viudez et al., 2017).

1.2. Disentangling the different procedures

The inconsistency between the results of studies showing that human behavior is strongly affected by instructions and those showing that descriptions are neglected in their tasks serves as the rational for the present work, as this discrepancy demands explanation. However, the experimental procedures of experiments are usually so different that it is difficult to disentangle which factors are causing it.

Nonetheless, one study from the experimental analysis of behavior field that found positive evidence of instructional control (Takahashi and Shimakura, 1998) used a procedure similar to those of the description-experience gap that didn't, as it explicitly examined choice behavior under concurrent VI schedules. On this procedure, on each component, the first response that is made after a variable time-interval is reinforced. For example, in a concurrent VI 60-s VI 30-s schedule, the first component will deliver a reinforcer when the individual makes a response on its response collector after an average time of 60 s has elapsed. Once the reinforcer is delivered, this cycle restarts. On the second component, the same rules apply, with the exception that it may deliver reinforcers with double frequency (i.e., after an average time of 30 s).

The authors used the following concurrent VI schedules: VI 480-s–VI 60-s, VI 180-s–VI 60-s, VI 60-s–VI 60-s, VI 15-s–VI 60-s, and VI 7.5-s–VI 60-s in a quasi-random order. 15 Participants were divided in three groups that differed on the type of instructions that they would get: one group faced instructions describing the structure of VI schedules by telling the participants that the number of presses is not relevant to get the points; another group faced instructions describing the relative frequencies of reinforcements that they would get on each lever; and the third group faced both types of instructions (see procedure of Experiment 2 for a detailed description of similar instructions) (Takahashi and Shimakura, 1998).

The authors assessed participants' choice behavior by evaluating its conformity to the matching law (Baum, 1974; Herrnstein, 1961) and found evidence of instructional control: participants facing instructions describing either frequencies of reinforcement or frequencies of reinforcement plus structure of VI schedules behaved more optimally –i.e., their response rates better matched reinforcement rates–, compared to participants facing instructions describing structure of VI schedules. Their method has some similarities with the description-experience gap paradigm, making the comparison of results meaningful.

Two main factors distinguish the procedures of the description-experience gap tasks (Jessup et al., 2008; Lejarraga and Gonzalez, 2011; Viudez et al., 2017) and the concurrent VI schedules (Takahashi and Shimakura, 1998): the type of schedule, and the relative expected values of the options. More specifically:

1. While using concurrent VI schedules makes the probability of reinforcement on each option dependent on time (see Figure A1), tasks from the description-experience gap involve probability schedules –prospects– that are independent of time (i.e., every time the participant chooses a particular prospect, the probability of getting reinforcement remains the same).

2. On the experiment from Takahashi and Shimakura (1998), both options delivered the same reinforcer, leading to different expected values due to the differences on reinforcers frequencies. However, on the description-experience gap experiments the magnitudes of the reinforcers are different, for different probabilities, so that expected values (magnitude X probability) for both options remain similar.

Therefore, having only two procedural differences between experiments from both research fields, the evaluation of their contribution to instructional control is straightforward: a two-factor table with two conditions on each factor was constructed to disentangle the situation of these differences (see Table A1). There is negative evidence of instructional control when the task involves probability schedules using options with similar expected values –as in the description-experience gap tasks– (Jessup et al., 2008; Lejarraga and Gonzalez, 2011; Viudez et al., 2017). On the other hand, there is positive evidence of instructional control when the task involves VI schedules using options with different expected values – as in concurrent VI schedules– (Takahashi and Shimakura, 1998). Consequently, the remaining two possibilities shall serve as the experimental procedures for the present paper in order to disentangle the specific contribution of each factor to the instructional control in choice tasks.

1.3. The Present Study

From a broader point of view, the relevance of the present study does not relies on the resolution of a particular contradiction in the literature, but on its contribution to delimiting the boundaries of instructional control: why some times instructions seem to be a strong factor to influence human behavior while other times they seem to be neglected? Furthermore, we aim to compare experimentally two different research fields by identifying their common points. *Descriptions* and *instructions*, and *experience* and *contingencies* can be understood as functionally similar, so they may serve as anchor for the present and future studies on these fields.

