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Published in:
Proceedings of the ACM on Human-Computer Interaction

DOI (link to publication from Publisher):
10.1145/3474662

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Publication date:
2021

Document Version
Publisher's PDF, also known as Version of record

Link to publication from Aalborg University

Citation for published version (APA):
Hougaard, B. I., Rossau, I. G., Czapla, J. J., Miko, M. A., Bugge Skammelsen , R., Knoche, H., & Jochumsen, M. (2021). Who willed it? Decreasing Frustration by Manipulating Perceived Control through Fabricated Input for Stroke Rehabilitation BCI Games. *Proceedings of the ACM on Human-Computer Interaction*, *5*, 1-19. https://doi.org/10.1145/3474662

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Who Willed It? Decreasing Frustration by Manipulating Perceived Control through Fabricated Input for Stroke Rehabilitation BCI Games

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Fig. 1. Fabricated input - an intervention for BCI - increases perceived control and reduces frustration, in the face of low input recognition.

To reduce frustration while performing no-risk tasks (e.g. in training and games) for BCI users, we propose increasing their perceived level of control through fabricated input - system-generated positive task outcomes. Two surrogate BCI studies injected fabricated input creating additional positive task outcomes to a 50% baseline. Users' perceived control increased significantly compared to the 50% baseline. In turn, frustration levels decreased. Fabricated input worked equally well in a game story context that provided an emotional stake in the protagonist's success and a simpler task lacking such incentives. People's number of input attempts during the tasks determined perceived control more than our controlled ratios of positive to negative task outcomes. Delays between users' input attempts and subsequent fabricated inputs further moderated their perceived control.

CCS Concepts: • **Human-centered computing** → **Empirical studies in HCI**; *Interaction paradigms*; *Interaction devices*; *Graphical user interfaces*; **Human computer interaction (HCI)**; *User studies.*

Additional Key Words and Phrases: Brain-Computer Interface; Frustration; Agency; Perceived Control; Illusion of Control;

ACM Reference Format:
Bastian Ilsø Hougaard, Ingeborg Goll Rossau, Jędrzej Jacek Czapla, Mózes Adorján Mikó, Rasmus Bugge Skammelsen, Hendrik Knoche, and Mads Jochumsen. 2021. Who Willed It? Decreasing Frustration by Manipulating Perceived Control through Fabricated Input for Stroke Rehabilitation BCI Games. *Proc. ACM Hum.-Comput. Interact.* 5, CHI PLAY, Article 235 (September 2021), 19 pages. https://doi.org/10.1145/3474662

1 INTRODUCTION

Brain-computer interface systems (BCIs) can be used to control devices using brain activity. In lab settings, BCIs can achieve fairly high input recognition rates, but require time-consuming setups

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2573-0142/2021/9-ART235. https://doi.org/10.1145/3474662

and experts for calibration. Despite 40 years of research, consumer-grade BCIs have only recently emerged, and face low adoption rates in real-world contexts such as home training, due to noise and low input recognition. Input recognition can be improved by using dependent BCIs, which require users to attend to an external stimulus such as a flickering light. Dependent BCIs allow for controlling assistive devices [39]. However, stimulus independent BCIs relying on movement-related brain activity (movement-related cortical potentials or event-related desynchronization) are the only known way to repair neural pathways when no residual movement is available in stroke patients [17]. The low recall rate of this type of BCI activation makes users not feel in control of their actions [22] causing frustration that 1) generates noise, 2) lowers BCI recall further [8], and 3) can lead to non-compliance with training [25]. To avoid frustration, designers of e.g. rehabilitation applications could increase people's perception of control by adding a percentage of fabricated input - concealed preprogrammed outcomes at desirable times, when BCIs do not recognize input attempts. However, introducing fabricated input comes with the risk of discovering such fabrication either from the absence of an attempt (cause) or the discrepancy in delay between cause and effect - constant for genuine, variable for fabricated input - reducing perceived control [18]. While previous work has partially understood the effects of these factors in isolation, their effects on perceived control and frustration when occurring in conjunction with real input have not been studied. Studies investigating the effect between actual control and perceived control have produced conflicting results. This calls for simpler studies e.g. both in terms of degrees of freedom of input devices and evaluating them in concrete task contexts.

BCI studies can not determine the ground truth of users' actual level of control from the unreliable input signal (ongoing brain activity). A surrogate input (i.e. which aim to simulate BCI input) is needed to uncover the relationship of actual control level to perceived control and frustration. The surrogate input should serve as an input signal that mimics a BCI input, but with access to the ground truth and ideally make the user believe the control is based on BCI-decoded brain activity. In this work we have used blinking as a surrogate input instead of actual movement-related brain activity from an independent BCI. We conveyed to the users that the blinking was detected by a BCI based on the ongoing brain activity although it was detected with an eye tracker. This paper presents two surrogate BCI studies aimed at understanding how fabricated input can be utilized to increase perceived control to reduce frustration. They highlight the need for a more nuanced measuring approach of the frames in which people evaluate their perceived control and frustration.

The paper contributes evidence that people experience higher levels of control and reduced frustration from added fabricated input, despite its longer and variable delay.

2 BACKGROUND

BCIs record the signal from voluntarily produced brain activities and translate this into device commands - e.g. by identifying discrete events through electroencephalography (EEG). BCIs have primarily served as means for communication and control, when other tools can not be used [39]. BCIs may be divided into three major categories: passive, dependent, and independent BCIs. Passive BCIs do not control an external device but are used as a means to assess brain states, e.g. for emotion recognition [40]. Dependent BCIs are controlled through externally evoked brain activity, e.g. by looking at screen flicker and are often used for controlling assistive technology [39]. Independent BCIs are controlled through internally evoked brain activity, e.g. spontaneous or motor imagery brain activity [29] and do not require external stimuli. This type of BCI is also used for control of assistive technology (but with lower input recognition rates) and for neurorehabilitation after e.g. stroke [17, 39]. Recordings of motor imagery are subject to extraneous noise reducing input recognition and impeding adoption [23]. BCIs further challenge users by not providing inherent feedback [38] in relation to their input attempts (e.g. compared to the sensory feedback from pressing a button [31]). Hence, users rely solely on the functional feedback, e.g. visual feedback, from BCIs' recognized input attempts.

This paper focuses on the context of games in stroke rehabilitation for motor recovery using independent BCIs. In this context, the BCI relies on movement-related brain activity with lower input recognition either from movement-related cortical potentials or event-related desynchronization. The paper does focus not on stimulus dependent BCI input signals such as steady-state visually evoked potentials or P300.

