UNDERSTANDING AND OVERCOMING BIASES IN JUDGMENT AND DECISION-MAKING WITH REAL-LIFE CONSEQUENCES

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UNDERSTANDING AND OVERCOMING BIASES IN JUDGMENT AND DECISION-MAKING WITH REAL-LIFE CONSEQUENCES

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Editorial: Understanding and Overcoming Biases in Judgment and Decision-Making With Real-Life Consequences

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Editorial on the Research Topic

Understanding and Overcoming Biases in Judgment and Decision-Making With Real-Life Consequences

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The study of judgment and decision-making is essential to understand human behavior and to inform policy affecting people's wellbeing in different domains, including health, finance, and the environment. Advances in research on judgment and decision-making over the last decades have helped to document a wide range of cognitive and affective biases that can affect decision-making, uncover the mechanisms underlying such biases, and identify moderating factors. However, a better understanding of the impact of biases on judgments and decisions beyond laboratory settings and ways to prevent negative real-world outcomes is still needed. With the current Research Topic, we aimed to bring together researchers from different fields and traditions to cover recent advances in these areas and bridge gaps between theoretical and applied work. We launched the Research Topic in 2020 as an initiative from the Society for the Advancement of Judgment and Decision-Making Studies (SEJyD), which was founded in Spain in 2014 with the aims of creating a new platform for sharing insights from research in this field, promoting interdisciplinary work, and fostering new international collaborations.

We were pleased to receive a diverse set of contributions, resulting in 14 published articles involving 57 authors from 7 different countries (Australia, Italy, Norway, Sweden, US, UK, and Spain). The contributions include theoretical and applied work reflecting expertise in different areas of psychology (experimental, clinical, and health psychology), health sciences, business management, organizational behavior, and sustainability, among other disciplines. The articles spanned diverse methodologies, including large scale laboratory and field experiments, cross-sectional and longitudinal surveys, and syntheses of previous research. Overall, the contributions can be grouped in four main areas of research, outlined below.

The first line of research investigates basic processes in causal learning and judgments of causal relationships in different contexts. Greenaway and Livesey report a contingency learning experiment where participants were presented with pairings of food items (varying in their a priori likelihood to produce allergic reactions) and allergy episodes. Results revealed that both prior knowledge and contingency information contribute to causal beliefs about foods and allergy, even when people are instructed to ignore prior knowledge. Relatedly, Blanco et al. report three contingency learning experiments investigating judgments about the effectiveness of medical treatments. Results indicated that treatments can be perceived as less effective than they are because patients' judgments are systematically biased by the base rate of symptoms. This tendency was driven by people's tendency to use relative, rather than absolute, measures of effectiveness to assess how well treatments work.

The second line of research focuses on investigating factors moderating biases in different domains and underlying cognitive and affective processes. In the domain of environmental decision-making, Threadgold et al. focus on the "negative footprint illusion", which refers to people's tendency to incorrectly believe that adding "eco-friendly" items (e.g., environmentally certified houses) to a set of conventional items (e.g., standard houses) reduces the carbon footprint of the combined set of items. Reduced susceptibility to the illusion was associated with actively open-minded thinking across two studies, but not with other reflective thinking dispositions. Relatedly, Muela et al. examine the role of individual differences in domain-general reasoning abilities in the context of problem gambling. Such reasoning abilities were mostly unrelated to sensitivity to gambling biases, suggesting that psychoeducation to improve domain-general reasoning could be insufficient to debias gambling-related beliefs and cognitions. Focusing on emotional and motivational factors underlying gambling-related biases, Philander and Gainsbury found that positive attitudes toward electronic gaming machines correlated with overconfidence in understanding how these machines work. However, a manipulation of the provision of accurate and inaccurate information about how outcomes were determined did not influence attitudes, suggesting that information-based interventions may be insufficient to reduce biases and positive attitudes toward gambling. Finally, Mayiwar and Björklund examined the interplay between psychological distancing and emotions in risky judgment and decision-making. The relationship between fear and risk-taking was found to be negative in the absence of psychological distancing but positive in the presence of distancing. These findings suggest that distancing may help to avoid excessive risk aversion caused by incidental fear.

A third area of research focuses on documenting the impacts of cognitive and affective biases on real-world outcomes and decisions. In the context of consumer behavior, Reutskaja et al. examine how price information affects choices concerning which denomination to use when paying for products (e.g., one €50 bill or five €10 bills) and choice of form of payment (cash vs. debit card). In a series of experiments, consumers exhibited the "price-denomination effect" whereby they anchor on prices when deciding which denomination to use. Using an Ecological Momentary Assessment (EMA) methodology, Colombo et al. investigated the relationship between affective forecasting biases and perceived psychological wellbeing. They found that positively biased forecasting of positive affect (i.e., overestimating positive emotional states) is associated with higher perceived psychological wellbeing and resilience. These results suggest that affective forecasting could function as an adaptive cognitive distortion that boosts people's resilience and mental health. On the other hand, Savioni and Triberti review the role of different cognitive biases in the decision-making and health management of patients with chronic diseases. The authors illustrate how different biases might influence the motivation and agency of patients and propose a process model of how cognitive biases can lead to suboptimal decisions. Garrido et al. studied decision delay–the time patients wait before seeking medical attention after symptoms have started–in acute coronary syndrome patients who survived their cardiac episode. They found that patients who had better knowledge of cardiovascular risk factors reported shorter decision delays, suggesting that knowledge of such factors could play a role in decision-making during an acute cardiac event (i.e., a heart attack). Finally, Sambrook et al. review the role of personal experience with extreme weather events in shaping climate change beliefs and action, as well as the influence of prior beliefs on people's perceptions of climate change impacts. The review highlights the importance of examining processes such as motivated reasoning to understand biases in the interpretation of personal experiences of climate change impacts.

A final line of research focuses on testing the effectiveness of strategies to overcome misconceptions, enhance probabilistic reasoning, and improve risky decision-making. Ferrero et al. examined the effectiveness of refutation texts at debunking misconceptions about education among teacher education students. Through a series of experiments, the authors show that refutation texts reduced teachers' endorsement of misconceptions in the short run but not in the long run. The study shows that, once adopted, misconceptions in education can be highly resistant to change. Focusing on probabilistic reasoning, Cruz et al. developed a graph-based Bayesian network tool representing probabilistic dependency relations between variables. The tool was effective to improve Bayesian reasoning in complex scenarios in which most individuals are prone to committing systematic errors. This tool may be used to improve probabilistic reasoning in risk-sensitive fields such as medical or forensic diagnostics and environmental or economic risk forecasting. Finally, Baltruschat et al. test the effectiveness of a mindfulness-based intervention in reducing risky driving behavior in a group of repeat traffic offenders. Participants who were trained in mindfulness did not show differences in emotional regulation, but showed improved performance in risk situations and had fewer accidents in comparison with control groups

Taken together, the studies in our Research Topic highlight the relevance of research in judgment and decision-making to understand human behavior and inform policies to improve wellbeing. This collection of insightful papers contributes to our understanding of the basic mechanisms underpinning different types of biases, circumstances under which such biases may be more likely to occur, and their real-world impact. The studies reviewed also highlight that more work is needed to understand the different factors that might protect people from biases and identify effective strategies to reduce their potential negative impact, particularly in the long term. We hope that our Research Topic will inspire future efforts along these lines, both in terms of specific issues in need of investigation and in terms of fruitful approaches to tackle these issues, including the combination of different methodologies and disciplines. It is also worth highlighting that many of the contributions endorsed open science practices, including publicly sharing study materials, data, and analysis code, and in some cases pre-registering study protocols. We believe that this sets an excellent example for future work that can help to enhance the transparency, reproducibility, and efficiency of research in this area, and at the same time promote collaborative efforts and quick knowledge transfer relating the important societal challenges addressed.

AUTHOR CONTRIBUTIONS

All authors listed have contributed to the conceptualization, writing, reviewing of the work, and approved the submitted version.

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Widening Access to Bayesian Problem Solving

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Bayesian reasoning and decision making is widely considered normative because it minimizes prediction error in a coherent way. However, it is often difficult to apply Bayesian principles to complex real world problems, which typically have many unknowns and interconnected variables. Bayesian network modeling techniques make it possible to model such problems and obtain precise predictions about the causal impact that changing the value of one variable may have on the values of other variables connected to it. But Bayesian modeling is itself complex, and has until now remained largely inaccessible to lay people. In a large scale lab experiment, we provide proof of principle that a Bayesian network modeling tool, adapted to provide basic training and guidance on the modeling process to beginners without requiring knowledge of the mathematical machinery working behind the scenes, significantly helps lay people find normative Bayesian solutions to complex problems, compared to generic training on probabilistic reasoning. We discuss the implications of this finding for the use of Bayesian network software tools in applied contexts such as security, medical, forensic, economic or environmental decision making.

Keywords: Bayesian networks, assistive software technology, reasoning, decision making, probabilistic

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THEORETICAL BACKGROUND

Most reasoning situations arguably take place under uncertainty: we cannot say for sure that the information from which we draw inferences is correct, but only believe it to a higher or lower degree (Evans and Over, 2013; Pfeifer, 2013; Gilio and Sanfilippo, 2014; Over and Cruz, 2019; Oaksford and Chater, 2020). Moreover, these uncertain pieces of information may be related to one another in intricate ways, so that it can quickly become difficult to foresee the implications that a change in our degree of belief in one piece of information may have on our degrees of belief in the others (Fernbach et al., 2010; Hadjichristidis et al., 2014; Rottman and Hastie, 2016; Bramley et al., 2017; Rehder and Waldmann, 2017).

But just like we can make use of tools like notepads and video recorders to aid our memory, there are tools that can help us navigate complex reasoning tasks in which we have to draw inferences from uncertain information. In particular, we can use probability theory to establish precise constraints between related degrees of belief (e.g., Gilio and Over, 2012; Politzer, 2016), and we can use Bayesian networks (BNs) to establish the precise implications of a change in the probability of one piece of information for the probability of other, related pieces of information (Pearl, 1988, 2000; Korb and Nicholson, 2011; Fenton and Neil, 2018).

Bayesian networks are graphical representations of probabilistic dependency relations between variables. Each variable is represented through a node, and arrows represent directed links from one node to another. Each node is associated with a probability table. The "parent" nodes in the network, which do not have arrows leading to them, have an unconditional probability table, with a single entry that represents their probability. The "child" nodes, which have one or more arrows leading to them, have a conditional probability table, which indicates the conditional probability of that node, given all possible combinations of the presence or absence of its parent nodes.

Figure 1 provides an example of a simple BN with three nodes, representing two causes that have a potential effect in common. In the figure, the presence of a delay is a function of the (inclusive) disjunction of two mutually independent causes, traffic and/or rain. There is a 40% probability of traffic (which when present on its own, leads to a delay in 90% of cases), and an 80% probability of rain (which when present on its own, leads to a delay in 60% of cases). The numbers in the example assume there are no unknown causes that could lead to a delay in the absence of both traffic and rain.

Once a network is built, it can be queried to assess for example what happens to the probability of a delay if an intervention is made to avoid traffic (such as traveling at a different time of the day).

Bayesian networks are finding increasing use in applied domains requiring people to make complex predictions and decisions on the basis of a range of uncertain and interconnected factors, ranging from forensic (Smit et al., 2016) over medical (Fenton and Neil, 2010; Constantinou et al., 2016) to meteorological contexts (Boneh et al., 2015). However, until now these methods have largely remained accessible only to experts in Bayesian probability theory or practitioners with extensive training (Nicholson et al., 2011; Smit et al., 2016).

In this study, we assessed to what extent the availability of a software tool to construct BNs with minimal training can help lay people solve complex probabilistic reasoning tasks, as might be faced in a range of real world problem solving situations in everyday and professional settings.

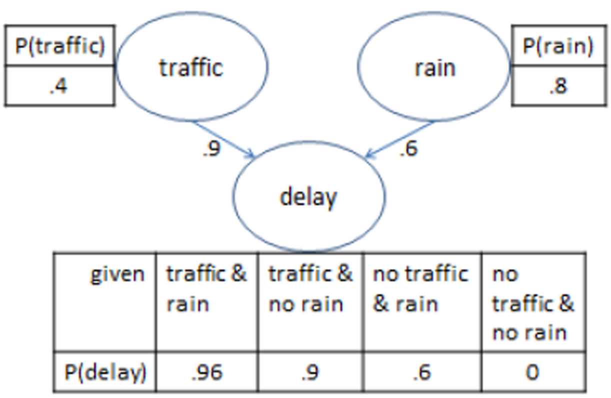

FIGURE 1 | Illustration of the components of a Bayesian network. See main text for details.

The BN software tool used was adapted from the AgenaRisk software[1] by Ann Nicholson, Erik Nyberg, Kevin Korb, and colleagues at the Faculty of Information Technology of Monash University, Australia (Nicholson et al., 2020, arXiv preprint available at https://arxiv.org/abs/2003.01207). This BN software tool, called BARD (for Bayesian Reasoning via Delphi), differed from AgenaRisk in three main respects relevant to the present study. (a) At the time of the study it implemented only a subset of the functionality of AgenaRisk. (b) The interface was structured in a different way, encouraging a workflow in which users first think of the variables relevant for a problem at hand, and then connect the variables to one another to form a causal network. Next users define the probability tables for each node in the network. Finally, users experiment with or "query" the network to obtain information from it, e.g., by setting one or more nodes to a particular value and assessing what impact this has on the values of the remaining nodes. (c) The software had an inbuilt training module featuring text and short videos, as well as inbuilt pointers to the functionality of each software element that could be accessed throughout the modeling process. The BARD software as a whole also includes features for people to build BNs collaboratively in groups, but we used a version of it, SoloBARD, for which the group related functionality was removed to focus on testing the usefulness of the software for individuals.

HYPOTHESES

We tested whether using the BARD software and training system for constructing BNs improves the ability of individuals to solve complex probabilistic reasoning problems, compared to a control group receiving only generic training in probabilistic reasoning. This research question was assessed through the following two hypotheses.

1. The treatment group using the BN software tool will produce higher proportions of correct responses than the control group, measured using predefined rubrics for each problem. The overall score in the rubrics was a composite based on marks awarded for responding to the questions explicitly asked for in the problem statement, alongside marks for providing background information about the problem, such as on the reliability and independence of sources, as well as for providing explanations for the responses given to the explicit questions. This hypothesis was assessed through the computation of effect sizes and confidence intervals.
2. The treatment group will produce higher proportions of correct responses than the control group in the section of the rubrics concerned with probability questions explicitly asked about in the problem statements. This hypothesis was also assessed through the computation of effect sizes and confidence intervals.

[1] www.agenarisk.com

METHOD

The study was preregistered with the Open Science Framework (OSF). The data, materials and analysis script can be found under: https://osf.io/28w9e/?view_only=d31e21706e4241839e27ea0dff51c98c

Participants

An initial sample of 72 participants was recruited from the participant recruitment pool of University College London, with 36 in the treatment and control groups, respectively. After accounting for some cancelations, the final sample consisted of 59 participants, 29 in the treatment and 30 in the control group. Participation was remunerated with £10 per hour. In addition, bonuses were given to the highest performing individuals in each group, with £250 to the single top scoring person, £100 to the top tenth percentile, £50 to the next tenth percentile and £25 to the next tenth percentile. All participants were residents of the United Kingdom and had not participated in a previous pilot study. Their mean age was 26.78 years (range 19–68). All indicated being native speakers of English, and 37 indicated having a Bachelor degree or above.

Materials

All participants worked through three complex probabilistic reasoning problems. These problems were created with the aim of covering a broad range of probabilistic reasoning features. Previous research suggests these are features that people often find difficult to spontaneously grasp (for examples and discussion see Juslin et al., 2009; Sloman and Lagnado, 2015; Rottman and Hastie, 2016; Rehder and Waldmann, 2017). The problems used in this experiment were "Black Site," "Cyber Attack," and "Kernel Error." These were the same problems as had been used in a pilot study aimed at obtaining an impression of baseline problem difficulty. The problem descriptions and the rubrics used to mark the solutions are included in the OSF repository for the study. The probabilistic features measured by each problem are summarized in **Table 1** (for more specific theoretical and empirical background to the problems see Dewitt et al., 2018; Liefgreen et al., 2018; Phillips et al., 2018; Pilditch et al., 2018, 2019).

Participants in the treatment group worked through the problems using the Bayesian network tool. Their training materials included guidance on how to identify relevant variables for a problem, formulate hypotheses about causal relationships between the variables, estimate the probability of each variable given the presence or absence of its potential causes, and strategies for querying the network to obtain candidate answers to the problem at hand.

Participants in the control group worked through the problems using blank Word documents, with access to the generic information on reasoning with probabilities that they were given during the training. This information included the advice to not only offer a direct answer to the explicit problem questions, but to also explain how and why this answer was arrived at, including a consideration of the reliability and consistency of the sources of information used to come to a conclusion, how likely this conclusion is considered to be, and what information might be missing which, if it became available, could change the assessment of the conclusion in relation to alternative conclusions that could have been drawn instead. Both groups also received guidance on the meanings of the technical terms "hit rate" and "false alarm rate."

TABLE 1 | Features measured by the three problems in the experiment.

	Black site	Kernel error	Cyber attack
General features			
Alternative hypothesis comparisons	x	x	x
Source reliability/accuracy	x		x
Conflicting evidence	x	x	x
Uncertainty encapsulation	x	x	x
Belief revision/updating		x	x
Base rates	x	x	x
False positive/negatives	x	x	x
Dependent evidence relations			x
Noisy-or	x	x	
Problem specific features			
Explaining away/discounting		x	x
Zero-sum fallacy	x		
Common cause vs. multiple independent explanations		x	

Design

The experiment followed a between participants design with one predictor variable: Participants were assigned to either the treatment group (receiving the Bayesian network training and software) or the control group (receiving generic information on reasoning with probabilities and a blank Word document).

There were two dependent variables (DVs): total scores on problem rubrics (includes points awarded e.g., for explaining reasoning steps and justifying conclusions arrived at), and question response scores (includes only points awarded for answers to explicit questions). Both dependent variables were measured as proportions of the maximum attainable marks for a problem.

For the above DVs, the study computed (a) effect sizes and (b) 95% confidence intervals (CIs) around the effect sizes. The above measures were complemented with (c) a linear mixed model analysis with random intercepts for participants. The mixed model was used to compute significance tests and CIs for the mean condition differences.

The method for computing effect sizes was chosen on the basis of whether or not the variances were equal in the treatment and in the control group. Equality of variances was assessed through the Levene test (using the leveneTest function of the car package in R). It was determined that if the test indicated that the variances were equal, then effect sizes would be computed using the Hedges' g measure for the pooled variance (Hedges' g is similar to Cohens' d but it corrects for a bias in the latter). If in contrast, the Levene test indicated that the variances were unequal in the two groups, then effect sizes would be computed using Glass' delta, a measure designed for situations of unequal variance. The linear mixed model analysis was performed in R (R Core Team, 2017) using the lmer function of the lme4 package (Bates et al., 2015).

Participants were assigned to one of the two groups in a pseudo-random way, based on the study dates for which they signed up. The same study advert was used for all study dates. Participants in both groups worked through the three reasoning problems. The order of presentation of the problems was counterbalanced between participants, so that overall each possible problem order occurred approximately equally likely in both groups.

Procedure

The testing took place in a computer based lab setting under exam conditions. Participants in the treatment group worked through the problems using the Bayesian network system, and their responses – in the form of written reports – were collected from within the system. Participants in the control condition worked through the problems using blank Word documents.

Each group was tested on two full consecutive days. The testing dates took place on different weeks for the two groups to facilitate blinding. Participants in each group were given 2.5 h. to work through each of the three problems, and they were offered lunch and coffee during the session breaks. No performance feedback was provided to participants in either group.

Rater Training

To ensure that participants' reports were marked in an impartial way, nine raters were recruited from university mailing lists, none of whom were associated with the project. The raters received ˜7 h of training. Rater training took place over a single full day. The day was split into four sessions, with the first three corresponding to the three problems administered. Within each problem session, raters first read the problem text and then discussed the problem structure as a group. Following this, raters read the rubric and were able to ask any questions and discuss any potential ambiguous elements as a group. Raters then rated a participant report from a pilot experiment. In the final session, raters rated three further reports, one for each problem, totaling six reports marked over the course of the day.

Participant Training

Participants in the control group were given 1.5 h to work through the generic training in reasoning with probabilities, and were able to access the training again at any point during the day. Participants in the treatment group were allowed 3.5 h to work through the training material embedded in the BN software. The difference in training time between groups was due to the experimental group having a wider range of material to work through than the control group. Both groups were given the opportunity to refresh their knowledge of training materials for 30 min on the second day of testing, prior to continuing with the problems.

Report Rating

Participant reports from both conditions were marked by the nine independent raters working with the problem specific rubrics. Reports were assigned randomly to raters, and all reports were marked by two different raters, with the mean score across the two raters used as the final variable. We took the mean of the two ratings rather than asking raters to discuss potential discrepancies until reaching an agreement in order to allow rater's judgments to be based on a larger amount of independent information (Hahn et al., 2019). Interrater reliability[2] was 0.789 for the treatment group and 0.636 for the control group. Raters were instructed to take ∼30 min to mark each report and were allocated 47 reports to mark each. The raters could not be fully blinded to condition because the two conditions used different templates for their answers. However, the raters were not informed about which template corresponded to which condition, nor of any of the study hypotheses.

RESULTS AND DISCUSSION

The average ratings of the two markers for each participant and problem were converted into proportions of the total attainable scores for each problem, separately for each of the two DVs.

Total Rubric Score

The average proportion of correct responses based on the total rubric score is shown in the left panel of **Figure 2** for each group. The CIs in the figure suggest that the variance was larger in the treatment group than in the control group. The Levene test showed that this difference was significant [$F_{(1,175)} = 13.782$, $p < 0.001$], thus Glass' delta rather than Hedge's g was used as effect size measure. Glass' delta and the CIs around it were computed using the smd.c and ci.smd.c functions, respectively, both from the MBESS R package. The effect size of the difference between groups on the total rubric scores was large: it reached 0.85 on average, with a 95% CI of [0.527, 1.166].

In accordance with the above results, the linear mixed model indicated that performance in the treatment group (estimated marginal mean = EMM = 0.479) was significantly higher than in the control group [EMM = 0.321; $t(57) = 3.546$, $p < 0.001$] and inclusion of the predictor for group in

[2]Interrater reliability was measured as intraclass correlation, in a two-way model of type agreement, using the icc function of the irr package in R.

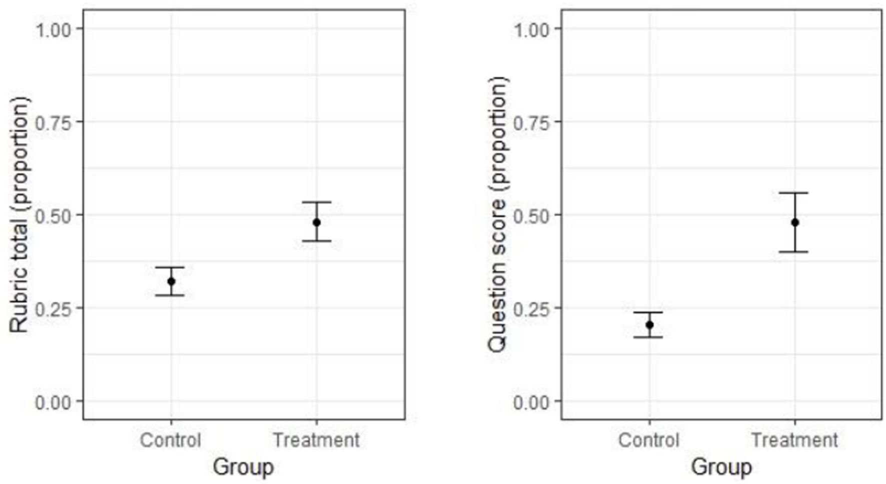

FIGURE 2 | Left panel: Means (and their 95% CIs) for the two groups on the total rubric score. **Right panel:** Means (and their 95% CIs) for the two groups on the explicit problem questions.

the model led to a significant improvement in model fit [$X^2(1) = 11.760, p < 0.001$].

Explicit Problem Questions

The average proportion of correct responses based on the explicit problem questions is shown in the right panel of **Figure 2** for each group. As in the previous analysis, the CIs in the figure suggest that the variance was larger in the treatment group than in the control group. The Levene test showed that this difference was significant [$F_{(1,175)} = 46.141$, $p < 0.001$], so that Glass' delta rather than Hedges' g was used as effect size measure. The effect size for the difference between groups on the explicit question scores was again large, and was numerically larger than that for the total rubric score. It reached 1.62 on average, with a 95% CI of [1.239, 1.996].

In line with the above finding, performance in the treatment group ($EMM = 0.480$) was significantly higher than that in the control group [$EMM = 0.203$; $t(57) = 4.752$, $p < 0.001$], and inclusion of the predictor for group in the model led to a significant improvement of model fit [$X^2(1) = 19.691, p < 0.001$].

Further corroborating analysis carried out separately for each problem can be found in **Appendix**.

CONCLUSION

In this study we investigated whether access to a Bayesian network modeling tool, together with a limited amount of embedded training resources in its use, can help lay people solve complex probabilistic reasoning problems, involving multiple dependencies between uncertain pieces of information that can dynamically change as more information becomes available.

The results were clear cut, providing strong evidence for an advantage in performance of the group with access to the Bayesian network tool compared with the control group having access only to generic training on probabilistic reasoning.

This finding provides a proof of principle that Bayesian network modeling can be made accessible to wider population sectors with minimal, self-directed training. Its introduction in areas such as intelligence analysis, medical or forensic diagnostics, as well as environmental or economic risk forecasting therefore likely constitutes less of an entry burden and uphill task than might be initially thought. Its wider use in these and other domains could bring about substantial benefits to its users given the normativity of the Bayesian framework, which allows people to minimize prediction error in a coherent way, preventing us from getting into situations in which any decision outcome leads to a sure loss (Ramsey, 1926/1990; Pettigrew, 2016; Vineberg, 2016). It can help increase our understanding of the relevant structure of a problem at the same time as the effectiveness with which our concomitant decisions help us to achieve our goals.

DATA AVAILABILITY STATEMENT

All datasets generated for this study can be found in the Open Science Framework under https://osf.io/28w9e/?view_only=d31e21706e4241839e27ea0dff51c98c.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the Research Ethics Committee, Department of Psychological Sciences, Birkbeck, University of London.

The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

NC: conceptualization, data curation, formal analysis, investigation, methodology, project administration, visualization, writing – original draft, and writing – review and editing. SCD: investigation, project administration, and writing – review and editing. SD, AL, KP, and MT: conceptualization, investigation, methodology, project administration, and writing – review and editing. UH: conceptualization, formal analysis, funding acquisition, investigation, methodology, project administration, resources, supervision, and writing – review and editing. DL: conceptualization, funding acquisition, investigation, methodology, project administration, resources, supervision, and writing – review and editing. TP: conceptualization, investigation, methodology, project administration, software, and writing – review and editing.

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Conflict of Interest: The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

APPENDIX

Additional Exploratory Analysis

We conducted an additional exploratory analysis not included in our preregistration. Its aim was to assess the generalizability of the findings across problems. The left panel of **Figure A1** displays the means and 95% confidence intervals of the total rubric score in each group, separately for each of the three problems. The right panel of **Figure A1** displays the same information as the left panel, but for the explicit problem questions.

The figures show clearly that for both dependent variables, the higher performance of the treatment group over the control group was not driven only by a subset of the problems used, but held across problems. This was corroborated by a linear mixed model analysis similar to the one reported in the confirmatory section of the results. This analysis showed that the main effect of group was significant not only overall, but also for each problem considered individually (for the total rubric score: lowest $t = 2.322$, highest $p = 0.022$; for the explicit problem questions: lowest $t = 3.453$, highest $p = 0.0004$; adjusted for multiple comparisons using the Sidak procedure).

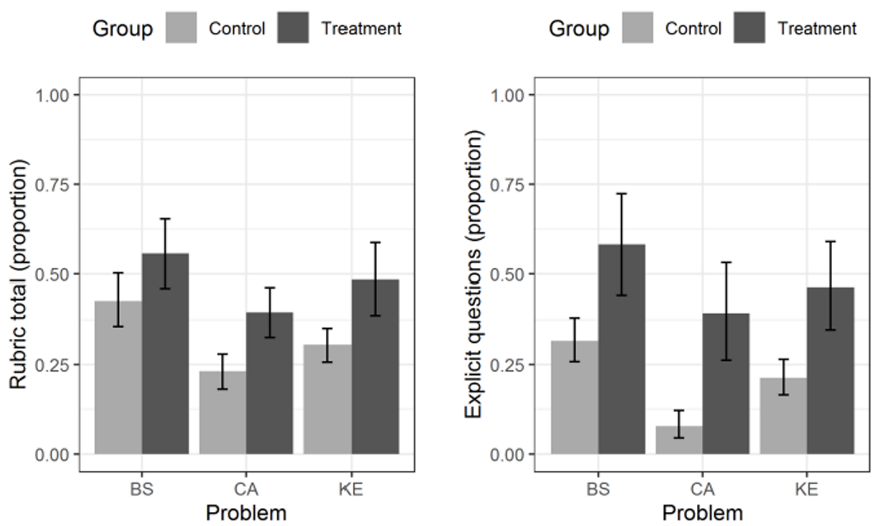

FIGURE A1 | Left panel: Means (and their 95% CIs) for the total rubric score in each group, separately for each problem. BS = "Black Site", CA = "Cyber Attack", KE = "Kernel Error". **Right panel:** Means (and their 95% CIs) for the explicit problem questions in each group, separately for the same three problems.

ORIGINAL RESEARCH
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Biased Affective Forecasting: A Potential Mechanism That Enhances Resilience and Well-Being

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According to a growing body of studies, people's ability to forecast future emotional experiences is generally biased. Nonetheless, the existing literature has mainly explored affective forecasting in relation to specific events, whereas little is still known about the ability to make general estimations of future emotional states. Based on existing evidence suggesting future-oriented disposition as a key factor for mental health, the aims of the current study were (1) to investigate the relationship between negative (NA) and positive (PA) affective forecasting biases and perceived psychological well-being, and (2) to explore whether positively biased predictions are associated with resilience and foster one's skills to cope with stressful events. To do so, we asked 85 undergraduate students to forecast PA and NA over 2 weeks, as well as to report their daily affect through a web-based Ecological Momentary Assessment. According to the results, positively biased PA forecasting (i.e., overestimating positive emotional states) was associated with greater perceived psychological well-being and higher resilience. When high levels of stress were experienced, participants holding an optimistic, yet biased, estimation of future PA were more likely to successfully manage stressors, thus maintaining lower levels of NA and higher levels of positive emotions. We suggest that positively biased PA forecasting is an adaptive cognitive distortion that boosts people's resilience and mental health, thus opening new avenues for the promotion of psychological well-being.

Keywords: affective forecasting, cognitive bias, ecological momentary assessment, psychological well-being, resilience

INTRODUCTION

As terms draws to a close and summer vacations stretch out ahead, people start to mentally imagine the upcoming holidays. For instance, they visualize themselves sleeping until late, having a brunch with some friends or leaving for a tropical destination. Beyond envisioning activities, people spontaneously imagine their own future emotions (Staats and Skowronski, 1992). That is, how happy and relaxed they will feel while taking a break from work, or the excitement they will experience while visiting a new place. As evidenced by a long tradition of research, people are indeed used to mentally time travel, and they always try to imagine and predict future emotional experiences (Kahneman and Snell, 1990; Gilbert et al., 2002; Gilbert and Wilson, 2009).

Despite some sort of insight is likely to exist (Buehler and McFarland, 2001; Wirtz et al., 2003), research generally suggests that inaccuracy between forecasted emotional states and future experiences is frequent: People are not good at forecasting feelings, and the affective states they anticipate do not match the actual future experience (Wilson and Gilbert, 2003). Sources of errors in affective forecasting may be connected either to the time at which the prediction is made or to the actual experience (Wilson and Gilbert, 2003). Regardless of the type of error, the result is a bias in affective forecasts. In this sense, the literature has shown that, while people are usually quite accurate at forecasting the valence of future emotional experiences (i.e., negative or positive) or the specific emotions they will experience (e.g., anger or fear) (Wilson and Gilbert, 2003), they are quite biased at estimating emotional intensity and duration, thus leading to the so-called durability bias (i.e., the tendency to overestimate the duration of an emotional reaction) (Gilbert et al., 1998) and impact bias (i.e., the tendency to overestimate the impact of a future event) (Gilbert et al., 2002; Wilson et al., 2003).

To date, a body of studies supports the idea that affective forecasting represents an important cognitive process, and predicting future feelings is an essential source of information to drive behaviors (Mellers et al., 1997; Crawford et al., 2002; DeWall et al., 2014). Accordingly, people use affective information to make judgments and take decisions about the future (Schwarz and Clore, 1983; Taquet et al., 2016; Colombo et al., 2020). In addition, there is also evidence supporting that affective forecasting is a regulatory process, that might serve as a resilience source in the presence of difficulties. Specifically, anticipating future feelings would be a future-oriented strategy to regulate emotions (Goodhart, 1985), which would lead people to directly or indirectly behave in order to match or change the forecasted emotional experience (Persson and Sjöberg, 1985). In that direction, Totterdell et al. (1997) asked thirty participants to predict daily and weekly mood, as well as to annotate daily affect at the end of the day. Results showed that, regardless of the presence of daily hassles, mood was more likely to improve when participants expected it to improve (i.e., when they predicted that they would have experienced a better mood), thus supporting the hypothesis of affective forecasting as a regulatory process and suggesting that mood forecasts may be considered "[...] *as part of a process that exerts some mental control over mood*" (Totterdell et al., 1997).

Based on the previous literature, it seems plausible that the way people anticipate affective states can have repercussions on different aspects of life, such as happiness and well-being (Dunn et al., 2007a; Gilbert and Wilson, 2009; Buchanan et al., 2019; Nasso et al., 2019), physical and mental health (Sieff et al., 1999; Riis et al., 2005), and interpersonal relations (Dunn and Laham, 2006). Consequently, biases in affective forecasting, either positive or negative, may entail several consequences for mental health. Indeed, positive illusions such as favorable self-evaluations, exaggerated perception of control, and unrealistic optimism have been shown to boost happiness and well-being (Taylor and Brown, 1988, 1994; Brookings and Serratelli, 2007). These cognitive biases are likely to increase the perception of owning successful copying skills (Brown, 1993), which in turn enhances motivation and enthusiasm while carrying out actions (Taylor and Gollwitzer, 1995). Similarly, a positive future-oriented disposition and openness to the future (i.e., having positive expectations and a general disposition of acceptance toward the future) have been shown to be protective factors for mental health and to be positively associated with well-being (Weinstein, 1980; Mikus et al., 2017; Botella et al., 2018).

In the present study, we aimed to explore affective forecasting in a sample of undergraduate students. Contrary to the previous literature that mainly focused on predicting emotions in relation to a specific future event, we explored affective forecasting as a future-oriented disposition in healthy individuals by asking for general future affective estimations. The main objective was to disentangle the association of affective forecasting with well-being and resilience. To do so, we asked 85 participants to forecast positive (PA) and negative (NA) affect over 2 weeks, and we monitored experienced daily mood by means of a web-based Ecological Momentary Assessment (EMA) design, which has been shown to be an adequate methodology to capture emotional dynamics in daily life (Colombo et al., 2019a,b).

First, we hypothesized that people with a more optimistic view of future affect and who tend to overestimate PA will show greater well-being. No significant association is expected in relation to NA forecasts, because overestimating negatively valenced emotions is known to be either an evolutionary rather than maladaptive copying mechanism (Miron-Shatz et al., 2009), or the consequence of a negative bias associated with anxiety and depressive conditions (Mathersul and Ruscio, 2019), which were excluded from the current study. Second, and in line with the previous hypothesis, we expected that PA but not NA forecasts will be associated with resilience. More specifically, we hypothesized that PA under-estimators would be less resilient than PA over-estimators. Finally, we hypothesized that biased PA forecasts would moderate the impact of stress on affect, consistent with the idea that holding positive expectations about the future represents a further source of resilience to cope with daily events.

METHODS

We reported how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study (Simmons et al., 2012).

Sample

The sample size was calculated considering the correlations as the main analyses of the study. Assuming an overall moderate effect size of 0.3 (correlation), a significance level of 5%, a statistical power of 80%, and a bilateral contrast, the sample size calculation resulted in a sample of n = 82. Calculations were made with G*Power (Faul et al., 2007).

In total, 91 undergraduate students were recruited via online advertisements at the Jaume I University (Castellon, Spain). Participants with a score above 14 on the Patient Health

Questionnaire (PHQ-9) (Kroenke et al., 2001) and/or the Generalized Anxiety Disorder (GAD-7) (Spitzer et al., 2006) were excluded from the study (i.e., individuals with moderate/severe clinical conditions). Accordingly, there is evidencing showing that patients suffering from Major Depressive Disorder (MDD) or Generalized Anxiety Disorder (GAD) are negatively biased in affective forecasting (Wenze et al., 2012; Mathersul and Ruscio, 2019), which would make their inclusion together with non-clinical individuals problematic. Accordingly, 6 participants were excluded, thus leading to a final sample of $n = 85$. The sample was composed of 72 females and 13 males, and their mean age was 20.81 years (SD = 2.26). In our sample, the PHQ-9's internal consistency was $\alpha = 0.73$, whereas the GAD-7's internal consistency was $\alpha = 0.82$.

This study was approved by the ethics committee of the Jaume I University (Spain) (certificate number: CD/57/2019; reference: 41EA95C7D3C8747F0A37), and informed consent was obtained from all participants.

Measures

Forecasted Positive and Negative Affect

Participants were administrated the Spanish adaptation (Díaz-García et al., 2020) of the Positive and Negative Affect Schedule (PANAS) (Watson et al., 1988). The PANAS is composed of 10 items to measure PA and 10 items to assess NA. Previous research has shown the validity and reliability of the questionnaire (Sandín et al., 1999). In the present study, the original instructions "*Indicate the extent you have felt this way over the past week*" were changed to "*Indicate the extent you think you will feel over the next two weeks*" to evaluate forecasted as opposed to retrospective affect. In our sample, both the PA and the NA subscales showed good internal consistency (PA: $\alpha = 0.91$; NA: $\alpha = 0.78$).