Taking into account the information given in Table A1, the first experiment will evaluate instructional control on a task using probability schedules and options with different expected values (e.g., a choice task between a prospect that gives 1 point with 40% of chance, and zero otherwise, and a prospect that gives 1 point with 20% of chance, and zero otherwise). The aim of the second experiment is to investigate instructional control on a task using VI schedules and options with similar expected values (e.g., a concurrent VI 60-s VI 180-s schedule where the former gives a reinforcer of 20$ while the later gives a reinforcer of 60$). Results will be evaluated at a group level on

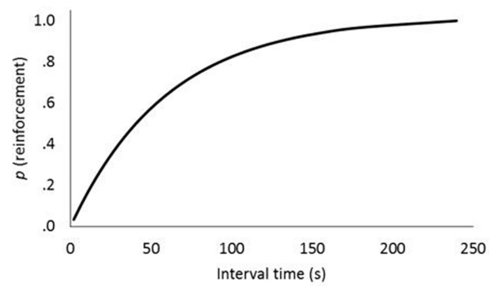

Fig. A1. Probability of a response getting reinforcement following a certain interval time in a VI 60-s schedule.

Experiment 1 and at an individual level on Experiment 2, following adequate data analysis for each experimental methodology that facilitates direct comparison with the studies we directly reference.

The results from both experiments will serve to complete the schema in Table A1 in order to know which factor is causing the contradiction in the results regarding instructional control, and therefore contribute to define the boundaries of instructional control.

2. Experiment 1

2.1. Method

Two choice tasks involving probability schedules were presented to the participants in a single-factor, between-subjects design with two levels of source of information available –only experience or experience and description–. Both options delivered reinforcers of similar magnitude, namely 1 point, with different time-independent probabilities. Thus, one option had a higher expected value than the other one (i.e. the option with a higher probability of reinforcement). Because of this property of the procedure, we expected the participants with objective information of the gambles (i.e. experience and description of the options) to display a more optimal behavior – this is, to have a higher proportion of choices on the option with higher expected value.

2.1.1. Participants

Our sample included 47 undergraduate students of Psychology from the University of Guadalajara (51% male). They were recruited by an announcement of their professor. Participants entered on a raffle, and the winner would get real money ranging from 100 to 300 Mexican pesos (approximately 5–15 USD) depending on the performance on the task. The Institutional Ethics Committee from the Neurosciences Institute of the University of Guadalajara approved this experiment.

2.1.2. Materials

Participants faced two choice tasks on a computer screen, presented in random order. They made their choices using the mouse of the computer. At the end of the experiment, participants had to fill a custom-made questionnaire about task comprehension and gambling habits. The experimental program was written using the OpenSesame software.

2.1.3. Procedure

Participants were randomly assigned to one of two groups: *experience* (n = 23) and *mixed* (n = 24). Groups differed on the form of presentation of the problems (see Table A2). Specifically, the *experience* group faced two unlabeled buttons, and had to choose between them 100 times. The *mixed* group was presented with both the probabilistic description of the problems, as labels near each button, and the experienced outcomes, as they also had to choose between them 100 times. Button position –left or right for the different types of options - was randomized for each participant.

In each task, the participants faced a *fixed* and a *changing option,* which varied across the two problems each participant was exposed to. The *fixed option* gave 1 point with a probability of 40% in both problems, while the *changing option* gave 1 point with a probability of 20% and 80% for problems A and B, respectively.

Every choice made by the participants had real consequences for them, as they got as many points as the sum of the outcomes of the 100 trials for both choice tasks (1 point = 2 Mexican pesos). Participants saw, highlighted in red, the amount of points they got in each trial, and also the amount of points they missed on the other option.

2.2. Results and Discussion

Figure A2 shows the average proportion of choices of the *changing option* in blocks of 25 trials for problem A (1 point with a probability of

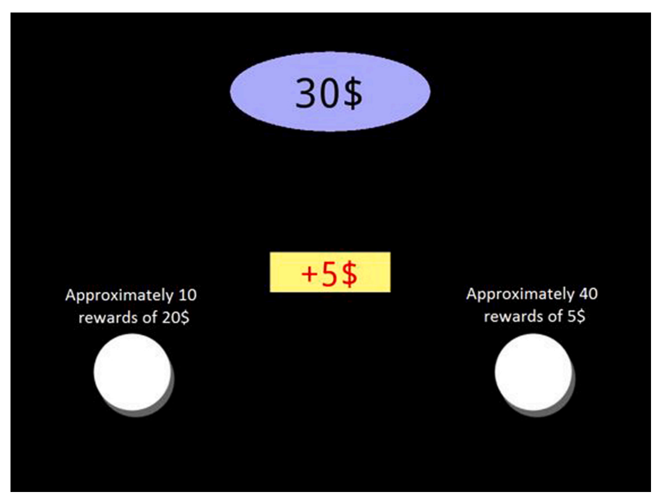

Fig. A3. Screenshot of the task for the mixed group: a counter of the total amount earned to that point (above), the display of the reinforcer that was earned just before (center), and the two response buttons with their respective labels. These labels were not available for the experience group.