In both dependent and independent BCI systems the literature has assumed a required level of 70% input recognition for reliable use [4, 19, 34], which can be achieved in laboratory settings with expert fitting and calibration. But an estimated 15-30% of BCI users cannot achieve such high recognition rates for independent BCI systems [4] despite controlled setups, while patients in home training contexts including self-fitting will face much lower recognition rates. BCI users perceive poor input recognition as system delays, erroneous behavior, and loss of control - all factors known to evoke frustration [7]. This emotion can last minutes and affect users' longer-term mood. Unfortunately for BCIs, frustration introduces more noise in the brain wave signals, reducing input recognition [16, 28].

The BCI literature has referred to input recognition as the level of control, i.e. the probability that a BCI input attempt results in a matching outcome. For example, activating a reliable control, e.g. a button, usually yields an outcome within 100 milliseconds [31]. But in the case of BCI, an input attempt could (after one second) yield an outcome only 75% of the time (true positives). The remaining 25% constitute false negative cases, and in some cases outcomes occur without input attempt (false positives). Studies have referred to this level of control as actual level of control [36, 41], input attempt recognition [26], or decoding accuracy [12].

We can differentiate between three different levels of control, dependent on the ratios of outcomes in three different frames: 1) Attempt outcomes, the direct outcome of an input attempt, e.g. whether the attempt triggers a response. 2) Task outcomes, the outcome of the task during which one or several input attempts were made. And 3) goal outcomes, the outcome of the overall goal, which could be e.g. winning/losing in a game context. Not all applications include all three frames or have an explicit differentiation between them. In some applications, the attempt- and task-frames will be the same, and some might consist of a series of tasks with no overall goal. BCIs usually include an 'input window', a specified time during which a task should be completed, making it easier to differentiate attempt and task outcomes.

Users become frustrated from their level of 'perceived control' [5, 13] - not from a reaction to the actual level of control (illustrated by the non-linear curves in Figure 2, left). When people seek successful outcomes or perceive situations to be skill-based, they become subject to an illusion of control [35]. When having a stake in the outcome, people overestimate how much control they have over it. Illusion of control occurs even in response to truly random events, such as coin tosses [10].

Having explained the relevance of frustration and perceived control to the design of BCIs, we turn our attention to the four studies our literature review identified which evaluated these variables in BCI contexts. These studies have yielded different relationships between the actual control or agency people had over outcomes, and the perceived control and frustration they experienced. All the studies were surrogate BCI studies - they simulated BCI input such that they could manipulate the level of control. Figure 2 (left) synthesizes normalized levels of frustration from two such surrogate BCI studies [36, 41]. Both studies aimed at finding actual control rates that minimized frustration and agreed on a necessary rate of 90%. But the shapes of the curves between actual control and frustration deviated. In van de Laar et al.'s online study [36], players steered a hamster with a keyboard, giving instant feedback by moving the hamster in four directions through a labyrinth. Actual level of control on both the attempt- and task-level was manipulated by making

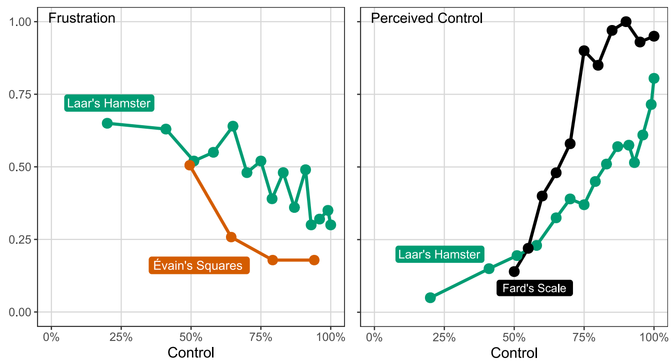

Fig. 2. Studies investigating effects of actual levels of control on frustration (left) [36, 41] and perceived control (right) [12, 36]. The measures are converted to decimal for comparison. Their data was partially reverse engineered from their figures. Van de Laar et al.'s mutual information (MUI) bits were converted from a logarithmic scale to 0 to 1. Note that the experiment structure and measuring variables differed, which limits the comparability of the results.

the hamster move in a different direction (or not at all) upon receiving keyboard input. The players' frustration started increasing once actual control dropped below 91%. Users in Évain et al.'s study [41] tried to select one out of three flickering squares through four-second eye fixations and received feedback when this window had ended. Participant input had no actual effect, as all outcomes were predetermined, allowing them to vary the level of control on the task-level. Their reported frustration at high levels of control were much lower and only started rising once actual control dropped below 83%.

What can account for such differences? The studies differed in a number of dimensions: 1) the number of controlled degrees of freedom (three vs. four control possibilities), 2) the session duration (two minutes [41] or until level completion [36]), 3) positive, negative, or neutral valence of outcomes, 4) keyboard input with immediate feedback [36] vs. the BCI equipment that lacked inherent feedback and provided delayed functional feedback [41], and 5) participants' expectations for the responsiveness of input devices. However, we believe the most salient difference was related to the frame within which the outcome was evaluated, i.e. the consequences participants encountered following an erroneous classification. While Évain's participants faced no further consequences (the wrong square was selected and the participant moved on to the next selection task, which provided no goal-frame), steering the hamster in the wrong direction could make completing the level (the goal) more difficult or time consuming. In Évain's study, the people could only evaluate their frustration on their (in-)ability to successfully select the correct square. In the absence of any stake, the frustration was therefore closely related to the cause-effect relationship. Unfortunately, the study did not measure perceived control.

Findings on perceived control, when comparing van de Laar's hamster game [36] and a study by Fard and Grosse-Wentrup [12], did not concur either. In the study by Fard, players moved a target to the right side of a scale to win, using a keyboard's arrow keys with a reduced recognition rate. Input attempts classified as unsuccessful stepped the target to the left, successful ones to the right. For both studies, the levels of perceived control started at similarly low rates of 50% actual control. However, for actual control above 50% the perceived control rose sharply and plateaued at around 80% in Fard's and rose more slowly but continuously until reaching 100% actual control in van de Laar's study (see Figure 2, right). Increasing actual control rates slightly above 50% made reaching the end of Fard's scale likely, and - while potentially slow - the progress must have been clear to players. Given the steep rise in perceived control and plateau for actual controls of 75% and higher, it seems probable that players were not rating their perceived control of the cause-effect relationship (their attempts or task outcomes) but the associated higher order outcome (the goal).

