Enhancing perceptions of control in depressed and non-depressed volunteers using a mobile phone intervention


1Centre for Social Issues Research, Department of Psychology, University of Limerick, Republic of Ireland.

2Health Research Institute, University of Limerick, Republic of Ireland.

3Department of Electronics and Computer Engineering, University of Limerick, Republic of Ireland.

4Computational Psychopathology Research Group, University of Oxford, United Kingdom.

Author Note

*Corresponding author: Rachel M. Msetfi, Department of Psychology, University of Limerick, Castletroy, Limerick, Republic of Ireland (tel: 00-353-61-202709; e-mail: rachel.msetfi@ul.ie).
Abstract

Background: Perceived control is strongly linked to healthy outcomes, mental healthiness and psychological wellbeing. This is particularly important when people have little control over things that are happening to them. Perceived control studies have been performed extensively in laboratory settings and show that perceived control can be enhanced by experimental manipulations. Although these studies suggest that it may be possible to improve people’s mental health by increasing their perceived control, to-date there is very little evidence to suggest that perceived control can also be influenced in the real world.

Objectives: The first aim of this study was to test for evidence of a link between non-control situations and psychological wellbeing in the real world using a mobile phone application. The second and arguably more important aim of the study was to test whether a simple instructional intervention on the nature of alternative causes would enhance people’s perceptions of their own control in these non-control situations.

Methods: We implemented a behavioral action-outcome contingency judgment task using a mobile phone application. An opportunity sample of 106 healthy volunteers scoring low ($N = 56$, no depression) or high ($N = 50$, mild depression) on a depression scale participated. They were given no control over the occurrence of a low or high frequency stimulus that was embedded in everyday phone interactions during a typical day lasting eight-hours. The intervention involved instructions that either described a consistent alternative cause against which to assess their own control, or dynamic alternative causes of the outcome. Throughout the day, participants rated their own control over the stimulus using a quantitative judgment scale.
Results: Participants with no evidence of depression overestimated their control, whereas those who were most depressed were also more accurate in their control ratings. Instructions given to all participants about the nature of alternative causes significantly affected the pattern of perceived control ratings. Instructions describing discrete alternative causes enhanced perceived control for all participants; whereas dynamic alternative causes were linked to less perceived control.

Conclusions: Perceptions of external causes are important to perceived control and can be used to enhance people’s perceptions. Theoretically motivated interventions can be used to enhance perceived control using mobile phone applications. This is the first study to do so in a real world setting.

Key words: Perception of control; Illusory control; Wellbeing; Depression; Health; Intervention; Causal learning;
Introduction

Perceived control is critical to health outcomes, mental healthiness and psychological wellbeing. For example, numerous studies have measured perceived personal control using psychometric questionnaire measures, and evidenced direct relationships to health outcomes (examples include cancer [1], diabetes [2], and heart disease [3], treatment adherence and effectiveness [4], with control mediating of the negative consequences of adverse conditions [5]. A key idea is that when healthy people have no control over events they tend towards an ‘illusory’ perception of control [e.g., 6]. This is thought of as a protective bias that supports people’s sense of control and therefore wellbeing when they cannot control things that are happening to them [e.g., 7]. Conversely, people with depression are held to recognize situations in which they have no control all too well [8]. This 'depressive realism' phenomenon may represent the absence of a healthy protective mechanism, with illusory control being an ingredient for positive physical and mental health [9, 10]. Given the importance of perceived control for health, the aim of the current study was to assess for evidence of this phenomenon outside of the laboratory using a mobile phone application and to test whether a simple theoretically motivated intervention could enhance people’s perceptions of control in a healthy manner.