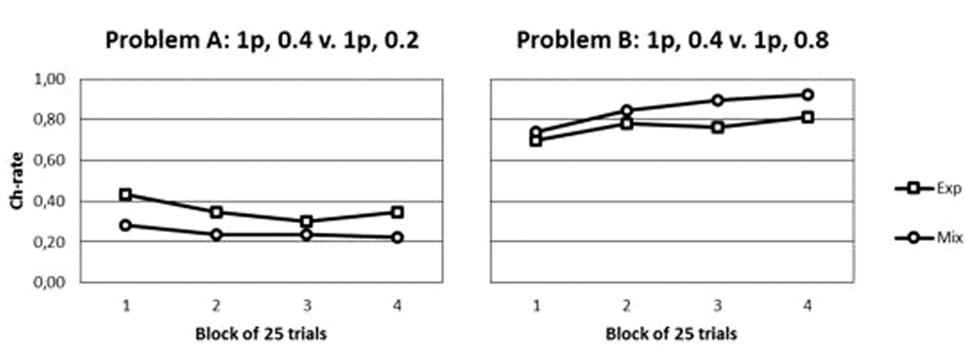

Fig. A2. Choice behavior expressed as changing option rate in blocks of 25 trials. Left panel shows data for problem A and right panel for problem B.