In summary, studies looking into perceived control and frustration produced conflicting results potentially due to methodological differences e.g. in framing the context for these measures. However, van de Laar's study provided evidence that increasing perceived control decreases frustration. As evidenced by three studies [12, 36, 41], both perceived control and frustration are affected by manipulating the level of actual control, which these surrogate BCI studies allow for but real BCI applications do not. So, how to make BCI tasks less frustrating in a training context, without requiring higher levels of actual control? While not providing design recommendations as to how, Bos et al. suggested reducing frustration from low recognition rates by deliberately manipulating people's perception of control [5]. But Haselager et al. argued that delegating parts of control to the system to allow for such manipulation could reduce people's sense of agency (their awareness of causing an effect) and perceived control [18]. Taken together, manipulating perceived control represents an unexplored design tension in BCIs, which we address through fabricated input.

3 FABRICATED INPUT

We propose to increase perceived control and reduce frustration by increasing the number of successful outcomes in a specific class of application - namely games, rehabilitation and training tasks, and other no-risk shared control [37] schemes. Barbero and Grosse-Wentrup showed that unwarranted positive feedback can even increase input recognition for people with low (around 50%) BCI input recognition [3]. BCI applications aware of favourable outcomes would, in the absence of recognized BCI events, fabricate suitable inputs communicated to the user by presenting functional feedback identical to feedback for genuine input. (see Figure 3, left).

While effects without cause (attempts) can greatly reduce perceived control, fabricated input can be concealed in BCI applications, because BCIs typically provide users with pre-defined *input windows* between 3-12 seconds [1, 27, 32]. During these, users are meant to and usually attempt input. Upon successful classification of an input attempt, the input window closes, and the BCI system presents feedback (task outcome). When it cannot classify an attempt, the BCI presents either no or negative feedback. A simplistic type of fabricated input mechanism could create input at the end of every input window in the absence of recognized input. However, fabricated input should be designed with the goal of making the game challenge fair, but not remove the challenge altogether.

We propose a fabricated input design as illustrated in Figure 3, left. The design addresses the concealment requirements by 1) being chance-based, e.g. letting fabricated input only occur in randomly selected input windows, and 2) occur randomly during the input window - potentially after some lead time, to give the user time to attempt input. Fabrication introduces variable delay (jitter) (Figure 3, case 1) and the risk of false positives (effects without input attempts, see Figure 3, case 2), the consequences of which have been studied both in network game contexts and causal learning studies.

Small scale latencies in the range of 50-300ms quasi-linearly degrade performance and general user experience in networked games [9]. The impact of longer variable delays (0-9 seconds) has been the focus of causal learning studies [6, 13, 15] in experimental psychology. People attribute causality between events based on them happening in succession (contingency) and their position in space-time (temporal contiguity [6]). Greville and Buehner studied manipulation of temporal contiguity in which participants chose whether and when to attempt input (free-operant paradigm)

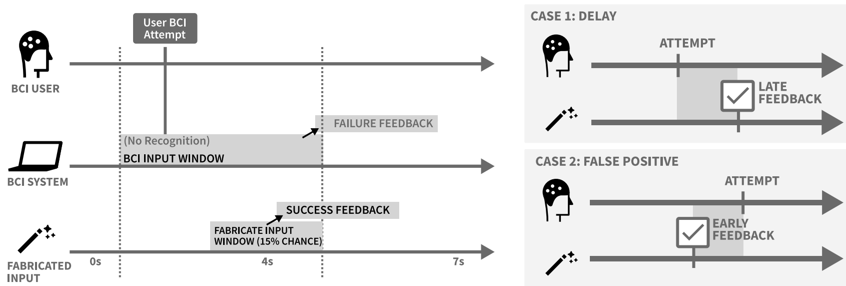

Fig. 3. Left: Timeline of how a BCI system fabricates input with 15% chance of activation. Right: Demonstration of variable delay and false positives, which occur with fabricated input. Poor input recognition prevents the system from knowing when attempts happen.

with 75% actual (attempt- and task-based) control, i.e. the probability *P(e|c)* that an effect *e* (a triangle flashing) will occur given a cause *c* (a button press), with no false positives [13]. Perceived control varied hugely with the response times of outcomes and decreased in a linear relationship with amount of delay and jitter in the range of 0-6 seconds [13]. However, their study used a different paradigm to the typical trial-based approaches used in BCI applications. Participants were allowed to press whenever, and any input had a 75% chance of yielding an outcome. Participants could e.g. press the button six times fast and get six instances of feedback in a row after a short delay, making it harder to connect causes to effects. Thus it is unclear to what degree this will translate into a typical binary BCI paradigm for rehabilitation, where only one input is accepted per trial (regardless of the amount of attempts), resulting in only one outcome.

Studies on background effects (random periodic preprogrammed triangle flashes) and yoked control (purely fabricated input replayed from other participants) indicated a large penalty, e.g. halving perceived control when some effects happened prior to causes [13]. However, perceived control was not nearing zero (23%), which the authors attributed to either sufficient number of participants' inputs coinciding with preprogrammed outcomes or participants' reluctance to rate at the extreme end of the Likert scales.

BCI designs employing fabricated input/outcomes should reduce the risk of outcomes occurring prior to causes/input attempts, and limit the variability of delays between them. But how adding fabricated input that incur a variable delay to an existing baseline of actual control will affect perceived control and frustration remains unclear. While we hypothesized that increasing the probability of positive task outcomes would increase perceived control and lower frustration, there was also a chance that the variable delay would result in the opposite effect if users became aware of the fabrication. We explored fabricated input in a simple binary input case, removed the confounds from effects without causes, and controlled the probability of successful task outcomes.

4 STUDY 1

This simulated BCI study explored the effects of injecting fabricated input in a game with a low level of actual control on players' perceived control and frustration. Unlike previous studies [12, 36, 41], this study kept both game duration and goal outcome (winning/losing) constant. We chose eye blinks as the input modality as people are less familiar with it than the keyboard. The modality made the low recognition rates more believable but allowed us to fully control task outcomes and understand input attempts. This ground truth access can only be obtained in surrogate BCI studies.

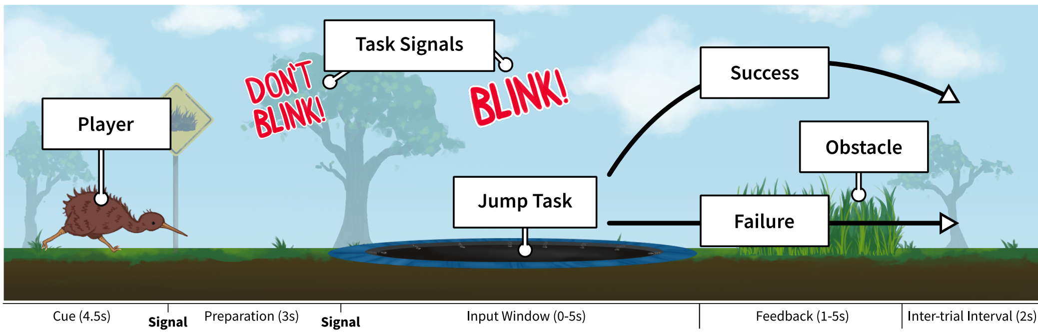

Fig. 4. Game screenshot. To succeed in a jump task, the player must blink while the kiwi crosses the trampoline.