Previous work

Despite its importance, perceived control research suffers from a lack of studies carried out in real world or applied settings. So far, laboratory-based research has been the only method of showing whether a person perceives that they have control when there is none. This is because the actual control a person has over a situation
needs to be known and adjustable by the experimenter, and an accurate, objective measure of people's experiences is required [11], which is almost never the case in the real world. Some methodologies used in this domain, for example comparisons between self and observer ratings of a situation [12] or between personal and population risk [of a cancer diagnosis for example, 13] have provided useful insight but cannot allow a definitive diagnosis of illusory perceived control.

An objective measure of available control is clearly present in laboratory tasks involving ‘contingency judgments’ as participants are exposed to carefully measured contingencies between their actions and outcomes.

**a) Generic contingency table**

<table>
<thead>
<tr>
<th>Action</th>
<th>Outcome</th>
<th>No Outcome</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>a</td>
<td>b</td>
</tr>
<tr>
<td>No action</td>
<td>c</td>
<td>d</td>
</tr>
</tbody>
</table>

Contingency $= \Delta P = \frac{a}{a+b} - \frac{c}{c+d}$

*NB: a, b, c, d refer to event frequencies*

**b) No control conditions**

<table>
<thead>
<tr>
<th>Action</th>
<th>Outcome</th>
<th>No Outcome</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>5</td>
<td>15</td>
</tr>
<tr>
<td>No action</td>
<td>5</td>
<td>15</td>
</tr>
</tbody>
</table>

$\Delta P = \frac{5}{5+15} - \frac{5}{5+15} = 0$

$P(O) = .25$

<table>
<thead>
<tr>
<th>Action</th>
<th>Outcome</th>
<th>No Outcome</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>15</td>
<td>5</td>
</tr>
<tr>
<td>No action</td>
<td>15</td>
<td>5</td>
</tr>
</tbody>
</table>

$\Delta P = \frac{15}{15+5} - \frac{15}{15+5} = 0$

$P(O) = .75$

*Figure 1*

Generic (a) and specific (b) contingency relationships between the occurrence of an action and the occurrence of an outcome.
With reference to Figure 1, the contingencies between a person’s actions and subsequent outcomes are defined using four event-outcome frequencies:

- a: a user action results in an outcome
- b: a user action does not result in an outcome
- c: a user in-action results in an outcome
- d: a user in-action does not result in an outcome

These are programmed to occur over a short period of time and are quantified using the normative \( \Delta P \) metric [14]. \( \Delta P \) is the difference between \( P(O|A) \), the probability of a user action (A) resulting in an outcome (O) and \( P(O|\sim A) \), the probability of the same outcome when the user does not perform the action. Positive and negative \( \Delta P \) values indicate the user has a certain control over the outcome, though in the case of a negative \( \Delta P \) the user inaction will be more likely to result in the outcome than user action. A \( \Delta P \) value equal to zero indicates the user cannot control the outcome through the action.

In both of the specific examples given in Figure 1b, the person has no control over salient outcomes (\( \Delta P = 0 \)) but the frequency or density of outcomes varies from low to high (low OD, high OD). Therefore, if accurate, people’s perceptions of control should not differ between these two conditions. However, numerous studies have shown that healthy people exposed to a high outcome density condition tend to over-estimate their control relative to the low outcome density condition [e.g., 15] and relative to people who are depressed [e.g., 8, 16]. These findings have provided evidence for the link between illusory control and healthy states.

However, the requirement for careful experimental control means that the basic experimental findings have never been tested outside the laboratory. This raises key
methodological concerns around external and ecological validity, of generalizability from one very specific control situation to the whole of life [e.g., 17] and of the difference between behavior instructed in the laboratory and that occurring naturally in the real-world [e.g., 18]. Such basic methodological critiques of perceived control research are well acknowledged [e.g., 19] and have limited the potential of this area of research to result in interventions for applied settings although laboratory based interventions have begun to be tested [20].