Psychological Well-Being

Psychological well-being was assessed using the Spanish adaptation (Díaz-García et al., 2020) of the Ryff's Psychological Well-Being Scale (Ryff and Keyes, 1995; Ryff, 2005), which explores six different dimensions of psychological well-being: Autonomy (i.e., independence from external judgments and social prejudices: "I have confidence in my opinions, even if they are contrary to the general consensus"; "I judge myself by what I think is important, not by the values of what others think is important"), environmental mastery (i.e., the ability to take advantage of the environment to achieve personal goals: "I am quite good at managing the many responsibilities of my daily life"; "In general, I feel I am in charge of the situation in which I live"), personal growth (i.e., the sense of continuous self-improvements thanks to life experiences: "For me, life has been a continuous process of learning, changing, and growth"; "I have the sense that I have developed a lot as a person over time"), purpose in life (i.e., the sense of meaning in life, owning clear personal values and life goals: "I have a sense of direction and purpose in life"; "I enjoy making plans for the future and working to make them a reality"), positive relations (i.e., satisfactory and trusting relationships, as well as empathetic and warm attitude toward others: "People would describe me as a giving person, willing to share my time with others"; "I know that I can trust my friends, and they know they can trust me"), and self-acceptance (i.e., positive attitude toward the current and past self, as well as acceptance of both positive and negative personal qualities: "When I look at the story of my life, I am pleased with how things have turned out"; "When I compare myself to friends and acquaintances, it makes me feel good about who I am"). This scale has shown good psychometric properties (van Dierendonck, 2004). In our sample, all subscales demonstrated good internal consistency, except for autonomy and environmental mastery (self-acceptance: $\alpha = 0.87$; positive relation: $\alpha = 0.83$; autonomy: $\alpha = 0.64$; environmental mastery: $\alpha = 0.67$; personal growth: $\alpha = 0.83$; purpose in life: $\alpha = 0.78$).

Resilience

Resilience was assessed using the Spanish adaptation (Notario-Pacheco et al., 2011) of the 10-item Connor-Davidson Resilience Scale (CD-RISC10) (Campbell-Sills and Stein, 2007), a self-report scale with good psychometric properties (Singh and Yu, 2017; Shin et al., 2018) that measures resilience over the previous 30 days ("I can deal with whatever comes my way"; "I think of myself as a strong person when dealing with life's challenges and difficulties"). In our sample, the CD-RISC10 showed high internal consistency ($\alpha = 0.85$).

Openness to Future

The Openness to the Future Scale (OFS) is a 10-item self-report questionnaire that measures orientation toward the future, including positive expectations, a sense of competence to cope with daily events, and the acceptance of what can't be predicted. Some examples include: "I calmly accept that good and bad things will happen to me in life"; "I am very excited about future opportunities and challenges"; "I feel hopeful about what the future may bring." This scale has shown good psychometric properties both in community and clinical samples (Botella et al., 2018). In our sample, the OFS showed good internal consistency ($\alpha = 0.80$).

Ecological Momentary Affect (EMA) Measures

At each daily evaluation, participants were asked to complete three 100-point numerical scales (0 = not at all; 100 = extremely) evaluating momentary PA ("*To what extent are you experiencing positive emotions at this moment?*"), momentary NA ("*To what extent are you experiencing negative emotions at this moment*?"), and momentary stress ("*How would you rate your current level of stress?*"). Participants were also asked to rate the momentary level of seven positive emotions (happiness, fun, hope, serenity, excitement, pride, gratitude) using a 1-5 Likert scale ("*To what extent are you experiencing the following positive emotions at this moment?*; 1 = not at all; 5 = extremely). The sum of the seven scales reflected the momentary level of positive emotions.

Procedure

Participants were recruited via poster advertisements at the Jaume I University (Castellon, Spain). Students interested in the study were invited to the laboratory in order to receive more information about the investigation. Participants who met the inclusion criteria were invited to sign the informed consent and to complete the affective forecasting measure with the PANAS.

Repeated daily assessments were collected by means of Qualtrics, a web-based platform that allows to create and send customized online surveys at specific time points during the day. In the present study, participants were semi-randomly prompted three times a day for 2 weeks (between 9:30 – 14:00; 14:00 – 18:30; and 18:30 – 23:00) by means of an email. After receiving the notification, participants had 60 min to enter the weblink and complete the evaluation.

At the end of the study, participants returned to the laboratory and completed the following questionnaires: The Ryff's Psychological Well-being Scale, the CD-RISC and the OFS. Additionally, participants were asked whether something significant unexpectedly happened in the previous 2 weeks. This included any sudden and unforeseen positive and/or negative event that significantly affected their mood, thoughts, or behaviors. This question was introduced in order to exclude participants that, during the study, experienced an event that was impossible to anticipate (such as a sentimental breakup, the death of a closer person, or being hired at a new job), thus creating a biased mismatch between the predicted and experienced affect. However, no participant reported such significant events and there was no need for exclusion. A remuneration of 10 euros was given to participants who completed more than 60% of the EMA assessments.

Data Analysis

A summary of all the variables included in the analysis and their abbreviations is reported in **Table 1**. Forecasted affect refers to the PANAS-PA and PANAS-NA subscale scores collected at baseline. Experienced affect refers to mean PA and NA levels experienced during the 2-week EMA, and it was obtained by calculating the mean of the 42 possible PA and NA assessments for each participant. Besides, EMA scores refer to the 42 possible NA, PA, positive emotions and stress repeated assessments collected throughout the 2-week study.

To distinguish between future affect overestimation or underestimation, delta scores were computed. To have the same range of scores for forecasted (PANAS: 1-to-5 Likert scale) and experienced affect measures (EMA: 0–100 scale), PANAS values were transformed to Percent of Maximum Possible (POMP) Scores (Cohen et al., 1999; Fischer and Milfont, 2010). POMP scores express raw scores in terms of the maximum possible score and can range between 0 and 100, thus facilitating the comparison of data when scales and scoring methods are not consistent. POMP scores are calculated as follows: 100 × (raw-min) / (max-min), with min and max indicating the lowest and highest scores possible according to the scale adopted. POMP scores of forecasted affect were calculated as follows: POMP scores: 100 × (raw - 10)/(50 - 10). Delta scores were therefore computed as follows: Delta = (POMP forecasted affect – experienced affect). Positive scores reflected future affect overestimation, whereas negative scores reflected future affect underestimation.

Correlation analyses were conducted to explore the association between forecasted and experienced NA, and between forecasted and experienced PA. Moreover, Generalized Estimating Equations (GEEs) with an unstructured correlation matrix structure and Huber–White standard error estimates were used, introducing forecasted PA and NA as predictors of daily EMA-NA and EMA-PA scores. GEEs are designed to examine longitudinal repeated-measures data. Furthermore, GEEs are adequate to draw inferences by considering not only variations in affective experience over time within individuals, but also variations in affective experience between individuals (Liang and Zeger, 1986; Pavani et al., 2016). Forecasted and experienced PA (Paired sample *t*-test) and NA scores (Wilcoxon Signed Ranks Test) were compared to test the participants' ability to predict future affect. Also, delta scores distribution was explored, and their association with depressive and anxiety symptoms was investigated.

To confirm the first hypothesis, correlation analyses were conducted to explore the association between forecasted/experienced affect, delta scores, well-being, and openness to the future. GEEs with an unstructured correlation matrix structure were used introducing forecasted NA, forecasted PA, daily EMA-PA and daily EMA-NA simultaneously as predictors of psychological well-being.

To explore the association between affective forecasting and resilience, correlation analyses were conducted. Besides, multiple linear regressions were performed using well-being measures as dependent variables and resilience as the independent variable; in a second block, delta scores were included to explore significant improvements in the model.

Consistent with the third hypothesis, we performed GEEs with an unstructured correlation matrix structure and Huber–White standard error estimates including delta scores, daily EMA-stress scores and the interaction term as predictors of daily affect.

TABLE 1 | Summary of all the variables included in the analysis and their abbreviations.

Abbreviation	Variable
Forecasted PA	Anticipated PA – PANAS at baseline
Forecasted NA	Anticipated NA – PANAS at baseline
Experienced PA	Global average of EMA-PA assessments
Experienced NA	Global average of EMA-NA assessments
EMA-PA	PA repeated EMA assessments
EMA-NA	NA repeated EMA assessments
EMA-Stress	Stress repeated EMA assessments
EMA-positive emotions	Positive emotions repeated EMA assessments
Delta PA	(Forecasted PA – Experienced PA)
Delta NA	(Forecasted NA – Experienced NA)

RESULTS

Forecasted and Experienced Affect

An overview of the recruited sample is reported in **Table 2**. Overall, high compliance was obtained (*M* = 80.47%; SD = 18.44%), considering previous research exploring the extent to which participants tend to answer EMAs

TABLE 2 | Detailed information about the recruited sample and affect measures (GAD-7: Generalized Anxiety Disorder; PHQ-9: Patient Health Questionnaire).

Sample (*n* = 85)	
Demographics	
Age	20.81 (±2.26)
Sex	72 female/13 male
GAD-7	5.12 (±3.47)
PHQ-9	5.69 (±2.93)
Compliance (%)	80.47 (±18.44)
Affect	
Forecasted PA-pomp	50.21 (±18.48)
Forecasted NA-pomp	18.71 (±11.76)
Experienced PA	55.60 (±18.46)
Experienced NA	22.06 (±12.26)

(Colombo et al., 2018; Van Genugten et al., 2020). Compliance was associated with depressive ($r = -0.21$, $p = 0.05$) and anxiety symptoms ($r = -0.21$, $p < 0.05$), but not with age ($r = 0.18$, $p = 0.11$).

Forecasted and experienced PA ($r = 0.45$, $p < 0.001$) and NA levels ($r = 0.43$, $p < 0.001$) were significantly correlated, thus indicating a good degree of participants' self-insight about future affect. Forecasted PA significantly predicted EMA-PA scores ($B = 1.27$, SD = 0.18, 95% CI [0.91, 1.63]; $p < 0.001$); similarly, forecasted NA significantly predicted EMA-NA scores ($B = 0.94$, SD = 0.18, 95% CI [0.58, 1.30]; $p < 0.001$).

Participants forecasted lower levels of PA than what they experienced (forecasted PA-POMP: mean = 50.21, SD = ±18.48; experienced PA: mean = 55.60, SD = ±18.46; t (84) = −2.57, $p < 0.05$). Similarly, a significant difference was observed between forecasted NA and experienced NA scores (forecasted NA-POMP: mean = 18.71, SD = ±11.76; experienced NA: mean = 22.06, SD = ±12.26; Z = −2.60, $p < 0.01$). Mean delta PA was -5.40 (SD = 19.37), whereas mean delta NA was -3.35 (SD = 13.29), thus indicating a general tendency to underestimate future affective states. No significant correlation was observed between delta PA and delta NA ($r = -0.13$, $p = 0.25$).

Participants with higher depressive symptoms anticipated to experience higher NA ($r = 0.36$, $p < 0.001$) and lower PA levels ($r = -21$, $p = 0.05$). However, delta values were not significantly associated with PHQ-9 scores (delta PA: $r = -0.11$, $p = 0.30$; delta NA: $r = 0.20$, $p = 0.07$), thus indicating that individuals with higher depressive symptoms forecasted and actually experienced lower levels of PA and higher levels of NA. Differently, forecasted NA ($r = 0.65$, $p < 0.001$) was significantly associated with anxiety symptoms, and delta NA significantly correlated with ($r = 0.28$, $p < 0.01$) and predicted delta NA [$R^2 = 0.11$; $F(1, 83) = 10.30$; $B = 0.83$, SE = 0.03, 95% CI [0.03, 0.14]; $p < 0.01$], highlighting greater overestimation of future NA in the presence of increased anxiety symptoms.

Affective Forecasting and Well-Being

Table 3 shows the association between psychological well-being measures and forecasted/experienced affect. Forecasted PA (self-acceptance: $r = 0.53$, $p < 0.001$; positive relations: $r = 0.32$, $p < 0.01$; autonomy: $r = 0.43$, $p < 0.001$; environmental mastery: $r = 0.44$, $p < 0.001$; personal growth: $r = 0.42$, $p < 0.001$; purpose in life: $r = 0.42$, $p < 0.001$) but not experienced PA significantly correlated with all Ryff's subscales, revealing that participants holding more optimistic predictions of future PA reported greater psychological well-being. Additionally, forecasted NA showed a significant negative association with Ryff's subscales of self-acceptance ($r = -0.37$, $p < 0.001$), autonomy ($r = -0.27$, $p < 0.05$), environmental mastery ($r = -0.33$, $p < 0.01$), and personal grow ($r = -0.23$, $p < 0.01$), while experienced NA did not correlate with any of the well-being measures.

Table 3 also shows the association between biased affective forecasting and psychological well-being. Delta PA was significantly correlated with all Ryff's psychological well-being measures (self-acceptance: $r = 0.33$, $p < 0.01$; positive relations: $r = 0.28$, $p < 0.01$; autonomy: $r = 0.38$, $p < 0.001$; environmental mastery: $r = 0.26$, $p < 0.05$; personal growth: $r = 0.28$, $p < 0.05$; purpose in life: $r = 0.33$, $p < 0.01$): That is, positively biased PA forecasting was associated with enhanced perceived well-being. Consistently with our hypothesis, delta NA did not correlate with any of the well-being measures. When simultaneously included in a regression model to predict psychological well-being, delta PA was the only significant predictor of self-acceptance [$R^2 = 0.11$; $F(1, 82) = 5.21$; delta PA: $B = 0.10$, SE = 0.03, 95% CI [0.03, 0.16]; $p < 0.01$; delta NA: $B = -0.19$, SE = 0.04, 95% CI [-0.11, 0.07]; $p = 0.70$], positive relations [$R^2 = 0.11$; $F(1, 82) = 5.11$; delta PA: $B = 0.09$, SE = 0.03, 95% CI [0.02, 0.15]; $p < 0.05$; delta NA: $B = -0.07$, SE = 0.05, 95% CI [-0.17,.02]; $p = 0.13$], autonomy [$R^2 = 0.14$; $F(1, 82) = 6.91$; delta PA: $B = 0.10$, SE = 0.03, 95% CI [0.04,.16]; $p < 0.001$; delta NA: $B = -0.02$, SE = 0.04, 95% CI [-0.10, 0.06]; $p = 0.64$], environmental mastery [$R^2 = 0.09$; $F(1, 82) = 3.97$; delta PA: $B = 0.05$, SE = 0.02, 95% CI [0.01, 0.10]; $p < 0.05$; delta NA: $B = -0.4$, SE = 0.03, 95% CI [-0.11, 0.02]; $p = 0.19$], personal growth [$R^2 = 0.10$; $F(1, 82) = 4.53$; delta PA: $B = 0.08$, SE = 0.03, 95% CI [0.03, 0.13]; $p < 0.01$; delta NA: $B = -0.01$, SE = 0.04, 95% CI [-0.09, 0.07]; $p = 0.81$], and purpose in life [$R^2 = 0.11$; $F(1, 82) = 4.94$; delta PA: $B = 0.09$, SE = 0.03, 95% CI [0.03, 0.15]; $p < 0.01$; delta NA: $B = -0.01$, SE = 0.04, 95% CI [-0.09, 0.06]; $p = 0.78$].

Besides, forecasted PA (forecasted PA: $r = 0.47$, $p < 0.001$), and forecasted NA ($r = -0.21$, $p = 0.05$) were significantly associated with OFS. Additionally, only delta PA was significantly associated with OFS ($r = 0.25$, $p < 0.05$), suggesting that participants who overestimated future PA were more likely to report greater openness to the future.

Using GEEs, forecasted affect and EMA affect scores were simultaneously included as predictors of Ryff's well-being measures (**Table 4**). Forecasted PA was the only significant predictor of positive relations ($B = 0.54$, SE = 0.08, 95% CI [0.09, 0.42]; $p < 0.01$), personal growth ($B = 0.27$, SE = 0.06, 95% CI [0.14, 0.39]; $p < 0.001$), and purpose in life ($B = 0.31$, SE = 0.07, 95% CI [0.17, 0.45]; $p < 0.001$), whereas both forecasted NA and forecasted PA significantly predicted self-acceptance (forecasted PA: $B = 0.39$, SE = 0.06, 95% CI [0.27, 0.51]; $p < 0.001$; forecasted NA: $B = -0.33$, SE = 0.09, 95% CI [-0.51, -0.15]; $p < 0.001$), autonomy (forecasted PA: $B = 0.28$,

UNDERSTANDING AND OVERCOMING BIASES IN JUDGMENT AND DECISION-MAKING WITH REAL-LIFE CONSEQUENCES

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TABLE 4 | Generalized Estimating Equation (GEE) models introducing forecasted PA, forecasted NA, daily EMA-PA and daily EMA-NA as predictors of well-being subscales.

	Self-acceptance		Positive relations		Autonomy		Environmental mastery		Personal growth		Purpose in life	
	B	*SE*	*B*	*SE*	*B*	*SE*	*B*	*SE*	*B*	*SE*	*B*	*SE*
Coefficients												
Forecasted PA	0.39***	0.06	0.25**	0.08	0.28***	0.07	0.24***	0.05	0.27***	0.06	0.31***	0.07
Forecasted NA	-0.33***	0.09	-0.24	0.14	-0.24*	0.11	-0.29**	0.09	-0.16	0.09	-0.15	0.11
Daily PA	0.000	0.002	-0.001	0.001	-0.001	0.001	0.001	0.0003	0.000	0.001	-0.001	0.001
Daily NA	-0.002	0.002	0.000	0.001	0.000	0.001	<0.001	0.0002	-0.001	0.001	0.000	0.001

*B = unstandardized regression coefficient; *p < 0.05, **p < 0.01, ***p < 0.001 (PA: Positive affect; NA: Negative affect).*

SE = 0.07, 95% CI [0.15, 0.41]; $p < 0.001$; forecasted NA: $B = -0.24$, SE = 0.11, 95%CI [-0.45, -0.02]; $p < 0.05$), and environmental mastery (forecasted PA: $B = 0.24$, SE = 0.05, 95% CI [0.14, 0.34]; $p < 0.001$; forecasted NA: $B = -0.29$, SE = 0.09, 95% CI [-0.47, -0.11]; $p < 0.01$). Interestingly, experienced daily affect did not predict any of the well-being measures.

Affective Forecasting, Resilience and Stress

Forecasted PA ($r = 0.62$, $p < 0.001$) and delta PA ($r = 0.37$, $p < 0.001$) but not experienced PA ($r = 0.19$, $p = 0.08$) significantly correlated with CD-RISC. That is, holding optimistic expectations regarding the future and overestimating PA were associated with higher levels of resilience. Besides, forecasted ($r = -0.27$, $p = < 0.05$) and experienced NA ($r = -0.27$, $p < 0.05$) but not delta NA ($r = 0.05$, $p = 0.67$) did show a significant association with resilience (**Table 3**).

Resilience was a significant positive predictor of psychological well-being (self-acceptance: $R^2 = 0.32$; $F(1, 83) = 39.87$; $B = 0.48$, SE = 0.08, 95% CI [0.33, 0.63]; $p < 0.001$; positive relations: $R^2 = 0.14$; $F(1, 83) = 13.54$; $B = 0.34$, SE = 0.09, 95% CI [0.16, 0.52]; $p < 0.001$; autonomy: $R^2 = 0.21$; $F(1, 83) = 21.82$; $B = 0.35$, SE = 0.07, 95% CI [0.20, 0.50]; $p < 0.001$; environmental mastery: $R^2 = 0.36$; $F(1, 83) = 46.93$; $B = 0.38$, SE = 0.06, 95% CI [0.27, 0.49]; $p < 0.001$; personal growth: $R^2 = 0.28$; $F(1, 83) = 32.22$; $B = 0.38$, SE = 0.07, 95% CI [0.28, 0.52]; $p < 0.001$; purpose in life: $R^2 = 0.35$; $F(1, 83) = 44.22$; $B = 0.46$, SE = 0.07, 95% CI [0.32, 0.59]; $p < 0.001$). The inclusion of delta PA significantly increased the variance explained by the model for autonomy ($R^2 = 0.62$, $\Delta R^2 = 0.05$, $F(2, 82) = 14.57$, CD-RISC: $B = 0.28$, SD = 0.08, 95% CI [0.13, 0.44]; $p < 0.001$; delta PA: $B = 0.07$, SD = 0.03, 95% CI [0.01, 0.12]; $p < 0.05$), and a close-to-significance trend was observed in the model predicting positive relations ($R^2 = 0.17$, $\Delta R^2 = 0.03$, $F(2, 82) = 8.40$, CD-RISC: $B = 0.28$, SD = 0.10, 95% CI [0.09, 0.47]; $p < 0.001$; delta PA: $B = 0.06$, SD = 0.03, 95% CI [-0.01, 0.13]; $p = 0.08$).

Finally, GEE analyses were conducted to explore whether EMA-stress scores and delta PA significantly predicted EMA-affect. EMA-NA was significantly predicted by EMA-stress level but not by delta PA (EMA-stress: $B = 0.46$, SD = 0.02, 95% CI [0.42, 0.51]; $p < 0.001$; Delta PA: $B = -0.04$, SD = 0.04, 95% CI [-0.11, 0.03]; $p = 0.26$), thus underlying the fundamental role of stress on NA affect ratings (i.e., the experience of higher stress was associated with higher levels of perceived NA). Similarly, EMA-stress scores but not delta PA significantly predicted positive emotion level (stress: $B = -0.09$, SD = 0.01, 95% CI [-0.11, -0.08], $p < 0.001$; Delta PA: $B = -0.02$, SD = 0.02, 95% CI [-0.07, 0.03], $p = 0.46$). Notably, a significantly different association between EMA-NA and stress was observed as a function of delta values (stress: $B = 0.45$, SD = 0.02, 95% CI [0.40, 0.50], $p < 0.001$; Delta PA: $B = 0.02$, SD = 0.04, 95% CI [-0.05, 0.09], $p = 0.60$; interaction: $B = -0.003$, SD = 0.001, 95% CI [-0.01, 0.00]; $p < 0.05$). As indicated by the negative beta coefficient of the interaction (Suso-Ribera et al., 2019), as delta PA becomes more positive (i.e., future PA is overestimated), the contribution of stress on NA is reduced. A significantly different association between EMA-positive emotion and stress was also observed as a function of PA delta values (stress: $B = -0.09$, SD = 0.01, 95% CI [-0.10, -0.07], $p < 0.001$; Delta PA: $B = -0.04$, SD = 0.03, 95% CI [-0.09, 0.01], $p = 0.12$; interaction: $B = 0.001$, SD = 0.0003, 95% CI [0.001, 0.002]; $p < 0.001$). As the interactive effect of delta PA and stress on positive emotion level is positive, this means that, as delta PA becomes more negative (i.e., forecasting becomes more negatively biased and future PA is underestimated), stress becomes more deleterious for positive emotions. In other words, it is possible to suggest that, despite the increase in experienced stress, subjects with positively biased PA forecasting (i.e., those who overestimated future positive affective states) reported lower NA levels and higher positive emotions.

Regarding delta NA, EMA-PA (stress: $B = -0.36$, SD = 0.03, 95% CI [-0.42, -0.30], $p < 0.001$; Delta NA: $B = 0.14$, SD = 0.11, 95% CI [-0.07, 0.36], $p = 0.19$) was significantly predicted by EMA-stress but not delta NA, whereas EMA-positive emotions were significantly predicted by both stress level and delta NA (stress: $B = -0.14$, SD = 0.02, 95% CI [-0.18, -0.09], $p < 0.001$; Delta NA: $B = 0.51$, SD = 0.16, 95% CI [0.83, 0.19], $p < 0.01$). No significant interaction effect was observed.

DISCUSSION

So far, a growing body of literature has explored people's ability to forecast emotional experiences in relation to specific future events. In the current study, instead, we investigated affective forecasting as a future-oriented disposition, asking participants to estimate their affect during a 2-week period.

The main aim of the present study was to explore whether biased affective forecasting was associated with perceived psychological well-being, consistently with the hypothesis that the ability to estimate future emotional experiences constitutes a future-oriented strategy to regulate emotions (Goodhart, 1985; Totterdell et al., 1997).

Aligned with the previous literature (Buehler and McFarland, 2001; Wirtz et al., 2003), participants in the present study showed a good degree of insight about their future PA and NA levels. A significant discrepancy between forecasted and experienced affect was also observed, and participants showed a somewhat pessimistic view of the future. These results diverge from what has been revealed by a growing body of literature exploring affective forecasting in relation to specific future events. People would indeed overestimate the impact of both positive and negative future events (Wilson and Gilbert, 2003), due to an excessive focus on a single event in isolation without considering the general context and background distractions (Wilson et al., 2000). This phenomenon, called *focalism*, does not occur when forecasting general emotional states, which could explain the dissimilar results observed in this study. Besides, our results confirmed the role played by depressive and anxiety symptoms on affective forecasting (Wenze et al., 2012), and the presence of mild symptoms was associated with a negative bias, which is consistent with the previous literature (Craske and Pontillo, 2001; Gotlib and Joormann, 2010; Colombo et al., 2019c). Specifically, depressive symptoms were associated with future NA overestimation and PA underestimation, whereas anxiety symptoms only significantly correlated with future NA overestimation. As evidenced by the tripartite model, indeed, depression and anxiety share the same pattern of enhanced NA, whereas low levels of PA and anhedonia are only typical of depressive conditions (Clark and Watson, 1991).

Coherently with the first hypothesis, participants holding more positive estimations of future PA and positively biased PA forecasting reported greater psychological well-being on almost all Ryff's subscales. Results also confirmed the hypothesis that biased NA estimations (i.e., underestimating or overestimating NA) were not significantly associated with well-being, which supports the idea that a bias in negative affective forecasting does not affect psychological well-being. Besides, it is of particular interest that psychological well-being was significantly predicted by forecasted but not experienced affect. In other words, our results suggest that psychological well-being is a grounded dimension: Rather than momentary affect and daily events, psychological well-being seems to be more strongly associated with resilience and coping skills, such as holding an optimistic, even if distorted, vision of the future. Accordingly, delta PA but not delta NA was significantly associated with OFS, which in turn has been found to be associated with better mental health (Botella et al., 2018).

Our results also confirmed the second hypothesis. Contrary to delta NA, forecasted PA as well as delta PA were strongly associated with resilience, and participants holding more positive estimations of future PA and overestimating future PA were found to be more resilient. The multiple regression analyses also showed that delta PA in addition to resilience improved the prediction of some well-being dimensions, thus confirming the idea that positively biased affective forecasting may constitute a coping skill that increases individuals' abilities to deal with daily hassles. Consistently, and confirming our third hypothesis, delta PA significantly moderated the impact of daily stress on daily affect. This means that, when experiencing high levels of stress, subjects who tended to overestimate future PA reported lower NA and higher positive emotions than subjects who showed a tendency to underestimate it. A positive attitude toward the future seems therefore to be an adaptive coping resource in highly stressful situations, allowing to maintain better levels of momentary affect despite the presence of intense stressors.

Even though a long tradition of research considered cognitive distortions as maladaptive mechanisms associated with worse mental-health (Jahoda, 1953), there is now increasing evidence revealing that, in certain circumstances, cognitive biases may rather be adaptive (Taylor and Brown, 1988). Specifically, people's perception of the future has been shown to affect mental health (Weinstein, 1980; Mikus et al., 2017), and openness to the future has been associated with higher positive emotions, psychological well-being, and self-esteem (Botella et al., 2018). This seems to be strictly connected to the construct of optimism, defined as "[…] *a mood or attitude associated with an expectation about the social or material future*" (Tiger, 1979), which has been shown to increase people's skills to deal with challenging events (Carver et al., 2012) and to be associated with higher subjective well-being, health and life success (Forgeard and Seligman, 2012). Beyond the conceptualization of optimism as an explanatory style (Seligman, 1991), the definition of optimism as one's disposition to hold favorable or unfavorable expectations and beliefs about the future seems to be more coherent with our results (Carver and Scheier, 2014). In this regard, we suggest that positively biased affective forecasting may in part reflect one's dispositional optimism, and it may constitute a mechanism that increases people's skills to deal with daily events, thus having a positive impact on psychological well-being.

Although optimism toward the future is likely to foster coping skills and promote well-being, it is important to note that holding a positively biased view of reality can also be maladaptive in certain circumstances (Chang et al., 2009). For instance, there is evidence showing that optimistic individuals are more likely to show gambling behaviors (Gibson and Sanbonmatsu, 2004) or to report lower motivation when trying to quit smoking (Weinstein et al., 2004). As suggested by Forgeard and Seligman (2012), "*the most adaptive outlook seems therefore to be mostly optimistic, tempered with small doses of realistic pessimism when needed*": For example, to avoid disappointment when idealizing something that it is quite improbable to achieve. A flexible rather than rigid positively biased perspective seems therefore to be the key of well-being. Future research should investigate the potential role of flexibility on affective forecasting and health-related outcomes.

Besides, the findings of the current study have to be considered in light of some limitations. In the present study, we excluded individuals with clinically relevant depressive and anxiety symptoms in order to control for the confounding effect of a pathological negative bias (Wenze et al., 2012; Mathersul and Ruscio, 2019). However, there are other individual factors, which have been shown to play a fundamental role in affective forecasting abilities. For example, personality has been found to explain 30% of the concordance between anticipated and experienced emotional experiences (Zelenski et al., 2013; Hoerger et al., 2016), and introverted as compared to extroverted individuals tend to anticipate more unpleasant emotions and less positive emotional states. Furthermore, there is evidence showing that people who are high in emotional intelligence are more accurate at encoding and predicting their emotional reactions (Dunn et al., 2007b; Hoerger et al., 2012). Altogether, these results suggest that affective forecasting is a complex cognitive phenomenon, in which many different factors are likely to reciprocally interact with each other. In addition to the previous, it is also possible to hypothesize that an individual's response style to positive emotional states (Feldman et al., 2008) could be an additional element that influences positive affective forecasting. Accordingly, habitual positive ruminators (i.e., those who tend to reflect on positive events, self-qualities, and pleasant emotions) might be more likely to be positively biased toward their future emotional states, as a result of an over-focus on positive emotional experiences and/or qualities. Future research is needed to prove this hypothesis and, more generally, to build a broader framework in which all the aforementioned factors are concurrently considered.

It is also important to note that the sample was mainly composed of undergraduate female students. Future research should investigate whether other factors such as sex or age may entail different effects on affective forecasting. To date, elderly people as compared to young individuals have been shown to recall more positive than negative information, a phenomenon called positivity effect (Reed and Carstensen, 2012; Carstensen and DeLiema, 2018). However, this positivity effect does not seem to influence elderly's affective forecasting (Nielsen et al., 2008), who have been shown to be accurate rather than positively biased in the estimation of future affective states, thus suggesting that "[...] *people may correct for this bias as they age*." Accordingly, the results observed in our study may not be generalizable to all populations, and it is possible that positively biased estimations of future states are more common in young-to-middle adulthood.

Additionally, the methodological nature of this study only allows to draw correlational conclusions, and more evidence is needed to clarify the potential causal role of biased forecasting on perceived well-being and resilience. Hence, experimental designs could complement existing evidence assuming causal inferences. Future studies should also consider the potential consequences of this cognitive bias on behaviors, exploring whether holding positive expectations about future emotions may also affect people's decisions in daily life. It might be possible, indeed, that biased affect predictions influence daily behavioral attitudes (such as avoiding or joining specific situations), which in turn may influence well-being. Finally, the use of single items to measure EMA-PA and EMA-NA might not capture the complexity of momentary affect, as opposed to the use of the PANAS for the assessment of affective forecasting. However, we decided to use single items in order to reduce participants' burden and increase adherence rates (Colombo et al., 2018, 2019c), similarly to previous studies (Suso-Ribera et al., 2018). Besides, the autonomy and environmental mastery Ryff's subscales showed low internal consistency, and analyses including both measures have to be taken with caution.

To conclude, the benefits of enhancing PA as a way to promote mental health and well-being has been widely supported (Pressman et al., 2018), thus suggesting the importance of developing specific interventions to potentiate people's strategies to regulate positive emotions. In particular, it is of utmost importance to clearly determine the importance of developing a positive bias as well as an optimistic rather than pessimistic attitude toward the future. Besides, it is arguable that the complex dynamic of emotional and cognitive processes that intrinsically conform the regulatory process of individuals does not need evaluative precision but rather intrinsic coherence that the future will be possible to cope with.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are openly available in OSF at https://doi.org/10.17605/OSF.IO/JFS3K (Colombo et al., 2020).

ETHICS STATEMENT

This study was approved by the ethics committee of the Jaume I University (Spain) (certificate number: CD/57/2019; reference: 41EA95C7D3C8747F0A37), and informed consent was obtained from all participants. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

DC contributed to the conception and design of the work; the acquisition, analysis and interpretation of data; and the drafting the manuscript. JF-Á and CS-R equally contributed to the critical revision of the work. PC, AG-P, CB, and GR contributed to writing—review processes, editing and supervision.

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Recognizing a Heart Attack: Patients' Knowledge of Cardiovascular Risk Factors and Its Relation to Prehospital Decision Delay in Acute Coronary Syndrome

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In acute coronary syndromes (ACSs), longer decision delay – the time patients wait before seeking medical attention after symptoms have started – increases the risk of complications and death. However, many patients wait much longer than recommended and research is needed investigating how patient decision delay can be reduced. In a cross-sectional study of 120 ACS survivors, we investigated the relationship between knowledge of cardiovascular risk factors and decision delay. Several days after the onset of a cardiac event, patients completed a questionnaire measuring demographics, decision delay, objective knowledge of cardiovascular risks factors and of ACS symptoms, and subjective perceptions of symptoms during the cardiac episode. Relevant clinical data were extracted from patients' medical records. In a multiple linear regression analysis, controlling for demographic and clinical factors, objective knowledge of cardiovascular risk factors and ACS symptoms, and subjective attributions of symptoms to a cardiac cause were related to shorter decision delays. Among patients with relatively high knowledge of risk factors, only 5% waited more than 1 h to seek help, compared to 22% among patients with relatively low knowledge. These results suggest that knowledge of the factors that increase the risk of developing cardiovascular disease could play a role in patient decision making during an acute cardiac event. We discuss methodological issues and potential underlying mechanisms related to decision heuristics and biases, which can inform future research.

Keywords: acute coronary syndrome, patient decision making, prehospital delay, knowledge, decision delay, heart attack, cardiovascular risk

INTRODUCTION

Cardiovascular disease is the most common cause of death worldwide, responsible for 25% of deaths in Europe and causing more premature deaths than cancer (Heron, 2016; World Health Organization, 2016; World Health Statistics, 2018). The majority of deaths from cardiovascular disease are due to coronary heart diseases including acute coronary syndromes

(ACSs) – responsible for 43% of deaths due to cardiovascular disease (Turpie, 2006; American Heart Association, 2016; World Health Statistics, 2018). ACSs usually manifest with chest pain or discomfort, pain in one or both arms, pain in the jaw, neck, back, or stomach, and shortness of breath, among others.

Rapid action is crucial in the management of ACS, because a longer prehospital delay – referring to the time from symptom onset to receiving treatment – has been linked to worse clinical outcomes and increased mortality (Rollando et al., 2012; Guerchicoff et al., 2014). However, results of previous interventions aiming to reduce patients' prehospital delay were mixed and it is not clear what components of these interventions increased their success (Mooney et al., 2012; Farquharson et al., 2019). Further research is needed to shed light on the factors that could reduce prehospital delays and thus improve patient outcomes.

Previous research has investigated the effect of socio-demographic, clinical, and situational factors on prehospital delay. For instance, older adults, females, patients with relatively low socioeconomic backgrounds and those with chronic diseases have longer prehospital delays (Moser et al., 2006; Khraim and Carey, 2009; Wechkunanukul et al., 2017). Similarly, patients who live alone or are alone at symptom onset, patients who do not call an ambulance but consult with a physician, and those who suffer the cardiac episode during daytime also have longer prehospital delays (Moser et al., 2006; Wechkunanukul et al., 2017).

A large body of research has also investigated cognitive and emotional factors related to prehospital delays. To illustrate, a recent systematic review of 57 studies conducted in 23 countries concluded that social concerns such as embarrassment in asking others for help or worry about troubling others were not systematically related to prehospital delays (Arrebola-Moreno et al., 2020a). In contrast, patients who attributed symptoms to a cardiac cause, perceived symptoms as serious, and felt anxiety in response to symptoms report shorter prehospital delays (Arrebola-Moreno et al., 2020a). Overall this literature indicates that symptom attribution to cardiac as opposed to other causes such as muscular, respiratory or digestion problems, is fundamental to speed up help-seeking.

Several studies showed that patient *decision* delay – the time elapsed between symptom onset and the moment patients decide to seek medical attention – is one of the major contributors to prehospital delays (see **Figure 1**; Ottesen et al., 2004; Moser et al., 2006; Mackay et al., 2014; Wechkunanukul et al., 2017). Thus, a potentially effective strategy for reducing prehospital delays would be to improve patient decision making. However, most previous studies measured total prehospital delay without differentiating the patient decision delay component (Mackay et al., 2014). In fact, reviews show that only between 18 and 33% of studies report patient delays in decision making (Mackay et al., 2014; Arrebola-Moreno et al., 2020a). This is an important shortcoming because factors such as patient knowledge or perceptions are unlikely to influence health system delays; thus, considering total prehospital delay instead of only patient decision delay to study the influence of patient-related factors introduces avoidable error variance. In the current research, we investigated patient decision delay and we focused on its relationship with patients' knowledge and perceptions.

To be able to assign the experienced symptoms to a heart problem such as ACS, patients would need to know what the typical symptoms of ACS are. Such symptom knowledge is usually assessed with objective measures (i.e., patients' correct recognition of the symptoms in a test-like questionnaire) or subjective measures (patients' self-reported knowledge of the symptoms before the cardiac event). However, both types of measures have shown mixed results in relation to prehospital delays (Arrebola-Moreno et al., 2020a).

It is possible that symptom knowledge is not sufficient to speed up decision making if individuals do not know that they are at risk of suffering an ACS. There are multiple risk factors that make it more likely to suffer coronary heart disease including older age, smoking, diabetes, hypertension, and obesity (American Heart Association, 2016). However, research shows that people are not generally aware of these risk factors (Erhardt and Hobbs, 2002; Jensen and Moser, 2008; Wartak et al., 2011), and those with relatively low knowledge may underestimate the probability of experiencing a cardiac event (Lefler and Bondy, 2004; Darawad et al., 2016).