4.1 Game Design

In the storyline of a custom-made game, participants were made to believe that non-recognized input attempts had a negative effect on the protagonist's (a kiwi bird) ability to save babies from an impending threat. Successful input attempts made the kiwi jump over obstacles, while unsuccessful input attempts slowed it down (see Figure 4).

Mimicking BCI task design conventions to minimize noise prior to input, the game made players aware of obstacles ahead of time via two signals. Three seconds before each obstacle, a "Don't Blink!" signal appeared (in the "Preparation" phase in Figure 4). A subsequent "Blink!" signal indicated the beginning of a five second input window. When the system processed a blink from the player during the input window, the kiwi immediately jumped and 'flew' across the obstacle (success). The jump took roughly five seconds when it happened at the beginning of the input window. When the system discarded the initial blink or received none, either A) the kiwi walked through the whole input window (for five seconds) and then slowly for four seconds through the obstacle (failure), or B) there was a chance that the system fabricated input at a random point throughout the input window and the kiwi jumped across the obstacle somewhere within two to five seconds after the *Blink!* signal. In the worst case, it took the kiwi five seconds to reach the end of the input window and three seconds for the subsequent obstacle jump. So, the delay of a fabricated jump lay anywhere between zero to four seconds from the last blink attempt prior and depended on the number of attempts a player made during the input window. The successful clearing of the obstacle consisted of the visual (see Figure 4) combined with a matching sound.

4.2 Experimental Design

The experiment was a within-subjects study, which measured self-reported perceived control and frustration for each of the three levels of fabricated input. The different levels were: A) 0%, B) 15% and C) 30%, which were added to a 50% baseline of successful task outcomes (see Table 2). We chose the 50% baseline based on and for comparison with earlier work [12, 36, 41] to induce low perceived control and frustration. The recognition rate was artificially lowered to 50% by discarding successful input attempts so that only 10 of 20 trials (tasks) resulted in successful outcomes. This was only for the sake of the experiment, as in a real BCI scenario we would not discard successful inputs. However, for this study we needed to be able to manipulate the level of control to get a stable 50% baseline recognition.

4.3 Participants, Apparatus and Procedure

The experiment relied on 20 university students (11 M, 9F) in the age range of 22-32 years (*mean* = 25) who had no prior experience with BCI. They were told that they would be playing three sessions of a video game that they could control by blinking but were not told any other details of the game or the experiment. The participants played the game on a laptop while wearing BCI equipment (MyndBand[1]). The instructions made them believe they could control the game through the BCI by blinking. While eye-blinking is typically considered a source of noise in BCI systems, this study deliberately used eye blinks as an input modality instead of the movement-related brain activity to gain access to the ground truth via an eye-tracker. The participants, however, were unaware that their blinks were picked up by an eye-tracker (Tobii EyeX[2]) mounted on the laptop, which provided a consistent level of control for blinks. The game registered blinks when the eye-tracker lost tracking of the pupils for more than 100ms. Having participants believe the input came through the MyndBand was aimed at making the low recognition rates more plausible. The players were instructed to adhere to common BCI constraints, including sitting still, and avoiding jaw movements and unnecessary blinking. Since inherent feedback from blinking (i.e. loss of vision and the feeling of closing their eyelids) might interfere with perceived control, participants were told that the system only accepted blinks of a certain kind. Its description included a requirement of duration (between 0.5 and 1.0 second), a one second interval between blink attempts, and purposefully vague terms such as 'determined' and 'specific' blinks.

Each game session consisted of 20 trials (each in-game obstacle constituted one trial), which lasted between 8.5 and 18.5 seconds with a two second inter-trial interval (see Figure 4). During 30 minutes, each participant played three sessions in counter-balanced order, for a total of 60 trials. Each session corresponded to one condition (see Table 1). The order of trial outcomes (success, failure, fabricated success) was randomized within each condition. After each condition, participants answered a questionnaire based on Évain et al. [41]. Participants used 7-step Likert scales to rate their frustration, based on the question '*How much frustration did you feel during this play-through?*' (absent (1), barely perceptible, faintly present, light, marked, pronounced, strongly pronounced (7)), and perceived control based on the prompt '*I felt I was in control of the kiwi while playing the game*' (strongly disagree (1), [...], strongly agree (7)). Ratings were given on a laptop, with a different page for each condition.

At the end, we prompted them for differences between the conditions. Their responses were coded into two categories: no idea and some idea, depending on whether they articulated any suspicion we were manipulating the feedback. After this, they received a debrief on the actual purpose of the experiment and feedback manipulation.

4.4 Results

Fabricated input decreased frustration - according to a repeated measures Friedman test (χ^2=13.4, p<0.001) with significant differences depending on fabrication rate. Post-hoc Wilcoxon signed rank tests showed significant differences between fabrication rates of 0% and 15%, (p=0.025, r=0.63) and 0% and 30% (p<0.001, r=0.72), with frustration scores for 0% being higher in both cases. After applying Bonferroni corrections, only the difference between 0% and 30% fabrication remained significant. See Table 1 and Figure 5 for the means of participant answers for the different fabrication rates. Participants were less frustrated when fabrication was added to the baseline 50% level of control.

[1] http://myndplay.com/
[2] http://tobii.com

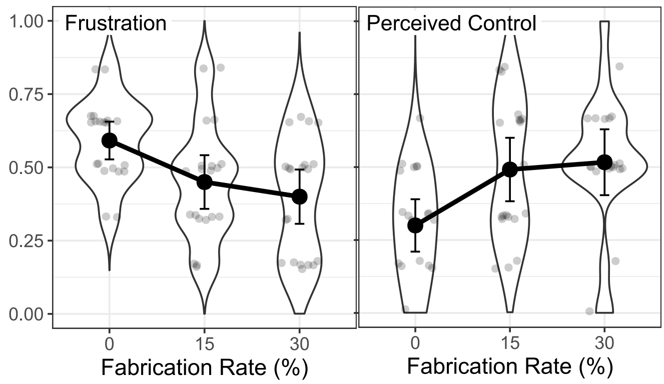

Fig. 5. Perceived control and frustration means (black) with 95% confidence intervals, raw data (grey) jittered for better visibility, and their distribution (violin plots) by fabrication rate added to the baseline of 50% successful task outcomes.