Where laboratory research has been helpful, is in shedding light on our understanding of the psychological processes underpinning perceived control and, theoretically, the factors that will enhance perceptions if used to formulate interventions. So, for example, we know that the perception of alternative causes of outcomes is a key moderator of perceived control. Whether a rule based normative model [14] or a process based associative model [21] is preferred, one’s own control is evaluated against the control exerted by alternative causes. Other potential controlling causes are numerous, both inside and outside the laboratory, and include the environment or context in which events occur. For example, if a person wanted to control the heat level in a room using heating controls (action), an important alternative cause of heat variation would be the room itself and the effectiveness of the central heating system therein. In other words, the context is a key conditionalizer of control experience [e.g., 22] and has been indicated as a key factor that discriminates healthy and depressed people in their control perceptions [16]. These findings lend themselves to interventions that will influence people’s perceptions of alternative causes and enhance their feelings of control [20].
**Goals of this study**

Given the ubiquity of mobile phones in everyday life, and their potential as data gathering and intervention devices, the goals of this study were to investigate perceived control in the participant’s normal everyday environment and to test potential interventions that may improve a participant’s perceived control, whether or not they are depressed.

To this end, we implemented a contingency judgment control task designed to run on Android mobile phones. We used the same conditions displayed in Figure 1b but experimental trials were embedded into participants' everyday lives through user-phone interactions programmed to take place during a typical day (see Figure 2). These phone interactions were prompted by a standard Android alert message and consisted of a user action followed by auditory stimulus at the programmed probability. We also asked participants to rate their control over the auditory outcome at five time points throughout the day. Ratings were performed by scrolling a wheel to a value between -100 and 100, with -100 indicating a perception of complete preventative control and +100 indicating complete generative control. Note that such ratings can be mapped onto the programmed $\Delta P$ metric.
The current study will be the first to do this.

**Methods**

**Recruitment**

Participants were 106 university students, who volunteered to participate by responding to an email advert and fulfilled the inclusion criteria. These were that they
were required to have access to an android mobile phone and be over the age of 17. Volunteers completed the Beck Depression Inventory [BDI: 26] during a visit to the laboratory after which they were supervised in downloading and installing the mobile application.

On the basis of their BDI scores, participants were categorized as members of the low BDI (BDI ≤ 5, n = 56, representing non-depression) or high BDI groups (BDI > 5, n = 50, representing mild depression). This cutoff value for group categorization has been used in other similar studies examining the effects of mild depression or dysphoria on contingency learning and represents the median BDI score in most samples [27]. Consistent with this, Table 1 shows that the BDI groups produced significantly higher scores on other depression relevant scales, including anxiety and stress. The two groups were matched on age and estimated IQ scores [28] but not digit span scores [29].

<table>
<thead>
<tr>
<th>Measure</th>
<th>Low BDI M</th>
<th>Low BDI SE</th>
<th>High BDI M</th>
<th>High BDI SE</th>
<th>MANOVA F</th>
<th>MANOVA P</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>22.98</td>
<td>0.98</td>
<td>25.68</td>
<td>1.03</td>
<td>3.60</td>
<td>0.061</td>
</tr>
<tr>
<td>Digit</td>
<td>7.61</td>
<td>0.18</td>
<td>6.90</td>
<td>0.19</td>
<td>7.47</td>
<td>0.007</td>
</tr>
<tr>
<td>Est IQ</td>
<td>113.00</td>
<td>0.83</td>
<td>112.33</td>
<td>0.88</td>
<td>0.30</td>
<td>0.582</td>
</tr>
<tr>
<td>BDI</td>
<td>2.30</td>
<td>0.65</td>
<td>12.56</td>
<td>0.68</td>
<td>119.21</td>
<td>&lt;.001</td>
</tr>
<tr>
<td>DASS-A</td>
<td>1.02</td>
<td>0.33</td>
<td>3.62</td>
<td>0.35</td>
<td>29.40</td>
<td>&lt;.001</td>
</tr>
<tr>
<td>DASS-D</td>
<td>1.05</td>
<td>0.41</td>
<td>4.96</td>
<td>0.43</td>
<td>43.01</td>
<td>&lt;.001</td>
</tr>
<tr>
<td>DASS-S</td>
<td>2.29</td>
<td>0.49</td>
<td>6.50</td>
<td>0.52</td>
<td>34.95</td>
<td>&lt;.001</td>
</tr>
</tbody>
</table>