In the current research, we investigated for the first time whether knowledge about cardiovascular risk factors is related to decision delay in patients experiencing an ACS. The focus on decision delay rather than total prehospital delay would be particularly relevant for the current research. The rationale is that the latter is not only influenced by patients' decision making but also by other factors that are out of patients' control (e.g., health system delays). To account for other factors that could influence patient decision making (Nguyen et al., 2010; Arrebola-Moreno et al., 2020a), we also investigated the effect of patients' objective knowledge of ACS symptoms, subjective attributions of symptoms to cardiac causes, perceived severity of symptoms, and demographic factors. Our hypothesis was that patients' knowledge about cardiovascular risk factors would be uniquely related to decision delay after accounting the effect of the other factors.

MATERIALS AND METHODS

This was a cross-sectional retrospective study of ACS patients admitted to the Cardiology Department of the University Hospital Virgen de las Nieves (Granada, Spain) who underwent a percutaneous coronary intervention (PCI) as part of the management of ACS between March 2017 and April 2019. The study was completed on average 4.67 days (95% CI 4.24–5.09) after the cardiac event.

All participants signed an informed written consent before participation and the Hospital Ethics Committee approved the study. The inclusion criteria were: (a) having been diagnosed with an ACS, (b) being younger than 75, and (c) being fluent in Spanish. The exclusion criteria were having an inflammatory disease or a neurological problem that prevented participation in the study. Patients were selected based on these criteria by a qualified cardiologist who extracted information about the

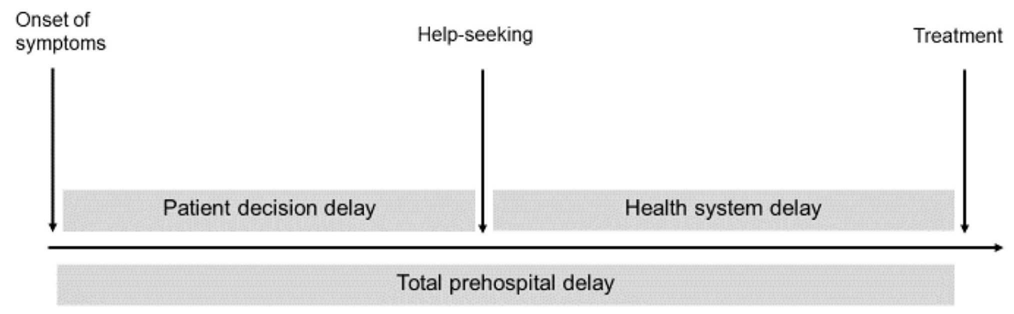

FIGURE 1 | Phases of prehospital delay.

final diagnosis. To minimize the exclusion of participants due to fatigue, illiteracy, or other reading difficulties the researcher offered help to all patients and gave detailed instructions.

Participants completed a survey that started with assessment of standard data for studies in ACS patients (demographics, family history of cardiovascular disease, anthropometric data, and healthy habits). Participants then completed the measures described below, including knowledge of ACS symptoms, knowledge of CV risk factors, prehospital delay, and part of the modified Response to Symptoms Questionnaire (based on Burnett et al., 1995; Dracup and Moser, 1997)[1].

A priori analysis with G*power[2] assuming alpha = 0.05, power = 0.80, and a total of 10 predictors indicated that to detect an effect size of $R^2 = 0.06$ for one tested predictor 126 participants would be required (see OSF: doi: 10.17605/OSF.IO/CEHN7). The choice of effect size was based on the average documented effect size of diverse psychological factors on prehospital decision delay in a previous study in this population (R^2 between 0.05 and 0.07) (Arrebola-Moreno et al., 2020b). Because we expected some participant attrition (e.g., due to missing clinical records, incomplete questionnaires or final diagnosis determined not to be ACS), we decided to invite a minimum of 150 patients (+20% of the required sample size).

Measures

Clinical Information

The following measures were obtained from patients' medical records: (a) number of days elapsed from cardiac event to completion of the questionnaire, (b) cardiovascular disease history – e.g., any previous myocardial infarction or ischemic disease, (c) smoking – i.e., non-smoker or smoker, (d) history of diabetes, (e) history of hypertension, (f) body mass index – i.e., weight (kg)/height (m)^2, (g) type of myocardial infarction – i.e., ST-segment elevation myocardial infarction (STEMI) or a non-STEMI, (h) obstructed arteries – i.e., number of obstructed vessels, (i) ejection fraction (EF) – i.e., the amount of blood that is pumped out of the ventricles, considering an EF of <35%, 35–45%, 45–55%, and >55% as very reduced, moderately reduced, slightly reduced, and normal respectively, and (j) revascularization – i.e, complete or incomplete revascularization.

Decision Delay

It was calculated as the time difference, in minutes, between symptom onset and the patients' decision to seek medical attention. Patients were asked to determine (1) at what time symptoms started and (2) at what time they decided to seek medical attention (e.g., when they decided to go to the hospital or call an ambulance), and we computed the difference between the two time points. This measure was validated in a previous study against patients' troponin levels on arrival at the hospital (see Petrova et al., 2017; Arrebola-Moreno et al., 2020b). Troponin is a protein that is released when the heart muscle has been damaged and is currently the gold standard for ACS diagnosis and management (ECS guidelines; Roffi et al., 2016). It has a known progression curve that make it a useful additional measure of the time elapsed from ASC onset.

Knowledge of Cardiovascular Risk Factors

This was assessed with a questionnaire measuring participants' knowledge of the effect of 52 factors on the risk of developing cardiovascular disease designed for this research. There were four types of factors: modifiable factors (24 items, e.g., smoking cigarettes and eating fresh vegetables), uncontrollable factors (7 items, e.g., age – e.g., older than 65), psychosocial factors (13 items, e.g., having social support), and fictitious causes/filler items (8 items, e.g., being bitten by a mosquito)[3]. The selection of factors was based on guidelines for the prevention of cardiovascular disease and further scientific literature on risk and protective factors for cardiovascular disease (Winkleby et al., 1992; Myrtek, 2001; Rosengren et al., 2004; Grande et al., 2012; Rozanski, 2014; Khera et al., 2016; Piepoli et al., 2016; World Heart Federation, 2017).

Participants were asked to indicate for each factor what they thought its effect was on the risk of developing cardiovascular disease using a 5-point scale ranging from "it reduces the risk very much" to "it increases the risk very much" with a neutral point indicating that "it has no effect." Items were scored as

[1]The survey contained a second part addressing a different research question regarding willingness to adhere to lifestyle recommendations, which will be reported elsewhere.

[2]gpower.hhu.de

[3]The fictitious causes were used as filler items. These were factors that the accumulated scientific evidence had discarded as potential contributors to cardiovascular risk and risk factors for other diseases such as cancer or infectious diseases that have no important bearing on cardiovascular risk.

correct if patients correctly identified whether the item was a risk factor (it increases the risk), a protective factor (it reduces the risk), or it has no effect. We calculated the sum of the number of correct answers for each category. The final score was the total number of correct answers excluding the filler items (i.e., modifiable + uncontrollable + psychosocial factors).

Knowledge of ACS Symptoms

This was measured using the ACS response index (Riegel et al., 2007) that lists 21 predefined symptoms, including arm pain, weakness/fatigue, sweating, and chest discomfort. Patients were asked to indicate whether they thought it was a symptom of heart attack (yes/no) or they did not know. The final score was calculated as the sum of the number of correctly identified symptoms.

Modified Response to Symptoms Questionnaire

Participants answered four multiple-choice questions evaluating (a) what symptoms they experienced, (b) where they were when the symptoms started, (c) whom they were with, (d) what they thought the problem was, and (e) the perceived symptom severity (i.e., how severe they thought the symptoms were at onset, ranging from 1 "not at all severe" to 6 "very severe") (Burnett et al., 1995; Dracup and Moser, 1997). From the responses to (d), the variable "attribution to a cardiac origin" was created, where responses indicating a heart problem were coded as 1 and the rest (e.g., stomach, muscular, dental problems, fatigue, etc.) were coded as 0.

Data Analyses

First, we describe our sample using descriptive statistics. The variable decision delay was positively skewed, so median and interquartile ranges were considered and the variable was log-transformed for analysis. Second, to investigate the relationship between prehospital decision delay and knowledge of cardiovascular risk factors, knowledge of ACS symptoms, attribution to a cardiac origin, and perceived symptom severity, we computed bivariate Pearson correlations, followed by multiple linear regression analyses.

TABLE 1 | Demographic and clinical characteristics of the sample (categorical variables).

	Number	Percentage
Age > 60 years	58	48
Age > 70 years	13	11
Sex: Male	83	69
Education		
Low (no or primary education)	66	55
Medium (secondary education)	12	10
High (tertiary education)	42	35
Acute coronary syndrome severity		
STEMI	53	41
Ejection fraction		
Very reduced	11	9
Moderately reduced	19	16
Slightly reduced	22	18
Normal	62	52
Complete revascularization	72	60
Risk lifestyle/classical factors		
Overweight (BMI ≥ 25)	102	85
Obesity (BMI ≥ 30)	49	41
Smoker	54	45
Cardiovascular disease history	22	18
Diabetes	31	26
Hypertension	63	53
Modified response to symptoms questionnaire		
Where were you when symptoms started?		
Home	81	68
Work	7	6
Car	7	6
Public place	15	13
Other	10	8
Whom were you with when symptoms started?		
Alone	30	25
Partner	53	44
Relative(s)	19	16
Friend(s)	7	6
Workmate(s)	7	6
Other	4	3
Attributed symptoms to cardiac origin	40	33

STEMI, ST-segment elevation myocardial infarction; BMI, body mass index.

RESULTS

During the study period the participating cardiologist identified 207 patients fulfilling the inclusion criteria of which 156 were invited to participate. From these, 140 agreed to participate and 120 returned completed questionnaires. Thus, final sample size was 120 (69.2% male, age μ = 59.87, *SD* = 8.80, range from 41 to 75). Descriptive statistics for all study variables are presented in **Tables 1**, **2**.

Patient Characteristics

As is typical for ACS patients, participants had characteristics consistent with high cardiovascular risk (see **Table 1**). The majority of patients were males, over 60 years, overweight, and with a history of hypertension. Forty-five percent were smokers, 26% had diabetes, and 18% had previous history of cardiovascular disease. In fact, considering age and the classical risk factors in **Table 1**, 98% of patients had at least one relevant cardiovascular risk factor, which put them at high cardiovascular risk; in particular, 8% had one, 28% had two, 19% had three, and 43% four or more risk factors.

Decision Making During the Cardiac Episode

From the whole sample, 40% (*N* = 48) reported a decision delay less or equal to 30 min; 16.7% (*N* = 20) reported a delay between 30 and 60 min; the remaining 43.3% (*N* = 52) reported a delay longer than 60 min. The majority of patients were at home, alone

TABLE 2 | Descriptive statistics for the sample (continuous variables).

	Mean	*SD*	Min–max	Range	Missing (%)
Age, years SD	59.86	8.80	41.00–75.00	–	1 (0.8)
Obstructed arteries	1.53	0.85	0–3	–	5 (4)
BMI, kg/m^2	29.33	4.94	20.31–53.53	–	0 (0)
Decision delay*	60.00	133.75	1.00–1440.00	–	4 (3)
Knowledge of cardiovascular risk factors					
Modifiable factors	20.37	2.05	13–23	0–24	5 (4)
Uncontrollable factors	5.19	0.95	2–7	0–7	5 (4)
Psychosocial factors	9.54	2.01	0–13	0–13	5 (4)
Total risk factors	39.58	3.76	23–45	0–44	5 (4)
Fictitious causes	3.52	1.29	1–7	0–7	4 (3)
Knowledge of ACS symptoms					
ACS symptoms	12.67	4.03	0–20	0–21	0 (0)
Modified response to symptoms questionnaire					
Perceived symptom severity	3.67	1.71	0–6	0–6	4 (3)

**Decision delay is presented as median and interquartile range; BMI, body mass index.*

or with a partner when symptoms started; and only 33% correctly attributed symptoms to a cardiac cause (**Table 1**).

Knowledge

Overall knowledge of ACS symptoms was low-to-average, with 53% of patients correctly identifying fewer than 14 out of 21 symptoms (see also **Table 2**). In contrast, knowledge of cardiovascular risk factors was relatively high, with a median of 41 (out of 44). The percentages of patients giving correct answers to each item from the risk factors questionnaire are presented in **Table 3**. Participants correctly recognized most of the modifiable risk and protective factors but only a few of the uncontrollable and psychosocial factors (although recognition was still high on average). Among the less recognized factors were HDL cholesterol, ethnicity, and locus of control. Importantly, age (one of the most influential risk factors) and gender were only recognized by 71 and 60% of patients, respectively. The fictitious causes subscale revealed that many patients incorrectly thought that factors that have a role in other diseases (e.g., cancer, infectious diseases) were related to cardiovascular disease.

Factors Related to Decision Delay

Bivariate Pearson correlations with the log continuous delay score are presented in **Table 4**. Shorter decision delay was related to more accurate knowledge of cardiovascular risk factors, more accurate knowledge of ACS symptoms, correct attributions of symptoms to a cardiac cause, and higher perceived severity. Those patients who had more accurate knowledge of cardiovascular risk factors also had more accurate knowledge of ACS symptoms. Finally, higher perceived symptom severity was related to accurate attributions of symptoms to a cardiac cause.

For our main analysis, we conducted a multiple linear regression analysis with decision delay as outcome variable. The rest of the variables (i.e., knowledge of cardiovascular risk factors, knowledge of ACS symptoms, attribution to a cardiac cause, and severity of symptoms) were included as predictors. Demographics (age, gender, and education), disease severity (type of ACS), and the number of days elapsed between the cardiac event and completion of the questionnaire were included as controls in this analysis. Knowledge of fictitious factors was not considered in the analysis as it was not related to decision delay (**Table 4**).

The results of the regression analysis are presented in **Table 5**, including standardized regression coefficients (βs) and the change in R^2 for each predictor. The model accounted for 32% of the total variance in prehospital delay, $F(9, 97) = 5.06$, $p < 0.001$. Knowledge of cardiovascular risk factors, knowledge of ACS symptoms, and attributing symptoms to a cardiac cause accounted for 19.4, 11.5, and 10.1%, respectively of the variability, whereby more accurate knowledge of cardiovascular risk factors, correct attributions of symptoms to a cardiac cause, and more accurate knowledge of ACS symptoms were related to shorter decision delay. The other predictors were not significant ($ps > 0.05$).

To illustrate the effects of the significant variables in the model, we considered the percentage of patients who waited more than 60 min to seek help after symptom onset, which is considered the "golden time window" for initiating treatment (Moser et al., 2006). **Figure 2** displays this percentage as a function of cardiovascular risk factor knowledge quartiles, showing that the percentage of patients waiting more than 60 min is significantly higher for patients with relatively lower knowledge of risk factors (i.e., knowledge below the median). In the case of ACS symptom knowledge, the protective effect was observed in the highest quartile, in which only 28% waited more than 60 min, compared to an average of 60% in the lower quartiles. Finally, among those who attributed symptoms to a cardiac cause, only 30% waited more than 60 min, compared to 62% among those who did not attributed symptoms to a cardiac cause.

DISCUSSION

To the best of our knowledge, this is the first study showing that patients' correct identification of cardiovascular risk and protective factors is related to shorter decision delays in seeking for help. This effect was independent of demographics and clinical characteristics and other important decision making factors such as symptom recognition and perceived severity of symptoms.

Previous research shows that people consistently underestimate the probability of experiencing negative outcomes (e.g., a disease). That is, they often show unrealistic optimism bias, which can reduce the accuracy of their risk appraisals and delay their help-seeking behavior (Weinstein, 1982; Blumenthal-Barby and Krieger, 2015). Previous research further showed that ACS patients tend to be overly optimistic regarding their risk of cardiovascular events (Dracup et al., 2008; Alfasfos et al., 2016; Thakkar et al., 2016). The current study raises the possibility that more accurate knowledge of cardiovascular risk factors might contribute to better decision making in patients with ACS by accurately increasing their perceived risk of suffering a cardiac event (Lefler and Bondy, 2004; Darawad et al., 2016), reducing

TABLE 3 | Knowledge of cardiovascular risk factors questionnaire: item responses.

Factor	Effect	Item text	Correct answer	
			N	**%**
Modifiable factors				
Obesity	R	Suffering obesity	113	94
Tobacco consumption	R	Smoking cigarettes	111	93
A diet high in salt	R	Eating food with lots of salt	108	90
Raised blood glucose	R	Having high blood sugar levels (glucose)	108	90
A diet high in saturated fats	R	Having a diet rich in saturated fats (e.g., butter, cream, pastries, processed meat)	108	90
A diet high in trans fats	R	Eating foods high in trans fats (e.g., hamburgers, cakes, chips)	107	89
Mediterranean diet	P	Following the Mediterranean diet: high consumption of vegetable products, bread and other cereals, with olive oil as the main fat.	107	89
High levels of triglycerides	R	Having high triglyceride levels (lipids, a type of blood fat)	107	89
Hypertension	R	Having hypertension (high blood pressure)	106	88
Consumption of fresh vegetables	P	Eating fresh vegetables	105	88
Low density lipoprotein (LDL) levels	R	Having high levels of low density lipoprotein (LDL) ("bad" cholesterol)	104	87
Alcohol consumption	R	Drinking alcohol excessively	104	87
Overweight	R	Being overweight	103	86
Diabetes	R	Having diabetes or prediabetes	102	85
A diet high in omega-3 fatty acids	P	Having a diet rich in omega-3 fats (e.g., fish, nuts)	101	84
Abdominal fat	R	Having a lot of abdominal fat (around the waist)	101	84
Soft sugary drink consumption	R	Drinking sugary drinks (for instance, coke, fanta. . .)	99	83
Fresh fruit consumption	P	Eating fresh fruits	99	83
Fish consumption	P	Eating fish	92	77
Sitting for prolonged periods of time	R	Spending many hours a day sitting (e.g., watching TV, driving)	91	76
Physical activity	P	Doing physical exercise (walking, running, dancing. . .)	87	73
High waist-to-hip ratio	R	Having high waist-to-hip ratio (e.g., a prominent belly)	83	69
Fiber consumption	P	Eating foods high in fiber (e.g., legumes, potatoes)	73	61
High-density lipoprotein (HDL) cholesterol	P	Having high HDL ("good") cholesterol	23	19
Uncontrollable factors				
Personal history of CVD	R	Having had cardiovascular disease previously (e.g., a heart attack or stroke)	110	92
Genetic predisposition	R	Genetic predisposition	108	90
Family history of CVD	R	Having a family history (a direct family member who has had or died from cardiovascular disease before age 55)	108	90
Passive smoking	R	Being exposed to tobacco smoke (e.g., being exposed to tobacco smoke from someone who smokes around you)	100	83
Age	R	Being older (por instance, more than 65 years old)	85	71
Sex	R	Being male	72	60
Ethnicity	R	Being form African or Asian ethnicity	17	14
Psychosocial factors				
Type-D personality (negative affectivity)	R	Feeling strong negative emotions frequently	106	88
Stress at work	R	Suffering stress at work	105	88
Stress at home	R	Suffering stress at home	104	87
Major stressful life events	R	Having experienced stressful life events in recent years (for instance, going through unemployment, divorce, or the death of a close family member)	104	87
Depression	R	Suffering depression	104	87
Anxiety	R	Suffering frequent anxiety	104	87
Financial stress	R	Suffering economic stress (for instance, not being able to make ends meet, having loans to return)	100	83
Social isolation	R	Feeling alone or socially isolated	93	78

(Continued)

TABLE 3 | Continued

Factor	Effect	Item text	Correct answer	
Type-D personality (social inhibition)	R	Not expressing the negative emotions one feels (keeping quiet or not mentioning them)	89	74
Social support	P	Having high social support (support from the people around you)	78	65
Being poor/Low income	R	Having low income	75	63
Education	P	Having high education (for instance, having gone to university and finished one's studies)	29	24
Locus of control	P	Thinking that you have the control over events that happen around you	7	6
Fictitious causes				
Type-A personality (urgency)	NE	Being impatient (e.g., feeling in a hurry and needing to go/act fast frequently)	79	66
Risky sexual behavior	NE	Having unprotected sex with multiple partners	71	59
Pregnancy	NE	Being pregnant (for women)	61	51
Mosquito bites	NE	Being bitten by a mosquito carrying a virus	60	50
Sun exposure	NE	Sunbathing excessively or using sunbeds	57	48
Radiation exposure	NE	Exposure to X-rays and other sources of radiation	37	31
Type-A personality (competitiveness)	NE	Being competitive in everything you do	32	27
Type-A personality (hostility)	NE	Being a hostile person, one who gets angry easily	13	11

Effect on cardiovascular risk: R, risk factor; P, protective; NE, no effect.

TABLE 4 | Pearson correlations between decision delay with the other variables of interest.

	1	2	3	4	5	6
1. Decision delay (log)	–					
2. Knowledge of CV risk factors	−0.44*	–				
3. Knowledge of fictitious causes	0.09	−0.12	–			
4. Knowledge of ACS symptoms	−0.34*	0.33*	−0.16	–		
5. Attribution to a cardiac cause (1 = yes; 0 = no)	−0.32*	0.18	0.02	−0.00	–	
6. Perceived severity	−0.26*	0.17	0.07	0.13	0.38*	–

*$p < 0.01$.

TABLE 5 | Linear regression analyses to determine the influence of each predictor on decision delay.

	Decision delay (log)				
	B	***SE***	**β**	***p***	***R*²**
Knowledge of cardiovascular risk factors	−0.060	0.018	−0.312	0.001	0.19
Knowledge of ACS symptoms	−0.048	0.017	−0.254	0.006	0.12
Attribution to a cardiac cause (0 = no; 1 = yes)	−0.280	0.139	−0.189	0.047	0.10
Perceived severity of symptoms	−0.024	0.038	−0.058	0.540	0.07
Age	0.001	0.007	0.012	0.888	0.01
Gender (0 = male; 1 = female)	0.022	0.140	0.014	0.876	0.02
Education	−0.035	0.047	−0.067	0.458	0.04
Type of ACS (0 = non-STEMI; 1 = STEMI)	−0.047	0.129	−0.033	0.716	0.00
Days elapsed between cardiac event and questionnaire	−0.020	0.028	−0.065	0.479	0.00

the typical unrealistically optimistic perceptions of these patients (Dracup et al., 2008; Alfasfos et al., 2016; Thakkar et al., 2016).

Another potential mechanism behind the effect of risk factor knowledge could be related to the "representativeness heuristic" (Kahneman and Tversky, 1972; Blumenthal-Barby and Krieger, 2015). According to this heuristic, the probability of an event (e.g., a person experiencing a heart attack) is inferred by comparing it to an existing prototype (e.g., the typical person who would suffer a heart attack). If people have relatively high knowledge of cardiovascular risk factors, they could detect the similarity between their own characteristics and those of a prototypical person who develops cardiovascular disease. This could increase the perceived probability of suffering an important cardiac event and speed up help-seeking. For instance, a male smoker toward the end of his 60s, who knows that male gender, older age, and smoking are risk factors for developing

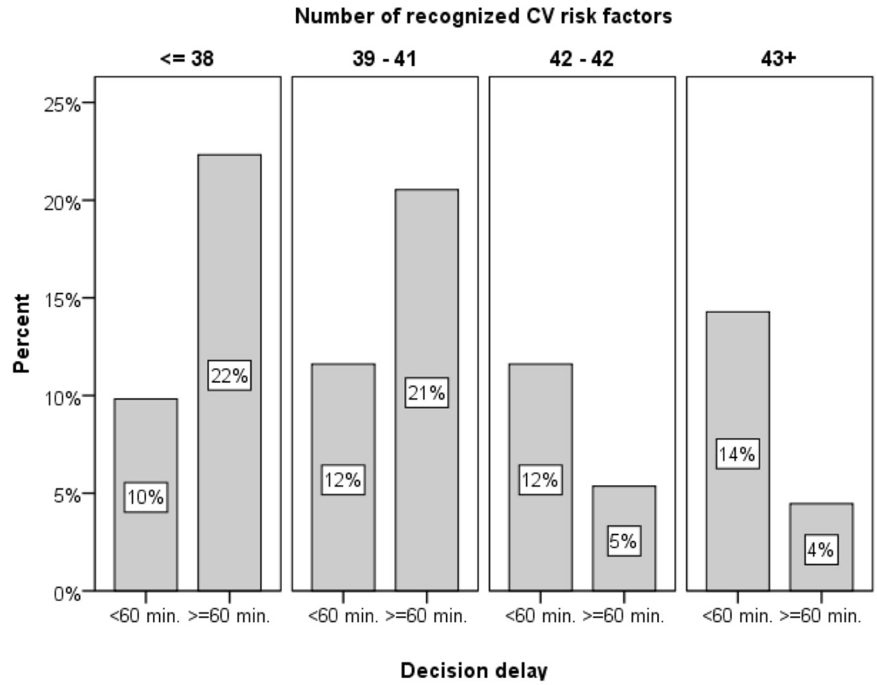

FIGURE 2 | Percentage of patients waiting more than an hour to seek help after symptom onset as a function of cardiovascular symptom knowledge quartiles.

cardiovascular diseases may be more likely to identify himself as someone who is likely to have a heart attack and thus make the decision to seek help sooner after symptom onset. This hypothesis should be investigated in future research.

Among the factors identified in previous research in relation to patient decision making during ACS is statistical numeracy: the ability to understand the mathematics of risk, including proportions, percentages, or probabilities (Cokely et al., 2014). More numerate patients were found to be three-to-four times more likely to have sought medical attention within 1 h after symptoms onset, independent of other cognitive, clinical, and demographic factors known to influence decision delay such as age and symptom severity (Petrova et al., 2017). Numeracy has been related to better knowledge and comprehension in diverse health contexts (Garcia-Retamero et al., 2019). It is possible that having more accurate knowledge of cardiovascular risk factors and more calibrated risk perceptions are among the mechanisms that make persons with higher numeracy more risk literate decision makers (Cokely et al., 2018). Future research can test this hypothesis in patients with ACS or other diseases.

In the current study, ACS patients from a hospital in Spain showed relatively good knowledge of cardiovascular risk factors. This result is in contrast with previous research in healthy and patient populations conducted in other countries showing that people tend to have very limited knowledge of their cardiovascular risk factors (Erhardt and Hobbs, 2002; Jensen and Moser, 2008; Wartak et al., 2011). Given these discrepancies, it would be best to further investigate the relationship between knowledge of risk factors and prehospital decision delay in other more diverse samples that also show more representative knowledge levels across the whole continuum of the scale. The risk factors questionnaire used in the current study was very detailed, including clinical, lifestyle, demographic, and psychosocial risk factors. The modifiable lifestyle and clinical factors were among the most recognized by participants – a result that it is not surprising given that they are the more frequently addressed in medical consultations and prevention efforts (ESC guidelines; Roffi et al., 2016). In contrast, many of the psychosocial factors are likely to be known only to medical professionals and researchers. Nevertheless, the current results show that many patients have correct intuitions regarding some of these factors, including the effect of stress, social isolation, and emotional tendencies (**Table 3**). These lay perceptions could be formed based on personal experience (e.g., based on the circumstances of family members or acquaintances who have suffered cardiovascular disease) or provided by physicians or the media. However, it is noteworthy that age, which is one of the strongest predictors of cardiovascular risk, was less recognized than many lifestyle factors – a result that suggest that interventions that effectively improve knowledge about cardiovascular risk facts might be useful.

In addition, results regarding symptom attributions are consistent with our previous findings (e.g., Arrebola-Moreno et al., 2020a), showing that patients who attributed their symptoms to a cardiac cause waited less to seek help. In contrast, the results regarding knowledge of ACS symptoms (i.e., that only very high knowledge appeared to have a protective relationship with decision delay) add evidence to a large number of previous studies showing mixed results (Petrova et al., 2017; Arrebola-Moreno et al., 2020a).

Such mixed findings could be due to the limitations of the retrospective methodology often used to study prehospital delays (see Arrebola-Moreno et al., 2020a, for more details). Patients recently diagnosed with ACS are often recruited shortly after the cardiac event to fill in a questionnaire. This methodology promotes several biases including memory biases (e.g., mild cognitive impairment is common after ACS, Saczynski et al., 2017), and selection biases (e.g., only survivors and clinically stable patients are included, thereby excluding the most vulnerable population). To illustrate, patients may not correctly remember exactly when their symptoms started or may not have interpreted the initial bodily sensations related to the cardiac episode as symptoms. Another limitation of this methodology is that it does not allow to control for the effect of learning (i.e., patients might learn from their experience with the disease). For instance, patient knowledge of symptoms could be influenced by the symptoms experienced during the cardiac episode and knowledge of risk factors could be influenced by their interaction with healthcare professionals, showing the hindsight bias, which is often referred to as the "I-knew-it-all-along" phenomenon (Christensen-Szalanski and Willham, 1991).

Despite these limitations, cross-sectional retrospective studies remain one of the most useful methods to study decision delay in ACS in a naturalistic setting. Unfortunately, prospective studies on prehospital delay, in which potential predictors of delay are recorded at baseline, are rare due to practical and financial issues stemming from the need to follow-up a very large number of individuals. In addition, factors directly related to decision making, such as perceptions and interpretations in the context of experiencing symptoms can only be recalled retrospectively (it could be impractical and even unethical to collect data during the experience of ACS).

As an alternative, studies with healthy populations in which participants report hypothetical decision delays could eliminate some of these biases and allow to study decision making processed in more detail (Arrebola-Moreno et al., 2020a). In this type of studies decision theories and knowledge regarding heuristics and biases could be used to understand and potentially improve patient decision making. Hence, we would like to encourage researchers from these fields to use their valuable expertise to solve this pressing societal problem. Coronary heart disease is the leading cause of death in Europe, causing about 1,739,000 deaths every year, which is 20% of all deaths (Wilkins et al., 2017).

Most interventions that aimed to improve patient decision making during ACS have focused on improving the recognition of symptoms in the population, raising awareness about successful treatment options, and giving instructions about what to do in case of symptoms (Mooney et al., 2012; Farquharson et al., 2019). Should the role of knowledge of cardiovascular risk factors be confirmed in future studies, then raising awareness about cardiovascular risk factors should be considered as a strategy in interventions and campaigns targeting patient delays during ACS.

DATA AVAILABILITY STATEMENT

This research is part of project "PySCA: Study on the impact of psychological factors in acute coronary syndrome" (Principal Investigator: JR-H). Individualized data from the project cannot be publically shared on a data repository due to the conditions of non-disclosure described in the hospital consent form signed by the participating patients. Study materials and detailed statistical results are available on the Open Science Framework (OSF) (doi: 10.17605/OSF.IO/CEHN7) and individualized data can be requested from the corresponding author.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the Ethics Committee of the University Hospital Virgen de las Nieves in Granada, Spain. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

DP, AC, JR-H, and RG-R conceived the study and prepared the study materials. DG and JR-H collected the data. DG managed the data and conducted the analyses together with DP. DG and DP wrote the first draft of the manuscript with input from all authors. All authors contributed to the interpretation of the results, provided critical revisions of the first draft, and approved the final version of the manuscript.

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Price-Denomination Effect: Choosing to Pay With Denominations That Are the Same as the Product Prices

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Building on past research on judgment anchoring, we investigate the effect of price information on consumers' choice of denomination when making a purchase. Across seven experiments, including two in the field (N = 4,020), we find that people tend to purchase with denominations that are the same as the product prices. They use larger denominations for higher priced products that are priced at the value of the denomination held, and smaller denominations for lower priced products that are priced at the value of the smaller denomination held. The effect is not explained by storage or purchase convenience. We propose the "price-denomination effect" is driven by consumers anchoring on product price and then choosing the denomination that matches the anchor. The effect replicates across participants from different continents (United States, Europe, and Africa) and samples (online panelists, and actual consumers), as well as prices in different currencies (United States $, €, and Nigerian Naira). We further demonstrate that people's preference for denominations also affects the choice of the form of payment used: cash versus card. Consumers are more likely to use cash (vs. card) when product price is exactly the same as a denomination held. We conclude with a discussion of theoretical and practical implications.

Keywords: subjective value of money, denomination choice, judgment anchoring, behavioral economics, field and lab experiments, price

INTRODUCTION

Recently, ATMs in the U.S and Europe have started allowing customers a choice of denominations when they withdraw money from their bank accounts; an option that was earlier only available at the teller. For example, when withdrawing $200, the customer has the option to withdraw ten $20 bills or four $50 bills. Can the type of denomination chosen affect how the money will be spent? And is the choice of denomination used to spend a function of the price of the products that customers intend to purchase?

The use of cash in the marketplace is an interesting phenomenon (Prelec and Simester, 2001; Raghubir and Srivastava, 2002; Amromin and Chakravorti, 2009). Despite the growing availability of new payment methods, and despite the arguments from some economists and policymakers for the phasing out of paper money (Rogoff, 2016), cash remains the most heavily used retail payment instrument (Matheny et al., 2016). In developed economies such as the USA, Japan, and Singapore, cash remains the dominant mode of payment, with around 85% of payments made with cash globally (Wheatley, 2017). Because cash continues to be relevant to people, understanding how people make decisions regarding the use of cash is a relevant pursuit.

It is particularly relevant given that there is a misalignment between the predictions of traditional economic model assumptions and people's actual behavior. On the one hand, according to standard economics, money is money, irrespective of payment method, currency, or denomination. On the other hand, research on the subjective value of money consistently shows otherwise. For instance, money is judged beyond its actual denomination value: people assign greater value to money based on physical properties, such as the size of coins (Bruce et al., 1983) and spend more when the face value of a foreign currency is a fraction of one's home currency (Raghubir and Srivastava, 2002). Additionally, people's perception of the economic value of bills is primarily affective, rather than numerical (Giuliani et al., 2018). Consumers also estimate higher purchasing power due to money familiarity (Alter and Oppenheimer, 2008), are more likely to spend, and spend more, when they have smaller rather than larger denominations (Mishra et al., 2006; Raghubir and Srivastava, 2009), and spend less when paying with cash versus credit cards (Prelec and Simester, 2001; Raghubir and Srivastava, 2008).

The research presented here falls within this program on the subjective value of money. This paper examines how people choose different denominations of cash: a new effect termed the "price-denomination effect." We propose that the decision of which denomination to use will be a function of the price of the product. Specific denominations will be more likely to be used to purchase a product when their value is equal to its price. For example, consumers will use a larger denomination to buy a product, which has the same price as the value of the denomination, even when smaller denominations held are inconvenient to store.

We examine how price information affects people's choice of denomination to pay for a purchase (e.g., with one €50 bill or five €10 bills) and choice of form of payment (cash vs debit card). Seven experiments (including two field experiments, $N = 4{,}020$), test our predictions using participants from different continents (United States, Europe, and Africa) and samples (online panelists, and Nigerian consumers), as well as prices in different currencies (United States $, €, and Nigerian Naira). We propose that once consumers have decided to purchase, they use price information as a judgment anchor and this tendency to anchor on price information influences their choice of denomination. For instance, if a consumer has five $10 bills and a $50 bill and has decided to purchase a $10 product, she is more likely to pay using the $10 bill; but if she has decided to purchase a $50 product, then she is more likely to pay using the $50 bill, violating the descriptive invariance of money principle. We test this proposition in study 1.

We further propose that anchoring on price information is one of the drivers of the effect and elicits faster responses when choosing the denomination with which to purchase, indicating that when there is a match between the price and the denomination carried, the decision of which denomination to use is faster to make (study 2). We demonstrate that having the price be exactly the same as the denomination carried is not a necessary condition for the effect to occur and that denominations should be "close enough" to the price anchor for the effect to hold (the effect holds for prices which are 10 – 20% below the denomination value, study 3). We further show that in spite of the fact that most people judge smaller denominations as easier to purchase with, the majority of participants decide to pay with larger, not smaller, denominations. Therefore, purchasing (transactional) convenience is unlikely to explain the results. We go on to further show that storage (carrying) convenience is also an unlikely mechanism behind the effect (study 4). Overall, the price-denomination effect holds even when controlling for purchasing and carrying convenience.

One alternative explanation for our effect may be value-matching. That is, people would use value as a heuristic to make a purchase. Both larger denominations and more expensive purchases might be seen as more valuable to the consumer, and, therefore, matching value of the product and value of denomination might drive our effect. Study 5 finds that matching of denomination value and value of the product to be purchased (e.g., a Valentine gift or a gift for a jerk boss) are unlikely to explain the effect. Finally, we suggest that the price-denomination effect could extend to the choice of form of payment (cash vs card). We demonstrate that consumers' payment preference (cash versus card) shifts depending on whether the price they encounter is the same as the denominations they hold. Consumers are more likely to pay with cash over card when the denomination they hold is exactly the same as the price of the product to be purchased, and they are more likely to use a card for their purchase when the price and denomination at hand are not the same. We note that our theory does not make predictions for all possible scenarios and mixes of prices and denominations that consumers can encounter. Our theory only makes clear predictions for situations in which a consumer wants to buy a product, the price of which is exactly the same as one of the denominations she holds. To examine the boundary conditions of our effects, we also include an examination of the price-denomination effect where we vary the price of the purchase by lowering it 10, 20, and 30% and, therefore, distance it from the denomination carried. We find that our effect holds also for prices that are up to 10% below the denomination at hand. In other situations, not examined in this paper, consumers might either be indifferent between the use of two denominations at hand, supporting the money invariance principle, or be more likely to spend smaller denominations, supporting the "denomination effect", which we discuss below.

Our work contributes to the research on the subjective value of money by demonstrating violations of the descriptive invariance of money in the domain of which denomination to pay with. We also contribute to the price anchoring literature by documenting a new downstream consequence of prices on consumers' behavior. Finally, our theory and supporting evidence are in line with the mental accounting perspective (Thaler, 1985; Prelec and Simester, 2001): consumers will use small denominations when prices are equal to the small denominations at hand, and large denominations for large purchases priced the same as the larger denomination. Our findings specifically add to the literature on the "denomination effect" (Raghubir and Srivastava, 2009) which suggested that one of the reasons people were more likely to make a purchase when they held smaller denominations was because they wanted to exert self-control and did not want to lose track of how much money they had (see also Raghubir et al., 2017). Other reasons proposed for the same effect include perceptual fluency (Mishra et al., 2006), and feelings of smaller notes being "dirty" as they are in greater circulation than larger notes (Di Muro and Noseworthy, 2013). The vast majority of the studies in previous investigations of the denomination effect had choice sets where the value of products was closer to the smaller denomination that participants had been given. It is plausible that the prices of the majority of products used in previous studies were close to the smaller denominations which led to an increased likelihood of spending in the lower (vs. higher) denomination conditions. As such, the match between price and denomination may be an additional factor explaining the denomination effect.