Similarly, perceived control was significantly different depending on which fabrication rate was used according to a Friedman test (χ^2=13.19, p<0.001). The participants felt more in control with added fabrication. Post-hoc exact Wilcoxon signed rank tests with Bonferroni corrections showed significant differences between the fabrication rate of 0% and both 15% (p=0.002, r=0.68) and 30% (p<0.001, r=0.69), with perceived control for 0% fabrication being lower. The fabrication rates of 15% and 30% did not differ significantly in terms of perceived control (see Figure 5, right).

Linear mixed models with random intercepts checked for carry-over effects from fabrication change, i.e. whether having experienced a higher or lower level of fabrication in the previous condition had an effect on the perceived control or frustration in the next condition. The ANOVAs found no significant differences between models that added fabrication change to models that included the fabrication rate. But models that included fabrication rate were significantly different (perceived control: χ^2=15.98, p<0.001, frustration: χ^2=15.42, p<0.001) from their null models, which supported the Friedman test results.

In the qualitative post-experiment probing into their experience, nine out of 20 participants articulated in one way or another that we somehow manipulated how responsive the system was to their input. But the majority of participants either thought the change was related to how they were supposed to blink or had no idea. None of the participants attempted to not blink during the input windows, but one participant - potentially distracted - missed one of the input windows and experienced one successful outcome fewer (9/20) than planned in the baseline (0%) condition. We re-ran all analyses without his data but found no differences and included his data in all reported results. One participant remarked about the fabricated input conditions: "*Sometimes you got another chance to make the kiwi jump*".

Table 1. The experimental conditions with added fabrication rates to a baseline of 50% successful task outcomes and the means of the participants' normalized ratings frustration and perceived control .

Control *P(e|c)*	Fabr. Rate	Frustr.	Perc. Control
10/20 (50%)	+0/20 (+ 0%)	0.65	0.40
10/20 (50%)	+3/20 (+15%)	0.53	0.56
10/20 (50%)	+6/20 (+30%)	0.47	0.59

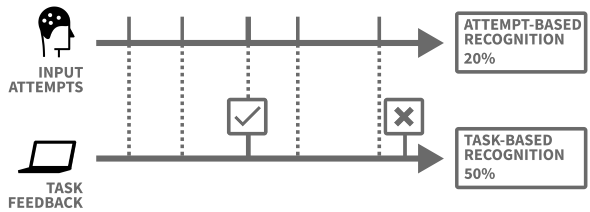

Fig. 6. Input-oriented recognition is a calculation of number of input attempts (5) to positive outcomes (1). (Top) Task-oriented recognition is a calculation of number of positive outcomes (1) to total outcomes (2). (Bottom)

4.5 Discussion

By fabricating favourable outcomes with variable delays from (discarded) input attempts, people felt more in control and less frustrated in the frame of avoiding obstacles and meeting the goal of the game. For low levels of added fabricated input (15%), this had a large effect on both variables but wore off for the higher rate (30%).

So, why did perceived control not decrease when adding fabricated input with similar variable delays as Greville's [13] study on the negative influence of feedback latency suggested? Greville's study kept the actual control rate of button press to light up triangle constant at 75%, while our fabrication increased the participants' control, resulting in more successful outcomes. Possibly subject to an illusion of control [35], our participants attributed the favourable outcomes to their own actions despite their longer delays. But our results provided evidence of a possible penalty stemming from increased variable delay by the reduction in perceived control for the higher (30%) fabrication rate. However, our data logs did not allow for exact measures between the time between blink attempts and subsequent feedback from fabricated input, which we addressed in Study 2.

Similarly to perceived control, low amounts of fabricated input had a strong effect in the desired direction of reducing frustration but higher rate did not yield further reductions. While experiencing fewer events that impede the goal of the game should decrease frustration, our results needed explanations for why this effect plateaued for the higher fabrication rate (30%). One reason that could also explain the plateauing of perceived control was insufficient statistical power and sampling noise in our data. As Figure 5 illustrates, the ratings of of frustration and perceived control varied substantially between participants. We computed intra-class correlation coefficients between the participants rating perceived control and frustration. We found poor agreement [21] even after removing mean differences between the participants (ICC3) for both frustration (kappa=0.31, $p<0.001$) and perceived control (kappa=0.36, $p<0.001$). People had difficulty judging their experiences reliably, potentially due to them not being able to consult previous ratings when submitting their current ratings. As mentioned before, we could not check whether differences in feedback delay from fabricated inputs accounted for some of the rating variance.

While this study showed that fabricated input in a BCI game could increase perceived control and decrease frustration, we could not answer how the narrative (goal) frame affected the relationship. To what degree were ratings based on participants reacting to attempt outcomes - their ability to making the kiwi jump - or to the task outcomes - them making the kiwi clear the obstacle for which they had multiple attempts (see Figure 6)? Would fabricated input have a different, or even opposite, effect in a non-game context without a narrative (goal outcome) frame? We addressed these concerns, along with methodological changes to increase inter-rater reliability, and added a higher fabrication ratio and a control of 100% control in a subsequent second study to better understand why gains of fabricated input plateaued for 30% fabrication rate.

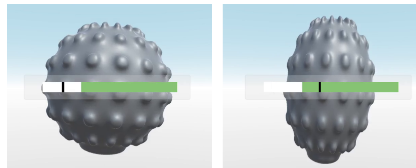

Fig. 7. (left) The colored ball shown to participants; (right) The ball "squeeze" shown to participants on successful blinking.

5 STUDY 2

While the framing of this experiment was different, it was otherwise structurally identical to the first study, with the same setup and the same measurements being taken.

5.1 Game Design

For this experiment, we used a simpler application with no narrative (no goal outcome), where the only goal was to make a ball shrink (task outcome) (see Figure 7). When participants made a correct input within the input window, the ball squeezed (as if their hand was squeezing it) for 0.8 seconds, and a pleasant harp sound played for 1.5 seconds. If they failed to do this, the ball would turn red, and a loud buzzer sound would play. A progress bar shown above the ball informed participants of when the input window was open. When the black progress indicator was in the white part of the bar, which was for two seconds per trial, they were to wait. When it was in the green part, they had a five second input window to perform the correct input. If they succeeded, the bar stopped and played the successful feedback. To ensure equal duration for the attempts and hence the conditions, the bar would would not go back to the beginning until five seconds had passed since the opening of the input window. Just as in the previous study, there was a chance that the system would fabricate input at a variable random time within two to five seconds of the input window. The blinking algorithm was changed to activate when people opened their eyes, rather than upon closing their eyes, to ensure the visual feedback was visible while allowing for measuring blink duration.