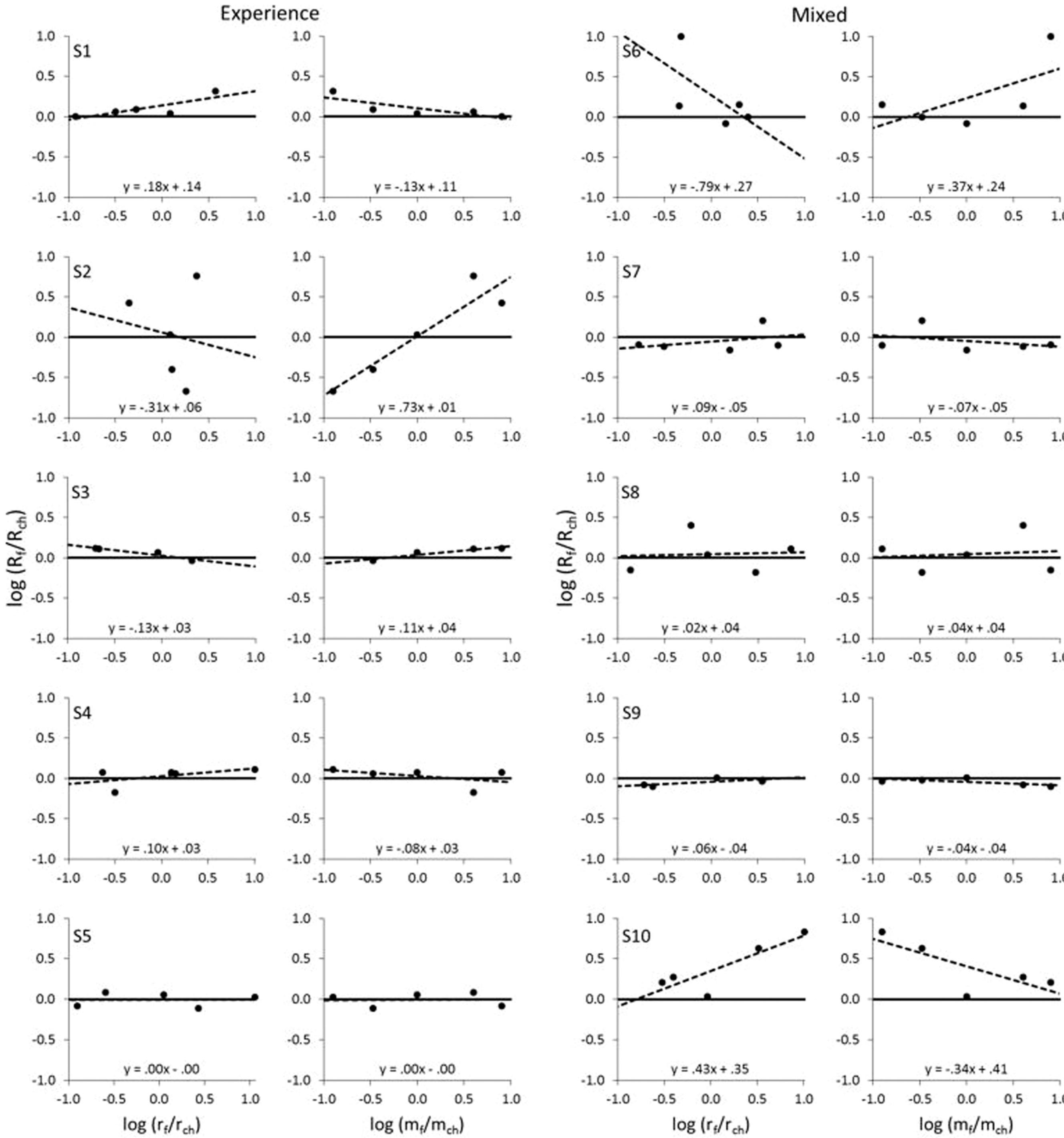

Fig. A4. Ratios of response rates in the two components plotted against the ratios of reinforcer frequencies (left-hand graphs) and against the ratios of reinforcer magnitudes (right-hand graphs) in the two components using double logarithmic coordinates (base 10), for the participants in experience (left panel) and mixed (right panel) groups. Data from the last 3 sessions of each condition were used. Solid lines represent predicted behavior by the Generalized Mathing Law, and dashed lines represent best fit linear functions. Equations for linear functions are represented at the bottom of each graph.

20%) on the left panel, and problem B (1 point with a probability of 80%) on the right panel. Our prediction was confirmed, as *mixed groups* displayed consistently, across problems and blocks, a higher preference towards the optimal option (i.e. *fixed option* in problem A and *changing option* in problem B). One-tailed *t*-tests corroborated this assertion both in problem A, $t(45) = 1.94$, $p = .029$, $d = 0.568$; and problem B, $t(45) = 1.79$, $p = .040$, $d = 0.523$.

Regarding post hoc power analysis, a design with group sample sizes of 23 and 24, can detect effect sizes of $\delta \geq 0.568$ and $\delta \geq 0.523$ with a probability of at least.61 and.55, respectively, assuming a one-sided criterion for detection that allows for a maximum Type I error rate of $\alpha = 0.05$. These power estimators are far from great, but given that we have rejected the null hypothesis, type II error is not to concern us at this moment.

Optimal behavior for tasks such as those used on the present experiment consists in evaluating which one is the best option, and sticking to it for the whole task. As the probabilities are static, the chances that a particular option will lead to reinforcement are the same

Table A1
Preliminary evidence of instructional control as a function of expected values of the options and type of schedule.

	Probability	Variable-interval
Similar expected values	Negative evidence (D-E gap studies)	Experiment 2
Different expected values	Experiment 1	Positive evidence (Takahashi and Shimakura, 1998)

Table A2
Description of the problems for the mixed group (original in Spanish).

Problem A	Problem B
Option A: win 1 point with a 40% chance or win 0 points otherwise. Option B: win 1 point with a 20% chance or win 0 points otherwise.	Option A: win 1 point with a 40% chance or win 0 points otherwise. Option B: win 1 point with an 80% chance or win 0 points otherwise.

Table A3
Fixed and changing option VI values and reinforcer magnitudes across conditions. Only mixed group faced False Condition.

Condition	*Fixed option*	*Changing option*
1	VI 60-s (20$)	VI 7.5-s (2.5$)
2	VI 60-s (20$)	VI 15-s (5$)
3	VI 60-s (20$)	VI 60-s (20$)
4	VI 60-s (20$)	VI 180-s (60$)
5	VI 60-s (20$)	VI 480-s (160$)
False*	VI 60-s (20$)	VI 180-s (20$)

Table A4
Final evidence of instructional control as a function of expected values of the options and type of schedule.

	Probability	Variable-interval
Similar expected values	Negative evidence (D-E gap studies)	Negative evidence (Experiment 2)
Different expected values	Positive evidence (Experiment 1)	Positive evidence (Takahashi and Shimakura, 1998)

on each trial. Hence, choosing invariably the *fixed* option in Problem A and the *changing option* in problem B would maximize profit for every participant.

In the questionnaire about task comprehension and gambling habits, one third of the participants, no difference between groups, mentioned the task to be following some sort of pattern so that prizes would rarely appear for a number of times in a row. Accumulated research (Tune, 1964) has shown that human participants tend to expect more short runs and less long runs in randomly generated sequences when compared to their mathematically expected distributions. In two-choice learning tasks, it has been suggested that participants may be interpreting the discrepancy between the distribution that they expected and the distribution that they encounter in every trial as if their probabilities were conditional (Beach and Swensson, 1967). This behavior has been documented since the summer of 1913 in a Monte Carlo casino, when most gamblers started to bet exaggeratedly on red, after fifteen blacks had shown up in a roulette wheel. However, the roulette kept on landing in black eleven times more. Gambler's behavior showed that they were assuming that the randomness of the wheel should compensate the long run and land on red (Lehrer, 2009, pp. 66).

Regardless of instructional control, this pattern –the gambler's fallacy– appears to be operating in our participants' behavior, too. Additionally, previous research suggests that participants do not comprehend properly the descriptions commonly used in probability schedules, as they behave differently when those are presented in a different format or in a different unit (Gottlieb, Weiss, and Chapman, 2007; Harman and Gonzalez, 2015; Hilbig and Glöckner, 2011; Viudez et al., 2017).