Knowing how the public uses different denominations also has important practical implications. For example, knowing how denominations are used is important for central banks who decide on money issuing and maintenance policies. In addition, banks can use this information to decide which denomination to stock at both the bank tellers as well as at ATMs. For example, at an upscale mall, retailers might benefit if ATMs issued larger denominations, while outdoor retail markets like food markets and farmers markets may benefit if ATMs closer to them issued smaller denominations. These results can also help managers decide on pricing policies for individual products and bundled options. For example, in countries where the commonly available denominations are 20, 50, and 100, managers may wish to create bundled or unbundled options that are closer to 50 or 100 than to 75. In the following sections, we first discuss past research on judgment anchoring. The studies are then described. We conclude with a discussion of the results, theoretical contributions, managerial implications, and opportunities for further research.

THEORETICAL FRAMEWORK AND HYPOTHESES

Judgment anchoring has been defined as an effect that occurs when individuals are biased toward an arbitrary value before making a numerical estimate (Jacowitz and Kahneman, 1995). The selective accessibility model proposed by Strack and Mussweiler (1997) argues that anchoring occurs when an anchor activates a target response, with anchors being illustrations of semantic priming. Priming refers to situations where information that is activated becomes more accessible when solving tasks compared to non-activated information. Applying their model to point-of-purchase decisions, we suggest that the prices consumers encounter can function as judgment anchors and affect subsequent judgments (cf. Mussweiler and Strack, 1999; Lin and Chen, 2017; Köcher et al., 2019).

While making a judgment, people resort to readily accessible information (e.g., the anchor), which carries through to their decision. Accordingly, responses tend to converge toward the anchor. Anchors increase the saliency of information irrespective of whether the anchor is arbitrary (Ariely et al., 2003), encountered in the environment (Jacowitz and Kahneman, 1995) or self-generated (Epley and Gilovich, 2005). Research in the consumer domain has shown that consumers anchor on price information when making purchase decisions (e.g., Morwitz et al., 1998). Anchoring effects involving price information have also been observed in bidding contexts. Consumers spend more and recall lower costs when exposed to a surcharge compared to control conditions (Morwitz et al., 1998; Chakravarti et al., 2002). More recently, Jung et al. (2016) demonstrated that when consumers anchored on higher bonus prices, they spent more on average than when they anchored on lower bonus prices. In addition, the proportion of consumers' spend allotted to charity versus the retailer was influenced by whether they encountered a higher or lower anchor. Price anchoring is just one of the many ways in which prices influence consumers' decisions or in which consumers use prices. Consumers use price cues to estimate product quality (Zeithaml, 1982). They also engage in price search in an attempt to increase savings (Stigler, 1961). Further, consumers' tendency to disregard prices with −99 endings leads them to underestimate the actual prices of products; thereby overspending when they are offered clearance sales (Schindler and Kibarian, 1996). Other research from Thomas and Morwitz (2005) shows that consumers misattribute the magnitude of price discounts to the ease of computing the difference between a regular price and a sale price. In a different but related context, Choi et al. (2019) found that numeric information with −99 endings increased consumers' unhealthy food consumption compared to numeric information with −00 endings. It, therefore, goes to reason that prices exert other forms of influence on consumers at a point of purchase.

Of particular concern to the present research, is the choice of which denomination to purchase with when consumers have decided to pay for a product or service. Relating the research on judgment anchors to the price contexts involving choice of denominations, we propose that, when making a purchase, price acts as a factor that is contextually available and affects choice of denomination. We argue that the prices consumers encounter represent salient environmental anchors both because such prices are readily available (Tversky and Kahneman, 1974) and because they are relevant (Davis et al., 1986). We suggest that because price information is salient in shopping contexts (Thomas and Morwitz, 2005), it activates the saliency of denominations consumers possess, provided prices and denominations match each other (e.g., price of \$50 and denomination of \$50). Given that people tend to rely on information that is activated in memory (Sedikides and Skowronski, 1991), they should also exhibit a tendency to purchase with the denominations the person possesses, which are activated by the price. This implies that if denominations are not the same as the price, consumers will be less likely to rely on price information in choosing the denomination with which to purchase. To put it more formally:

H1: Consumers are more likely to purchase with the denomination that is a match with the price of the product when selecting between two denominations at hand.

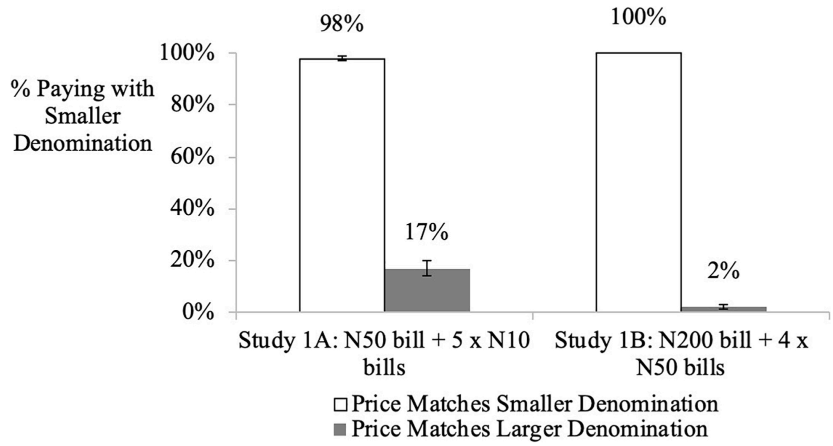

FIGURE 1 | Influence of "price-denomination effect" on denomination choice (studies 1a and 1b). Error bars denominate std. errors. The results were similar for studies 1A and 1B. For instance, in study 1A, when the price was N10, 98% of consumers chose to purchase with the matching N10 bill they possessed. However, when the price was N50, the proportion of consumers purchasing with the same N10 bills decreased to 17%.

By "match" we mean the price that is exactly the same as the denomination at hand. We further use the term "match" and the expression "price is exactly the same as the denomination" interchangeably.

Epley and Gilovich (2001) demonstrated that people respond faster the more they rely on anchors that readily come to mind. In contrast, they respond slower when their final responses deviate from readily available anchors. Thus, in line with existing theory, we further hypothesize:

H2: People decide on which denomination to choose faster when prices are a match with the denomination they possess, compared to when they are not.

EMPIRICAL OVERVIEW

We conducted seven studies to test the price-denomination effect and the role of price as an anchor in the choice of denomination (we also report three additional studies in the **Supplementary Appendix** replicating the studies in the main manuscript under slightly different conditions and contexts). In field studies 1A-1B, we test H1 using actual consumers in Africa where we could take advantage of the lower cost of living, and use relatively large denominations in an economically feasible manner. Study 2 tests our proposed mechanism – judgment anchoring. Studies 3-4 test whether results can be extended beyond exact matching (study 3) and storage convenience (study 4). Study 5 rules out that the transfer of ownership value from denominations to the product moderates our proposed effect. Finally, study 6 extends the price-denomination effect to choice of payment form: cash vs. card. **Table 1** shows a summary of all the study descriptions. Data, study protocols and analysis codes are publicly available at https://osf.io/syvrm/ (the flows of the field studies are described in detail in the text directly).

The method of analysis across all studies is to examine differences in the percentage of participants using a given denomination across price conditions. Additionally, we examined whether the percentage of participants who chose the non-matching denomination varied significantly from 50% (given there was a choice of two denominations, 50% represents a random choice or guessing). In no study was there any evidence for over 50% of participants purchasing with the non-matching bill (see **Table 1**).

A meta-analysis testing mean p-values using Rosenthal's (1978) approach shows that the price-denomination effect is significant [$z = 8.78$, if we use $N = 26$; or $z = 5.17$, if we use $N = 9$, where N = number of studies used for analysis, $ps < 0.001$ for both, see **Supplementary Appendix 8** for details.]

STUDY 1. ESTABLISHING THE EFFECT WITH ACTUAL PURCHASES

The objective of studies 1A-1B was to test the price-denomination hypothesis (H1) in actual retail settings using relatively large sums of money for the participant population. We took advantage of the cost of living in Nigeria and conducted our studies in Lagos. We used the local currency (naira, denoted as "N") with an exchange rate of \$1: N360.68. By conducting the study in Nigeria we were able to use large denominations in a real life setting (e.g., participants handled a 200N bill which is the 3rd largest denomination in the country) – something that would not be possible to accomplish in a Western European country or North America due to budget constraints. Study 1A uses lower denominations and price levels than study 1B. In line with our theoretical framework, we expect consumers to choose the denomination that matches product price.

Study 1A: Method

Participants and Design

Three hundred and ninety-nine students and workers (female = 51%) who were responding to an on-campus sales promotion at two Lagos universities were assigned at random to a two-cell (price: N10 vs. N50) between-subjects design.

TABLE 1 | Study Descriptions (1 – 6).

Study	Denominations	Spend Level	Products	Prices	*N*	Results
1A	N50 & 5 × N10	N100	One pen	N10	399	Results[1]: 98%
			A set of five pens	N50		17%
1B	N200 & 4 × N50	N400	phone voucher	N200	388	2%
			A set of five pens	N50		100%
2	1 × $50 & 5 × $10	$100	Shampoo	Exact Match: $10	1,204	Results[2]: 10.04 secs
				Equidistant: $30		11.03 secs
				No price		10.61 secs
			Perfume	Exact Match: $50		7.03 secs
				Equidistant: $30		9.35 secs
				No price		9.87 secs
3	$100 & 5 × $20	$200	Compact Camera	$100, $90, $80, $70	881	Results[3]: 70%, 83%, 60%, 56%,
	$50 & 5 × $10	$100	Perfume	$50, $45, $40, $30		88%, 87%, 55%, 51%
4	$50 & 50 × $1	$100	Perfume	$50	550	Results[1]: 29%
			Shampoo	$10		85%
	$100 & 100 × $1	$200	Camera	$100		32%
			Shampoo	$10		93%
5	5 × €10 & 1 × €50	€100	Gift: Jerk boss	€10	438	Results:[1] 90%
			Gift: Valentine	€10		91%
			Gift: Jerk boss	€50		24%
			Gift: Valentine	€50		19%
6	10 × $10	$100	Taxi ride	$10	160	Results[4]: 76%
	10 × $10			$50		40%
	2 × $50			$10		46%
	2 × $50			$50		63%

[1]Proportion paying with the smaller denomination, [2]Participants' reaction time when choosing which denomination to purchase with, [3]Proportion paying with the larger denomination, [4]Proportion paying with cash.

Materials and Procedure

Upon arrival, participants received N100 (≈ $0.28¢ or €0.23¢) in cash in an unsealed brown envelope: one N50 bill and five N10 bills. They were told they must purchase one product and could select one of two products available at the promotion, depending on the price condition they were assigned to, using their money and could leave with the rest of their money. In the price = N10 condition, participants saw three pens (blue, black and red), and had to purchase one of these pens. In the price = N50 condition, they had to purchase one of two sets of five pens (set 1: two black, two blue and one red pen versus set 2: four red and 1 black pen). Finally, each participant was issued a receipt and thanked. Assistants, blind to the study's objective, manually recorded the choice of denomination (N10 or N50) used to pay for the N10 or N50 purchase using the duplicate copy of the receipt. Four observations were excluded from the analyses due to errors in recording participants' responses leading to a usable sample of $N = 395$.

Results

The proportion of participants purchasing with the smaller N10 bill was 98% (versus 50%; $z = 13.46$, $p < 0.001$) in the N10 price condition versus 17% in the N50 price condition (versus 50%; $\chi^2(1) = 263.34$, $z = -9.38$, $ps < 0.001$), see **Figure 1**. This provides initial support for H1.

Discussion

Participants chose the denomination that matched the price of the product they had to purchase, consistent with H1. Study 1B examines the generalizability of the effect using a higher spending level, a higher set of prices, and larger denominations.

Study 1B: Method

Participants and Design

The design was identical to study 1A, with the exception of the denominations and prices, which were higher. Three hundred and eighty-eight students and workers participated in an on-campus sales promotion. Participants received N400 in a single N200 bill and four N50 bills. Participants were assigned, at random, to one of two price conditions in a between-subjects design. In the price = N50 condition, participants chose between two sets of pens, similar to study 1A. In the price = N200 condition, participants chose a telephone voucher from one of the four existing telecoms providers. We could not use the same product in different price conditions as prices are different and there was no equivalent product available at both price levels.

Results

In the price = N50 condition, 100% of participants purchased with the smaller N50 bill, as compared to only 2% (versus 50%) in the price = N200 condition ($\chi 2(1) = 185.25$, $z = -13.68$, $ps < 0.001$, significantly less than 50%), see **Figure 1**.

Discussion

Study 1B replicated the effect of study 1A with high spending levels and higher prices. Overall, study 1 demonstrated in the field that people are likely to use the denomination that is the same as the price of the product they need to purchase. This is consistent with H1. We note that the number of options varied between conditions as it was not feasible to use the same number of options: we used 3 single pens vs 2 sets of pens, or we used all the colors of the pens available (3) and all the vouchers available in the store (4). We could not limit the number of vouchers to three to match the number of pens because, otherwise, some of the participants would be at a disadvantage, as they would not have the option of the phone voucher of their network. We, however, did not expect that this difference in number of options would create a systematic bias, but we recognize this as a limitation of this study. Later studies use an identical number of choice options. **Supplementary Appendix 1** reports an additional study testifying to the generalizability of this effect using samples from a Prolific Academic panel, the euro currency, a wide variety of products, and constant number of options across conditions.

One of the limitations of the field study is that we were unable to elicit the mechanism(s) driving the price-denomination effect. In studies 2-5 we explore possible processes underlying the effect. To begin, Study 2 examines price anchoring as a potential mechanism. It explores whether the effect persists when the price anchor is removed and whether consumers are faster at choosing denominations that match the prices encountered. We also include controls for product familiarity and attitude, as these can change the extent to which people think intuitively (Zajonc and Markus, 1982).

STUDY 2. JUDGMENT ANCHORING

The price-denomination effect proposes that prices that are the same as the denominations at hand are relevant contextual cues that will affect the choice of denomination to use. Previous research suggests that people respond faster to readily available anchors (Epley and Gilovich, 2001). Accordingly, in this study we explore whether consumers choose between denominations at hand similarly when a price anchor is available (vs not available) and whether consumers make faster decisions when there is a closer match between prices and denominations.

Method

Participants and Design

One thousand two hundred and four Amazon Mechanical Turk participants (female = 49%, $M_{age} = 38.02$, $SD = 12.95$) were recruited to participate in this study in exchange for modest compensation. They were assigned at random to one of six conditions using a between-subjects design that manipulated the price levels of two products (shampoo vs. perfume) and the type of price information (exact match vs. equidistant vs. no price).

Materials and Procedure

Participants read instructions asking them to imagine they wanted to purchase either a luxury shampoo or a perfume. Participants were further asked to imagine they held \$100 as a \$50 bill and five \$10 bills, and were shown images of the bills they had. Depending on condition, participants saw one of the three price anchors. In the shampoo conditions the price levels were: \$10 (exact match anchor), \$30 (equidistant anchor) or no price. In the perfume conditions, the prices were \$50 (exact match anchor), \$30 (equidistant anchor) or no price. Though the "no price condition" is not realistic as consumers usually know how much each product costs, from a theoretical point of view, this condition allows for a test of whether there is a main effect of product and whether people have approximate price anchors in their minds. Participants then indicated their choice of denomination. We measured their response latencies (in seconds). Following this, participants responded to product attitudes ("How much do you like ___?") adapted from Irmak et al. (2010) and product familiarity ("How often do you use ___?") adapted from Teixeira et al. (2014) measures, both measured on 100-point slider scales (0 = "not at all"/100 = "very much"). Finally, participants indicated their age, gender, and income level. Fifty-nine respondents (5% of participants) failed an attention check and were excluded from the analyses, leaving a usable sample of $N = 1145$.

Results

Denomination Choice

The dependent variable was the proportion of participants who purchased with the denomination that was equal to the product price (coded "1"; otherwise "0"). As shown in **Figure 2A**, the proportion of participants who purchased with the denomination when the denominations matched price (\$10 for shampoo, \$50 for perfume) was significantly higher (85%) than those purchasing with \$10 bills and \$50 bills respectively, when the price was \$30, or equidistant from the two denominations (50%, $\chi 2(1) = 109.15$, $p < 0.001$), as well as those in the no price anchor condition (57%, $\chi 2(1) = 76.06$, $p < 0.001$). There was a marginally significant difference between the no price and price \$30 conditions (50% vs. 57%, $\chi 2(1) = 3.44$, $p = 0.063$). In the no price anchor condition, respondents were indifferent in their choice of denominations when purchasing shampoo (49%, $\chi 2(1) = 0.048$, $z = -0.22$, $p = 0.413$), but were more likely to use their \$50 bill to purchase the perfume (64%, $\chi 2(1) = 15.67$, $z = 3.96$, $p < 0.001$; overall 49% vs. 64%: $\chi 2(1) = 8.77$, $p = 0.003$). We also conducted logistic regressions estimating denomination choice as a function of price anchors controlling for covariates. The results indicate that our findings are robust. Participants were more likely to use larger denomination for purchasing high priced product (log-odds = 1.50, $p < 0.001$, **Supplementary Table A4**) and less likely to use larger denomination for purchasing a low priced product (log-odds = -1.56, $p < 0.001$, **Supplementary Table A5**) in comparison to the no-price condition when an exact price anchor was available. For both high- and low- priced products,

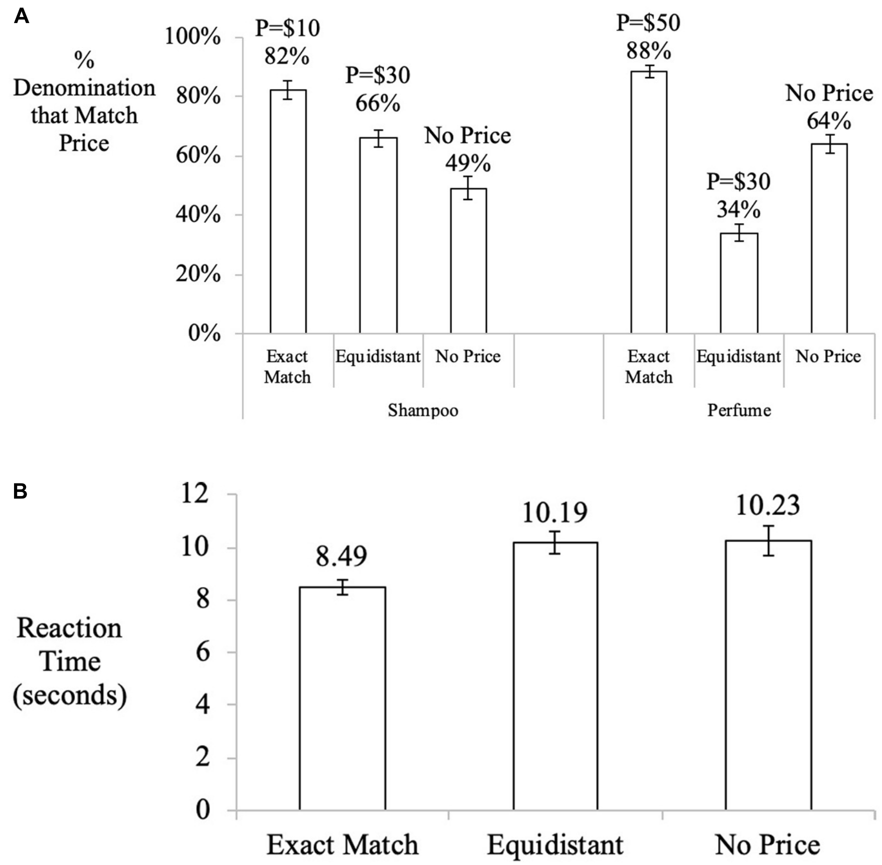

FIGURE 2 | (A) Study 2. Influence of price-denomination effect on denomination choice for six conditions that manipulate price levels. Error bars denominate std. errors. All participants were endowed with a \$50 bill and five \$10 bills. The *y*-axis represents the proportion of participants purchasing with denominations that "matched" different product conditions (that is, matched \$10 in shampoo condition, and \$50 in perfume condition). The *x*-axis represents the type of price information encountered and the product conditions participants were assigned to. **(B)** Study 2. Evidence for judgment anchoring mechanism on price-denomination effect using response latencies on purchase decisions as the dependent variable (in seconds). Error bars denominate std. errors.

people were less likely to use the larger denominations when an equidistant price anchor was available in comparison to the no-price condition (log-odds = -1.23, $p < 0.001$ for high-priced product; and = -0.70, $p = 0.001$, for the low-priced product, **Supplementary Tables A4, A5**). We found no significant effect of the covariates of familiarity and attitude. We also did not find any significant effects for gender or income. The effect of age was significant, but small, for the higher-priced product (log-odds = 0.02, $p = 0.038$, see **Supplementary Appendix 2**, **Supplementary Tables A3–A5**).

Response Latencies

Participants' response latencies (in seconds) per condition are presented in **Figure 2B**. We performed an analysis on the log transformation of the response latencies as the response latencies were skewed. The results are robust when the ANOVA analysis is done on the non-transformed response latencies (see **Supplementary Appendix A2**). A one-way ANOVA on a log transformation of reaction time as a function of price information including all covariates, revealed a main effect of price information ($F(2, 1137) = 8.10$, $p < 0.001$, $\eta^2 = 0.01$), product attitude ($F(1, 1137) = 4.05$, $p = 0.044$, $\eta^2 = 0.00$), product familiarity ($F(1, 1137) = 8.07$, $p = 0.005$, $\eta^2 = 0.01$), age ($F(1, 1137) = 102.99$, $p < 0.001$, $\eta^2 = 0.09$), gender ($F(1, 1137) = 5.24$, $p = 0.022$, $\eta^2 = 0.00$), and income ($F(1, 1137) = 7.64$, $p = 0.006$, $\eta^2 = 0.01$), indicating that the speed at which participants responded to the product varied depending on whether prices matched denominations held. The effect of matching was similar when the covariates were excluded from the model ($F(2, 1142) = 7.63$, $p = 0.001$, $\eta^2 = 0.01$).

On average, participants who faced a price that was an exact match for the denomination they carried responded faster ($M = 1.95$) compared to those who saw the \$30 price ($M = 2.11$, $t(763) = -3.57$, $p < 0.001$, Cohen's $d = 0.26$, *t*-test of log-transformed time in seconds), or those who saw no price ($M = 2.10$, $t(762) = -3.23$, $p = 0.001$, Cohen's $d = 0.23$).

We also conducted regressions estimating the log of response time as a function of price anchors controlling for covariates. The results indicate that our findings are robust and that participants in the exact match condition responded faster than in the non-price condition (log-odds = -0.30, $p < 0.001$), see **Supplementary Appendix A2**, **Supplementary Table A6**. The regression analysis also indicates that this difference is mainly driven by the high-priced product (log-odds of interaction of low-priced Shampoo_Product*Exact_Match_Anchor = 0.34, $p < 0.001$). There is also a significant, though small, positive effect of attitude (log-odds = 0.002; $p = 0.026$), and age (log-odds = 0.01; $p < 0.001$) and a small negative effect of income (log-odds = −0.01, $p = 0.008$).

Discussion

Study 2 results show that people choose denominations that match the price of purchases being considered more often than denominations that do not match those prices, and make these decisions faster. These results are consistent with the idea that prices serve as judgment anchors. An interesting result was when no price was present: consumers were equally likely to choose either denomination in the case of the shampoo but they were more likely to use their $50 bill for the perfume. This suggests the possibility that people may have approximate price anchors in their minds, with some products having more salient price anchors than the others. For example, participants might believe that perfume is more likely to be priced around $50. However, it might be harder for people to rapidly assess the price of a luxury shampoo as the prices of luxury shampoos might vary. Without a clear price anchor in mind, participants might simply use either denomination at random to make a purchase. Exploring the role of implicit anchors may be an interesting avenue for future research. A reasonable criticism of our designs so far is that in typical retail settings prices are not an exact match of the denominations that consumers hold. For example, the .99$ phenomena (see, Schindler and Kirby, 1997; Thomas and Morwitz, 2005, 2009), is wide-spread, as are prices that require a mix of denominations (e.g., $80, $40). Janiszewski and Uy (2008) have also observed that precise price anchors led individuals to overestimate costs of a wide range of products more than rounded anchors. Study 3 investigates the boundary conditions of the effect of price anchors by using prices that are 10, 20, and 30% below the denominations at hand.

STUDY 3. PRICES LOWER THAN THE DENOMINATION VALUE

Method

Participants and Design

Eight hundred and eighty-one participants (female = 51%, M_{age} = 37.54, SD = 11.91) were recruited from Amazon Mechanical Turk in exchange for modest compensation. Participants were assigned at random to one of eight conditions in a between-subjects design, where participants encountered one of two possible products (camera versus perfume) and four possible prices.

Materials and Procedure

We adopted the same cover story used in study 2. In the camera conditions, participants encountered one of four possible prices (starting at $100 and reducing by $10 [10%] for each condition: $100, $90, $80 and $70). Similarly, in the perfume conditions, participants encountered one of four possible prices (starting at $50 and reducing by $5 [10%] for each condition: $50, $45, $40, $35). Participants were told that they had a hypothetical spending budget of $200 and $100 in the camera and perfume conditions respectively. In the camera conditions, the denominations were a single $100 bill and five $20 bills. In the perfume conditions, participants had a single $50 bill and five $10 bills. We expected participants to be more likely to purchase with the larger denomination held in each price condition.

In addition to the measures on product familiarity and product attitude used in study 2, we aimed to control for purchase and storage convenience. Participants rated both the large and the small bills on purchase convenience ("How convenient is $__ bill for making purchases?"), and on storage convenience ("How convenient is $__ bill for carrying in a wallet?"), both on a continuous scale from 1 ("not very much") to 10 ("very much"). Finally, participants indicated their age, gender, and income levels.

Results

Denomination Choice

The dependent variable was the proportion of participants paying with the larger denomination. As shown in **Figure 3A**, participants in the camera conditions were more likely to purchase with the larger denomination when the prices were up to 20% below the largest denomination value ($100 camera: 70% [$\chi 2(1) = 17.6$, $z = -4.20$, $p < 0.001$]; $90 camera: 83% [$\chi 2(1) = 47.13$, $z = -6.86$, $p < 0.001$]; $80 camera: 60% [$\chi 2(1) = 4.77$, $z = -2.18$, $p = 0.015$]. However, for the $70 camera, the effect was attenuated to being marginally significant (56%, $\chi 2(1) = 1.75$, $z = -1.32$, $p = 0.093$).

Participants in the perfume conditions were more likely to purchase with the larger denominations but only when the price was reduced by ten percent of the larger denomination value ($50 perfume: 88% [$\chi 2(1) = 64.15$, $z = 8.01$]; $45 perfume: 87% [$\chi 2(1) = 61.13$, $z = 7.82$], ps < 0.001). However, when prices were further reduced by 20% and 30%, participants were indifferent in their choice of denomination ($40 perfume: 55% [$\chi 2(1) = 1.11$, $z = 1.05$, $p = 0.146$]; $35 perfume: 51% [$\chi 2(1) = 0.08$, $z = 0.29$], $p = 0.387$). Logistic regressions controlling for the convenience measures as well as other covariates indicate that the effect is robust, suggesting that people facing prices that matched exactly or were 10% below the larger denomination at hand were more likely to use larger denominations than when price was 30% below the denomination value (contrasts P = $100 vs P = $70, log odds = 0.70, $p = 0.020$; P = $90 vs P = $70 log odds = 1.57, $p < 0.001$; P = $50 vs P = 35$ log odds = 1.97; $p < 0.001$; P = $45 vs P = 35$ log odds = 1.98; $p < 0.001$, **Supplementary Appendix 3**, **Supplementary Tables A12, A13**). We find that no covariates were consistently significant for both products used in this study. However, some individual covariates were significant for one product but not the other.

In order to understand the influence of convenience, we compare how people judge larger and smaller denominations in

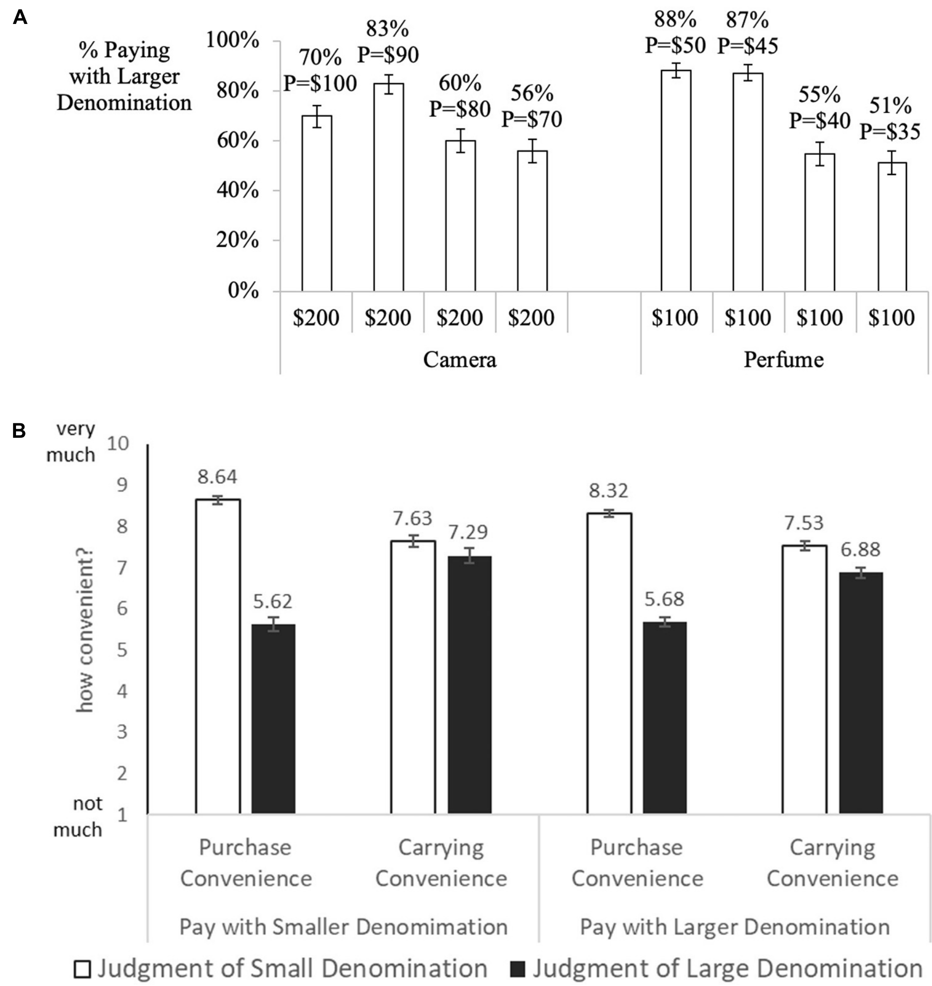

FIGURE 3 | (A) Study 3. "Price-denomination effect" when prices fall below 10% - 30% of the denomination value. Error bars denominate std. errors. The *y*-axis represents the proportion of participants purchasing with the larger denomination. The *x*-axis represents the respective amounts participants in the camera and perfume conditions were endowed with. **(B)** Study 3. Judgement of purchase and storage convenience by type of bill one paid with. Error bars denominate std. errors.

terms of their carrying and purchase convenience. On average, people judged smaller denominations to be more convenient to carry ($M_{\text{smaller\_denom}}$ = 7.56 vs. $M_{\text{larger\_denom}}$ = 7.01, $t(880)$ = 4.09, $p < 0.001$) and more convenient to purchase with than the larger denominations ($M_{\text{smaller\_denom}}$ = 8.42 vs. $M_{\text{larger\_denom}}$ = 5.66, $t(880)$ = 23.48, $p < 0.001$). We further compared the responses from participants who chose to pay with the larger denominations (n = 607) to those who chose to pay with the smaller ones (n = 274). Participants who chose the smaller denomination rated smaller denominations equally convenient to carry as the larger ones ($M_{\text{smaller\_denom}}$ = 7.63 vs. $M_{\text{larger\_}denom}$ = 7.29, $t(273)$ = 1.48, p = 0.139), and participants who chose the larger denomination rated smaller denomination more convenient to carry ($M_{\text{smaller\_denom}}$ = 7.53 vs. $M_{\text{larger\_denom}}$ = 6.88, $t(606)$ = −3.91, $p < 0.001$). Regarding purchase convenience, participants in both groups rated the smaller denomination as more convenient for making purchases than the larger denominations (those who chose the small denomination: $M_{\text{smaller\_denom}}$ = 8.64 vs. $M_{\text{larger\_denom}}$ = 5.62, $t(273)$ = −15.66, $p < 0.001$; those who chose the larger denomination: $M_{\text{smaller\_denom}}$ = 8.32 vs. $M_{\text{larger\_denom}}$ = 5.68, $t(606)$ = −18.03, $p < 0.001$), see **Figure 3B**.

Discussion

This study confirms and builds on the results of the previous two studies. We replicated and extended the price-denomination effect for prices that were 10% below the denomination value. Thus, exact match of denomination and price is not a necessary condition for the "price-denomination effect" to occur. Prices that are not an exact match but are close matches to denominations held may also serve as anchors guiding the choice of denomination to use. Study 3 also provides preliminary evidence that when prices are 20–30% lower than the denomination carried, consumers are indifferent in terms of choosing which denomination to pay with. Said differently, the diagnosticity of prices as anchors reduces if they are not an exact or a close match to the price.

We replicated the results of study 3 in a lab setting using prices ending at .99 and in a field setting in Africa using a simpler design and an actual purchase (for full study details, see Appendices 4 and 5). The studies lend further support that price and denomination do not need to be exactly the same for the effect to hold.

Results on the measure of convenience further indicate that purchase convenience is unlikely to explain the effect reported here. A majority (69%) of respondents choose to pay with larger denominations even though larger denominations were consistently judged as less convenient to purchase with than the smaller bills. On the other hand, results from study 3 on storage convenience are not conclusive. Larger denominations were viewed as equally or less convenient to carry than smaller denominations. Therefore, an unanswered question is whether the price-denomination effect could be explained by consumers' tendency to manage the amount of cash they have at hand. Thus, in study 4, we manipulate the storage convenience of smaller denominations (and make smaller denominations far less convenient to carry than larger ones) and test whether storage convenience could drive consumers to get rid of several smaller bills at the earliest opportunity (Mishra et al., 2006).

STUDY 4. DOES STORAGE CONVENIENCE PREDICT THE EFFECT?

The objective of study 4 was to test whether the "price-denomination effect" is influenced by storage convenience. We stretched the number of smaller bills to an extreme: we examined conditions in which participants had fifty or one hundred $1 bills. Holding so many $1 bills is not realistic; nevertheless, the aim was to experimentally determine the role of storage convenience versus prices that serve as an anchor. We predict that, contrary to the intuition that consumers will get rid of so many smaller bills for the sake of storage convenience, people are more likely to purchase with denominations that serve as anchors and match the prices they encounter.

Method

Participants and Design

Five hundred and fifty participants (female = 45%, M_{age} = 36.41 years, SD = 11.70) were recruited from Mechanical Turk in exchange for modest compensation. Participants were assigned at random to one of four conditions in a 2 (purchase price: price matches the larger denomination vs. not) × 2 (spend level: $100 vs. $200) between-subjects design.

Materials and Procedure

The cover story was adapted from study 2. Depending on their assigned condition, participants were asked to imagine they had decided to purchase a product whose price was either the same as the larger denomination carried ($50 perfume in the $100 spend level condition, or $100 camera in the $200 spend level condition), or was $10 (shampoo for both spend level conditions). Participants were told how much money they hypothetically possessed and the denominations in which they had it. As with earlier studies, participants had to decide which denomination they would use, given they could only purchase one unit of the product and had no other means of payment.

Participants in the $100 spend level conditions were shown images of a $50 bill and fifty $1 bills, and indicated which denomination(s) they would use to buy either a perfume (price = $50) or a shampoo (price = $10). In the $200 spending level conditions, participants were provided with a $100 bill and one hundred $1 bills, and decided which denominations to use in purchasing either a camera (price = $100) or a shampoo (price = $10).

In addition to the measures on product familiarity and product attitude used in study 2, participants also responded to two measures on purchase convenience: "How convenient is $__ bill for making purchases?", "How convenient is $__ bill for carrying in a wallet?"; denomination familiarity: "How often do you purchase items using [. . .] bill?" (1 = "never"/10 = "very often"); and product affordability: "How affordable did you find the [product]?" (1 = "not affordable"/10 = "very affordable"). Finally, participants indicated their age, gender, and income levels.

Results

The dependent variable was the proportion of participants paying with the smaller denomination. As shown in **Figure 4A**, when participants had to purchase products with prices that matched the larger denomination at hand, they were less likely to use the smaller $1 bills they had ($50 perfume: 29% [$\chi2(1)$ = 28.20, z = −5.31]; $100 camera: 32% [$\chi2(1)$ = 13.37, z = −3.66], ps < 0.001), and overwhelmingly chose to purchase using the denomination that matched the price of the product. It is worth noting that they could have eliminated their stack of $1 bills that are inconvenient to carry and store; instead, participants chose to hold on to them and pay with the larger denomination, which is evidently easier to carry and store.

On the other hand, when participants had to purchase the $10 shampoo, they were more likely to use their smaller $1 bills compared to using their larger bills ($100 spend level: 85% [$\chi2(1)$ = 81.06, z = 9.0]; $200 spend level: 93% [$\chi2(1)$ = 82.29, z = 9.07], ps < 0.001). Logistic regressions controlling for the convenience measures as well as other covariates indicate that the effect is robust and that people were less likely to get rid of large number of smaller denominations when the high price anchor was available (log-odds = −2.39,for $50 product, and log-odds = −4.38 for $100 dollar product, ps < 0.001, see **Supplementary Appendix 6**). We do not find any covariates consistently significant for both $100 and $50 product, however some individual covariates were significant for an individual product (see **Supplementary Appendix 6**).