5.2 Experimental Design

Similarly to the first one, Study 2 followed a within-subjects design, measuring frustration and perceived control in a surrogate BCI context. The experiment included five conditions. Four conditions had an actual level of (task-based) control of 50% with an added A) 0%, B) 15% , C) 30% and D) 50% fabricated feedback. The last condition had a 100% level of actual control, to allow participants to compare to what they would expect from a 'normal' input method (e.g. a button press), and to verify that participants made full use of the Likert scales when rating their frustration and perceived control.

5.3 Participants, Apparatus and Procedure

The study involved 26 participants (18M, 8F), all employees at a university. Participants were told we were testing out different 'algorithms' for blink detection with the MyndBand but nothing about the fabricated input. We used the same surrogate BCI hardware setup as in the previous experiment (i.e. the MyndBand as the 'fake' input device and the Tobii EyeX as the real input device).

Participants played through five sessions corresponding to five conditions throughout 20-30 minutes, in a counter-balanced order. Each session consisted of 20 trials for a total of 100 trials per participant. Each trial lasted for seven seconds (a two second cue phase, a zero to five second input window and a zero to five second inter-trial interval). If inputs were received early, the system extended the inter-trial intervals to achieve consistent trial lengths. The instructions about input included that blinks should be of a certain length (between a half and a full second), with a one second interval between attempts if the first attempt failed. To make it easier for participants to distinguish between the different conditions, the color of the ball varied between conditions. The color of the ball for the different conditions was randomized between participants. After completing each condition, participants were asked to rate their perceived control and frustration using the Likert scales from Study 1. All ratings were given on one A4 sheet of paper, allowing participants to compare ratings between conditions. After all conditions, they pointed out, which conditions they thought were "best" and "worst," and what they thought differed between conditions.

5.4 Data Preparation

We logged every blink attempt in order to verify that the level of control we exposed people to was the actual level of control they experienced. Participant blink data and task event data (feedback activation, input window state) were collected with timestamps down to 10 milliseconds of accuracy. Data post-processing calculated the timestamps of the eye opening based on when the eye tracker lost pupil tracking and the duration for which pupils were visible. Blink events happening at the same time as feedback received a delay of zero. In 11 of the 494 fabricated input events, participants experienced false positives, i.e. they received feedback from fabricated input 40-570ms (M=290ms) before they had blinked.

We checked that the actual control and fabrication rates the participants experienced met our factorial design in Table 2 through the data logs. In some cases participants were unconsciously being outside the eye trackers range, often resulting in in 0% blink recognition. Twelve of 130 recorded sessions were discarded this way because they did not meet the assumed recognition rate and fabrication rate due to poor recognition, affecting six participants. Regression analysis verified that neither the color of the ball nor the condition order had an effect on the dependent variables. Subsequent analysis of the dependent variables did not include these factors (color and order) as controls.

5.5 Results

A repeated measures Friedman test confirmed that fabricated input had a significant effect on user frustration (χ^2=32.9, p<0.001) and perceived control (χ^2=40.4, p<0.001). Figure 8 (left, middle) shows how added amounts of fabrication rates increased perceived control and decreased frustration (see Table 2 for numerical values and blink attempt recognition rates). Post-hoc Wilcoxon signed rank tests showed significant differences between most fabrication rates, except 30% against 50% in terms of frustration (p=0.215) and 15% against 30% in terms of perceived control (p=0.312). There was a strong negative correlation (r=-0.76) between perceived control and frustration (see Figure 8, right). Given the low rater reliability in Study 1 we checked inter-rater reliability, which were moderate to high [21] for both frustration (ICC3: 0.72) and perceived control (ICC3: 0.78), showing that the ratings of participants were much more consistent than in Study 1.

We wanted to see whether the scores of frustration and perceived control were affected by the different framing in Study 1 and 2. A Welch two sample t-test which compared the ratings from conditions the two studies had in common (0%, 15% and 30% fabricated input) found no significant difference between the studies. The ratings did not seem to factor in that they were related either to making a kiwi pass obstacles or a ball squeeze.

In line with our expectations, all participants rated the condition with 100% recognition as being the ‘best’, citing that it responded “*immediately*” or “*quickly*” and “*worked every time*”. Almost all rated the condition with 50% recognition and no fabricated input as the ‘worst’, with three

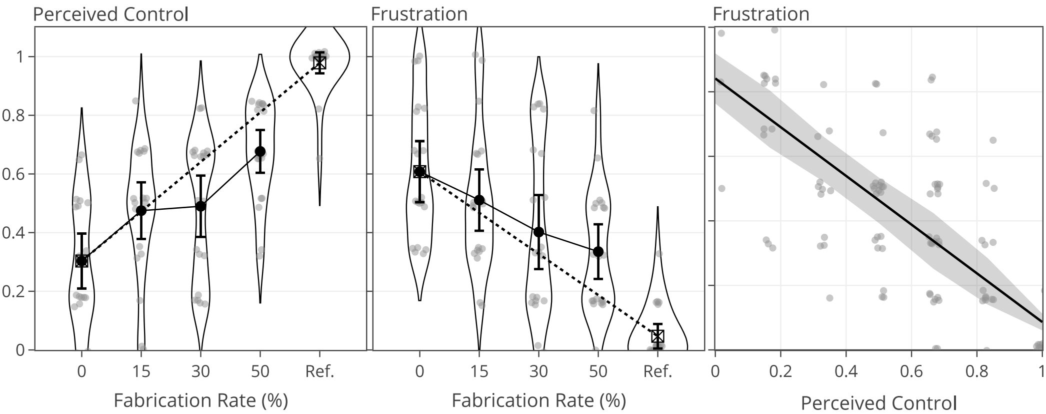

Fig. 8. Participant ratings of perceived control (left) and frustration (middle) at different fabrication rates on top of 50% control and the reference condition with 100% control. Error bars and grey band denote 95% confidence intervals. The dotted line shows an assumed relationship between 50% and 100% level of control with no input fabrication. The right figure shows the linear relationship between perceived control and frustration.

Table 2. Normalized mean control and frustration in five conditions with different fabrication rates and a 50% baseline.