3. Experiment 2

3.1. Method

Different concurrent VI schedules were presented to the participants in a single factor, between-participants design with two levels of source of information available –only experience or experience and description–. Two schedules delivered reinforcers of different magnitudes with different time-dependent probabilities. Reinforcer magnitude for each schedule was adjusted so that both options had identical expected values.

3.1.1. Participants

Our sample included 10 undergraduate students of Psychology from the University of Guadalajara (80% women) that were divided in two groups of 5, following the study serving to the rational of the present work, which also included 5 participants per group (Takahashi and Shimakura, 1998), to facilitate direct comparison of the results. The smaller sample of participants of this experiment is justified by the higher reliability of the measure on choice behavior (McClelland, 2000) that compensates sample size, and it is common practice when researching choice behavior using concurrent VI schedules (Athens and Vollmer, 2010; Martens and Houk, 1989; McDowell, 1988; Neef et al., 1992; Neef et al., 1994). Furthermore, the statistical analysis used by Takahashi and Shikamura (1998) have been complemented with Bayesian methods to improve the accuracy in interpretation of results.

Participants were recruited by an announcement of their professor, and earned virtual money in each session. At the end of the experiment, one session of one participant would be randomly drawn and that participant would get that same amount of real money, ranging approximately from 100 to 400 Mexican pesos (approximately 5–20 USD) depending on the execution on the task. The Institutional Ethics Committee from the Neurosciences Institute of the University of Guadalajara approved this experiment.

3.1.2. Materials

Participants faced the concurrent VIs schedules on a computer screen, where their choices would be made using the mouse of the computer. At the end of the experiment, participants had to fill a custom-made questionnaire about task comprehension and gambling habits. The experimental program was written using the OpenSesame software.

3.1.3. Procedure

Each experimental session lasted 10 min, except the very first ones for some participants, due to an error in the program code. Therefore, additional sessions were run by those participants on the first experimental condition to ensure data consistency. After each session, participants could take a break of 5 min if they wanted to. The median number of sessions per day, and the average number of days they came to the lab were 6 and 3.5, respectively.

Participants were randomly assigned to one of two groups: *experience* and *mixed.* Groups differed on the instructions they were given at the beginning of each session and the presentation format of the problems. VI values, instructions and presentation format were adapted from the work of Takahashi and Shimakura (1998), in order to facilitate the comparison of results.

Instructions for the *experience group* only mentioned the basic elements of the task, while instructions for the *mixed group* also included reference to the functioning of the VI schedules. Furthermore, the task included informative labels above the buttons in the *mixed group*, reporting the average number of reinforcers that could be earned in one session, and their magnitude (see Figure A3).

Instructions appeared at the beginning of each session and there was no time limit for reading them, before starting the task. Instructions for the *experience group* read as follows (original in Spanish):

Your task consists in increasing the counter in the superior part of the screen by clicking on the buttons of the screen. Sometimes, when you click on the buttons, you will get rewards whose amount will be displayed at the central box, and the counter will increase accordingly. There are two buttons, and each one of them can increase the counter independently. You can choose between them freely.

This session will last 10 min, and within this period you can get money. Please, get as much money as you can. Once the experiment is over, a raffle will be carried out between the participants, and the winner will get the money obtained in one of his/her sessions, randomly chosen.

The instructions for the *mixed group* were identical, with the exception that the following paragraphs were added between the previous paragraphs:

The number of clicks on the buttons is not relevant to increase the counter. The situation in which clicking a button will raise the money counter appears at some moments on each button. You can increase the money counter by clicking on the button in that moment. This situations appear at random intervals on each button.

You can get the rewards shown above each button by clicking on the left and right buttons. The rewards above the right button can be obtained by clicking the right button and the rewards above the left button can be obtained by clicking the left button. You can obtain these rewards during one session.

The order of the different conditions for each participant followed a quasi-complete Latin square order. In each condition, the participants faced a *fixed option* on the left key, and a *changing option* on the right key. The *fixed option* provided 20 pesos (1 USD approximately) following a VI 60-s on every condition, while the *changing option* had different VI and reinforcers values on each condition. *Changing option* parameters were adapted so that expected values on both options remained identical, i.e., longer VI schedules delivered proportionally higher reinforcers and vice versa (see VI and reinforcers values for each condition in Table A3). A total of 30 interreinforcement intervals were arranged for each VI schedule following the method of Catania and Reynolds (1968).