Note: Est IQ = estimated IQ; Digit = digit span score; BDI = Beck Depression Inventory score; DASS-A = DASS anxiety, -D = depression, -S = stress; 'DF= 1, 104.
Design

This study used a mixed $2 \times 2 \times 2 \times (5)$ factorial design, in which BDI group (2: low BDI, high BDI), outcome density (2: low, high), and alternative cause instructions (discrete, dynamic) were the between groups variables, and judgment block (5: 1 to 5) was the repeated measures variable. Participants made ratings of the control their actions had over the occurrence of the outcome, and these were made using a $+100$ (complete control) through 0 (no control) to $-100$ (preventative control) scale, presented to the participant as a wheel as shown in Figure 2. In addition, we recorded the number of actions made by each participant, and calculated the contingency (actual $\Delta P$) and outcome density experienced (based on the number and types of trials experienced), and the number of trials missed.

Measures

Participants completed the BDI, the Depression, Anxiety, Stress Scales [30], the digit span test and a number of other demographic items, which are described in detail elsewhere [31]. In addition, participants completed a perception of control task, that was administered using a mobile phone and is described in detail below.

Perception of Control Task: The task was implemented using an application developed using an Android library for the context-aware delivery of messages to users’ mobile devices [32]. The application required wireless or network connectivity with the server initially in order for randomization to groups to take place and the experimental condition settings to be downloaded to the phone. Once this was complete, the application functioned independently and did not require continuous wireless or network connectivity (details of the architecture and information flow are
given in Supplementary Information 1 and https://osf.io/4aqdk/). Incremental data upload to the server was programmed to take place as soon as connectivity was available, both during the task and once the task was finished.

The task was implemented as a discrete trials contingency judgment task, including 40 experimental trials lasting 6 seconds (s) each, divided by inter-trial intervals lasting on average 12 minutes [calculated using the Fleshler-Hoffman progression: 33]. Each trial was prompted by a standard Android ‘notification’ message (similar to those delivered to alert a user that a text message has arrived and is waiting to be read) that included a brief auditory and visual signal. A time limit of 2 minutes was given for participants to access the alert after which the trial would be categorized as a ‘miss’. Under these circumstances the alert would be removed from the screen and the next inter-trial interval would commence. This procedure was used to ensure that the procedure lasted for the same duration for all participants. If the participant accessed the alert within the 2-minute time frame, the onscreen button would appear on the screen in 2s. They would then have the opportunity to press a touch-screen button for 3s. Whether or not the button was pressed within 3s, an auditory outcome would sound for 1s, or not, depending on the programmed probability. Following each of these experimental trials, the inter-trial interval would commence. Action-outcome contingencies were programmed as in Figure 1b.

Participants were either randomized to the low outcome density group ($P(O|A) = .25, P(O|\sim A) = .25$) or the high outcome density group ($P(O|A) = .75, P(O|\sim A) = .75$).

Therefore for all participants, the programmed contingency was zero and they had no control over the sound’s occurrence. Every 8 trials, the participant would be asked to rate their own control using the previously described wheel (see Figure 2). The
procedure was programmed to last for approximately 8 hours and function during the
participants' typical day, in a similar manner to typical mobile phone interactions.