Control *P(e|c)*	Fabr. Rate	Frustration	Perc. Control	Accepted Blinks	Blink Recognition
10/20 (50%)	+ 0/20 (+ 0%)	0.64	0.30	255 /1054	21%
10/20 (50%)	+ 3/20 (+15%)	0.52	0.49	264 / 839	32%
10/20 (50%)	+ 6/20 (+30%)	0.41	0.53	375 / 866	43%
10/20 (50%)	+10/20 (+50%)	0.33	0.70	437 / 665	66%
20/20 (100%)	+ 0/20 (+ 0%)	0.01	0.97	437 / 437	100%

participants saying that it seemed to be "*random*" when it worked. One participant said about this condition that there was "*no correlation between blink and squeeze*" (despite all squeezes being a direct consequence of their blinking). While participants noted occasional differences in the temporal relationship between input and feedback from fabricated input, they still believed to have some control over the ball squeezing. For example, participants believed that for some attempts you had to blink several times or in different ways in order for it to work, i.e. that they just had to change something in their approach in order for it to work: "*What you did for one of [the colors] didn't work for the others*". Others thought the feedback was simply "*delayed*". Their belief that they were still in control was expressed in what they perceived to be the difference between conditions. Several participants thought it was about the "*speed*" of the feedback, or "*how quickly it responds*". Three participants mentioned experiencing the ball squeezing without a blink attempt.

Our study set out to answer how fabricated input (incurring variable feedback delays) mixed with real control (with immediate feedback) affected perceived control. While our study was not designed to determine the exact effect of feedback delays on perceived control, our data allowed for a first glimpse into this relationship. We measured how much delay occurred between each input attempt and matching positive task outcomes (fabricated or real feedback). In case of multiple attempts occurring before a fabricated feedback, we matched the feedback with the closest preceding attempt.

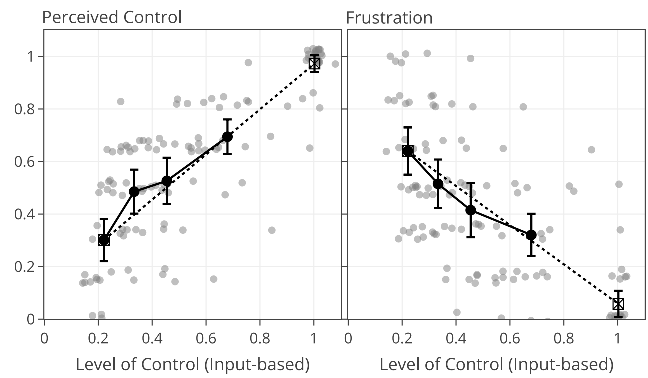

Fig. 9. Participant ratings of perceived control (left) and frustration (right) with respect to individual eye-blink input recognition, based on different fabrication rates and the reference condition with 100% control. The dotted line shows the assumed relationship between the measured 50% and 100% level of real control with no input fabrication. Error bars represent a 95% confidence interval.

Attempts occurring prior to this selected attempt did not receive a delay and did not contribute to the measurement of feedback delays. Participants received real feedback typically within 20 milliseconds of their blinks, while for fabricated inputs delays ranged between 0-2 seconds. We calculated input recognition rates (attempt-based control) based on how many blink attempts were followed by feedback and divided this by the total number of blink attempts for each condition per person (see Table 2 for the totals per condition). We can think of the measures as the effectiveness and feedback delay as the responsiveness of the input device. We tried to model perceived control ratings through multiple linear regressions. We first evaluated, which of the control-paradigms (attempt or task) was a better predictor of perceived control. Attempt-based control (Figure 9) was a significant predictor ($p<0.001$, $\beta=79.2$, intercept=16.8) that explained 60% of the variance in perceived control ratings. Task-based control (Figure 8), while a significant predictor, only explained 48% of the variance ($p<0.001$, $\beta=104.32$). When adding feedback delay to the attempt-based control model, both attempt-based control ($p<0.001$, $\beta=0.87$) and feedback delay ($p=0.015$, $\beta=0.59$) were significant predictors of perceived control. Moreover, the analysis found an interaction effect ($p=0.001$, $\beta=-130.65$). Adding feedback delay and the interaction to the model explained an additional 3% of the variance in perceived control ratings ($R^2=0.63$) - a minor improvement. We interpret the interaction effect in the following way: for high levels of (attempt-based) control, feedback delays reduced perceived control more than for low (attempt-based) control.

6 DISCUSSION

We manipulated people's perceived control through fabricated input even when they faced the same baseline of 50% actual control. Input fabrication improved both perceived control and reduced frustration along the tested range of fabrication rates. The comparison between Study 1 and 2 showed that games and rehabilitation tasks do not have to employ narrative frames to cover up fabricated input - in our cases fabricated input manipulated perceived control and frustration with and without narrative player incentives. Participants had much higher level of agreement in their ratings which, given the high inter-rater reliability, provides evidence for the construct validity of our measures. We attributed the improvements in reliability over Study 1 to the methodological changes, i.e. allowing for consulting previous ratings. For both perceived control and frustration, the shape of the data, when inspected from a task outcome perspective, suggested a penalty from fabricated input (see Figure 8, left and middle). The post-hoc regression analysis showed that perceived control in Study 2 could be better explained by the attempt-based level of control (c.f. Figure 9) and to some degree the feedback delay.

Our results underscore an important trade-off decided by the size of the BCI input window. Shorter windows will make it harder for BCI users to attempt inputs and are limited by the BCI classification software settings but will reduce input attempts and the delays incurred from fabricated input and will increase perceived control. Our findings were based on a five second input window, resulting in between zero and seven blink attempts (2.1 on average) and delays between zero and a maximum of 3.2 seconds (average 0.83 seconds). Bigger input windows might change perceived control/frustration relationship. Our model of perceived control does not suggest a penalty of delay similar to that found by Greville and Buehner [14], but instead a more complex interaction that requires more research.

Surrogate studies decide outcomes beforehand, but BCI systems will not know whether the user's next trial will fail or succeed. This unknown means that BCI systems would inject fabricated input into a percentage of all trials - even trials in which the BCI would otherwise have recognized the input attempt. In real BCI systems, other forms of fabrication may also be suitable - e.g. if trials without input is a desired outcome, then it might be sensible for BCI systems to fabricate suppression of input.