Participants in the *mixed group* faced one extra condition with false descriptions that was not presented to the *experience group*. The purpose of this phase was to have an extra measure of the presence or absence of instructional control. On this condition, the *fixed option* remained the same as in the rest of the experiment, while the *changing option* was accompanied by a false description. The label of this option was identical to the *fixed option* VI 60-s (i.e., "about 10 rewards of 20 pesos"), while its actual schedule was a VI 180-s with a reinforcer of 20 pesos. Therefore, the label was false regarding reinforcer density: it stated that reinforcer was 3 times more frequent than it actually was. This extra condition was added to the procedure to have another measure of evidence of instructional control on the task.

Each condition remained in effect until at least 3 sessions were run and the standard deviation of the response rate on the *fixed option* (i.e., number of responses on the *fixed option* divided by the total number of responses) on the last 3 sessions was smaller than.1. Finally, a 5-s changeover delay was programmed throughout the whole experiment; therefore responses emitted during the first 5 s following a changeover didn't deliver any contingencies.

3.2. Results and Discussion

Participants from both groups showed a similar, high response rate across the experiment, $F(1, 8) = 3.78$, $p = .088$, average response rate being 2.6 responses / sec, even for participants from *mixed group,* whose instructions stated that the number of responses was not important to get the reinforcers. We were informed by personal communication with dr. Takahashi that participants in their experiment had moderate response rates (Takahashi and Shimakura, 1998). This contrast may have been caused by the difference between our apparatus: while they used two small levers of 2.2 × 2.0 × 0.6 cm for the participants to respond, we used standard pc mouses.

Regarding choice behavior, Figure A4 shows the ratios of the response rates in both components (i.e., average response rate on the *fixed option* divided by average response rate on the *changing option*), plotted against the ratio of the reinforcer frequencies and against the ratio of the reinforcer magnitudes, from the last 3 sessions of each condition. Similar results were obtained when the ratios of the allocation times in both components were used instead of response rates. When, in a particular session, the total number of reinforcers were obtained just on one side, this data was not considered on the data analysis (Takahashi and Shimakura, 1998). This treatment resulted in the analysis of 92% of the data. However, condition VI 60-s – VI 480-s from S3 was completely neglected, as the participant never obtained one single reinforcer from the *changing option* (i.e., VI 480-s) on the 3 sessions that were run.

When the slopes of both groups were compared following the statistical analysis from Takahashi and Shimakura (1998), *t*-tests revealed them to be similar, both when the independent variable was the ratio of reinforcer frequencies, $t(8) = 0.02$, $p = .985$, $d = 0.017$; or the ratio of reinforcer magnitudes, $t(8) = 0.69$, $p = .508$, $d = 0.438$. Regarding post hoc power analysis, a design with a sample size of 5 in each group can detect effect sizes of $\delta \geq 0.017$ and $\delta \geq 0.438$ with a probability of at least.05 and.09, respectively, assuming a two-sided criterion for detection that allows for a maximum Type I error rate of $\alpha = 0.05$. Finally, the estimated Bayes factors (null/alternative) for reinforcer frequencies and magnitudes suggested that the data were 2.03:1 and 1.75:1, respectively, in favor of the null hypothesis assuming that instructions did not affect participants' behavior with an error percentage lower than 0.01%.

These power estimators and Bayes factors might seem weak at first, as they are quite affected by the sample size; but we should recall here that the representativeness of the data in a concurrent schedules experiment is ensured by the procedure, rather than by increasing sample size: each participant remained in each condition until at least three 10-min sessions were run and the standard deviation of the response rate on the fixed option (i.e., number of responses on the fixed option divided by the total number of responses) on the last 3 sessions was smaller than.1. Therefore, we can conclude that every participant exhibited sub-optimal behavior on this experiment, as none of them matched choice with reinforcer frequency (Baum, 1974; Herrnstein, 1961). Except for S2, every participant in our experiment appeared to be insensitive to both reinforcer frequency and magnitude.

However, our interpretation of the linear models slopes as not different from zero relies on the prediction from the Generalized Matching Law (Baum, 1974) according to which participants should be sensitive to both components. Due to the properties of the VI schedules that were used in our experiments, these components "cancel each other": if one option delivered reinforcers twice as frequently as another one, those reinforcers were also halved in their magnitude. Therefore, both options had identical expected values and this made participants to show matching behavior.

The only exception to this pattern was S2, which consistently matched her choice behavior to reinforcer magnitude, $t(3) = 4.70$, $p = .018$. This pattern resulted in a more sub-optimal behavior: she got less reinforcers than she would if she had been indifferent between both options, or if she had matched her behavior to reinforcer frequency (Baum, 1974; Herrnstein, 1961). Although applying alpha value correction using Holm–Bonferroni method yields a non-significant p-value, visual inspection of the data shows a clear matching choice behavior to reinforcement magnitude, and an estimated Bayes factor (null/alternative) suggested that the data were.284:1 in favor of the null hypothesis, or rather, 3.5 times more likely to occur under the model assuming that the participant matched her choice behavior to reinforcement magnitude. Nevertheless, the remaining 4 participants showed a straightforward absence of preference between options.