We further analyze participants' responses to the convenience measures. First, independent of the condition, all participants (550) judged $1 bills as less convenient to carry than larger bills ($M_{\text{smaller_denom}}$ = 4.83 vs. $M_{\text{larger\_denom}}$ = 7.78, $t(549)$ = 16.44, p < 0.001). This indicates that our manipulation worked: driving the number of the smaller denominations to the

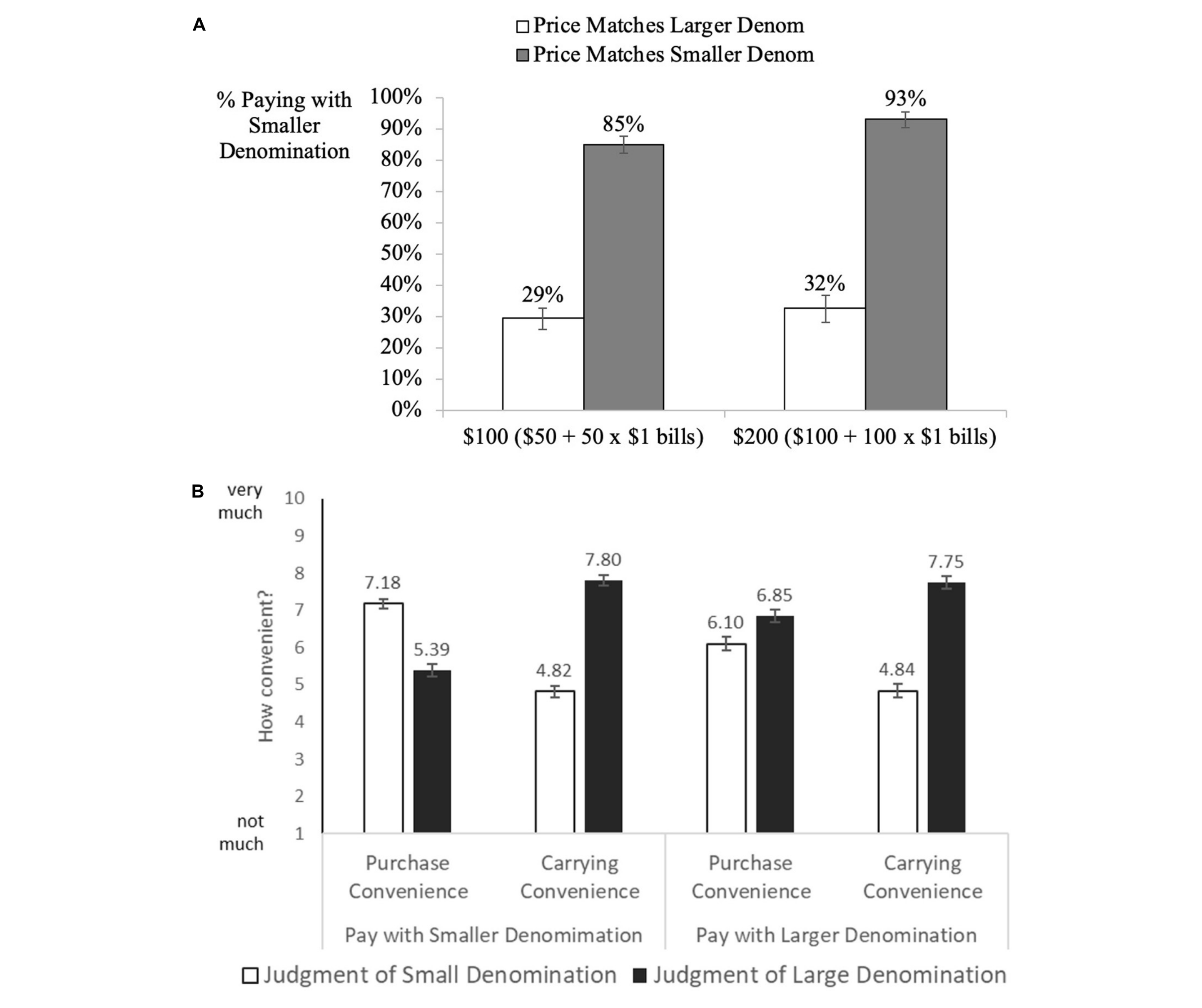

FIGURE 4 | (A) Study 4. Influence of the price-denomination effect on denomination choice manipulating storage convenience. Error bars denominate std. errors. **(B)** Study 4. Judgement of purchase and storage convenience by type of bill one paid with. Error bars denominate std. errors.

extreme made people judge smaller denominations as less convenient to carry (compared to carrying convenience in study 3). Regarding purchase convenience, similarly to study 3, on average, participants judged smaller denomination to be more convenient to purchase with than the larger denominations ($M_{smaller\_denom}$ = 6.75 vs. M_{larger_denom} = 5.98, $t(549) = -4.46$, $p < 0.001$). Next, we compared the responses from participants that chose to pay with the larger denominations ($n = 222$) to those who chose to pay with the smaller ones ($n = 328$). Participants in both groups rated larger denominations more convenient to carry (those who chose the smaller denomination: $M_{smaller\_denom}$ = 4.82 vs. M_{larger_denom} = 7.80, $t(327) = 12.47$, $p < 0.001$; those who chose the larger denomination: $M_{smaller\_denom}$ = 4.84 vs. M_{larger_denom} = 7.75, $t(221) = 10.72$, $p < 0.001$), indicating that carrying convenience did not drive their choice of denomination. With regards to purchase convenience, participants who chose the smaller denomination rated it more convenient for making purchases ($M_{smaller\_denom}$ = 7.18 vs. M_{larger_denom} = 5.39, $t(327) = 8.47$, $p < 0.001$) while the opposite was true for those who chose the larger denomination: $M_{smaller\_denom}$ = 6.10 vs. M_{larger_denom} = 6.85, $t(221) = -2.96$, $p = 0.003$), see **Figure 4B**.

Discussion

These results lend further support for the price-denomination effect. We replicate the effect even when the number of smaller denominations is stretched to the extreme and when we control for carrying convenience. The findings run contrary to the storage convenience explanation that consumers would get rid of large numbers of smaller denominations. Rather, we find that when the price of the product ($50 perfume or $100 camera) matches the value of the larger denominations, a majority of participants prefer to pay with the larger denomination carried, despite the inconvenience of carrying fifty or one hundred $1 bills.

Studies 2-4 provided initial evidence that "price-denomination effect" can be explained by anchoring, and that the effect is robust beyond storage or purchase convenience (see also **Supplementary Tables A2, A3, A12–A15** for storage and purchase convenience results in regressions and results of **supplementary studies**). However, one potential explanation for the price-denomination effect is "value-matching" between the subjective value assigned to the product and the denomination. That is, because higher priced products as well as larger denominations might be perceived as more valuable, larger denominations might be used to purchase higher priced products due to value-matching of the perceived product and denomination value. Similarly, people might prefer to buy lower-priced products with smaller denominations because both lower priced-products and smaller denominations have lower "perceived value" in the eyes of the customer. We examine this potential mechanism in study 5. Specifically, in the next study, we investigate whether consumers have a tendency to transfer the value of ownership from the physical money they possess to a product.

STUDY 5. TESTING VALUE MATCHING

Study 5's aim was to test a potential value matching explanation. Research has shown that people have a sense of higher ownership value for larger bills compared to smaller bills under conditions of increased social presence (Di Muro and Noseworthy, 2013, study 2). Therefore, it is possible that, given such conditions, consumers transfer the value of ownership for money bills they possess to the purchase. This would result in an alternative route that would also predict that consumers will use a lower valued denomination to pay for a lower valued purchase. On the contrary, the price-denomination effect predicts that price information influences choice of which bills consumers choose to purchase with, regardless of perceived product value.

Method

Participants and Design

Four hundred and thirty-eight participants (female = 32%, M_{age} = 26.75, SD = 8.89) from the Prolific Academic panel were recruited in exchange for modest compensation. Participants were assigned at random to either condition in a 2 (purchase price: €10 vs. €50) × 2 (gift type: gift for a jerk boss vs. valentine gift) between-subjects design. The amount of money received including the denominations was held constant across treatment conditions.

Materials and Procedure

Participants in the jerk boss' gift condition imagined that, after receiving a €100 cash bonus (in a single €50 bill and five €10 bills), their work colleagues informed them of their boss' upcoming anniversary. They all decided to contribute cash (€10 or €50, depending on the assigned condition) towards purchasing a gift for the boss. However, no one, including the participant, liked the boss. Those in the Valentine's Day gift condition imagined they wanted to purchase a Valentine's Day gift for a friend. Similar to studies 2 and 4, participants were told how much money they had and the denominations in which they had them. Participants were asked to indicate which denomination they preferred to use in purchasing the gift. This served as our dependent variable. Next, participants responded to a survey that included a measure of purchase convenience based on their response to the denomination choice question: "How convenient was it to pay [€10/€50] for your [boss'/friend's] gift using a [€10/€50] bill?" (1 = "not at all convenient"/10 = "extremely convenient"), an attention check, gender, age, and income level. Five responses were excluded for failing an attention check, leaving a usable sample of N = 433.

Results

Denomination Choice

As shown in **Figure 5**, participants were more likely to pay using the smaller €10 bills when the price of the purchase matched their smaller bills, regardless of how much they valued the recipient (jerk boss' gift: 90% [$\chi 2(1) = 70.4$, $z = 8.39$]; Valentine gift: 91% [$\chi 2(1) = 70.74$, $z = 8.41$], $ps < 0.001$). On the other hand, participants were less likely to use the smaller bills when the donation/purchase matched the larger denomination (jerk boss' gift: 24% [$\chi 2(1) = 28.27$, $z = -5.32$]; Valentine gift: 19% [$\chi 2(1) = 41.18$, $z = -6.42$], $ps < 0.001$). See further results on convenience in **Supplementary Appendix 6.**

Discussion

Study 5 continues to support and build on previous results. On one hand, we replicate the price-denomination effect under different circumstances. In previous studies 1-4, participants were presented with scenarios where they decided on purchasing items for themselves. We replicate our previous findings in study 5 where participants decided on purchasing a gift for a less valued recipient. Study 5 also eliminates the explanation that greater value being placed on the ownership of larger bills influences our results. Rather, we replicate the price-denomination effect

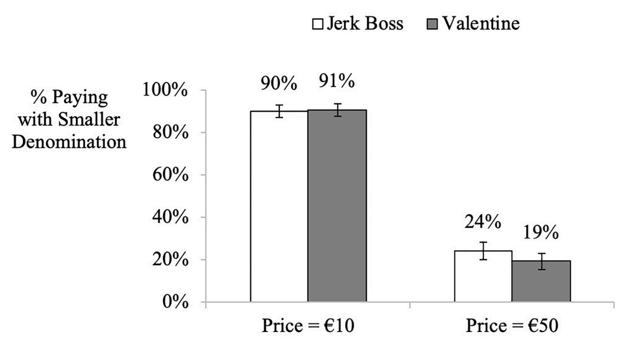

FIGURE 5 | Study 5. Influence of price information on denomination choice manipulating gift type. Error bars denominate std. errors. The y-axis represents the proportion of participants who chose to pay with the smaller denomination they possessed. All participants possessed a single €50 bill and five €10 bills.

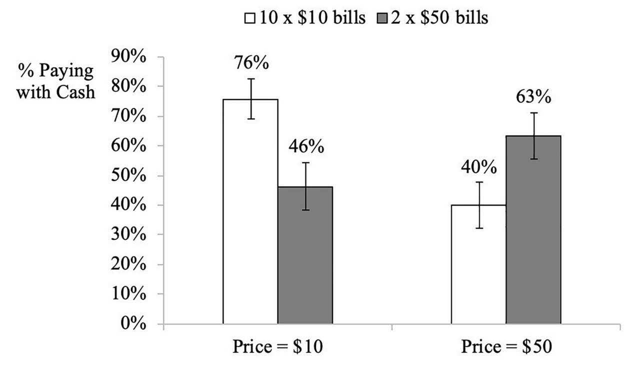

FIGURE 6 | Study 6. Influence of price-denomination effect on payment choice. Error bars denominate std. errors. The *y*-axis represents the proportion of participants who chose to pay with cash.

for a low valued recipient but highly priced product. The results indicate that denomination choice is unaffected by value matching tendencies. Instead, participants' choice of which denomination to purchase the gifts with was determined by the extent to which they relied on price cues.

The results so far suggest that the price-denomination effect can be extended to evaluations of payment mechanisms. It raises the question of whether price cues that trigger paying with denominations will influence payment choice in the presence of a more convenient option – a debit card. Research suggests that if individuals have the option of paying with a card vs cash their willingness-to-pay increases (Prelec and Simester, 2001). Can, therefore, payment with the card (vs. cash) attenuate the price-denomination effect? We suggest that the price-denomination effect would imply that people will use cash when denomination at hand is the same as the price to be paid, while use card when denomination at hand does not match the price. We test this idea in study 6 using an online panel.

STUDY 6. CHOICE OF PAYMENT FORM

Study 6's aim is to explore whether individuals are more likely to pay with cash (vs. a debit card) when possessing both payment forms. Previous research suggest that consumers prefer paying with card compared to cash because the former is more convenient (Feinberg, 1986) and associated with lower pain of paying (Prelec and Loewenstein, 1998; Raghubir and Srivastava, 2009). However, the price-denomination effect predicts that individuals will be more likely to pay with cash when the price matches the value of the money bill they hold, but not when it does not match it.

Method

Participants from Mechanical Turk (n = 160; 49% female; M_{age} = 40, SD = 11.95) were assigned at random to one of four conditions in a 2 (Denominations: two $50 bills vs. ten $10 bills) × 2 (Price: $10 vs. $50) between-subjects design. Participants read a vignette asking them to imagine they traveled by taxi and when they got to their destination, the taxi driver informed them of how much the taxi cost. Participants were told they possessed $100 dollars in either two $50 bills or ten $10 bills, depending on the condition to which they are assigned. Those in the lower price condition were told the taxi ride cost $10 while those in the higher price condition were told the ride cost $50. To control for the decoupling effect (Prelec and Loewenstein, 1998) and cash back benefits (Feinberg, 1986; Soman and Cheema, 2002), participants were further informed that they possessed a debit card, which could easily cover the cost of the ride, and which they could pay with if they so preferred. Next, participants indicated how they would choose to pay for the taxi ride (1 = cash/0 = debit card), as the main dependent measure.

Participants also responded to four convenience measures: "How convenient is it to use a [denomination] to pay for a taxi that costs [price]?"; "How convenient is it to use [debit card] to pay for a taxi that costs [price]?"; "How convenient is it for the taxi driver to receive payment in a [denomination] for a ride that costs [price]?"; "How convenient is it for the taxi driver to receive payment with debit card for a ride that costs [price]?", all anchored (1 = "not at all convenient"/9 = "very convenient"); and pain of paying: "How much pain are you feeling right now about spending money on the taxi?" (1 = "not painful at all"/9 = "very painful"), adapted from Xu et al. (2015). Finally, participants indicated their age, gender and income levels. Two responses (1% of our sample) were excluded from the analyses due to failed attention check, leaving a usable sample of N = 158.

Results

The dependent variable was the proportion of participants who purchased with cash. As predicted, when the taxi ride cost $10, participants with ten $10 bills (76%) were more likely to pay with cash (vs. debit card) compared to those who had two $50 bills (vs. 46%: $\chi 2(1) = 7.31$, $p = 0.006$). In contrast, when the taxi ride cost $50, participants who had two $50 bills (63%) were more likely to pay with cash (vs. debit card) compared to those who had ten $10 bills (40%: $\chi 2(1) = 4.18$, $p = 0.041$), see **Figure 6**.

We also examined whether convenience ratings varied when participants paid with cash (vs. card). One-way ANOVAs revealed that participants who paid with cash (M = 7.82, SD = 1.69) compared to those who paid with card (M = 6.09, SD = 2.54) indicated higher convenience for cash payment ($F(1, 156) = 26.37$, $p < 0.001$, $\eta^2 = 0.17$), and higher convenience for others receiving cash payment (M_{Cash}: 7.56 vs. M_{Card}: 6.86, SDs: 2.07 vs. 2.45, $F(1, 156) = 3.85$, $p = 0.052$, $\eta^2 = 0.02$). Conversely, participants who paid with cash (M = 6.85, SD = 2.15) compared to those who paid with card (M = 8.20, SD = 1.51) indicated lower convenience levels for card payment ($F(1, 156) = 19.69$, $p < 0.001$, $\eta^2 = 0.13$), and lower convenience for others to receive card payment (M_{Cash}: 6.61 vs. M_{Card}: 7.59, SDs: 1.99 vs. 1.86, $F(1, 156) = 10.13$, $p = 0.002$, $\eta^2 = 0.06$). We observed no difference in pain of paying between choice of payment form ($F < 1$). It is unclear whether the convenience ratings reflect the choice that has been made (Cesario et al., 2004), or drive it.

Discussion

Study 6 results add further evidence of a price-denomination effect. The results suggest that consumers' payment preference (cash versus card) may shift depending on whether the price they encounter matches the denominations they hold.

GENERAL DISCUSSION

In this paper we investigated the price-denomination hypothesis and found that individuals are likely to choose the denomination that is exactly the same as the price of the product to be purchased (studies 1A-1B). We further examined the role of anchoring on price as a potential mechanism for this effect and found that people make faster decisions when they choose to pay with a denomination that matches the price (study 2). Study 3 showed that the price-denomination effect holds for prices 10% below the denomination value, controlling for storage and purchase convenience and suggested that purchase convenience is unlikely to explain the results. Study 4 replicated the effect even when the number of smaller denominations was stretched to an unusually high number, showing that storage convenience is also unlikely to explain our results. In study 5, we tested whether the value-matching between the product and denomination reduced the price-denomination effect and found no evidence that the effect is driven by value matching. Finally, study 6 tested the extension of the effect to situations in which consumers choose between paying with cash or a debit card. We found that consumers are more likely to use cash over debit card when the price matched the cash denominations the customers held, suggesting that price-denomination match also influences the choice of payment method.

Despite the rise of mobile payments, cash as a payment method is as strong as ever. A majority of consumers pay with cash in most countries (Bagnall et al., 2016; Rosenbaum, 2018). In fact, the amount of cash in circulation has increased over time. In the United States, cash in circulation has grown at $\geq 5\%$ for the past two decades. The number of notes in circulation has doubled to 40 billion between 1996 and 2016. In Europe, the growth is even higher at 6%. It is, therefore, important to understand how consumers handle cash (Matheny et al., 2016; Wheatley, 2017). One of the main features of cash is its denomination. Studying how consumers deal with larger denominations is of particular importance given recent trends in currency circulation – demand for larger denominations rose drastically in the United States after 2008(Matheny et al., 2016; Wheatley, 2017)- as well as recent calls for the elimination of large paper-money (Rogoff, 2016). Does the difference in denominations affect the way we use them? The work we present here suggests that it does, and it is a function of the price of the product one is purchasing. In particular, we find that people pay with denominations that are the same as product price, even when it is more convenient to get rid of smaller denominations. In our studies people consistently showed a tendency to use larger denominations for higher priced products, and lower denominations for lower priced products: the price-denomination effect. This effect replicates across consumers from different continents, online, in the lab and in the field, and using different currencies.

We propose that one of the possible routes through which the effect operates is that a price serves as an anchor that guides the choice of denominations. Results of study 2, which tested anchoring as a possible mechanism as well as measured reaction times, are consistent with this proposed explanation. The theory we present here focuses on specific situations where the price of the product is exactly the same as one of the denominations carried. We also present empirical evidence that adds to the boundary conditions of our findings and shows that this effect goes beyond exact match and holds for prices up to 10% below the denomination. Beyond this deviation, the effect is not stable and attenuates or disappears. How people select denominations in other possible scenarios (for example, when the price is 40% below the larger denomination) is out of scope of this paper but is necessary to study in future research in order to build an all-encompassing theory of the choice of denominations used in different scenarios. We now discuss the implications of this research for theory and practice.

Theoretical Contributions

Our contribution to previous research is twofold. First, this research contributes to the literature on anchoring on price information. Previous research suggested that consumers use price information as an anchor, and that this anchor influences their information processing, their internal reference price, and ultimately their belief about the value of the product (Morwitz et al., 1998; Thomas and Morwitz, 2005; Chandrashekaran and Grewal, 2006). Our research shows that product price also affects the choice of the denomination consumer pays with. That is, we demonstrate yet another influence that price exercises on consumers as an anchor. Consumers holding small and large denominations choose the small denomination when it is the same as the low price, and the large denominations when it is equal to the high price. Consumers anchor on the price when deciding which denomination to use. In fact, when the product price matches the denomination being held, consumers decide on which denomination to use faster. Our results are aligned with research on judgment anchoring, which also relies on response latencies to examine anchoring effects (Epley and Gilovich, 2001; Mason et al., 2013).

Second, our work contributes to the stream of research on the subjective value of money. Standard economic theory would posit that money is ultimately money, regardless of what we could call circumstantial specificities, such as the method of payment (cash, credit card, etc.), the currency (dollars, euros), or the denomination (ten $10 bills or one $100 bill). But numerous researchers have uncovered violations of this descriptive invariance principle. For instance, when people think about money, they tend to do so in nominal value rather than in real monetary value (Shafir et al., 1997). Relatedly, when people value a product in a foreign currency they have a tendency to be more influenced by the face value, without making the necessary adjustments for the exchange rates (Raghubir and Srivastava, 2002). Also, the salience of money (whether money is introduced earlier or later in a decision) influences how people discount money, which in turn influences their choices and decisions (Jiang et al., 2016). Another way in which consumers violate the descriptive invariance principle is by spending more when they use a credit card versus when they use cash (Raghubir and Srivastava, 2008), and spending more when holding smaller versus larger denominations (Mishra et al., 2006; Raghubir and Srivastava, 2009). Our research is particularly relevant to an area within this body of work: the impact of prices that match denominations (smaller or larger bills) posed on consumer behavior. We investigate whether price influences the choice of denominations, and, accordingly, add to the literature on the subjective value of money.

Implications

Our work has implications for policymakers as well as businesses. Paper money seems to be at a crossroads. On the one hand, it is not only widely used but actually its circulation increases over time. On the other, there are important (even moral) reasons why certain stakeholders ask for it to being phased out, particularly large bills (Rogoff, 2016). Consider for instance the EU decision in 2016 to discontinue production and issuance of €500 bills, or the Indian Government's decision in 2016 to demonetize the Rs 500 and Rs 1000 bills and replace them with new Rs 500 and Rs 2000 bills. It is unclear how these moves affect consumer spending in the longer term. Will they lead to lower priced purchases in the EU, and higher priced purchases in India? This research suggests that the denominations in circulation may skew purchases of products and services that match those denominations. Policy makers may, therefore, benefit from a nuanced understanding of how consumers use cash. We hope our research can inform central banks' decisions on what denominations to issue and keep in circulation. Results from our research could also help commercials banks decide on how to dispense the appropriate denominations from ATMs; a decision that may be contingent on the retail context in which the ATM is located.

Our findings can also be used by managers willing to make their pricing and offerings more persuasive. They could consider adjusting their prices to the amounts represented by the bills in circulation in their particular market. This can be done in different forms: bundling or unbundling, modifying packaging sizes, etc. Such policies might also differ across countries. First, different countries have different denomination values and, therefore, pricing and bundling of products might be influenced by denominations available in the country. Second, one can speculate that stores in those countries where cash is used more often than other payment forms could benefit from such policies more than countries where other electronic methods of payment are dominant. By the same token, consumers would also benefit from understanding the price-denomination effect, and remain vigilant against overspending.

The results also have implications for when people choose between payment forms. Past research suggests that since parting with cash is psychologically more painful than parting with other money forms (Prelec and Loewenstein, 1998; Duclos and Khamitov, 2019), people will spend more using cards as compared to cash (Prelec and Simester, 2001; Soman, 2003; Raghubir and Srivastava, 2008). Our findings from study 6 suggest that, different from what previous work has documented, there is no difference in the pain of parting with money between the cash condition and the card condition. Therefore, the price-denomination effect can possibly be a boundary condition for the "pain of payment" feeling: consumers may choose cash payment over more convenient electronic payment forms when the price is the same as the denomination at hand.

Areas for Future Research

We believe there are several interesting directions for future research. First, it would be interesting to study the affective consequences of the choice of denominations. The consequences of paying with denominations matching the price of the product may lead to positive emotions. Based on the work by Mishra et al. (2006), it is possible that in situations where consumers have the exact denomination matching the price of the product, they may feel the transaction was more perceptually fluent when they paid using the matching denomination (Alter and Oppenheimer, 2008). This should lead them to feel happier about the transaction and contribute to a positive shopping experience. We suggest this topic as a promising avenue for further research.

In this paper we identified one possible mechanism behind price-denomination effect: price anchoring. Though we found no evidence for some potential explanations of the price-denomination effect (storage convenience, purchase convenience, exact price matching or value being placed on the ownership of larger bills), we could not rule out other alternative explanations. Since most of the effects are multiply-determined, another interesting avenue for further research is to examine the other possible antecedents of price-denomination effect.

What are the possible moderators of price-denomination effect is another topic worth investigating. It would be interesting to explore whether the price denomination effect is weakened/strengthened under condition of time pressure, purchase of healthy vs unhealthy items, or situations where deliberation is encouraged. For example, it is possible that competence in manipulating cash or different abilities in doing mental arithmetic could moderate the price-denomination effect, especially under conditions of cognitive load, or time pressure. For example, imagine that you are running late and need to pay a taxi driver. It is plausible that you would aim to minimize the cognitive effort of doing mental arithmetic, as well as minimize possible errors of the transaction and would be more likely to use the same denominations as the price of the service, not only because of anchoring, but also because of cognitive load and time pressure. The exacerbating effects of time pressure and cognitive load on the price-denomination effect are suggested as future avenues for research.

In the studies reported here, we found that a minority of participants chose to pay for a higher-priced items with smaller denomination even when an exact-matching larger denomination was available, as well as vice versa. This could be a result of noise due to making a random choice in an online study. Consistent with this speculation, the percentage of mismatched denomination choices was the lowest in the field studies 1A and 1B. The minority that did choose to pay with a mismatched bill was always below 50% (likelihood of choosing one of two denominations totally at random, see **Table 1**). In the studies reported in the **Supplementary Appendix 1**, however, when the denominations were held in the form of coins (versus notes) a significantly higher proportion of participants than would be indicated by random choice, chose to pay with their smaller denominations. One can speculate that the use of coins may be a boundary condition for the price-denomination effect. While for bills carrying convenience was not at play, coins may be more inconvenient than paper bills, which may affect people's decisions to choose to pay with them if possible. This research was not designed to compare coins to bills, but it can be an interesting area of future research.

As noted above, our results can also be interpreted using a mental accounting perspective. Mental accounting was primarily conceptualized as a cognitive process "to organize, evaluate, and keep track of financial activities" (Thaler, 1999). Mental accounting has since then been applied to a large number of financial and non-financial behaviors (for a recent review, see Zhang and Sussman, 2018.) While we note this intriguing possibility, we believe future research could formally test whether consumers do, indeed, assign different mental accounts for different denominations: for example, $1's and $5's for tips and coffee, $10's and $20's for outdoor food, fruit and flower shopping, and $50's and $100's for contractor's services.

One of the limitations of our empirical work is that, for the most part, it assumes that consumers are aware of the bills they carry in their wallet. However, there are systematic biases in the recall of denominations held in one's wallet that favor the recall of fewer larger denominations over the more numerous smaller ones (Raghubir et al., 2017). This opens up interesting lines of future work. How do consumers react if they realize they do not carry the denomination that matches the price? Do they finalize the purchase? Is their evaluation of the purchase intact? We also did not test how customers feel about their purchases (apart from pain of paying in study 6). Studying the influence of the price-denomination match on satisfaction with purchase is an interesting avenue for further investigation. For example, one could speculate that if a product costs $50 and it turns out that the consumer does not carry this denomination in her wallet and, instead, needs to use smaller bills, she might be less satisfied with the purchase. In addition, prior research has established that consumers have a stronger memory trace of expenses made with cash as compared to credit cards, leading to greater future spending with credit cards as compared to cash (Srivastava and Raghubir, 2002). Therefore, it would be interesting to examine whether the same price and denomination lead to a weaker memory trace than a purchase made when they are different, and consumers engage in cognitively effortful mental arithmetic. Assessing memory and subjective states of the customers facing such purchases are interesting avenues to explore in future research.

Finally, we mainly measured purchase convenience for the purchasing agent directly. Another potential mechanism could be examining the transactional convenience for the person receiving money as a payment (see **Supplementary Appendix 7** for some preliminary results for the party receiving cash might matter). For example, Di Muro and Noseworthy (2013) demonstrated that students were more likely to break crisp (vs. worn) bills in a social context, when others could observe their transaction due to pride in ownership of the crisp bill. In a "private" context, however, pride in ownership made participants more likely to purchase with the worn bills. In our context, in study 4, it is possible that people did not want to get rid of fifty $1 bills so as not to be perceived as "cheap" by the experimenter or the party receiving the money.

More generally, it would be interesting to investigate whether the price-denomination effect influences spending decisions, which was out of the scope for our paper. Differences in spending could happen in at least two ways: (*1*) do consumers spend more or less, depending on whether the price of an item matches their denomination at hand?, and (*2*) does carrying specific denominations influence consumer choices of specific products?

We believe these are interesting and relevant questions, and we encourage future research to examine them.

DATA AVAILABILITY STATEMENT

Data, study protocols and analysis codes are publicly available at https://osf.io/syvrm/. The flows of the field experiments are described in the text directly.

ETHICS STATEMENT

Participants in laboratory and online experimental studies involving human participants provided their written consent to participate in the studies. Participants in the field studies did not provide any written consent due to the nature of the studies (they were not aware of participating in the experiment and rather responded to the promotion of the local retailers with which we partnered for the study). The studies involving human participants were reviewed and approved by Comité de Ética de la Investigación de la Universidad de Navarra.

AUTHOR CONTRIBUTIONS

ER, JI, and PR contributed to the idea, design of the studies, data analysis, and results interpretation. JI did the data collection. ER, JI, PR, and IG did the write-up of the manuscript. All authors contributed to the article and approved the submitted version.

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SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2020.552888/full#supplementary-material

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An Attempt to Correct Erroneous Ideas Among Teacher Education Students: The Effectiveness of Refutation Texts

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There is sound evidence about the high prevalence of misconceptions about education among pre-service teachers. This trend continues after students complete the degree in education and once they are in the exercise of their profession. In fact, several studies show that these misconceptions are widespread among in-service teachers. Erroneous ideas about education may divert material and human resources to poor grounded methods and teaching tools, compromising the quality of education. Strategies to debunk misconceptions among future teachers, who may not have a firm position about many educational issues, might contribute to reversing this trend. The main goal of the present study was to assess the efficacy of refutation texts in the correction of misconceptions among pre-service teachers. As in previous studies with in-service teachers, refutation texts were effective in reducing participants' endorsement of misconceptions. But this effect was short-lived and did not affect participants' intention to use educational methods that are based on the misconceptions addressed in the refutation texts.

Keywords: misconceptions, refutation texts, intervention, pre-service teachers, education

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INTRODUCTION

One of the most important strategies to guarantee high quality teaching is to endow teachers with subject-matter knowledge and a repertoire of evidence-based pedagogical skills (Ingvarson and Rowe, 2008). However, teacher education students in many training colleges are often invited to rely on observation and hard-earned experience rather than on rigorous, high-quality research and evidence when selecting educational methods for the classroom (Seidenberg, 2013). Moreover, teacher education programs do not always include authoritative educational research findings (Moats, 1999; Gersten, 2001) nor content knowledge about how research is conducted and how to interpret its findings (Levin, 2013; Seidenberg, 2013; Hammersley-Fletcher and Lewin, 2015). At the same time, there is a huge market of courses, workshops, and books that offer a wide range of pseudoscientific theories and methods about how to improve learning, such as Brain Gym® (Hyatt, 2007) or The Glenn Doman Method (Edkin, 1987). Not surprisingly, many pre-service teachers hold a substantial number of erroneous ideas about education. For instance, it has been shown that many of them believe that hemispheric dominance can explain individual differences among students (i.e., Fuentes and Risso, 2015; Tardif et al., 2015) or that letter reversal is a common symptom of dyslexia (i.e., Washburn et al., 2014; Soriano-Ferrer et al., 2016). This trend continues after students complete their degree in education and during the exercise of their profession. In fact, the high prevalence of erroneous ideas among in-service teachers has been widely documented all over the world (e.g., Dekker et al., 2012; Ferrero et al., 2016).

The prevalence of misconceptions among pre-service and in-service teachers can have serious consequences in the quality of education, as these beliefs pave the way for ill-grounded methodologies and might impede the adoption of effective procedures of teaching (Goswami, 2006; Busso and Pollack, 2014). To mention just a few examples, the popularity of learning styles has motivated many teachers to divert their time and resources to adapting their way of teaching to the learning styles of their students. However, there is sound evidence against this practice (Coffield et al., 2004). Similarly, the groundless idea that reading disabilities are caused by abnormal eye movements has often favored the use of optometric exercises on children with dyslexia (Handler et al., 2011), at the expense of training in well-founded aspects of literacy such as alphabetic principle or word recognition (National Reading Panel, 2000).

One possible solution to this problem is to explicitly address the erroneous ideas among teachers (Pintrich et al., 1993). Unfortunately, the available evidence shows that, once adopted, misconceptions can become quite resistant to change (Lewandowsky et al., 2012), even when they have been already recognized as erroneous by the target audience (Johnson and Seifert, 1994). In addition, not all methods to address misconceptions are equally valid and, in some cases, they can even backfire, that is, they can strengthen the target ideas instead of challenging them (i.e., Nyhan and Reifler, 2010; Nyhan et al., 2013; Nyhan and Reifler, 2015), although this finding has not always been replicated (Haglin, 2017; Swire et al., 2017; Wood and Porter, 2019).

In this context, refutation (or refutational) texts have received special attention as a simple means to change misconceptions (Guzzeti et al., 1993; Tippet, 2010; Lewandowsky et al., 2012). Refutation texts are defined as those that describe a common theory, belief, or idea, refute it, and offer a satisfactory alternative (Guzzetti, 2000). In general, the evidence collected indicates that refutation texts are a powerful tool for addressing erroneous ideas (Guzzeti et al., 1993; Tippet, 2010; Lewandowsky et al., 2012). This might be due to their effectiveness in creating some of the conditions necessary to induce a conceptual change among people. More precisely, according to Posner et al. (1982), refutation texts can provoke dissatisfaction with current conceptions and provide an alternative explanation to the audience. Preferably, this explanation must be *intelligible*, and not more difficult to understand than the current conceptions (Lombrozo, 2007); *plausible*, that is, it must be helpful to resolve the problem generated and also consistent with other knowledge; and *inspiring* to open up new areas of inquiry (Posner et al., 1982).

Refutation texts have received some attention in teacher education. For instance, Hynd et al. (1997) analyzed the changes induced by these texts in the conceptions of pre-service teachers about projectile motion. Likewise, Salisbury-Glennon and Stevens (1999) and Kutza (2000) tested the effectiveness of refutation texts to elicit a conceptual change in motivation knowledge among teacher students. Finally, Gill et al. (2004) addressed the epistemological beliefs of pre-service teachers about mathematics through the use of this tool. In Salisbury-Glennon and Stevens (1999), refutation texts were tested alone, while in the rest of studies they were assessed in combination with other elements such as real demonstrations (Hynd et al., 1997), alerts about conflicting information (Gill et al., 2004), or rewards for adjusting conceptual change to the expert opinion stated in the refutation texts (Kutza, 2000). All in all, the evidence gathered in these studies showed that refutation texts enabled the correction of erroneous ideas among teacher students although in general a full correction was not achieved and in some cases their effectiveness depended on the addition of extra elements (Kutza, 2000; Gill et al., 2004). The only study that measured the effects of refutation texts in the long run found that their impact remained significant 2 months later (Hynd et al., 1997).

In a recent experiment, we tested the use of refutation texts to correct some of the most prevalent misconceptions about education among in-service teachers (Ferrero et al., 2020). Along with this, we aimed to determine if the inclusion of information discrediting the origin of the misconception had any influence on the effectiveness of refutation texts. The results showed that, in the short run, refutation texts were effective at debunking misconceptions about education among in-service teachers, although the addition of information discrediting the origin of the misconceptions did not increase their impact. However, all the effects disappeared in a month and, most importantly, the manipulation failed to change teachers' intention to use educational methods based on misconceptions. Overall, the results of Ferrero et al. (2020) converge with those of previous research showing that, once adopted, misconceptions are highly resistant to change.

As mentioned above, teacher education students hold a large number of misconceptions which prevail over time and might affect the exercise of their profession (Goswami, 2006; Busso and Pollack, 2014). Because of their continuous exposure to ideas and educational practices of dubious validity, in-service teachers may show positions radically opposed to the message presented in refutation texts. In contrast, pre-service teachers may not yet have firm positions on several educational issues and hence an intervention focused on debunking misconceptions in this sample might yield more promising results. Following up on this hypothesis, the aim of the present study was to replicate the results of Ferrero et al. (2020) with teacher education students. In brief, the experiment consisted of three phases. During Phase 1, we measured the prevalence of different misconceptions through a multiple-choice questionnaire. During Phase 2, we exposed each participant to three conditions (refutation text with information about the origin of the misconception, refutation text alone, and no text) and immediately afterward we measured again the prevalence of the target misconceptions. During Phase 3, we measured for a last time the prevalence of the misconceptions. We introduced two modifications in comparison with Ferrero et al. (2020). First since the degree of endorsement for misconceptions had no effect in the preceding experiments with in-service teachers, we did not consider this variable in the analyses. Second, as the timing of the present study coincided with participants' completion of their undergraduate degree, we included two additional questions aimed at exploring whether participants had received or searched for extra information about the target misconceptions over the course of the experiment.