For BCI designers, this poses a challenge because the lack of ground truth access impedes tracking action attempts and they have to rely on task outcomes as a measurement of input recognition. Surrogate studies provide researchers an opportunity to study the ground truth, but their transferability relies on the way which they simulate real BCI. This study did not interpret motor imagery signals, but only used blinking as a control input with similar temporal characteristics (length of cue and activation phases etc.) as a BCI operated using motor imagery. One of the main factors that we expect could affect the transfer of results between the surrogate BCI in this study and results obtained from a real BCI is that it is difficult to know exactly when the user intends to activate the BCI and therefore the system cannot guarantee to provide the fabricated input immediately after the attempt. It is unknown how this would affect the ratings of the questionnaires in a real BCI. However, design solutions of the interaction and instructions given to the user (e.g. keep trying to activate the BCI with motor imagery in the entire input window) could reduce the impact of this potential problem. The surrogate studies covered in this paper do not investigate their transferability [12, 36, 41], but we expect that the results in the current study will not differ vastly from those obtained using a real BCI based on motor imagery. However, this needs to be validated in future studies.

This study was designed as a stepping stone to creating a less frustrating experience when training with BCI for e.g. stroke rehabilitation. Patients who undergo rehabilitation are typically elderly and may suffer from cognitive impairments and have problems maintaining concentration throughout a BCI training session. Their perception of control and frustration might differ from the healthy participants in our study in different ways. The young healthy participants in our study may have higher expectations for the interaction compared to an elderly stroke patient who may not be as familiar with games and human-computer interaction. Stroke patients may also have a lower state of perceived control already compared to the healthy participants. Lastly, the motivation of healthy participants to participate in such studies as the current one is likely to be different from the scenario where a stroke patient is training to regain motor functionality and perform activities of daily living that have been lost due to the injury. Our study with healthy adults was a first exploration of fabricated input's effect on perceived control and frustration and allows for comparisons with previous studies that used similar participants.

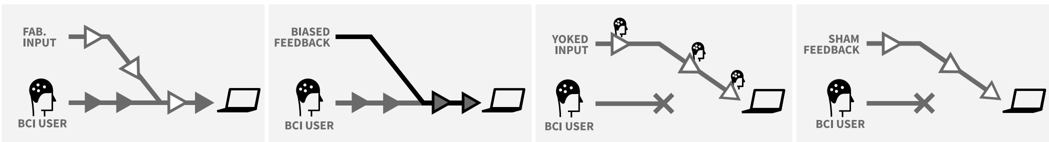

Fig. 10. Fabricated input is system input mixed into real input (far left), Biased feedback alters the BCI classification (middle left), Yoked input is input replayed from previous trials (middle right) and Sham feedback is system input with no input from the user.

6.1 Future Work

There is ample opportunity in the design of BCI systems for the CHI community to embrace. We have investigated fabricated input in two surrogate studies, which allow us to access the ground truth and measure input attempts in a way not possible with BCI. While results from surrogate studies will not apply unaltered in real BCI settings, we need to first understand the mechanisms and relationships between the salient variables to better interpret results obtained in BCI settings lacking ground truth access. Future studies need to evaluate whether the effect of fabricated input holds for even higher amounts, e.g. when fabricated feedback outnumbers feedback from genuine attempts. It may be worth exploring how to best conceal fabricated input in long-term interactions in which users have a higher chance of discovering fabricated input. Self-reported frustration and perceived control ratings could benefit from triangulation using physiological measurements, such as pupil dilation, electrodermal activity or heart rate [20].

The influence on perceived control from the amounts of variable feedback delay from fabricated input and its interplay with real feedback delays, which could be randomly delayed for better concealment, should be explored systematically - our study provided only a first glimpse into this topic. In BCI-based motor imagery training contexts, proprioceptive feedback and brain intentions need to be closely coupled in time to harness neuroplasticity to reestablish neural pathways [33]. For such contexts, understanding the effect of the mismatch between delays for a neuroprosthesis and feedback delay in the game world on perceived control would be important. Given the positive effect of fabricated input, this could open up BCI applications to a wider range of mechanisms for providing help to users struggling with poor performance. Video games have drawn on this approach under the moniker of 'skill assistance' [11] or 'difficulty adjustment' [2]) to accommodate for poor performance on the player's part rather than the system. A popular example of this are power-ups in Mario Kart[3], which offer short-term benefits to players doing poorly. Furthermore, designers can cover up poor BCI performance by combining fabricated input with more reliable controls (c.f. [30]).

6.2 Related Work and Concepts

The concept of fabricated input shares similarities with sham feedback in neurophysiology [24, 33] (for experimental control conditions), background effects or yoked input (preprogrammed outcomes with no cause) in experimental psychology [13, 15], and biased feedback [3] in neuroengineering, but differs in purpose and scope (see Figure 10). Neurophysiological studies employ sham feedback (e.g. in a situation in which the benefit of timed tactile feedback for motor learning gets contrasted with a control condition which includes feedback at random times) to achieve balanced neurofeedback between conditions. Unlike fabricated input, sham feedback typically does not occur together with genuine feedback to the input signal and would hence be misleading to use for our case. In yoked

[3]Nintendo. 1992. Super Mario Kart.

input, participants receive feedback events based on the timing of inputs from previous participants and not their own input attempts. This can result in yoked feedback bearing little similarity to a person's input attempts and thereby provides poor experimental control as to what participants experience. Background effects produce feedback at either fixed or random time intervals at constant rate, which can lead to effects without cause. The goal behind background effects is to dilute users' ability to discern their own input, and is hence inappropriate to use to describe our manipulation. Biased feedback alters the interpretation of the brain signal to weigh more towards giving users positive (or negative) feedback. Barbero and Grosse-Wentrup defined biased feedback only in the context of continuous input. In contrast to these, fabricated input is a system-created binary input which is mixed into an existing signal of binary BCI input. Fabricated input can be added in more or less amounts measured in percentage and has a variable random delay, to the user's perceived cause.

7 CONCLUSION

Given low risks or control for users not attempting input at all, BCI designers can employ fabricated input to boost perceived control and reduce frustration in non-risk, or training contexts. In our lab-based surrogate studies, we evaluated mixes of up to the same number of fabricated input (50%) to real inputs (50%). Fabricated input incurred variable delays (0-2 seconds) and close to no delay for genuine feedback in the input window. When deciding on the length of input windows, BCI designers need to trade-off the difficulty of producing genuine input during a short amount of time (the longer the window the easier) against the cost of false positives, perceived number of attempts, and increased delay in fabricated input (the shorter the window the better for perceived control). Fabricated inputs require only changes to application code and can be applied to a wide range of contexts such as games outside BCI in which people struggle with issuing correct input, for example in voice- or gesture-based interfaces.

8 ACKNOWLEDGEMENTS

Thanks to the anonymous reviewers for their very thoughtful and constructive feedback that helped improving the manuscript. This project was partially funded by VELUX FONDEN (project number: 22357).

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Received February 2021 ; revised June 2021 ; accepted July 2021