Mixed group participants' behavior on the condition with false descriptions was compared to their behavior on the condition VI 60-s – VI 60-s, in order to have another analysis of instructional control, as both conditions had identical descriptions, while different contingencies. The condition of false instructions comprised identical descriptions to the condition of VI 60-s – VI 60-s, 20 pesos in each option, while the actual contingencies were VI 60-s – VI 180-s, 20 pesos in each option (i.e. the description of the reinforcer magnitude was inaccurate, but the description of the reinforcement frequencies was not). Data analysis from the last 3 sessions from both conditions revealed differences on the ratio of response rates on both options, $F(1, 28) = 9.55$, $p = .004$. This result was also supported by a Bayes factor (null/alternative) of.107:1, suggesting that the data were 9.4 times more likely to occur under the model assuming that the instructions affected participants' behavior. Therefore, participants adapted their behavior when contingencies changed, resulting in a more optimal behavior than if they had followed the false descriptions. Specifically, on the condition VI 60-s – VI 60-s, the average rate of response ratios was.96. That is, participants distributed their responses fairly between the two options. However, on the condition with false descriptions, the average rate of response ratios was 2.02. This is, participants responded twice as much to the *fixed option* (VI 60-s) than to the *changing option* (VI 180-s, described as VI 60-s).

Therefore, our interpretation that participants were being influenced by both properties of the options –frequency and magnitude– finds support on these results too. Optimal behavior on this condition would be to respond three times as much on the *fixed option* than on the *changing option*, as their only difference was that the former delivered three times more reinforcers than the latter. Participants showed a still sub-optimal matching behavior to reinforcer frequencies by responding twice as much on the *fixed option* compared to the *changing option.*

As R^2 values measure the proportion of variation of the dependent variable explained by the independent variable, and most of the linear models we got were consistent with the null hypothesis, this measure was not used because it gave artificially low values of model fitting (e.g. the R^2 values for both regression models of S5 were 0, even if the model clearly fits the data). Results showed that both groups behaved similarly, and the slopes of their models were not significantly different from zero.

4. Discussion

On the one hand, participants from Experiment 1 showed evidence of instructional control in a choice task using probability schedules with different expected values: the *mixed group* exhibited a more optimal behavior across blocks and problems, compared to the *experience group.*

On the other hand, participants from Experiment 2 showed no sign of instructional control when confronted with concurrent VI schedules, where both options had identical expected values: most of our participants, regardless of their experimental condition, distributed their responses evenly between both options. Moreover, in the condition with false descriptions where both schedules were described likewise, they exhibited a preference towards the option with a higher expected value.

These results, taken together (see Table A4), suggest that the difference between the instructional control that was found on previous work using concurrent VI schedules (Takahashi and Shimakura, 1998) and the absence of it on the experiments in the description-experience gap paradigm (Jessup et al., 2008; Lejarraga and Gonzalez, 2011; Viudez et al., 2017) was caused by the relative expected values that were adopted: negative evidence of instructional control was found on the studies that used options with similar expected values, while positive evidence of instructional control was found on the studies that used options with different expected values, without regard of the type of schedule. Therefore, the situations where instructions describe options with different expected values to the participants seems to be a relevant factor to understand why sometimes instructional control appears while other times it does not.

Our interpretation of the data suggests that relative expected values seem to be important because, given the parameters used on the present experiments, it facilitated optimal behavior. Therefore, discrimination of the relative expected values is important to put participants' behavior under instructional control as long as they make one option clearly better than the other one. Thus, the descriptions of the options clarify this difference and make the participants choose more optimally. When both options have similar expected values, adding information about them does not affect choice behavior.

Our assertion is congruent with previous literature demonstrating instructional control in response rates (Galizio, 1979; Matthews et al., 1977; Shimoff et al., 1981), in choice behavior under concurrent probability schedules (Barron, Leider, and Stack, 2008; Braveman and Fischer, 1968; Fantino and Esfandiari, 2002), concurrent time schedules (Hackenberg and Joker, 1994), concurrent ratio and interval schedules (Catania et al., 1982; Matthews et al., 1985), and the Prisoners' Dilemma (Baker and Rachlin, 2001). In all of them, instructions signaled a behavior pattern to increase reinforcement frequency. A recent study on risk-taking also showed results coherent with our hypothesis, as subjects' behavior was affected by instructions only in the condition using different expected values for the options (Newell et al., 2016).