MATERIALS AND METHODS

Participants

As in Ferrero et al. (2020), to recruit participants for the study, we sent personal invitations to the headmaster of each college by email. After accepting to participate, we jointly established the schedule of the research. The day before the start of each phase, the first author (MF) sent the link of the corresponding experimental task to the teachers who agreed to collaborate in the experiment. On the intervention days, students received the link from their teachers and completed the tasks during class time.

Due to the difficulty in recruiting the target sample, our intention was to test the maximum number of participants that we could reach using the same recruitment strategy as in Ferrero et al. (2020). The power analysis conducted in Ferrero et al. (2020, Experiment 2) shows that at least 23 participants are needed to detect an effect of the manipulation on misconceptions in Phase 2 in a two-tailed test with 85% power. The final sample included 64 elementary education majors (40 female) from two different education colleges in the Basque Country, Spain. The mean age of the sample was 20.47 ($SD = 1.52$). Participants were enrolled in the second (42%) and third year (58%) of the college degree.

Materials

Unless noted otherwise, the materials were identical to those of Experiment 2 in Ferrero et al. (2020). All these materials are available in the **Supplementary Material**.

Phase 1

We employed a three-part questionnaire. The first part contained an informed-consent form and requested background information about the participants. The second part contained 36 statements about education and neuroscience applied to education. Eighteen of them hold well-grounded evidence and the remaining half are based on null or very weak evidence and can be considered misconceptions. Participants were asked to judge the validity of each statement using a 5-point Likert scale ranging from 1 (Definitively false) to 5 (Definitively true). Although the questionnaire assessed endorsement for 36 statements, only nine of them were addressed in the experimental manipulation described below (Phase 2). Responses to the remaining 27 items were ignored in the statistical analyses. The third part of the questionnaire included 18 educational interventions. Half of these approaches referred to well-grounded practices, while the remaining nine referred to practices with very poor or null evidence that corresponded to the nine target misconceptions addressed during the intervention. Participants were asked to rate their intention to use or recommend each methodological approach through a 6-point Likert scale ranging from 1 (Definitively not) to 6 (Definitively yes). Only responses to the nine interventions addressed in the experimental manipulation (described below) were considered in the statistical analyses.

Phase 2

During this phase, nine of the 18 misconceptions included in the 36-item questionnaire were addressed (for the selection of the nine target misconceptions, see Ferrero et al., 2020). For each misconception, there was one refutation text with three different versions: (a) refutation text with an explanation of the origin of information and its credibility (TO); (b) refutation text alone (TA); (c) no text (NT). All the texts followed the same structured: At the beginning, the target misconception was introduced and, immediately afterward, it was refuted. Next, the origin of misinformation was discredited (only in the text-and-origin condition). Then, the alternative (and correct) information was presented. Finally, a rhetorical question was formulated.

Phase 3

As in Experiment 2 in Ferrero et al. (2020), during this phase, we employed the same questionnaire of Phase 1 with three additional questions. In the first two questions, students had to report whether they had searched or received additional information about the nine target misconceptions during the participation in the study. For each misconception, there were four response options: (1) I have not searched for information; (2) I do not remember having searched for information; (3) I have searched for information and it runs in the same direction of the refutation text; (4) I have searched for information and it runs in the opposite direction of the refutation text. To assess whether participants had received any information regarding each misconception during the study, in the second question "search for" was replaced by "received." The third question was aimed at measuring the level of difficulty of the refutation texts as perceived by students. To this aim, there was a Likert scale which ranged from 1 (Extremely easy) to 10 (Extremely difficult).

Design and Procedure

We conducted a within-subject study which consisted of three phases. During Phase 1, participants completed the on-line questionnaire described in the "Materials" section. Average completion time for Phase 1 was approximately 15 min.

As explained above, during Phase 2, nine misconceptions were assigned to three types of refutation texts described above (TO, TA, and NT). Consequently, each participant read six refutation texts in total (3 TO and 3 TA). Texts were presented in a random order for each student. Students could read each text as many times as they wished. Immediately after reading the texts, participants completed the same questionnaire used in Phase 1 for a second time. Average completion time for these two tasks (reading the texts and completing the questionnaire) was approximately 25 min. Between Phase 1 and Phase 2 there was a delay of 6 to 7 weeks.

During Phase 3, 30 days after Phase 2, participants completed the same questionnaire used in Phase 1 and Phase 2 for a third time along with the three additional questions described in the "Materials" section.

Participants completed the three phases of the study in a computer room of their college within the usual schedule. All the sessions were supervised by a teacher. The materials were presented on-line.

RESULTS

Figure 1 (left panel) plots the average endorsement ratings for the nine target misconceptions across experimental Conditions and Phases. The first observation that stands out is that utilizing refutational texts seems to have an effect on the rate of statement endorsement, as there is a decrease from Phase 1 to Phase 2. However, this effect does not seem to be long-lasting as there is an increase in endorsement rates in Phase 3.

We analyzed the data using a linear mixed-effects model with Condition (within-subjects; three levels: TO, TA, and NT) and Phase (within-subjects; three levels: Phases 1, 2, and 3) as fixed effects, and participant-specific random intercepts. The analysis showed a significant interaction between Condition and Phase, $\chi^2(4) = 28.60$, $p < 0.001$, suggesting that timing of intervention is crucial for the actual effect of intervention: specifically, as expected in Phase 1 there is no difference between conditions (all *post hoc* pairwise comparisons *ps* > 0.50). In Phase 2, the effect of the refutational texts causes a decrease in the rate of endorsement in the TO and TA conditions (both significantly different from the NT condition, *ps* < 0.001), but there is no difference between these two conditions, $t(1672) = 0.11$, $p = 0.63$. The effect of the refutation text (TO and TA conditions) diminishes in Phase 3 as there is an increase in the endorsement rates compared to Phase 2 (which reaches the endorsement ratings before any intervention is provided - as in Phase 1). While ratings in TO and TA conditions are still significantly different than the NT condition (both *ps* < 0.04), the present results suggest that temporal proximity to the actual intervention is an important determinant of the effectiveness of such interventions. Also, the main effects of Condition, $\chi^2(2) = 30.08$, $p < 0.001$, and Phase, $\chi^2(2) = 152.95$, $p < 0.001$, were significant.

We conducted the same type of analysis for the intention-to-use scale. **Figure 1** (right panel) presents a similar picture as with endorsement ratings, that is, a decrease in the intention-to-use ratings in Phase 2 followed by an increase in Phase 3. Overall, we observed a significant main effect of Phase, $\chi^2(2) = 72.76$, $p < 0.001$, suggesting that the intention to use was lowest in Phase 2 and highest in Phase 1. Similarly, the main effect of Condition was significant, $\chi^2(2) = 8.17$, $p = 0.017$: this is driven by a significant difference between TO and NT, $t(1669) = 2.62$, $p = 0.02$, whereas the remaining two pairwise differences (TA vs NT and TA vs TO) were not significant (both *ps* > 0.05). The interaction did not reach significance, $\chi^2(4) = 5.36$, $p = 0.252$. In addition, we included the judged difficulty as a covariate in both mixed-effects models (endorsement and intention-to-use ratings), but it did not result in better predictive power (likelihood ratio tests, both *ps* > 0.45).

We also explored whether searching or receiving information about the presented statements had an effect on the endorsement and intention-to-use ratings. For this analysis, we treated the

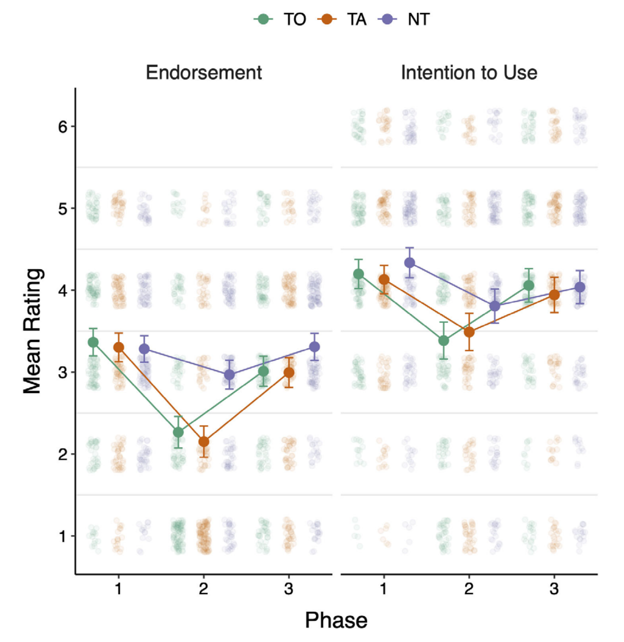

FIGURE 1 | Participants' endorsement (**left** panel) and intention-to-use ratings (**right** panel) on Phases 1–3. TO, TA, and NT refer to Text + Origin, Text Alone, and No Text, respectively. Error bars denote 95% CIs.

four response options (see section "Materials and Methods," *Phase 3*) as a categorical predictor in a linear mixed-effects model which also included Condition (and their interaction) as a fixed effect and participant-specific random intercepts. This is a rather exploratory piece of analysis as the independent variable is created based on participants' responses and it is not the result of usual methods of experimentation (i.e., random allocation to conditions). For example, in the search behavior question, the majority of participants had searched for additional information, with categories 3 and 4 accounting for 80% of all responses. For the endorsement ratings, we found an effect of search behavior, $\chi^2(3) = 13.01$, $p = 0.005$. Following up the significant main effect with pairwise *post hoc* tests (Tukey's adjustments), the only difference was observed between the extreme responses 1 ("I have not searched for information"; $M_{R1} = 3.33$) and 3 ["I have searched for information and it runs in the same direction of the refutation text"; $M_{R3} = 2.87$; $t(499) = 3.24$, $p = 0.007$]. This result suggests that searching for information which is consistent with the refutation text can potentially decrease endorsement ratings for inaccurate statements. This effect does not seem to be moderated by condition as both the main effect and the interaction did not reach significance (both *ps* > 0.25). The results about the intention-to-use ratings are similar: the main effect of search was significant, $\chi^2(3) = 16.85$, $p < 0.001$: as in the endorsement ratings, those statements that left unexplored received highest usage ratings ($M_{R1} = 4.38$) as opposed to those statements that were searched for and for which the information found was in line with the refutation text [$M_{R3} = 3.86$; $t(586) = 2.83$, $p = 0.025$]. There were also significant differences between response categories 1 and 2

[$M_{R2} = 3.39$; $t(560) = 3.77, p = 0.01$], and 2 and 4 [$M_{R4} = 4.12$; $t(552) = 2.66, p = 0.041$].

The pattern of results when considering whether participants had received any information about the misconceptions was similar to that of search behavior. The responses for this question are more balanced than the search behavior (R1 = 30.08%; R2 = 9.22%; R3 = 35.13%; R4 = 25.57%). In terms of endorsement ratings, the main effect of receiving information was significant, $\chi^2(3) = 25.46, p < 0.001$, with reliable pairwise differences between response category 1 ($M_{R1} = 3.22$) and 3 [$M_{R3} = 2.64$; $t(501) = 4.74, p < 0.001$], and 3 and 4 [$M_{R4} = 3.03$; $t(375) = 3.15, p = 0.0097$]. The interaction with condition was not significant, $\chi^2(6) = 4.64, p = 0.59$. For the intention-to-use ratings, we observed the same pattern: the main effect of receiving information is significant, $\chi^2(3) = 46.17, p < 0.001$, with reliable pairwise differences between response category 1 ($M_{R1} = 4.35$) and 3 [$M_{R3} = 3.59$; $t(587) = 6.69, p < 0.001$], 3 and 4 [$M_{R4} = 3.98$; $t(573) = 3.65, p = 0.0017$], and 1 and 4, $t(569) = 2.62, p = 0.044$.

DISCUSSION

The high prevalence of misconceptions about education among teacher education students is well-documented (i.e., Tardif et al., 2015; Soriano-Ferrer et al., 2016). These erroneous ideas, which are usually not corrected during college years and are even promoted through different channels, might jeopardize the adoption of effective methods in the classroom. Despite this, until now only a handful of studies have directly tried to combat this type of ideas (Im et al., 2018; Ferrero et al., 2020). The aim of the present study was to replicate the research of Ferrero et al. (2020) in a sample of pre-service teachers.

The results showed that refutation texts might reduce the number of misconceptions among teacher education students. Specifically, when presented within a refutation text, participants significantly reduced their belief in those erroneous ideas in comparison with the beliefs that were not refuted. As in Ferrero et al. (2020), adding information about the origin of the misconceptions (TO) did not produce better results than not providing it (TA). Once again, this result runs in the opposite direction of some studies which found that undermining the reliability of the misinformation or its source might promote beliefs correction (Lewandowsky et al., 2005; Guillory and Geraci, 2013). Interestingly, we found that the effect of refutation texts did not last over time. Thirty days after the intervention, the effects of refutation texts had decreased significantly. These results are in perfect agreement with the study performed with in-service teachers (Ferrero et al., 2020) and suggest that the effectiveness of refutation texts is largely determined by temporal proximity to the intervention. The reason that could explain the differences between these results and those obtained in the study of Hynd et al. (1997), where long-term effects were found, may lie in the type of ideas that were discredited in each case. In the latter, the misconceptions were about physics. Unlike educational topics, natural phenomena can inspire more confidence in expert voices and, in turn, not be so dependent on a community's cultural heritage.

Along with the reduction on the number of misconceptions, we were also interested in measuring the impact of refutation texts on the reduction of participants' intention to use educational practices that were based on the misconceptions refuted in the texts. Our results do not lend support to the hypothesis that the refutation texts changed participants' willingness to adopt educational practices that were based on the misconceptions. Although, intention-to-use ratings were numerically lower in the two conditions with refutation texts (TO and TA) than in the control condition (NT), these differences were already present in Phase 1, although not significant. And, in any case, there is no evidence whatsoever that those differences persisted in Phase 3. These results are also in line with our previous experiment with in-service teachers.

In the present study, we also explored whether after reading the refutation texts participants searched or were presented with additional information about the target misconceptions. About 80% of them stated that they searched actively for information and 61% stated that they had received information about the misconceptions. In general, those who searched or received information challenging the misconception showed lower endorsement and intention-to-use ratings than participants who did not search or receive this information or received information supporting the misconception. These results confirm that students receive a substantial amount of information about these misconceptions in their field of education (Moats, 1999; Gersten, 2001) and that this information does not always challenge the myth. In our analyses, whether or not students encountered information for or against, each misconception did not interact with the experimental manipulation. But it did have a main effect on endorsement ratings and intention-to-use ratings. Participants who actively searched for information and found that it run in the same direction as the refutation text showed, overall, lower endorsement and intention-to use ratings than participants who did not search for information. And participants who (passively) received information in agreement with the refutation text gave lower endorsement and intention-to-use ratings than those who did not receive any information at all or received information supporting the misconception. This fact is not trivial because teachers prefer known and nearby sources (Landrum et al., 2002; Cook and Schirmer, 2003) and, therefore, the rigor of the information sources closest to the centers play a crucial role. These findings have been confirmed in the present study, where challenging information found by students have had an effect on their beliefs.

Regardless of domain knowledge, misconceptions have been proven to be extremely resistant to change (Lewandowsky et al., 2012). In fact, individuals persist in relying on them even when they can recall a correction (Johnson and Seifert, 1994). Faced with this, some researchers have suggested that efforts to correct misinformation should target only to people with moderate rather that strong beliefs (Ecker et al., 2014). To some extent, the results of the present experiment support this recommendation. Participants in the study reduced their belief in misconceptions after reading the refutation text, but this effect disappeared shortly after the intervention and did not change their intention to use practices based on the refuted misconceptions. Future research should explore alternative means to extend the effects of refutation texts in the long run both on beliefs and educational practices. In this regard, it would be interesting to test the efficacy of refutation texts combined with other strategies that may maximize their impact, such as discussion groups, training in the scientific method, or inoculation. The latter is proving to be a promising strategy in several disciplines such as health or politics (Banas and Rains, 2010) and might be a welcome option to correct misconceptions among pre- and in-service teachers. For instance, this technique could be tested by warning participants that they are about to be fooled by incorrect information. In the same line, it would be valuable to measure the effects of refutation texts, alone or accompanied by other strategies, at different intervals to determine which is the most effective formula to get a verifiable and permanent impact on educational ideas and practices among pre- and in-service teachers.

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

ETHICS STATEMENT

This study was reviewed by the King's College London Ethics Committee. The participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

All authors listed have made a substantial, direct and intellectual contribution to the work, and approved it for publication.

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SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2020.577738/full#supplementary-material

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ORIGINAL RESEARCH
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When Success Is Not Enough: The Symptom Base-Rate Can Influence Judgments of Effectiveness of a Successful Treatment

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Patients' beliefs about the effectiveness of their treatments are key to the success of any intervention. However, since these beliefs are usually formed by sequentially accumulating evidence in the form of the covariation between the treatment use and the symptoms, it is not always easy to detect when a treatment is actually working. In Experiments 1 and 2, we presented participants with a contingency learning task in which a fictitious treatment was actually effective to reduce the symptoms of fictitious patients. However, the base-rate of the symptoms was manipulated so that, for half of participants, the symptoms were very frequent before the treatment, whereas for the rest of participants, the symptoms were less frequently observed. Although the treatment was equally effective in all cases according to the objective contingency between the treatment and healings, the participants' beliefs on the effectiveness of the treatment were influenced by the base-rate of the symptoms, so that those who observed frequent symptoms before the treatment tended to produce lower judgments of effectiveness. Experiment 3 showed that participants were probably basing their judgments on an estimate of effectiveness relative to the symptom base-rate, rather than on contingency in absolute terms. Data, materials, and R scripts to reproduce the figures are publicly available at the Open Science Framework: https://osf.io/emzbj/.

Keywords: causal learning, cognitive bias, patients' beliefs, base-rates, causal judgment

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INTRODUCTION

A great deal of health-related decisions, such as deciding whether or not to quit a treatment, or whether to replace it by an alternative option, depend on the patients' beliefs about their symptoms and diseases, and particularly about the effectiveness of their treatments. For instance, one of the main reasons for treatment drop-out is the belief that the treatment is producing little or no observable benefit (Leventhal et al., 1992; Dilla et al., 2009). Thus, understanding how patient's beliefs form and evolve is critical to developing strategies aimed at improving the trust and adherence to the prescribed treatments, and therefore fostering well-being among patients and users.

Previous research on experimental psychology suggests that many of these health-related decisions such as treatment adherence, or therapeutic choices, can be better understood as a result of causal learning (Rottman et al., 2017). That is, the users' beliefs about the effectiveness of the treatment are causal in nature, i.e., "the treatment causes the symptom remission," or "the treatment prevents me from falling ill." Thus, it is possible to study the patients' beliefs of treatment effectiveness through causal learning experiments (see reviews in Matute et al., 2015; Matute et al., 2019). This possibility offers a number of advantages. To begin with, we can study the formation of beliefs under highly controlled settings, by using fictitious scenarios and computerized tasks. This would be impossible in real life, in which researchers cannot manipulate parameters such as the frequency with which a treatment is used, its actual effectiveness, or the severity of symptoms. Thus, ecological studies would be limited because it is often impossible to run an experiment that unveils causal relationships between different factors and health beliefs, and most research would be limited to uncontrolled, observational studies. The second advantage of using causal learning experiments is that we can study health beliefs in a safe context, without putting the participant's health at risk. As this research normally involves using treatments with no actual benefit, or even inducing false beliefs of effectiveness, it would be unethical to conduct such studies with real health outcomes. Additionally, it is sometimes possible to use samples of real patients who deal with fictitious or imagined health outcomes in the context of a causal learning experiment (Meulders et al., 2018), which helps to alleviate the limitations of ecological validity while using highly controlled procedures.

This line of research that uses causal learning experiments to study health beliefs has shown some promising advances. For example, it is possible to predict which conditions will make patients and users more vulnerable to pseudomedicine and bogus health claims (Blanco et al., 2014; Blanco and Matute, 2020), to discover situations in which previously acquired beliefs interfere with actual effectiveness (Yarritu et al., 2015), to investigate how health beliefs are affected by biases in Internet search (Moreno-Fernández and Matute, 2020), to explain why certain patients are hypersensitive to pain symptoms (Meulders et al., 2018), and to improve the effect of placebos (Yeung et al., 2014). This knowledge has the potential to offer a valuable foundation for designing interventions aimed at debiasing dysfunctional beliefs in real life settings (Lewandowsky et al., 2012; Macfarlane et al., 2020).

Exploring Health Beliefs in the Laboratory

Most causal learning experiments exploit a basic principle of causality: causes and effects (outcomes) correlate with each other, unless a third factor masks this relationship. Since causality cannot be directly observed (Hume, 1748), people use this simple principle and rely on a proxy measure, the contingency between the cause and the outcome, to estimate causality (Allan, 1980; Wasserman et al., 1996; Vadillo et al., 2005; Blanco et al., 2010). In a simple situation with only one binary cause and one binary outcome, the contingency can be computed by means of the Δp index (Allan, 1980). This is simply the result of subtracting the probability of the outcome occurring given that the cause occurred, $P(O| C)$, minus the probability of the outcome occurring given that the cause did not occur, $P(O| \sim C)$. Large values of Δp correspond to situations in which the cause increases or decreases the probability of the outcome beyond the base-rate, $P(O| \sim C)$. The larger this difference is, the stronger the association between cause and outcome, and therefore the higher the chances that there is a causal link. According to previous research, this is how probabilities could produce causal beliefs in many situations (Perales et al., 2016).

In the context of judging a treatment's effectiveness, this reasoning amounts to computing how often the symptomatic episodes appear during the treatment, $P(O| C)$, compared to how frequent they are without the treatment, $P(O| \sim C)$. This comparison renders fairly in randomized controlled trials, in which two comparable groups of patients are recruited (i.e., experimental vs. control, or treatment vs. placebo). That is, clinicians often form their judgments on the effectiveness of a treatment after carefully comparing the two groups, and ensuring that occurrences of symptom remission are more frequent in the treatment group than they are in the control group. However, although this reasoning applies well to clinicians and researchers, patients often lack the resources to base their decisions on such complete information. Rather, they must form their beliefs of effectiveness on the basis of a more limited comparison: how often symptoms were observed before the treatment started vs. how often they occur during the treatment, on the same patient (usually, themselves). Most causal learning experiments do not take into account this limitation, and instead provide participants with information about a series of different patients (Blanco et al., 2014; Matute et al., 2019). This is useful to investigate the formation of causal knowledge in general, but it is not realistic when applied to the case of patients' beliefs of effectiveness, as the procedure clearly departs from the actual experience of patients with their own treatments. In the current research, we propose a more natural setting to investigate the formation of beliefs of effectiveness, by presenting information of a single patient previous to, and during, a treatment (see a related approach in Blanco and Matute, 2020).

Previous experiments that used causal learning paradigms suggest that people can often be accurate in their judgments of causality (Shanks and Dickinson, 1987; Wasserman, 1990; Blanco et al., 2010), being generally sensitive to the actual contingency presented in the experiments. However, researchers have also reported systematic deviations, or biases. In particular, when the probability of the desired outcome is high, judgments tend to be higher even in null contingency conditions (Alloy and Abramson, 1979; Buehner et al., 2003; Blanco et al., 2014, 2020; Chow et al., 2019), contributing to what has been called a "causal illusion." This is a bias consisting of the belief in a causal link that is actually inexistent (Matute et al., 2015; Matute et al., 2019). The causal illusion bias share some features with other phenomena like the classical illusory correlation effect (Chapman and Chapman, 1967, 1969), and pseudocontingencies (Kutzner et al., 2011; Fiedler et al., 2009).[1] Despite their different explanations and

[1] In the typical illusory correlation paradigm, which is often framed in a social context, two groups of people (a minority group and a majority group) possess either of two traits (a common trait and an uncommon trait). Although the two

assumptions, all these phenomena coincide in the importance of event probabilities, such as the probability of the cause and the probability of the outcome, when judging causal relationships.

Thus, the causal illusion (as well as the other related biases) has been suggested to underlie many beliefs related to treatment effectiveness, and in particular those concerning pseudomedicines. These are treatments claiming to be effective, despite the lack of scientific evidence supporting levels of effectiveness higher than those of placebo (Lilienfeld et al., 2014; Macfarlane et al., 2020). The rationale is that, when diseases have a high chance of spontaneous remission, people systematically overestimate the effectiveness of treatments, even of those treatments that are completely unable to produce an effect. This could have serious consequences in real-life, as patients may grant undeserved trust and reliability to treatments that produce no actual benefit, thus losing the therapeutic opportunity (Freckelton, 2012).

By contrast, little research has paid attention to another possibility: that patients may also underestimate the effectiveness of actually valid treatments. As we will show, we have reasons to expect that causal learning can also produce this underestimation effect under some circumstances (see an example in Yarritu et al., 2015). For instance, by virtue of the biasing effect of the probability of the remissions that we described above, a treatment might appear as not effective when used on a disease with frequent symptomatic episodes, compared to a mild disease with less frequent symptoms.

Overview of the Experiments

In the current research, we use a causal learning procedure to experimentally study how people form beliefs of effectiveness for a fictitious treatment. Specifically, we present a medicine that is able to produce a moderate improvement in symptoms (i.e., a medicine with moderate contingency with symptom remission), and compare the perceived effectiveness in two situations: a disease with a high probability of symptomatic episodes, and a disease with a low probability of symptomatic episodes. Since the medicine equally works to reduce the frequency of episodes in both scenarios, one would expect similar ratings of effectiveness. However, the probability of the outcome (in this case, the observation of symptom remissions) could bias the judgments, producing the impression that the medicine is working better in the group in which symptoms had lower base-rates. In contrast with most previous studies on causal learning, we provide the information of the treatment effectiveness on a more natural fashion, which implies: (a) describing first how likely symptoms are before the treatment, and then how they respond to the introduction of the treatment, and (b) that the information given through a series of trials concerns only one patient, observed through time. This presentation format aims to mirror the chronology and generalization ability of the observations made by patients in real life.

ETHICS STATEMENT

The procedure was revised and approved by the Ethical Review Board of the University of Deusto. The participants were informed before the experiment that they could quit the study at any moment by closing the browser window. No personal information (i.e., name, IP address, e-mail) was collected. We did not use cookies or other software to covertly obtain information from the participants. All measures, groups and conditions are disclosed. Data, materials, and R scripts for the three experiments are publicly available at the Open Science Framework: https://osf.io/emzbj/.

EXPERIMENT 1

Experiment 1 uses a causal learning task to investigate the question of whether the effectiveness of a medicine can be underestimated if the disease has a high base-rate of symptomatic episodes. We expect that diseases that produce frequent observations of symptoms would create the impression that the treatment is not working as effectively as a treatment used for a disease with less frequent symptomatic episodes.

Method

Participants

We initially planned a sample of 100 participants, which would allow for the detection of effects of $d \geq 0.57$ in the difference between two groups at 80% power. However, data from one subject were not recorded due to technical errors. Thus, 99 Internet users (45 male, with age $M = 31.38$, $SD = 9.88$) participated anonymously through the Prolific Academic platform (Palan and Schitter, 2018), in exchange for money (0.80£ for about 10 min). The program randomly assigned 52 participants to the Infrequent group, and 47 to the Frequent group.

Procedure and Design

We adapted the standard trial-by-trial contingency learning task (Wasserman et al., 1990) that is extensively used to study human learning. The experiment was programmed in *JavaScript* to run online using a web browser. The instructions (available at the Open Science Framework, https://osf.io/emzbj/) asked participants to imagine that they were suffering from a fictitious disease called *Hamkaoman Syndrome*, which produces severe headaches. However, this symptom appears from time to time. Participants were told that the fictional drug *Batatrim* was a potential treatment for this disease if taken on a daily basis, but it may not work equally well for all people (i.e., "*Perhaps it works in your case, but we don't know until we try*"). The goal of the task was to use the information to find out whether Batatrim works to stop the headaches.

groups have identical trait distributions, it is often concluded that the majority group possesses the common trait to a greater extent than does the minority group (Hamilton and Gifford, 1976). Another paradigm proposed to understand biases in causal learning and illusory correlations is pseudocontingencies (Fiedler et al., 2009; Kutzner et al., 2011), in which people incorrectly use the marginal probabilities of events (e.g., the probability of the cause and the probability of the outcome) as a hint to infer the individual-level contingency, falling prey to an equivalent to the ecological fallacy. In practice, this means that scenarios in which the probability of the cause and the probability of the outcome are skewed in the same direction would produce stronger causal judgments.

Then, the training started by presenting a series of 40 records sequentially. Each record corresponded to one day, and displayed information about (a) whether the patient took Batatrim that day and, after a delay of 1 s, (b) whether the patient reported a headache (see **Figures 1A,B**). This information remained on the screen until the button "Next" was clicked, which proceeded to the next trial (after an inter-trial-interval of 500 ms).

The training comprised two consecutive phases. During Phase 1, as the instructions indicated, participants observed the records corresponding to the time before the treatment had started ("*In the first round of records, you will observe the diary entries corresponding to the time before you had any treatment, when you were just waiting for the doctor to give you Batatrim.*"). That is, Phase 1 contained 20 medicine-absent trials, in which either the patient reported a headache or not, and did not take any drug, therefore it conveyed the information to compute $P(O| \sim C)$. Then, in Phase 2, participants started observing the 20 records that corresponded to the time after the treatment had started ("*You have already learned about the symptoms produced by the Hamkaoman Syndrome when no treatment is given. Now, your pills have arrived, and you will start taking Batatrim on a daily basis.*"). This means that only medicine-present trials were shown

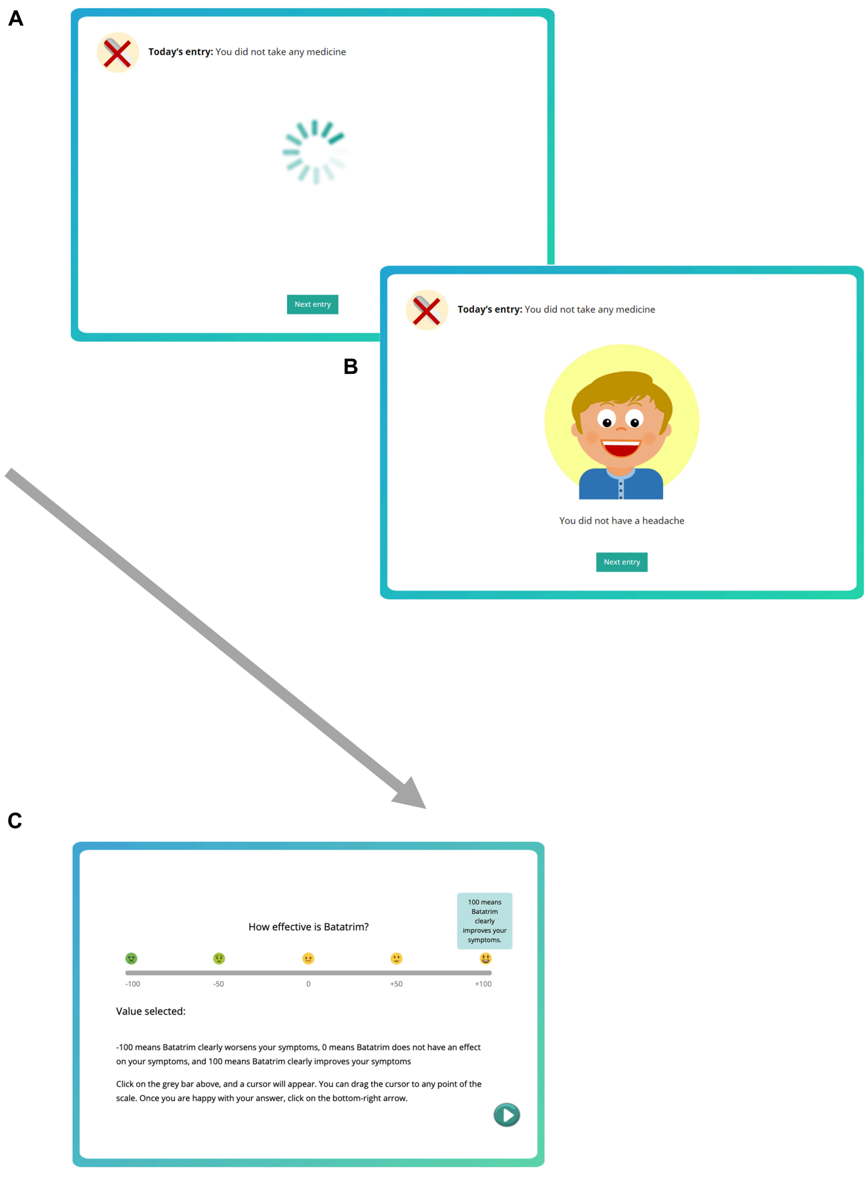

FIGURE 1 | Screenshots showing the contingency learning task. **(A)** At the beginning of the trial, the information about the medicine (top part of the screen) is shown for 1 s. **(B)** Then, the information about the presence or absence of the symptoms is shown in the center of the screen (in this example, the patient did not report symptoms). Pressing the "Next entry" button leads to next trial after a delay (ITI) of 500 ms in which the screen is cleared. **(C)** After the training session, we collect an effectiveness judgment on a –100 to +100 scale.

in Phase 2, which serves to compute P(O| C). The order of the trials within each phase (outcome-present or outcome-absent) was randomly determined for each participant.

Table 1 summarizes the experimental design. In the Frequent group, the symptoms were initially very frequent: 14/20 trials in Phase 1 (before treatment), and 8/20 in Phase 2 (during treatment). By contrast, in the Infrequent group, the symptoms were reported less often: 8/20 trials in Phase 1, and 2/20 in Phase 2. However, the objective contingency between treatment and symptom occurrence was the same in both groups. In the Frequent group, the contingency is computed as P(O| C) – P(O| ∼C) = 0.4–0.7 = −0.3; and in the Infrequent group it yields the same number, P(O| C) – P(O| ∼C) = 0.1–0.4 = −0.3. That is, according to the contingency rule for determining effectiveness (Δp), the two groups were depicting a medicine that was equally effective (a difference of 30% in the symptoms occurrence, in absolute terms), although they differed in the symptom base-rate.

After the sequence of 40 trials (20 in each phase), participants were asked several questions. First, we collected an effectiveness judgment (i.e., "*How effective is Batatrim?*"), which was our main dependent variable. The judgment was collected on a scale from −100 ("*Batatrim clearly worsens your symptoms*") to 0 ("*Batatrim does not have an effect on your symptoms*"), to +100 ("*Batatrim clearly improves your symptoms*"). To help interpret the response scale, we included five evenly separated small pictures of faces ranging from −100 (sick face) to +100 (happy face). When participants hovered the mouse pointer over these pictures, a small box appeared with a verbal label as shown in **Figure 1C**. No time constraints were imposed to answer these questions.

Second, we asked two conditional probability questions (in random order for each participant): P(O| C) judgment ("*Imagine a different person who suffers from the same syndrome. This person takes Batatrim on 100 consecutive days. Out of these 100 days in which the person takes Batatrim, on how many of them will the person report having headaches?*"), and P(O| ∼C) judgment ("*Imagine a different person who suffers from the same syndrome. This person does not take Batatrim on 100 consecutive days. Out of these 100 days in which the person does not take Batatrim, on how many of them will the person report having headaches?*"). These two pieces of information, combined, serve to compute the contingency between treatment and symptoms, and hence are necessary to correctly assess effectiveness. By examining these two questions, we will be able to detect whether participants correctly encode the two probabilities.

Finally, we requested a judgment about the tendency to opt for an alternative treatment different from Batatrim ("*If you had the chance, would you stick to your current treatment with Batatrim, or would you try a different treatment?*"). This was answered on a scale with five options ("*I'm sure I would stick to Batatrim*" / "*I would probably stick to Batatrim*" / "*I don't know*" / "*I would probably try a different treatment*" / "*I'm sure I would try a different treatment*"). We expected that participants who felt that the medicine was not working well would be more likely to stop taking it and try a different treatment.

Results and Discussion

The main results are those obtained from the effectiveness judgments, depicted in **Figure 2**. Although the medicine was identically effective in both groups according to the contingency information, the effectiveness judgments were significantly higher in the Infrequent group (which featured a lower symptom rate before the medicine was taken) than in the Frequent group, $t(97) = 4.96$, $p < 0.001$, $d = 0.998$. This suggests that those diseases that course with frequent symptomatic episodes will produce an underestimation of the actual effectiveness of the treatment relative to those with less frequent symptoms.

Next, we examine the judgments measuring the tendency to switch to alternative treatments, whose descriptive statistics appear in **Table 2**. The judgments could range between 1 ("*I'm sure I would stick to Batatrim*") and 5 ("*I'm sure I would try a different treatment*"). These judgments were significantly higher in the Frequent group than in the Infrequent group, $t(97) = 4.22$, $p < 0.001$, $d = 0.850$. That is, those participants who observed a disease with frequent symptomatic episodes were not only more likely to produce lower estimates for the effectiveness of the medicine, but they were additionally less willing to adhere to the treatment with Batatrim, despite the medicine being identically effective in the two groups.

Finally, we analyzed the conditional probability judgments to gain insight into how participants learned these two pieces of information, the probability of symptoms when the medicine was taken, P(O| C) and the probability of symptoms when no medicine was taken, P(O| ∼C). These judgments are depicted in **Figure 3**. We conducted a mixed 2 (Group) × 2 (Probability), revealing a main effect of Group, $F(1,97) = 117.0$, $p < 0.001$, $\eta_p^2 = 0.55$. Overall, probability judgments were greater in the Frequent group than in the Infrequent group, which is consistent with the actual symptom probabilities in each group. We also found a main effect of Probability, $F(1,97) = 327.91$, $p < 0.001$, $\eta_p^2 = 0.77$, which just reflects the fact that the symptoms reduced their frequency from Phase 1 to Phase 2 (i.e., the medicine was effective). Importantly, there was no interaction, $F < 1$. To better interpret these results (and those of subsequent experiments, with additional groups), we computed a "perceived contingency score" by subtracting the two conditional probability judgments

TABLE 1 | Design of Experiment 1.

Group	Phase 1	Phase 2	P(O| ∼C)	P(O| C)	Contingency (Δp)
Frequent	Symptoms reported: 14/20 trials	Symptoms reported: 8/20 trials	0.70	0.40	−0.30
Infrequent	Symptoms reported: 8/20 trials	Symptoms reported: 2/20 trials	0.40	0.10	−0.30

The two groups differ in the base-rate with which the symptoms appeared. The medicine was equally effective in both groups according to the contingency rule Δp, because the difference in the symptom probability before and after treatment was the same in both groups, in absolute terms.