Procedure

After having been fully briefed on arrival at the lab and having given informed
consent, participants were asked for their demographic details and completed a series
of questionnaires about their mood and performed the digit span test. Following this,
the experimenter assisted participants in downloading the app onto their own mobile
phone and guided the participant through installing and activating the app. Once the
application was activated, the participant was prompted to enter a code that would act
as a unique identifier to allow matching of lab and app generated data. Following this,
the instructions (see Supplementary Information 2 and https://osf.io/4aqdk/) were
presented and told participants that they would interact with the phone during the
course of the day which would be opportunities to test if their button pressing
controlled the sound occurrence. Following each block of 8 trials and the
corresponding control rating, participants received an intervention message to prompt
the participant to consider the influence of their context on their control. In the
‘discrete’ condition, participants were told to think about the, "…control external
factors have .... this could be factors related to the phone, the mobile network or
anything apart from your actions." In the dynamic context condition, participants were
asked to think about, “…control the place you are located in has.... It’s important to
note that you will change your location throughout the day. The place you are in could
affect whether the boing sound occurs, regardless of your actions." When the
procedure was complete, the app provided debriefing information and links to support
information provided on a webpage.
**Statistical analysis**

**Power**

We conducted a priori power analyses, which indicated that a sample of 152 was required for a power of .8 to detect medium sized between-group effects and a sample of 24 and 48 to detect repeated measures effects and interactions respectively. However, only 106 volunteers kept appointments at the laboratory to participate in the study. Based on the achieved sample size, compromise power to detect the repeated measures effects and interactions that were the focus of our hypotheses were high (> .99), whereas power to detect main effects of between-group variables was lower than we planned (0.70).

Due to participants completing the task at the same time as their everyday activities, we anticipated missing data, in terms of trials and ratings, as well as issues such as loss of mobile phone battery. In this data set, 10.57% of judgment values were missing. We therefore carried out multiple imputation, which involves replacing missing data with values generated from a series of multiple regression analyses, which include standard error, and use available parameter estimates. The fifth and most conservative iteration was used for the analyses reported here (the data can be accessed at: [https://osf.io/4aqdk/](https://osf.io/4aqdk/)).

**Perception of Control Task Validity Data:** Participants missed 11.38 trials (SE = 0.79) of the programmed 40 on average and, as instructed, pressed the button on around half of the trials they engaged with (press proportion $M = 0.58$, $SE = .019$). The actual contingency ($ΔP$) experienced was close to 0 as programmed ($M = .04$, $SE = .02$). Participants in the low outcome density condition experienced outcomes on 29.2% of trials ($SE = 2.7\%$) whereas participants in the high outcome density condition
experienced outcomes on 69.2% of trials (SE = 2.5%). Overall, the recorded engagement with experimental trials and contingency experience was as programmed.

**Results**

Participants' ratings of their own control over the outcome are shown in Figure 3. We chose not to display the data by judgment block, as there were no effects or interactions involving this variable.

![Figure 3](image)

**Figure 3**

Mean ratings of control as a function of outcome density, BDI group and alternative cause intervention. *Error bars correspond to standard errors of the mean.*
An illusory control effect is demonstrated when participants’ ratings of high OD conditions are higher than ratings of low OD conditions, as suggested in some groups in Figure 3. In order to test the hypotheses, that (i) illusory control and depressive realism effects would be present, and (ii) that the intervention would enhance ratings of control, a mixed factorial analysis of variance (see SOM 3 and https://osf.io/4aqdk/), which included actual ΔP experience as a covariate, was carried out. The alpha level was held at .05 throughout unless stated otherwise.

**Illusory Control and Depression Effects**

There was a significant main effect of BDI group, which showed that the non-depressed low BDI groups produced significantly higher ratings of control than more depressed participants, $F(1, 91) = 4.05, MSE = 6671.40, p = .047, \eta_p^2 = .04, 90\% \text{ CL} [.0004, .1271]$. On average, low BDIs rated their control as 14.90 ($SE = 5.37$) whereas mildly depressed high BDIs rated their control as nearer to zero ($M = 1.16, SE = 5.98$). Subsequent single samples t-tests comparing ratings to a criterion accuracy value of 0 showed that, for low BDI participants, 4 out of 5 action ratings were significantly higher than 0, all $t$s $> 2$, all $ps < .05$, evidencing illusory control. For more depressed, high BDI participants, 5 out of 5 action ratings were not reliably different to 0, all $t$s $< 1.16$, all $ps > .25$. This shows that non-depressed participants tended toward perceiving that they had more control over the occurrence of the auditory stimulus than depressed participants did whose ratings represented lower levels of perceived control.