The hypothesis that we derive from our experiments is of special relevance to define the extent to which instructions affect human behavior. Previous studies have demonstrated that instructions stop affecting behavior when the contingencies in effect contradict the instructions, as in Galizio (1979). The present paper adds another limit to instructional control, this is, when they do not facilitate optimal behavior in choice tasks.

4.1. Limitations

Only two choice tasks were used in Experiment 1 so the results could be compared with those studies on the description-experience gap that are fundamental for the current experiment (Jessup et al., 2008; Lejarraga and Gonzalez, 2011). Further research would be needed using additional tasks to add more validity to our analysis.

The collapsed choice proportion across all trials was used as the dependent variable for data analysis in Experiment 1. A more sophisticated data analysis, such as a multilevel modelling approach might have shown different conclusions. However, we deem more important, given the objective of these experiments (i.e., to be useful to be compared with related research) to use the same methods and data analysis than the other studies. In this case, we refer to Lejarraga and Gonzalez (2011) experiment, where they compared groups using this same measure. Therefore the criteria remains constant across studies, allowing for a more comprehensive comparison.

A plausible impact of the difference between our apparatus and those used by Takahashi and Shimakura (1998) on Experiment 2 participants' response rates could not be measured; as their participants' response rates was not available.

First sessions of Experiment 2 were shorter than 10 min. This was caused because the program required a time limit together with a trial number limit (i.e., loops, or iterations). We initially set a limit of 1000 trials which still would allow for a response rate of 1.5 responses / sec. However, due to the higher response rate of 2.6 responses / sec that our participants exhibited, this limit was overpassed on the first sessions. Therefore, it was changed to 2500 so it would not be reached before the 10 min time limit. Still, additional sessions were run to ensure data consistency before introducing a new condition.

No criterion of absence of directional trend in choice proportions was introduced in Experiment 2. Yet data analysis excluding conditions that could be affected by such a criterion was conducted and similar results were obtained on this experiment. Nonetheless, we encourage researchers to include it on further research.

While subjects from Experiment 1 were told about the change to a new problem, this was not the case in Experiment 2 for *experience group*, following the method from Takahashi and Shimakura (1998). However, given that these participants' behavior was not different from *mixed group*, which had that information, this procedural difference seems unimportant.

5. Conclusion

In conclusion, the present work showed that (1) positive evidence of instructional control was found in a choice task with probability schedules and different expected values between options; (2) negative evidence of instructional control was found in a choice task with VI schedules and similar expected values between options; and (3) when taking these results together with previous research (Jessup et al., 2008; Lejarraga and Gonzalez, 2011; Takahashi and Shimakura, 1998; Viudez et al., 2017), relative expected values was demonstrated to be a fundamental factor on understanding the presence of instructional control in choice tasks. We conclude that the relevance of the present paper is based on its contribution to the definition of the boundaries of instructional control: our data suggest that the appearance of instructional control is influenced by their capacity to make participants discriminate better which is the optimal behavior.

Therefore, future lines of work may include studying a choice task where both reinforcers magnitudes and options' expected values are different to further test our hypothesis, as we would expect to find instructional control. Additionally, we have treated for the purpose of the present paper the relative magnitudes of the reinforcers as a binary factor –similar or different–, but further research should examine its contribution to instructional control in choice behavior by establishing a continuum of magnitude differences: it may well be that not all differences in the world are equal.

We would like to add a final remark related to the description-experience gap field when looked from the point of view of Analysis of Behavior. A meta-analysis conducted by our team (Viudez et al., 2021) revealed that the usual methodology on description-based tasks involves probabilistic or graphic descriptions of the options and asking participants to make a choice. On the other hand, experience-based tasks are set as two buttons for the participants to make repeated choices, no other information available. Therefore, research on the description-experience gap actually compares two different functions. One of them depends on the previous learning history of the subject –description-based tasks–, specifically with the learning history of mathematical/economical instructions following behavior. On the contrary, the other function depends on the history that is formed during the task –experience-based tasks–, specifically with the engagement of the subject on concurrent probability schedules. From our point of view, it is no surprising that there is a "gap" on the results, as they are different things. Finally, from this argument we could deduce an interesting future experiment: People with a broader history of following probabilistic instructions and choosing between them, such as economists or mathematicians, should demonstrate a higher control by instructions compared to the participants from studies on the description-experience gap (Jessup et al., 2008; Lejarraga and Gonzalez, 2011; Viudez et al., 2017).

Funding

This study was conducted at Psychology University Center for Biological and Agricultural Sciences, University of Guadalajara, and supported by the Portuguese Foundation for Science and Technology and the Portuguese Ministry of Education and Science through national funds and when applicable co-financed by FEDER under the PT2020 Partnership Agreement (UID/PSI/01662/2013).

Appendix

See Appendix Figs. A1–A4.
See Appendix TablesA1–A4.

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