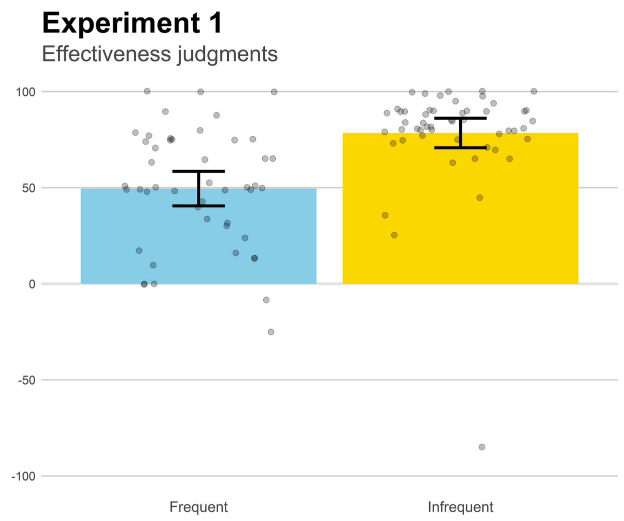

FIGURE 2 | Mean effectiveness judgments in Experiment 1. Higher positive values indicate a strong belief that the medicine works to reduce the symptoms. Jittered data points are superimposed to the plot (to avoid overplotting, the placement of data points of a given condition along the *x*-axes is random). Error bars depict 95% confidence intervals for the mean.

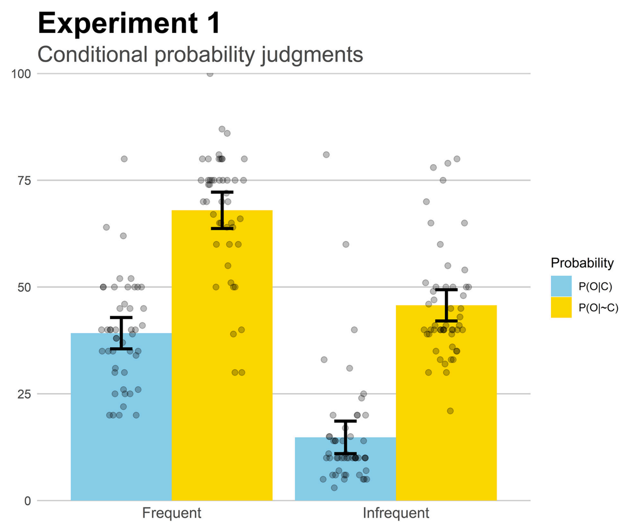

FIGURE 3 | Mean conditional probability judgments in Experiment 1. Jittered data points are superimposed to the plot (to avoid overplotting, the placement of data points of a given condition along the *x*-axes is random). Error bars depict 95% confidence intervals for the mean.

TABLE 2 | Descriptive statistics for the alternative treatment judgments in the three experiments.

Experiment	Group	Mean	SD
Experiment 1	Frequent	2.85	1.23
	Infrequent	1.90	1.00
Experiment 2	Frequent-Experimental	2.66	1.18
	Infrequent-Experimental	1.73	0.83
	Frequent-Control	3.96	0.99
	Infrequent-Control	3.87	0.95
Experiment 3	High Continency-Large Change	1.98	0.91
	Low Continency-Large Change	1.91	0.85
	Low Continency-Small Change	3.03	1.07

The judgment was collected on a scale from 1 to 5 (1: "I'm sure I would stick to Batatrim"; 2: "I would probably stick to Batatrim"; 3: "I don't know"; 4: "I would probably try a different treatment"; 5: "I'm sure I would try a different treatment").

following the Δp rule, i.e., P(O| C)-P(O| ∼C). These scores can then be interpreted as the amount of contingency that a participant perceived, based on the conditional probability ratings. The resulting values showed no differences between groups, $t(97) = 0.65$, $p = 0.51$, $d = 0.13$, indicating that the perceived contingency was the same in both base-rate groups, as the conditional probability estimations only differed between groups in their absolute values. Taken together, the results suggest that participants were able to capture accurately the probabilities involved in the computation of contingency, as the mean estimations were close to the actual values presented in the task. Therefore, the underestimation of effectiveness that we reported above cannot be explained as a failure to learn the conditional probabilities.

EXPERIMENT 2

Experiment 1 successfully showed that the base-rate of the symptomatic episodes can bias the judgments of treatment effectiveness: diseases with a higher probability of symptoms produced lower perceived effectiveness, even if the actual contingency was identical. This aligns with the evidence obtained in different situations (e.g., null contingencies), and also with results from experiments conducted in related paradigms (e.g., pseudocontingencies, Kutzner et al., 2011).

Still, our results could be interpreted as if our participants were simply ignoring the contingency information, guiding their judgments by the probability of symptoms only. That is, it could be possible that if a medicine drives the probability of symptoms close to zero, it would be judged as effective even if the initial base-rate without treatment was also small, as people could just ignore the initial base-rate. In fact, as we mentioned above, there is ample empirical evidence indicating that judgments of causality can be strongly biased by the probability of the outcome, at least in null contingency situations (Alloy and Abramson, 1979; Buehner et al., 2003; Blanco et al., 2014; Chow et al., 2019; Blanco and Matute, 2020).

Experiment 2 aims to replicate the findings of Experiment 1, while introducing two control groups in which the actual contingency between the treatment and symptom remissions is zero: In these two control groups, the probability of the symptoms is the same before and after the treatment (i.e., the medicine does not work at all). These two probabilities match those of the two experimental groups when taking the medicine, P(O| C), which are identical to those used in Experiment 1. That is, for half of the participants, symptoms will be frequent, and for the other half they will be infrequent. Orthogonally, for half of the participants, the medicine will work (by reducing the symptom probability in

30%, in absolute terms), whereas for the other half it will not work at all. Thus, if participants judge the effectiveness of the treatment only by attending to the frequency of the symptoms and ignoring the contingency, then the control groups would not differ from the experimental groups, revealing that participants are only biased by the base-rate of the effect. Conversely, if participants do take into account contingency, they should note that control medicines are not effective.

Method

Participants

The planned sample size was $N = 200$, which allows detecting effects of $d \geq 0.57$ at 80% power. Data from three participants were not recorded due to technical errors. The final sample consisted of 197 anonymous Internet users (105 male, 91 female, 1 non-binary, with age $M = 30.8$, SD = 11.3), who participated through Prolific Academic (Palan and Schitter, 2018) in exchange for money (0.80*textsterling* for about 10 min). The program randomly assigned 52 to the Frequent-Control group, 47 to the Frequent-Experimental group, 47 to the Infrequent-Control group, and 51 to the Infrequent-Experimental group.

Procedure and Design

The procedure was identical to that in Experiment 1. The only change was the inclusion of two new groups that work as control conditions (see the design in **Table 3**). In these groups, the actual contingency between medicine and recovery from the symptoms was null, which means that the medicine was completely ineffective. That is, in addition to the two groups already present in Experiment 1, we had the Infrequent-Control group, which showed a base-rate of symptomatic episodes of 0.10 (i.e., 2/20 trials), both in Phase 1 and in Phase 2; and the Frequent-Control group, which showed a base-rate of symptomatic episodes of 0.40 (i.e., 8/20 trials), both in Phase 1 and in Phase 2. In sum, now we have included null-contingency controls for the two base-rate conditions that were previously tested. This will allow us to compare the two factors: will judgments depend on the symptoms base-rate, or on contingency (or both)?

Results and Discussion

The mean effectiveness judgments are displayed in **Figure 4**. They were submitted to a 2 (Base-rate) × 2 (Contingency) factorial ANOVA. The main effect of Contingency was significant, $F(1,193) = 392.4$, $p < 0.001$, $\eta_p^2 = 0.67$, indicating that participants were sensitive to contingency, producing higher judgments when the medicine was effective (Experimental groups) than when it was not effective (Control groups). The main effect of base-rate was also significant, $F(1,193) = 12.3$, $p < 0.001$, $\eta_p^2 = 0.06$, meaning that the infrequent groups produced stronger beliefs of effectiveness. Finally, the interaction, $F(1,193) = 10.0$, $p = 0.002$, $\eta_p^2 = 0.05$, indicated that, while the two experimental groups were sensitive to base-rate, meaning that we successfully replicated the effect reported in Experiment 1, $t(96) = 5.67$, $p < 0.001$, $d = 1.15$, the two control groups did not differ from each other, $p = 0.827$. That is, base-rate information only affected the effectiveness judgments in the two contingent groups.

The judgments about the likelihood to switch to an alternative treatment (**Table 2**) aligned with the previous conclusions. They showed, again, the main effect of Contingency, $F(1,193) = 148.55$, $p < 0.001$, $\eta_p^2 = 0.43$, the main effect of base-rate, $F(1,193) = 13.08$, $p < 0.001$, $\eta_p^2 = 0.06$, and the interaction, $F(1,193) = 8.92$, $p = 0.003$, $\eta_p^2 = 0.04$. The two experimental groups differed from each other as in Experiment 1, $t(96) = 4.56$, $p < 0.001$, $d = 0.92$, thus replicating the previous result, while the two controls did not differ, $p = 0.648$. In sum, the results concerning the alternative treatment judgments were consistent with those of the effectiveness judgments: participants in the control groups were more likely to try a different therapeutic option, while in the experimental group the symptom base-rate mattered, so that the higher the symptom base-rate, the more unlikely they were to adhere to the treatment.

Finally, we analyzed the conditional probability judgments (**Figure 5**). The two Experimental groups replicated the results from Experiment 1: P(O| C) was estimated higher than P(O| ∼C) in both base-rate levels, $t(46) = 11.0$, $p < 0.001$, $d = 1.60$ (Frequent), and $t(50) = 10.6$, $p < 0.001$, $d = 1.49$ (Infrequent), while overall both probabilities were close to the actual values. In the control groups, there were no differences between the two conditional probabilities, which is consistent with the low effectiveness judgments, $p = 0.41$ (Frequent), and $p = 0.29$ (Infrequent). Like in Experiment 1, to make the interpretation of these results easier, we decided to compute a "perceived contingency" score by subtracting the judgments to the P(O| C) and to the P(O| ∼C) questions, thus following the contingency equation Δp. A 2 (Base-rate) × 2 (Contingency) ANOVA on these perceived contingency values revealed a main effect of Contingency, $F(1,193) = 156.89$, $p < 0.001$, $\eta_p^2 = 0.45$, with no other significant effects or interaction (both Fs < 0.2). The effect of contingency means that the two experimental groups (who were exposed to a positive contingency) perceived higher

TABLE 3 | Design of Experiment 2.

Group	Phase 1	Phase 2	P(O| ∼C)	P(O| C)	Contingency (Δp)
Frequent-Experimental	Symptoms reported: 14/20 trials	Symptoms reported: 8/20 trials	0.70	0.40	−0.30
Infrequent-Experimental	Symptoms reported: 8/20 trials	Symptoms reported: 2/20 trials	0.40	0.10	−0.30
Frequent-Control	Symptoms reported: 8/20 trials	Symptoms reported: 8/20 trials	0.40	0.40	0.00
Infrequent-Control	Symptoms reported: 2/20 trials	Symptoms reported: 2/20 trials	0.10	0.10	0.00

In addition to the two experimental groups, identical to those in Experiment 1, this experiment included two control groups in which the probability of the symptoms during the treatment were the same as in the experimental groups, but the contingency was null (the medicine did not work at all).

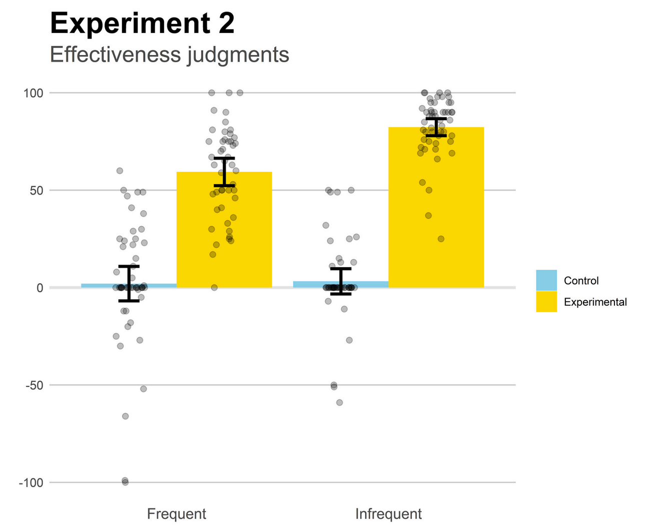

FIGURE 4 | Mean effectiveness judgments in Experiment 2. Higher positive values indicate a strong belief that the medicine works to reduce the symptoms. Jittered data points are superimposed to the plot (to avoid overplotting, the placement of data points of a given condition along the *x*-axes is random). Error bars depict 95% confidence intervals for the mean.

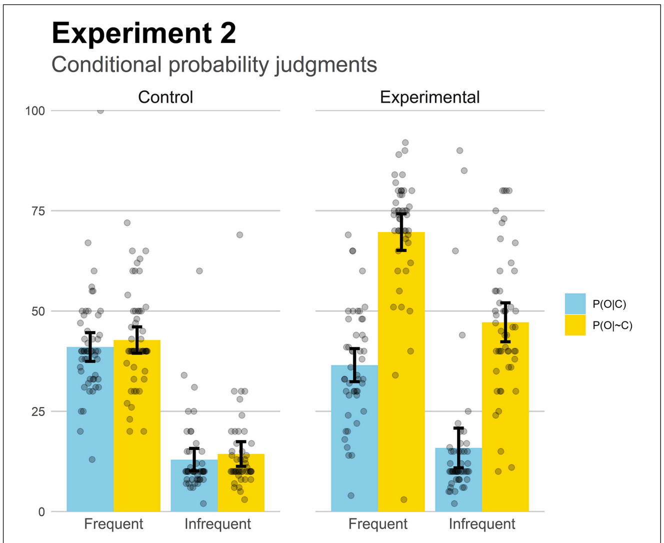

FIGURE 5 | Mean conditional probability judgments in Experiment 2. Jittered data points are superimposed to the plot (to avoid overplotting, the placement of data points of a given condition along the *x*-axes is random). Error bars depict 95% confidence intervals for the mean.

contingency levels than did the two control groups (who were exposed to a null contingency), irrespective of the differences in base-rate. Thus, the experimental groups replicated the results of Experiment 1, by not finding an effect of base-rate on the perceived contingency: it seems that the perceived contingency was the same regardless of the frequency of presentation of the symptoms.

EXPERIMENT 3

The results of Experiment 2 suggested that the effectiveness judgments produced by participants were affected by the symptom base-rate. However, participants were not completely ignoring the contingency information, as they, at least, were able to discriminate between a low/moderate contingency level (0.30) and a null contingency (0). The question is: how do participants use base-rate information to form their judgment?

Contingency, as described in section "Introduction," is an objective rule used to assess treatment effectiveness, which in principle allows the comparison of treatments for different cases, with different levels of symptom frequency. The two previous experiments suggested that participants, however, produce effectiveness judgments that are not only determined by contingency, but also biased by the frequency of the symptoms. It is possible to further investigate the way in which people use symptom base-rates when judging effectiveness. In fact, in our previous experiments, we fixed the contingency level to a given value of 0.30 (or zero in the control groups in Experiment 2), which means that the treatment always produced the same amount of change in the symptom probability *in absolute terms*. However, the groups differed in the amount of change in the symptom probability *relative to the base-rate level*. That is, when the treatment reduces the symptom occurrence from 0.70 to 0.40 (i.e., group Frequent), the absolute difference, or contingency, is 0.30, but the amount of reduction relative to the base-rate is 43%, i.e., (0.40–0.70)/0.70 = 0.43. By contrast, when the treatment reduces the symptom occurrence from 0.40 to 0.10 (i.e., group Infrequent), the absolute change remains 0.30 but the relative change is larger, 75%, i.e., (0.10–0.40)/0.40 = 0.75. Thus, it is possible that participants in our previous experiments were judging effectiveness by using the change in the symptoms proportional to the base-rate, rather than by using the absolute difference (contingency). This would be a different strategy to deal with effectiveness information that takes into account base-rates, and that could explain our results so far (note that using this strategy can also explain the results from the two groups with a null contingency).

Therefore, we designed Experiment 3 to test for this possibility. In Experiment 3, the groups differed either in the contingency level (high vs. low) or in the amount of change proportional to the base-rate (small vs. large). The parameter constellations were chosen such that the two possible drivers for participants' judgments (absolute differences vs. relative differences) could be pit against each other.

Method

Participants

We planned a sample size of $N = 150$ for this design of three groups (50 participants per group), which allows detecting effects for the difference between pairs of groups of $d \geq 0.57$ at 80% power. Data from one participant were lost due to technical errors/connection issues. The final sample consisted of 149 participants (70 women, 79 men, with age $M = 27.3$, SD = 8.66), recruited in the same way as in the previous experiments. The program randomly assigned 55 to the High Contingency-Large Change group, 57 to the Low Contingency-Large Change group, and 37 to the Low Contingency-Small Change group.

Procedure and Design

The procedure was identical to the previously reported experiments, except for the probability of observing symptoms during the training, which was manipulated across the three groups to obtain two different levels of contingency and two different levels of the change proportional to the base-rate (**Table 4**). That is, in the High Contingency-Large Change group, the contingency between the treatment and the symptom occurrence was high (−0.60) in absolute terms, and the change proportional to the symptom base-rate was large (a reduction of 75% from the initial symptom base-rate); in the Low Contingency-Large Change group, the contingency was low (−0.30), but when considered as a proportion of the initial symptom base-rate, the change was still large (a reduction of 75% of the initial symptoms); finally, in the Low Contingency-Small Change group, the contingency was low (−0.30), and the change proportional to the base-rate was small (a reduction of 37.5% of the initial symptoms). By comparing these groups pairwise, as they share one of the parameters (either contingency or proportional change) but not the other, we can eventually find out which of the two parameters more clearly affects judgments of effectiveness.

Results and Discussion

Figure 6 contains the mean effectiveness judgments in the three groups of Experiment 3. We were only interested in the comparisons between the groups that shared one parameter value (either contingency or proportional change) and differed on the other. The Low Contingency-Large Change and the Low Contingency-Small Change groups, despite having identical contingency, differed significantly, $t(92) = 5.87$, $p < 0.001$, $d = 1.24$, suggesting that contingency was not a key aspect for effectiveness judgments, and rendering plausible that the proportional change played a role in this effectiveness assessment. This possibility was further reinforced by the finding that the Low Contingency-Large Change and High Contingency-Large Change groups, which shared the same proportional change but show different contingency, did not significantly differ from each other, $p = 0.81$.

The judgments about the likelihood to switch to an alternative treatment (**Table 2**) showed the same pattern as the effectiveness judgments: In the Low Contingency-Large Change group, participants were significantly more likely to stick to the treatment than they were in the Low Contingency-Small Change group, $t(92) = 5.61$, $p > 0.001$, $d = 1.18$. As it happened with effectiveness judgments, no differences were found in the likelihood to adhere to the actual treatment when comparing groups with similar proportional change, i.e., Low Contingency-Small Change vs. High-Small, $p = 0.92$.

Finally, **Figure 7** depicts the conditional probability judgments for Experiment 3. Once again, the judgments were close to the actual values presented in the training. In all three groups, the difference between P(O| C) and P(O| ∼C) was significant (all ps < 0.001), consistent with the perception of at least some degree of effectiveness. Additionally, we used these conditional probability judgments to reconstruct the perceived contingency (by subtracting the two conditional probabilities) and the perceived proportional change between phases (by computing the contingency and dividing it by the symptom base-rate before the treatment). We found that groups with different contingency levels showed different perceived contingency scores: the High Contingency-Large Change group produced a larger difference between the conditional probabilities than did the other two groups, both ps < 0.007. On the other hand, groups with an identical contingency level did not differ in this measure: Low Contingency-Large Change vs. Low Contingency-Small Change, $p = 0.95$. Concerning the perceived proportional change, this score was higher for the groups with larger changes, even if they implied the same contingency: Low Contingency-Small Change differed both from High Contingency-Large Change and from Low Contingency-Large Change (both ps < 0.030). By contrast, groups with similar proportional change did not differ in this measure: Low Contingency-Large Change vs. High Contingency-Large Change, $p = 0.998$.

In a nutshell, it seems that effectiveness judgments were sensitive to proportional change in the conditional probabilities, but not to their absolute differences. This effect was also found in the desire to replace the treatment by an alternative. However, conditional probabilities seemed to be accurately captured.

TABLE 4 | Design of Experiment 3.

Group	Phase 1	Phase 2	P(O| ∼C)	P(O| C)	Contingency (Δp)	Change (%)
High Contingency-Large Change	Symptoms reported: 16/20 trials	Symptoms reported: 4/20 trials	0.80	0.20	−0.60	75
Low Contingency-Large Change	Symptoms reported: 8/20 trials	Symptoms reported: 2/20 trials	0.40	0.10	−0.30	75
Low Contingency-Small Change	Symptoms reported: 16/20 trials	Symptoms reported: 10/20 trials	0.80	0.50	−0.30	37.5

In this experiment, the probability of observing a symptom was manipulated between groups so that two of them showed the same low level of contingency (groups Low Contingency-Large Change and Low Contingency-Small Change), contrasting with a high contingency group (High Contingency-Large Change). Additionally, the amount of change between phases proportional to the symptom base-rate was identical (large) in two groups (High Contingency-Large Change and Low Contingency-Large Change), despite they diverged in their contingency level, and different from the Low Contingency-Small Change group.

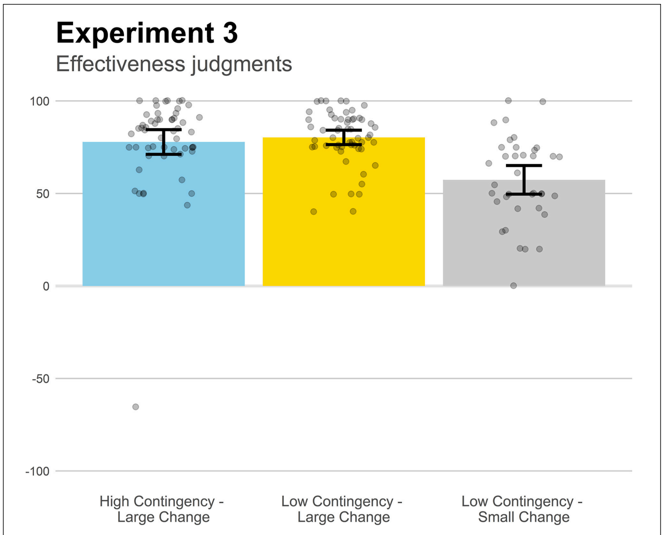

FIGURE 6 | Mean effectiveness judgments in Experiment 3. Higher positive values indicate a strong belief that the medicine works to reduce the symptoms. Jittered data points are superimposed to the plot (to avoid overplotting, the placement of data points of a given condition along the *x*-axes is random). Error bars depict 95% confidence intervals for the mean.

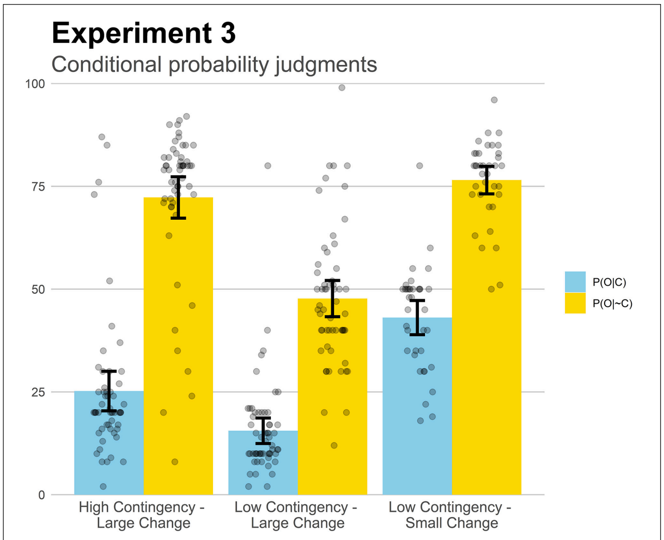

FIGURE 7 | Mean conditional probability judgments in Experiment 3. Jittered data points are superimposed to the plot (to avoid overplotting, the placement of data points of a given condition along the *x*-axes is random). Error bars depict 95% confidence intervals for the mean.

GENERAL DISCUSSION

Which Is the Rule for Estimating Effectiveness?

Beliefs of treatment effectiveness can be understood as the result of causal learning (Rottman et al., 2017), under the assumption that an effective treatment produces a change in the likelihood of symptom improvement compared to a control condition (e.g., taking no treatment). This allows us to investigate effectiveness beliefs by means of causal learning experiments, and to advance predictions based on the results described in this literature. Previous studies have focused on how completely ineffective medicines (e.g., pseudomedicines) can appear to be effective under some circumstances (Blanco et al., 2014; Matute et al., 2019). However, fewer experiments have been conducted to explore the possibility that actually effective treatments are seen as less effective due to the biases described in the causal learning literature.

Here, we have reported how beliefs of effectiveness are sensitive to the base-rate of the symptomatic episodes in a way that does not conform to the rule for computing contingency, Δp. That is, in Experiments 1 and 2, a fictitious medicine with a low/moderated contingency with health improvement (reduction of 0.30 in the probability of symptoms, in absolute terms) was tested in two different scenarios: a disease with high base-rate of symptoms and a disease with low base-rate of symptoms. Our results indicated that base-rates affected the judgments of effectiveness, so that a valid medicine was judged as less effective when the symptoms were very frequent before the treatment. This would modulate the perceived effectiveness of a treatment as a function of the symptom frequency, which could lead to mistaken conclusions when patients examine their treatments' effectiveness, or when they compare between diseases or patients with diverging symptom base-rates. In fact, according to our results, it is those patients who show symptoms with greater probability who will be more likely to underestimate the effectiveness of a moderately valid treatment. The implication of this is that these patients who suffer from frequent symptomatic episodes should be carefully supervised, as we know that treatment effectiveness beliefs are core to treatment adherence (Leventhal et al., 1992; Dilla et al., 2009). Additionally, those patients who underestimate the effectiveness of their treatment will be probably at risk of replacing their scientifically valid treatment by a different, probably less effective one, or even by a pseudomedicine, as our experiments also reveal through the alternative treatment question. Not surprisingly, lack of trust in scientific medicine is one of the predictors of pseudomedicine usage (Macfarlane et al., 2020).

The underestimation of the effectiveness when the symptom base-rate is high (Experiment 1) could be due to participants judging effectiveness on the basis of how infrequent the symptoms are when the treatment is taken. That is, any medicine that drives the probability of symptoms close to zero (i.e., complete healing) would be judged as effective, while the initial base-rate without treatment could be ignored. This possibility was examined in Experiment 2, which included control groups with null contingency: that is, the symptom-base-rate was kept identical before and during the treatment. Since participants in Experiment 2 were able to discriminate between the two contingency levels while still replicating the bias reported in Experiment 1, it seems that people's judgments are not entirely driven by the symptom level obtained at the end of the treatment.

Finally, Experiment 3 tested a potential way in which people could be using the symptom base-rate information when making their judgment, which is different from contingency. As we have described it, contingency is simply the difference between the symptom probability before and during the treatment, in absolute terms. Thus, it is an objective measure that is independent of the initial base-rate level. That is, a reduction of symptom probability from 0.70 to 0.40 is the same as one from 0.40 to 0.10. In this type of scenario, a contingency index, Δp (Allan, 1980), has been used as the traditional benchmark to assess causality and, hence, treatment effectiveness. However, people could be focusing on the reduction in symptom probability relative to the initial base-rate value, instead of in absolute terms. That is, when symptoms decrease from 0.40 to 0.10, they are reducing in 75% of the initial value. Experiment 3 presented three groups varying in either their contingency or their change proportional to the base-rate. Judgments were systematically guided by the change proportional to base-rate, rather than by contingency, suggesting that this is the way people use base-rate information to estimate effectiveness in this type of experiments. The explanation is compatible with all the results that we report in this article.

Is it reasonable to use proportional change, rather than absolute change (contingency) when assessing treatment effectiveness? In fact, researchers commonly use proportional change as a success index when testing the effectiveness of an intervention (especially in repeated-measures designs). For example, a treatment for depression could be regarded as useful if it reduces depressive symptoms by 10% from the baseline (see an example of the use of percent change from baseline, Lin et al., 2013). This is the logic underlying likelihood ratios (e.g., probability of the outcome given the treatment, relative to a control condition with no or other treatment) and odd ratios, which are common to estimate treatment effectiveness, test sensitivity, and risk in scientific studies (the same rationale is also present in the widely used Bayes Factors, Kass and Raftery (1995), which represent the support for one hypothesis relative to the null by means of an all-purpose likelihood ratio, although their computation is completely different). However, when used directly to assess the effectiveness of a treatment from the observation of the conditional probabilities, this approach can be problematic, and methodologists recommend to avoid it in most cases (Vickers, 2001; Tu, 2016). First, proportional change makes sense only with variables measured in ratio scales, in which zero is a meaningful value (fortunately, this condition holds in our case, as we are comparing probabilities). Additionally, note that, while contingency is an effectiveness measure that is insensitive to the symptom base-rate, the proportional change will strongly depend on this piece of information, so that those patients or conditions in which symptoms appear very often (i.e., high base-rate) will produce systematically smaller proportional changes than those in which symptoms are less frequent. Indeed, research works using this proportional change as outcome variable usually report strong correlations between the effectiveness of the manipulation and the baseline level (Tu and Gilthorpe, 2007; Tu, 2016), so that higher baseline levels apparently "reduce" the effectiveness. Moreover, despite it appearing to be an intuitive concept, presenting the information as proportional change can be confusing for patients. For example, when laypeople are presented with the results of a study on risk factors in terms of proportional change from baseline, they tend to erroneously interpret it as change in absolute terms (e.g., a reduction of 10% is interpreted as if a baseline score of 50 were reduced to 40, rather than 45) (Bodemer et al., 2014). Admittedly, there are situations in which proportional change could be a more useful measure of effectiveness than is direct difference (e.g., causes that produce a multiplicative effect), but most of times changes expressed as proportions are hard to generalize, as they depend on baseline levels that can vary between conditions or individuals (e.g., a change of 0.3 points in absolute terms can be small when the baseline is 0.9, but large when the baseline is 0.35). Thus, a direct difference measure such as the Δp index could be more versatile than likelihood ratios and related measures based on proportional change. In sum, proportional change from baseline is neither an accurate index for assessing treatment effectiveness, nor a good way to communicate it, at least in most situations. Hence, using proportional change could be considered a strategy that measures effectiveness, but in a suboptimal way that could lead to erroneous conclusions in some circumstances.

However, the finding that people spontaneously tend to use proportional change as an effectiveness index (as the results of Experiment 3 indicate) is interesting for theoretical reasons. Research on human causal and contingency learning has traditionally focused on objective measures such as Δp or similar rules (Perales and Shanks, 2007), not considering the possibility that participants use proportional change as a direct cue to causality assessment. Nonetheless, certain Bayesian theories of causal induction such as Causal Support (Griffiths and Tenenbaum, 2005) formalize causal inference in a way that involves likelihood ratios, that is, the probability of observing the data given one hypothesis (and model) relative to the probability of observing the data given an alternative one, which is structurally similar to a Bayes Factor (Kass and Raftery, 1995). For example, Causal Support computes a ratio of the likelihood of observing the current data under the model that assumes a causal link between cause and outcome, relative to the model that assumes no causal link, $P(\text{data}|\text{ hypothesis, model}_1)/P(\text{data}|\text{ hypothesis, model}_0)$. The computation of Causal Support is more complex than merely comparing the two conditional probabilities, and it involves additional assumptions about causality. However, we mention it here because there could be some structural resemblance between the way the model computes causal strength (and the way Bayes Factors express support for a hypothesis) and the strategy apparently exhibited by our participants. Our experiments were not designed to investigate these questions, but the findings of Experiment 3 could inspire further studies to better understand how people incorporate base-rates to assess effectiveness and causality.

In fact, there is evidence that people use proportional change as a cue in completely different paradigms. For example, when they compare two numbers, people's responses are affected by the ratio between the two quantities (i.e., "numerical size effect"; Moyer and Landauer, 1967). Additionally, studies on Bayesian reasoning also show that participants can use the information expressed as likelihood ratios to elaborate their judgments [although these judgments are often incorrect, especially when the information is given in terms of probabilities rather than natural frequencies (Gigerenzer and Hoffrage, 1995;

Hoffrage et al., 2015)]. Nevertheless, this paradigm is quite different from ours: Bayesian reasoning tasks first provide the conditional probabilities and base-rates, and then ask about the probability that an individual observation corresponds to a given category (which requires using the base-rate information), whereas our contingency learning task provides a sample of observations already classified, and then requests a generalized rule (i.e., whether there is a causal link or not) that in principle should hold regardless of the particular base-rate observed. Further studies should examine the potential similarities and connections between these numerical cognition effects and contingency learning phenomena.

It is also worth discussing the results concerning the conditional probability judgments. Across the three experiments, we found that the departure from contingency was detected in effectiveness judgments, formulated as a causal question, but not in the conditional probability judgments. This is in line with recent studies on the causal illusion (Chow et al., 2019) and coincides with previous claims that, generally, causal estimations are more prone to bias than are other types of judgments, such as predictions (Vadillo et al., 2005). This also has theoretical implications: some authors have proposed that biases in causal learning are the result of processes that appear in the moment of emitting the judgment, rather than in the encoding phase (Allan et al., 2008). Indeed, in our experiments, the basic pieces of information needed to compute the contingency index Δp, $P(O| C)$ and $P(O| \sim C)$, seem to have been correctly acquired. Therefore, the effects we have described in this article might be explained by the strategies or rules that people use to combine the information and form their judgment (e.g., using proportional change instead of contingency), rather than by learning or encoding phenomena. However, we must remain cautious when interpreting the conditional probability judgments, as they were always requested after the effectiveness judgment, and therefore they could be contaminated.

Methodological Aspects

Additionally, these experiments included several procedural and methodological innovations that depart from most previous literature, and that deserve discussion. First, most experiments using causal learning tasks in medical scenarios present the four types of trial (i.e., medicine-healing, medicine-no healing, no medicine-healing, and no medicine-no healing) in intermixed, often random, orders. Additionally, the information given on each trial concerns usually a different patient. Thus, the traditional task resembles a clinical study in which a sample of patients is examined, in no particular order. This causal learning task has advantages. For example, it prevents participants from assuming that trials are autocorrelated (i.e., that there is dependency between trials, so that the outcome of one trial can be affected by previous trials) and avoids order effects by randomizing the trial order. However, this procedure does not capture well the experience of patients who judge their own treatments, which is a highly common situation in real life. Patients cannot normally access a sample of participants to test the treatment. Rather, they can only test the effectiveness on themselves, and the information is, most of the time, examined in a particular order: first, they know how often the symptoms appear before the treatment, i.e., they observe $P(O| \sim C)$. Then, they start the treatment and may check if this base-rate is affected, i.e., they observe $P(O| C)$. In our two experiments, we tried to present a situation that mirrors this natural setting, by observing instances of symptom occurrences on a single individual (additionally, the task was described in second person, to help the participants imagine that they were the patients), and by arranging the information in two phases, one before and one during the treatment.

This choice to split the training session into two phases, $P(O| \sim C)$ and $P(O| C)$, seems to have yielded interesting results. In most similar studies with the traditional task (with the trials arranged in random order), a common finding is that null contingencies are overestimated when the probability of the outcome is high (see reviews in Matute et al., 2015, 2019). Here, Experiment 2 presented a null contingency condition with high chances of remission: in fact, the training in the Infrequent-Control group in which the symptoms were absent in 90% of the trials is almost identical to previous studies that showed strong overestimations of effectiveness, or causal illusions (Blanco et al., 2014; Blanco and Matute, 2019), except for the fact that the trials were separated into two phases, one for $P(O| C)$, and one for $P(O| \sim C)$. This difference seems to have abolished the causal illusion, as Experiment 2 shows clearly that most participants correctly identified the null contingency. We can only speculate as to why this procedural change makes such a big effect on judgments. One possibility is that, by arranging the trials in separate phases, the working memory demands are lower than in the usual experiment, thus making the task easier to solve. A previous study by Willett (2017) tested a related argument. In her experiment, the contingency information was presented in a summarized, pictorial format (depicting faces that represent the cases), rather than trial by trial. The design featured two levels of $P(O)$, high and low, in a null contingency situation. Critically, the pictorial information could be presented in either an "organized" way (which groups together the pictures of faces corresponding to the outcome, on the one hand, and the pictures that represent the no-outcome, on the other hand), or in a "scrambled" way (which intermixed the pictures in a random fashion). We can see a similarity between the scrambled condition and the usual contingency training with intermixed trials, and between the organized condition and our two-phases procedure. This experiment showed that the overestimation of contingency was stronger in the scrambled condition than it was in the organized condition, although the results were only marginally significant. However, one must be cautious when interpreting this evidence, as the information was presented in table format in Willett's experiment, whereas our experiments used the trial-by-trial format. Future experiments should further investigate the potential sensitivity of causal illusions to cognitive demands on standard trial-by-trial procedures. A second option to interpret the reduced illusion that we found in our Experiment 2 is that, by separating the phases, we are highlighting that outcomes can occur in two different contexts (i.e., in the presence and in the absence of the treatment), hence, implicitly inviting participants to compare them, as in the Δp rule (Allan, 1980). This possibility could be explored in future studies.

The second methodological change from most previously published experiments is the use of a bidirectional scale. As the association between two variables can be either positive or negative, contingency (usually assessed with the Δp index) can take on either positive or negative values, which translates to causally generative scenarios and causally preventive scenarios (Perales et al., 2016). Consequently, the response scale in our experiments was bidirectional, from −100 (the medicine worsens the symptoms) to +100 (the medicine improves the symptoms). Note that most research carried out on contingency learning biases have used the unidirectional scale, from 0 (no effect) to +100 (perfect effectiveness), see, e.g., Matute et al. (2019). The bidirectional scale that we used here has the advantage of correctly capturing the potential range of the contingency and causality values. However, it is also more difficult to understand for some participants. Previous research has suggested that, in general, both types of scale are valid to capture common contingency learning phenomena (see, e.g., Blanco and Matute, 2020, who report the same effects with unidirectional and bidirectional scales).