**Alternative Cause Intervention Effect**

Figure 3 suggests that, as we predicted, instructions on the nature of the alternative cause enhanced perceptions of control. Only when participants were instructed that the alternative cause was a discrete entity did they show evidence of the healthy
enhanced perception of control. The analysis supported this observation because the interaction between instructions and outcome density was reliable, $F(1, 91) = 7.05, MSE = 6671.40, p = .009, \eta^2_p = .07, 90\% \text{ CL [.01, .1674]}$. Follow up simple effects analyses confirmed this pattern, and showed that the high outcome density conditions only received higher ratings than low outcome density conditions with discrete cause instructions, $F(1, 91) = 6.06, MSE = 1334.28, p = .016, \eta^2_p = .062, 90\% \text{ CL [.0064, .1548]}$ and not with dynamic cause instructions, $F(1, 91) = 1.85, MSE = 1334.28, p = .177, \eta^2_p = .02, 90\% \text{ CL [0, .0887]}$. These findings show that perceived control can be enhanced by providing participants with simple instructions about the nature of alternative causes. Participants who compared their own control to discrete alternative causes produced higher control ratings.

Discussion

We used a mobile phone application in order to embed and measure perceived control occurring in participants' everyday lives as naturalistic mobile phone interactions. The application was designed such that participants' actions had no control over the occurrence of an auditory stimulus. Participants without signs of depression over-estimated their control whereas participants showing mild levels of depression rated their control as close to zero. This study provides the first objective demonstration of illusory control and depressive realism in a real-world setting.

Furthermore the instruction intervention significantly influenced all participants’ ratings of control. As we predicted, when the alternative cause was described as a discrete constantly present entity (the mobile phone network), people judged that they had more control when the auditory stimulus occurred frequently [e.g., 15]. Conversely, when people were informed that alternative causes were dynamic (i.e.
place) and would change throughout the experience, there was no evidence of illusory control effect. This finding suggests that the intervention used in this study, which was based on theoretical accounts of how people use alternative causes to evaluate their own control, was effective and successful.

**Limitations**

It is important to consider whether the experimental procedure was a valid test of the contingencies that we planned for participants to experience. This is because it has been acknowledged that changes in participant behavior can actually affect the contingencies they are exposed to [18, 34, 35]. Our concern here was missed trials. However, careful scrutiny of the data recorded on each trial showed that even though participants missed some trials, they experienced the contingencies as programmed. This alleviates an important concern.

Another limitation is that our implementation of the contingency judgment task was an extremely crude analogue of user-mobile phone interactions. However, it was important to implement a real life contingency task that was as similar as possible to lab procedures in order to provide the replication required. Our future studies will not only be able to provide a more sophisticated interface but will also collect richer data, including concurrent natural activities and behaviors, as well as mood and wellbeing ratings over longer periods of time.

**Conclusion**

The findings of this study show convincingly that when perceptions of control are measured in relation to an objective standard, biased estimates of control do correlate with mental healthiness, with illusory control being the healthiest type of control [e.g.,
Important, the simple theoretically motivated intervention, which was designed to influence people’s ability to learn about the power of the alternative cause in contrast to their own control, was effective in enhancing people’s perceptions of being ‘in control’. Finally, this study further demonstrates the power of mobile phone technologies for use in experimental and intervention research. This not only provides the opportunity to test psychological theory in novel ways embedded in a person’s everyday environment but also to collect ever richer data about natural behaviors.
References