Finally, in addition to effectiveness judgments and conditional probability estimations, we also collected judgments about the likelihood of using an alternative treatment, aimed at measuring the desire to quit the treatment and look for alternatives. Since the results were the same as those found in the effectiveness ratings, we could conclude that beliefs of effectiveness generalized to this question: participants who saw the disease with high symptom base-rate underestimated the effectiveness of the medicine, and were less willing to adhere to it. Our alternative treatment question contributes, thus, to fill the gap between causal estimations that are typically collected in contingency learning experiments and actual decisions made by patients when dealing with real diseases. The practical implication of our finding is that those patients who underestimate their treatment's effectiveness are less satisfied, and perhaps are more vulnerable to the offer of alternative options such as pseudomedicines and fraudulent health products (Macfarlane et al., 2020).

Practical Implications

More generally, we can outline a few implications of our research to clinical practice, although they involve some degree of speculation. Since our procedure is more ecological than the traditional causal learning experiment in certain aspects (order of the information that is presented, observation of only one patient instead of samples…), these experiments are well endowed to inform decisions and insights for real patients using real medicines. The first one is that people use, at best, inefficient methods for assessing effectiveness. Either they are biased by the symptom base-rate directly (as Experiments 1 and 2 initially suggested), or they use proportional change from the symptom base-rate (as Experiment 3 indicated), which is better but still biases the effectiveness assessment, producing lower estimations of effectiveness with larger symptom base-rates. Thus, it is necessary that practitioners watch their patients closely to prevent them from underestimating their treatments' success, and consequently abandoning the treatment or resorting to pseudomedicine. As mentioned, the patients who are most vulnerable to the effectiveness underestimation are those who initially experience frequent symptoms. Perhaps the misestimation of effectiveness could be reduced if clinicians try to make patients aware that changes in symptom rate proportional to the baseline can be in fact misleading, and provide them with more objective statistics such as absolute differences, when they are available. Previous research suggests that giving this information in frequency format (Bodemer et al., 2014) or pictorial format (Tubau et al., 2019) can greatly improve patients' comprehension and the chances of communication success. On the other hand, as Experiment 2 shows, the chronological order in which patients usually know the contingency information in natural settings (i.e., first they get to know the symptom base-rate without treatment, then they experience the symptom occurrence rate during the treatment) seems to alleviate other effectiveness estimation problems such as the causal illusion (Matute et al., 2015, 2019) that is more easily observed when the trials are presented in random order. Thus, in this case the natural presentation order works in our favor to prevent the overestimation of effectiveness.

DATA AVAILABILITY STATEMENT

The experiment materials, the datasets analyzed for this study, and the R scripts to reproduce the tables and figures can be found in the Open Science Framework: https://osf.io/emzbj/.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the Ethical Review Board of the University of Deusto (CEUD). The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

FB, MM-F, and HM contributed to the conception and design of the study. MM-F programmed the experiment. FB performed the statistical analysis. FB wrote the first draft of the manuscript. All authors contributed to the manuscript revision, read, and approved the submitted version.

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OPINION
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Cognitive Biases in Chronic Illness and Their Impact on Patients' Commitment

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INTRODUCTION

Cognitive biases are constructs based on erroneous or deformed perceptions which produce systematically distorted representations with respect to some aspects of the objective reality, such as prejudices (Haselton et al., 2005). Biases impact everyday life because they affect decisions and behaviors. For example, one may persist in an unhealthy behavior (e.g., smoking) because he selectively overestimates evidence that feeds up a pre-existing conviction (e.g., "smoking boosts my concentration") (Masiero et al., 2019): this is known as confirmation bias (Hernandez and Preston, 2013).

While some biases appear inherent to human cognition, others are situation–specific. Several studies have shown that there are cognitive biases typical of people who live with a chronic illness and continually attend to health management (Lichtenthal et al., 2017). These biases influence information processing about the disease and consequently decision making (DM), impacting the health and quality of life (Khatibi et al., 2014). The objectives of the present contribution are to synthetize information on biases in chronic illness and to highlight the possible effect of biases on health management. The last sections will explore how biases could influence not only the information processing, but also the motivation and agency within the patients' healthcare journey.

COGNITIVE BIASES IN CHRONIC ILLNESS

DM in chronic illness is complex because patients find themselves in a state of uncertainty (Reyna et al., 2015), and have to take life-relevant decisions in an emotionally-charged situation (Szekely and Miu, 2015; Mazzocco et al., 2019). People are averse to the unknown and risk (Tversky and Kahneman, 1986), and this may lead them to choose suboptimal treatments because they are perceived as less risky. For example, a patient may decide to refuse a treatment as it involves unlikely yet feared risks, this way failing to consider the benefits (Fraenkel et al., 2012; Pravettoni et al., 2016). The biases most frequently highlighted in the literature on chronic illness are attentional (Bar-Haim et al., 2007; Chan et al., 2011), interpretation (Ouimet et al., 2009; Lichtenthal et al., 2017), and recall biases (Karimi et al., 2016). Attentional bias is defined by Schoth et al. (2012) as the selective attention to specific information, failing to consider the alternatives because of the interference of pre-existing sensitivity. Interpretation bias is the patients' tendency to interpret an ambiguous information in an illness–related fashion and to catastrophize (Crombez et al., 2013; Khatibi et al., 2015). Recall bias consists in distortions in the accuracy of the recollections retrieved ("recalled") about events or experiences from the past (Last, 2000).

These biases have, in common, the tendency to prioritize information connected to the disease/illness experience, at any level of information processing and DM. For example, individuals tend to selectively focus on threat or pain–related words or pictures (Bar-Haim et al., 2007; Crombez et al., 2013). Attention to threatening stimuli and illness–related interpretation can lead to biased decisions in terms of treatment and lifestyle: subjects with chronic pain will tend to focus on pain–related information and consequent preoccupation (Bar-Haim et al., 2007; Hakamata et al., 2010; Schoth et al., 2012), this way preferring healthcare options that are less likely to cause pain, independently of their overall effectiveness or value. Similarly, they would avoid certain activities they feel potentially pain–inducing, with the consequence of social isolation and reduced social support (McCracken, 2008; Schoth et al., 2012). Negative interpretation of information influenced by interpretation bias could promote a greater pessimism about the potential control of a disease and, therefore, lower the implementation of control behaviors which are considered ineffective (Miles et al., 2009; Everaert et al., 2017).

Studies in psycho-oncology have shown that biases play a role in the fear of recurrence (FOR) (Miles et al., 2009; DiBonaventura et al., 2010). The fear that cancer may return, an important aspect to monitor in cancer survivors (Marzorati et al., 2017; Tsay et al., 2020), features a cognitive component related to the survivor's difficulty in processing disease–related information, thus, reducing the understanding of pathology and treatment. Patients with FOR tend to focus on the negative aspects within the doctors' explanation (Wenzel and Lystad, 2005; Davey et al., 2006; Han et al., 2006). Possible consequences entail detriment to the patient-doctor alliance (Ha and Longnecker, 2010), patient's inability to take into account all aspects of medical information to take good decisions (Kee et al., 2018), and, in the long run, the tendency to resort to options alternative to traditional medicine patients feel reassuring (Dobrina et al., 2020).

For what regards recall bias, people with past experience of pain or suffering create memory traces that distort the memory of a stimuli associated with those sensations (Karimi et al., 2016). Some studies on patients with chronic pain have shown propensity to recall pain–related information (Pincus and Morley, 2001; Rusu et al., 2012). Studies have demonstrated a recall bias for somatic symptoms showing a retrospective overestimation of symptom severity (Broderick et al., 2008; Walentynowicz et al., 2015). Lindberg et al. (2017) showed that breast cancer survivors' perception of past quality of life is significantly worse than it actually was (physical and cognitive functioning, fatigue, and pain). Patients with depression and pain recalled negative health–related information to a greater extent than the non-depressed controls and patients with depression or pain only, showing that the recall bias is exacerbated both bythe psychopathological and physical condition (Rusu et al., 2012). While there is less information on the direct influence of recall bias on health management, the propensity to recall negative information may affect the patients' self-efficacy or their belief to be able to manage their own health, in that memory of successful management ("mastery") is crucial to the maintenance of motivation (Hiltunen et al., 2005). In other words, it would hinder the perception of an effective self-agency which is necessary to implement healthy behaviors and treatment adherence, especially when it requests effort on the patient's side.

Biases in Self-Perception

The tendency to focus on a threatening stimuli may affect a chronic patient's cognition on a deep level. According to literature, this tendency may be rooted in self-perception. Self-perception is defined as the "cognitive generalizations about the self, derived from past experience, which organize and guide the processing of self-relevant information contained in the individual's social experience" (Markus, 1977, p. 64). Self-perception may be distorted (Alloy et al., 1988; Walfish et al., 2012). Chronic patients may develop self-perception focused on illness–related memories, such as viewing themselves as "sick" or "injured." Indeed, chronic disease implicates years of experience, adaptation to a disease of varying severity, making this information highly accessible. On one hand, self–related biases influence distorted tendencies in information processing such as those outlined above (attentional, interpretation, and recall biases) (Derry and Kuiper, 1981; Clemmey and Nicassio, 1997; Guzman and Nicassio, 2003). On the other hand, illness–related self-representation could be directly associated with mental health outcomes, such as anxiety and depression (Triberti et al., 2019), especially when the current ("actual") self is perceived inconsistent with other coexisting self-representations (e.g., the "ideal self" or the person one would like to be), a phenomenon known as "self-discrepancy" (Higgins, 1987, 1989). This result emerged for example in a research where oncological patients were asked to create digital avatars representing their multiple facets of the self (Triberti et al., 2019), as well as in qualitative and quantitative research focused on the chronic patients' self-perception (Clemmey and Nicassio, 1997; Bailly et al., 2015; Michaelis et al., 2019). Recent reviews highlight that self-discrepancy represents a contributory factor in psychiatric disorders (Mason et al., 2019) and negatively affects the patients' quality of life (Kwok et al., 2016).

Social Biases

Full consideration of biases within the chronic illness context requires taking into consideration those related to social cognition. DM rarely occurs in isolation. Indeed, the decisions in a chronic illness are often influenced by others (Ellickson et al., 2005; Germar et al., 2014). Others' influence on decisions can often lead to a wrong evaluation of the choices with a tendency to take a greater risk (Gardner and Steinberg, 2005; Muchnik et al., 2013). Social biases can occur within the social context. Several studies have dealt with the study of group psychology (Bar-Tal, 2012; Hogg, 2012; Thibaut, 2017); for example, the classic experiment by Asch (1951) showed that a subject will tend to conform his opinion, even when clearly untrue, to that of the other members of the group he feels part of because of social pressure. Groups may exert an influence on the cognitive processes and decisions just by a conformity effect. Certainly, such classic experiments may be criticized today, for example,

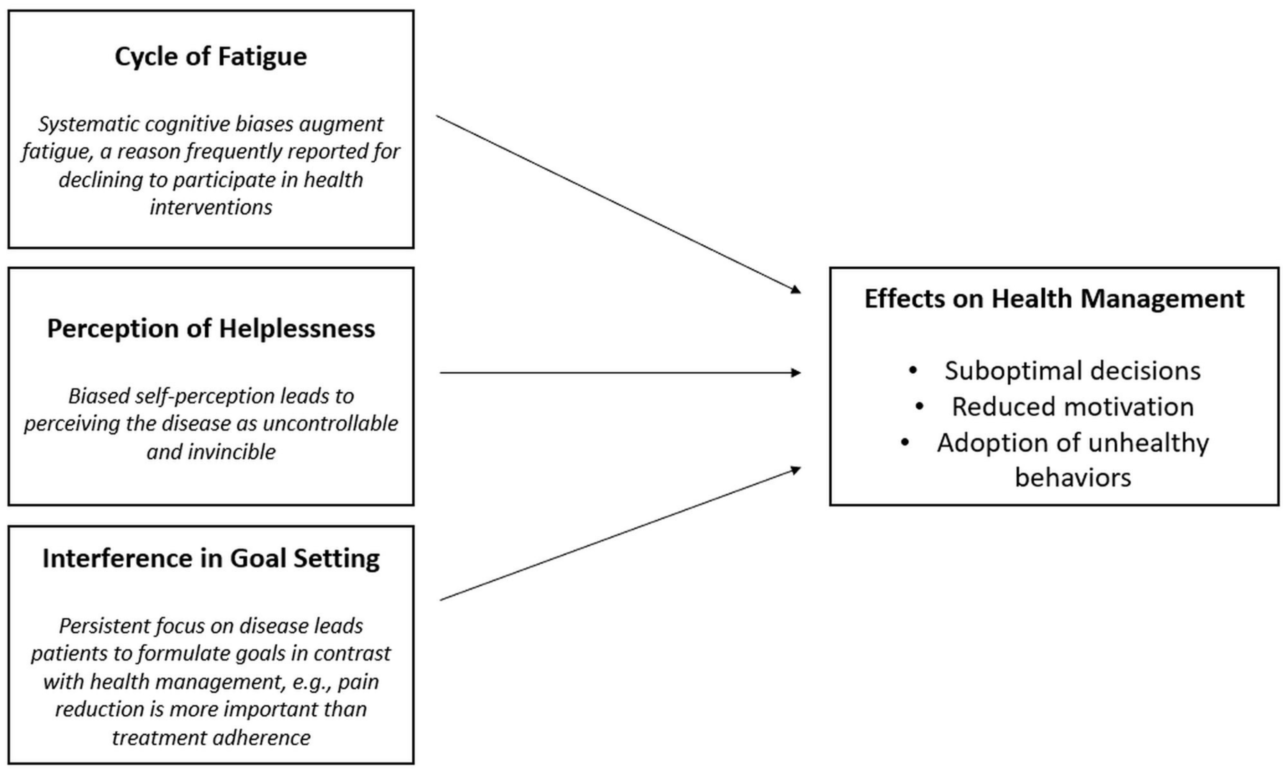

FIGURE 1 | Three main processes that influence patients' health management.

because they rely on abstract tasks and artificial settings and have a low ecological validity (Arjoon, 2008). Yet, it is well-known that groups belonging could promote biases in reasoning. Chronic patients are influenced by caregivers, family, and close friends, who often have different preferences regarding the treatment (Laryionava et al., 2018). Furthermore, health and medicine have now become an increasingly shared context online; patients have access to information that is not always reliable and evidence–based, and they may join groups more easily, often with the aim to share experiences, receive advice, and empathic support. The well-known example of anti-vaccine groups and related studies (Jolley and Douglas, 2014) show that the exposure to conspiracy theories within groups may sensitively affect the patients' health decisions. Even in the case of chronic patients, a social bias can, therefore, lead the patients to change their attitudes and opinions in favor of those shared by relevant groups.

THE INFLUENCE OF BIASES ON THE PATIENTS' DECISION MAKING

Biases can influence the DM process in chronic illness (Gorini and Pravettoni, 2011; Lucchiari and Pravettoni, 2013). Some cognitive biases in chronic illness could enhance attention to and the salience of symptoms which tend to be perceived as uncontrollable and incurable (Moss-Morris and Petrie, 2003), so that they negatively influence the patients' decisions regarding treatment and health management. Furthermore, patients affected by biases in self-perception may find themselves in a situation of perceived helplessness and self-derogation, which affects their ability to manage their own health and possibly augments the risk of mental health issues, such as anxiety. Psychologically vulnerable chronic patients could also refer to others and groups to make health decisions, which is a risky strategy especially when unprofessional opinions are involved.

It is possible that biases in chronic illness could influence DM and the formation of effective motivation to engage in healthy behaviors. Many psychological interventions are conducted to help patients manage their own health, as well as to recover a sense of authority and control over their life, this way addressing the biases' effects (Kondylakis et al., 2017). However, the patients' decision to take part in such interventions could be influenced by biases as well. Among the multiple possible mechanisms, we hypothesize that this happens because of three main processes (**Figure 1**). The first involves fatigue as psychological process directly related to biases. Recent studies have underlined that a reason to decline participating in a psychological intervention or resorting to psychological support is feeling tired or weak (Bernard-Davila et al., 2015; Aycinena et al., 2017). Indeed, it exists as a reciprocal interaction between the systematic biases and perception of fatigue: on the one hand, fatigue (physical and cognitive) leads to a careless information processing which augments the likelihood of biased reasoning (Boksem and Tops, 2008; Howard et al., 2015); on the other hand, symptom focusing and the way chronic patients interpret disease–related information are demonstrated to augment their perception of fatigue (Wiborg et al., 2011; Hughes et al., 2016).

Another relevant process regards the perception of helplessness as a self-perception component. Helplessness leads subjects to perceive symptoms like chronic pain as uncontrollable, unpredictable, and immutable, and to generalize these to daily functioning (Abramson et al., 1978; Evers et al., 2001). Along with passive coping (activity avoidance and persistent worrying), this contributes to perceiving the disease as uncontrollable and invincible, reducing self-efficacy, and the motivation to react to it (Samwel et al., 2006; Verhoof et al., 2014).

Finally, it is possible that the influence of systematic biases is pervasive to the point that it influences motivation formation. While motivation is often conceptualized as a dynamic force or pull (e.g., drive, instinct, intention), it could be structured as the declarative, explicit course of actions and outcomes to achieve, namely *objectives* or *goals* (Ryan, 2012; Triberti and Riva, 2016). Goal setting is a fundamental component of any care plan (Vaughn et al., 2016). Goal setting allows patients to identify the short- and long-term objectives to achieve, taking into account the patient's needs and lifestyle (Wade, 2009; Levack et al., 2015; Smit et al., 2019). Biases and, in particular, the tendency to focus on the negative factors may lead the patients to formulate goals to avoid the negative symptoms (e.g., pain), instead of pursuing the long-term personal growth objectives (e.g., "I will not participate in the intervention because it's tiring: I just need to rest").

On this basis, it is possible that systematic cognitive biases in chronic illness do not only influence the treatment decisions but also the motivation to resort to interventions that could help in reduce their detrimental effects. In other words, the repeated influence of the cognitive biases may be associated with a "vicious circle" that reduces the patients' motivation to recognize and address the same mental health issues that influence their DM.

CONCLUSION

The present contribution explored the ways biases could influence the motivation and agency within the patients' healthcare journey. By considering of chronic illness biases, we hypothesized that DM and motivation are directly altered, leading to a reduced patient engagement in their own healthcare. The strength of this hypothesis lies in the possibility to test it by quantitative research focused on the prevalence of specific biases in patient populations characterized by a low engagement and/or by the tendency to decline participation in health interventions. On the other hand, its weakness lies in the possibly reciprocal interaction between the biases and engagement: patients may incur in frequent biased cognition exactly because they are not adequately supported in their care process. Furthermore, the three mechanisms hypothesized here do not exhaust all the possible influences of biases so that future research should provide evidence to build a more complete model of their effects on the patients' decision making. This would allow the practitioners to understand how to address dysfunctional cognition to improve the accessibility and effectiveness of health engagement interventions.

AUTHOR CONTRIBUTIONS

LS conceived the ideas presented in the article and wrote the first draft. ST contributed with the discussion on the ideas presented and supervised the writing. Both authors contributed equally to the revision.

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Can We Set Aside Previous Experience in a Familiar Causal Scenario?

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Causal and predictive learning research often employs intuitive and familiar hypothetical scenarios to facilitate learning novel relationships. The allergist task, in which participants are asked to diagnose the allergies of a fictitious patient, is one example of this. In such studies, it is common practice to ask participants to ignore their existing knowledge of the scenario and make judgments based only on the relationships presented within the experiment. Causal judgments appear to be sensitive to instructions that modify assumptions about the scenario. However, the extent to which prior knowledge continues to affect competition for associative learning, even after participants are instructed to disregard it, is unknown. To answer this, we created a cue competition design that capitalized on prevailing beliefs about the allergenic properties of various foods. High and low allergenic foods were paired with foods moderately associated with allergy to create two compounds; high + moderate and low + moderate. We expected high allergenic foods to produce greater competition for associative memory than low allergenic foods. High allergenic foods may affect learning either because they generate a strong memory of allergy or because they are more salient in the context of the task. We therefore also manipulated the consistency of the high allergenic cue-outcome relationship with prior beliefs about the nature of the allergies. A high allergenic food that is paired with an inconsistent allergenic outcome should generate more prediction error and thus more competition for learning, than one that is consistent with prior beliefs. Participants were instructed to either use or ignore their knowledge of food allergies to complete the task. We found that while participants were able to set aside their prior knowledge when making causal judgments about the foods in question, associative memory was weaker for the cues paired with highly allergenic foods than cues paired with low allergenic foods regardless of instructions. The consistency manipulation had little effect on this result, suggesting that the effects in associative memory are most likely driven by selective attention to highly allergenic cues. This has implications for theories of causal learning as well as the way causal learning tasks are designed.

Keywords: associative learning, causal learning, prediction error, heuristics, reasoning, attention

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INTRODUCTION

Causal reasoning refers to the process or set of processes by which we arrive at judgments about cause and effect in a wide range of situations. The process by which we acquire the knowledge on which causal reasoning is based is referred to as causal learning. Since at least the British empiricists of the eighteenth century (Hume, 1978), causal reasoning has been linked to mental (and statistical) association. The study of human associative learning concerns how we acquire information about covarying events in our environment and the inferences and decisions that we make as a consequence of experiencing those events. As a field, its aims overlap with those of causal reasoning, though they are distinct in important ways. For instance, human associative learning research has typically been more concerned with how we acquire information about the covariation of events, than how this information is translated into judgments about cause and effect. The assumption generally taken is that (other factors being equal) the stronger the association between a cue and outcome, the stronger the causal judgment (Thorwart and Livesey, 2016; Le Pelley et al., 2017). The notion that we often interpret associations between events as evidence of a causal relationship has stimulated plenty of debate about the nature of causal learning as well as the nature of associative learning in humans in other domains (e.g., Mitchell et al., 2009). Formal models of associative learning make predictions about how covariation between events leads to differences in the strength of associations (e.g., Rescorla and Wagner, 1972) or the strength of retrieved memories (e.g., Stout and Miller, 2007). Many studies have shown that people make causal judgments that are sensitive to factors, which are not captured by these models (e.g., see Shanks, 2007; Mitchell et al., 2009 for reviews). In particular, instructions that manipulate the relevance of various properties of the learning context can influence whether causal judgments conform to the predictions of associative learning models. For instance, prior understanding of how causal relationships work within a given domain can determine whether and to what extent causal judgments exhibit competition among simultaneously presented cues (Waldmann and Holyoak, 1992; Waldmann, 2001). This study examined a related but distinct question, whether instructions to ignore or to use prior knowledge can control competition for associative memory in the same way that they control cue competition in causal judgments.

Cues that occur together interact lawfully in ways that suggest competition for a limited amount of learning that an outcome can support. For instance, two novel cues presented in compound and followed by an outcome (AX+) appear to support less learning than when each cue is paired with the outcome separately (A+/X+). When A is more salient than X, learning about the relationship between A and the outcome appears to heavily "overshadow" learning about X (Mackintosh, 1975). However, when the cues are of approximately equal salience (as is often the case in human learning experiments), this competition appears to be reciprocal such that neither cue achieves the level of association that it would if trained in isolation (Mackintosh, 1976 or see, e.g., Mitchell et al., 2006, for evidence of this in human causal learning). This mutual competition between cues is widely referred to as overshadowing (e.g., McLaren et al., 2014). Another prominent example of cue competition is the blocking effect. In the typical procedure one cue, B, is reliably followed by an outcome in an initial stage (B+). Later, this cue is presented in compound with a novel cue (Y) and again is followed by the outcome (BY+). In this instance, responding to the target cue Y is often observed to be weaker than responding to control cues A or X, suggesting that learning about the target cue Y is blocked by previous learning that B reliably predicts the outcome (Kamin, 1968). Cue competition effects were initially discovered in animal conditioning; however, there is now a wealth of evidence to suggest that they also occur in human causal learning (e.g., Shanks, 1985; Aitken et al., 2000; Livesey et al., 2013; Le Pelley et al., 2017).

Cue competition effects like blocking and overshadowing are consistent with the idea that the strength of learning is determined by prediction error or the discrepancy between the expectancy of an outcome and what is actually experienced (Rescorla and Wagner, 1972). Prediction error models of learning that assume a summed error term (in which the expectancy of an outcome is a product of the summed associative strength of all cues present on a trial) have been highly influential in the development of associative learning theory and its application to human causal learning (Shanks, 2007). By such accounts, blocking occurs because the pretraining of cue B leads to a strong expectation of the outcome on BY+ trials. When the outcome occurs (+), prediction error is minimal and therefore Y does not develop a strong association with the outcome (Rescorla and Wagner, 1972). Other associative models assume that attentional processes are implicated in the blocking effect (e.g., Mackintosh, 1975; Pearce and Hall, 1980). For instance, Mackintosh (1975) proposed that the predictive validity of a cue determines its associability and thereby the attention paid to it. Thus, pretraining renders B more salient in the context of the task and blocking is at least partly the result of a lack of attention to the blocked cue. Consistent with this account, a blocking procedure does appear to produce biases in attention away from the blocked cue in human causal learning tasks (e.g., Beesley and Le Pelley, 2011). Reduced prediction error should limit learning about those specific aspects of an outcome that are predicted. Predictions that are based on prior learning should not restrict further learning about an unexpected outcome or an unexpected property of the outcome. It should be noted that individual models like Rescorla-Wagner and Mackintosh are unable to provide a complete account of associative learning, but as Le Pelley et al. (2017) recently argued, the failings of any one these models in the context of human causal learning does not undermine the evidence that the general principles of associative learning (in particular, the assumption that prediction error determines the strength of learning) apply to human learning regardless of whether the relationships under question are causal or non-causal.

When studying human causal learning in the laboratory, researchers often use hypothetical scenarios to establish a framework for learning novel relationships. In many such studies, researchers capitalize on existing knowledge by selecting scenarios that are intuitive and familiar. Participants are asked to disregard their prior knowledge when making judgments in the task, but the assumption made by researchers is that new learning will proceed quickly because the cues are easy to identify and discriminate. One widespread example is the allergist task (Wasserman, 1990). In this task, participants assume the role of an allergist who is trying to identify the specific allergies of a fictitious patient (Mr. X). On each trial, a meal (consisting of one or more foods) that Mr. X has eaten is presented, and participants are asked to predict whether or not Mr. X will suffer an allergic reaction. After each prediction, they receive corrective feedback. Finally, participants are asked to judge which foods are causing Mr. X to suffer an allergic reaction, and this judgment may also take the form of a probability or cued recall judgment. The food allergist task has been used to study cue competition effects such as blocking and overshadowing (e.g., Shanks and Lopez, 1996; Aitken et al., 2000; Lovibond et al., 2003; Beckers et al., 2005a; Mitchell et al., 2005, 2006; Vandorpe et al., 2007; Livesey et al., 2013, 2019b; Luque et al., 2013; Uengoer et al., 2013), learning of preventative relationships such as in the case of conditioned inhibition (Karazinov and Boakes, 2004, 2007), complex rule learning tasks such as the patterning task (Shanks and Darby, 1998; Wills et al., 2011; Don et al., 2020), as well as a host of phenomena related to learned attentional changes including the learned predictiveness effect (Le Pelley and McLaren, 2003; Don and Livesey, 2015; Shone et al., 2015), the inverse base-rate effect (Don et al., 2019), outcome predictability effects (Griffiths et al., 2015; Thorwart et al., 2017), and other related transfer effects (Livesey et al., 2019a). Food allergies are relatively commonplace such that, by the time they enter the laboratory, participants have a lifetime of experience with food and its ability to cause allergic reactions in oneself or others. These properties not only support learning new relationships established in the experiment but also mean participants bring to the experiment prior knowledge or biases that may not be easily set aside.

In this study, we examined the extent to which participants are able to suppress prior knowledge and beliefs, when instructed to do so, in a causal learning task. We tested the extent to which prevailing cue-outcome associations (i.e., associations that people typically hold about certain foods and types of allergies) influenced cue competition expressed in both causal ratings and in the strength of associative memory. We first surveyed an independent sample to identify a number of foods commonly (and uncommonly) associated with allergic reactions. We then used this information to create a pseudo-blocking design in which the pretraining was replaced by prevailing beliefs about food allergies. That is, the foods rated as highly allergenic and those given a low allergenic rating were paired with foods that given a moderate allergenic rating to create two compounds; high + moderate and low + moderate. Each type of compound was then associated with the allergic reaction outcomes. Participants were either told to use or ignore their prior knowledge of foods and allergies. For participants told to use their knowledge, we assumed that the presence of a high allergenic food in high + moderate compounds would generate a prediction that an allergic reaction was going to occur, and that when it did, participants would attribute this outcome to the high allergenic food more than the moderate food. We expected to see evidence of this in both causal ratings and associative retrieval such that, for this "use your prior knowledge" group, moderate foods paired with low-allergenic foods would have higher causal ratings and higher associative memory scores than moderate foods paired with high allergenic foods. The key question was to what extent this pattern changed when participants were told to *ignore* their prior knowledge. In other words, do people have control over whether prior beliefs influence their current judgments and is that control the same for causal and associative memory judgments? As mentioned, past studies have shown that causal judgments are sensitive to instructions that inform how covariation information applies in a given context (Waldmann and Holyoak, 1992; Waldmann, 2001). Thorwart and Livesey (2016) argued that such instructions could also affect how that covariation information is acquired in the first place.

In order to achieve this end, we conducted an initial survey of a separate sample of undergraduate psychology students from the University of Sydney. The survey included 30 common food items and participants were asked to indicate the extent to which they associated each food with an allergy as well as which specific allergenic symptoms they associated with allergies to the foods in question. From the survey, we were able to identify three categories of foods, those strongly associated with allergy, those weakly associated with allergy, and some moderately associated with allergy.

If prevailing beliefs about food allergies lead participants to generate predictions about the likelihood of an allergic reaction, then prediction error models of learning would predict that compounds with high allergenic foods should generate less prediction error than compounds with low allergenic foods. Given the evidence outlined above, we therefore expected that food cues commonly associated with allergy would produce greater competition for association with the outcome than foods infrequently associated with allergy, thereby leading to poorer learning for the moderate cue-outcome relationship in the high + moderate than the low + moderate compounds.

The highly allergenic foods identified in the survey formed two subcategories based on the kind of symptoms most strongly associated with them; the first were associated with anaphylactic type symptoms (for example, difficulty breathing, swelling, and rash) and the second, gastrointestinal symptoms (such as stomach ache, cramps, and nausea). These two subcategories enabled us to manipulate the specific properties of the outcome of high + moderate compounds. Specifically, the experienced allergic reaction could be either consistent or inconsistent with the category of symptoms commonly associated with the high allergenic food in question. This is important because the highly allergenic foods may increase competition for learning because they reduce prediction error or they may simply increase competition because they are more salient in the context of the allergist task. That is, highly allergenic foods could strongly overshadow their moderate competitors as their prior predictive validity in such scenarios would lead to an attentional bias toward them in the manner proposed by Mackintosh (1975). If so, then the relative consistency of the type of allergy that follows should not affect the strength of learning, and we should see no difference in associative memory among the high + moderate compounds. On the other hand, experiencing an allergic reaction that is inconsistent with expectations ought to generate a larger prediction error than one that is consistent. Therefore, we may see greater competition produced by an outcome that is consistent than one that is inconsistent with expectations. Prediction error driven learning would therefore predict greater overshadowing when the experienced symptom of the allergic reaction is commonly associated with the competing cue than when it is not.

Using the food cue and allergic reaction outcome categories identified from this survey, we constructed the design shown in **Table 1**. As noted earlier, two kinds of compound formed the basis of the pseudo-blocking cue competition design: high + moderate and low + moderate. The high + moderate compounds were paired with a specific reaction that was either consistent or inconsistent with expectancies. For example, a food commonly associated with gastrointestinal symptoms, milk, for example, could be paired with a reaction that was either consistent with this pattern (e.g., stomach ache) or inconsistent with this pattern (e.g., rash). Half of the sample was given the typical instruction to ignore what they know about food allergies in the real world (group *ignore*), and the remainder were told that their existing knowledge of food allergies would be useful and that they should use that knowledge to inform their judgments (group *use*). In other words, the instructions were intended to encourage or discourage participants from relying on their prior knowledge of food allergies when making predictions in the task. If participants are able to set aside their existing beliefs when making judgments about the causal relationships in the task, then those who are instructed to use their prior knowledge of food allergies should show greater competition, and therefore, a stronger distinction in ratings for the moderate cues paired with high vs. low allergenic foods, than those who are told that their prior knowledge is not informative. Given the evidence outlined above, we expected causal ratings to be sensitive to instructions to use or ignore prior knowledge. The question was whether associative memory judgments would reflect the same level of control.

TABLE 1 | Experiment design.

Compounds	Training	Test
High + Moderate	**A**R – O1$_{con}$	**A**, R
	BS – O2$_{inc}$	**B**, S
	CT – no O	**C**, T
	DU – O3$_{con}$	**D**, U
	EV – O4$_{inc}$	**E**, V
	FW – no O	**F**, W
Low + Moderate	*H*X – O5	*H*, X
	*I*Y – O6	*I*, Y
	*J*Z – no O	*J*, Z

Letters represent food cues randomly assigned within subcategories: Those in ***bold*** *(A–F), those in italics (H–J), and those in regular font (R–Z) are foods identified as strongly associated with allergies, weakly associated with allergies, and moderately associated with allergies, respectively. Outcomes 1–6 fall into two subcategories, stomach-related (nausea, cramps, and stomach ache) and anaphylaxis-related reactions (difficulty breathing, rash, and swelling). Cues A–C are those foods generally associated with anaphylactic symptoms and D–F gastrointestinal symptoms. Highly allergenic foods were paired with an outcome either consistent or inconsistent with expectations, e.g., if A were peanuts and B were almonds (two foods associated with anaphylactic symptoms), then O1 would be difficulty breathing and O2 would be stomach ache. This relationship is denoted by the subscript on the outcome: "con" for consistent and "inc" for inconsistent.*

MATERIALS AND METHODS

Participants

We recruited 137 undergraduate psychology students from the University of Sydney to participate in the experiment. Sixty-six of these students completed the experiment as part of a tutorial assessment for an advanced psychology course in learning and behavior, and the remainder were recruited from the first year psychology subject pool and received partial course credit as compensation for their participation. Of these, two were excluded on the basis of failing to meet the learning criterion (set at <50% accuracy across training) and further 11 were excluded for failing the manipulation check (described below). The remaining sample of 124 had a mean age of 21.02 years and was predominantly female (N = 85). Half were randomly assigned to the *ignore* condition (n = 62) and half to the *use* condition (n = 62).

Stimuli

An independent sample of 74 undergraduate psychology students from the University of Sydney were asked to complete a short survey of their knowledge or intuitions of common food allergies in exchange for course credit. Of these, six were excluded for non-compliance, and leaving a final sample of 68 participants. Participants were shown 30 common food items and were asked to complete two questions about each food in turn. The first was to rate the extent to which each item was associated with an allergy of any kind on a scale ranging from "not at all" to "strongly associated." They were then asked to identify any specific symptoms associated with an allergic reaction to the foods in question, for example, difficulty breathing. Participants were instructed to select one or more symptoms from a list (which included "NA" as an option for those foods that were not associated with an allergy or for which there was no specific symptom that they associated with that food allergy) and were given the option to specify any that were not listed. Eighteen foods were selected for this experiment, six from among the highest mean association with allergy, three of the lowest mean association with allergy, and the remaining nine from the cues that fell in the moderate range between these two extremes. The results of this survey are provided in the **Supplementary Material**.

Procedure

The allergist task was programed with the Psychophysics Toolbox for MATLAB (Kleiner et al., 2007). Participants were given a hypothetical scenario in which they were asked to play the role of an allergist trying to determine the allergies of a particular patient, Mr. X. Half of the participants were given the standard instructions that usually accompany such tasks, that is, they were told to ignore what they know about food allergies and use only the information presented to make their judgments. The remaining participants were told that the patient's allergies were based on real world examples, and thus they should assume that any knowledge of food allergies that they possess would be useful in making judgments about this patient's allergies. Training consisted of four blocks of 18 trials in which each compound in **Table 1** was presented twice per block. On each training trial, participants were presented with a meal consisting of two foods that Mr. X had eaten and were asked to predict which allergic reaction (if any) would occur as a result. They used the mouse to select an outcome from among seven possible options (*nausea*, *cramps, stomach ache*, *difficulty breathing*, *rash*, *swelling*, or *no allergic reaction*). After a choice was made, corrective feedback was presented onscreen for 2 s before the next trial began. In order to reduce recency effects in the memory test, participants completed a short filler task after the training phase. The filler task was a set of eight syllogisms adapted from the belief bias task (Markovits and Nantel, 1989). Each item presented a conclusion that was either believable but invalid or unbelievable but valid. The syllogisms were presented one at a time, and participants were asked to judge if each conclusion was logically true or false. The results of the filler task are reported in the **Supplementary Material**.

At test, participants were presented with the cues from the training phase one at a time and were asked to make two different judgments about each cue. First, they were asked to recall the outcome that had followed the cue in question. All seven possible outcomes were presented onscreen and participants made a selection by clicking on the corresponding label. Once they had done so, they rated their confidence in their choice by indicating on a linear analogue scale ranging from "not at all confident" to "very confident." Second, they were asked to make a causal judgment about the cue in question. Another linear analogue rating scale appeared onscreen and participants were asked to indicate to what extent they believed the cue caused an allergic reaction of *any* kind in the fictitious patient Mr. X. Following this, a manipulation check was administered in which participants were asked to indicate which version of the critical instructions they had received at the beginning of the experiment. Three options were presented on screen, each of the critical instructions in full or "neither of the above." Twelve participants who failed to correctly report the instruction they received were excluded from the analysis, seven of these were from the ignore group and the remaining five from the use group.

Finally, to gauge the extent to which our participant's prior beliefs about food allergies aligned with those of the independent sample used to inform this design, participants completed a questionnaire about their prior knowledge or understanding of the allergenic properties of the foods used as cues in the experiment. They were instructed to ignore the events of the experiment when answering the questionnaire and respond based on their knowledge about these foods *before* the experiment began. Of course, giving such instructions presupposes that participants can successfully set aside what they experience in the experiment when responding on this questionnaire, the very question we aimed to address in this study. To anticipate the results, we found that prior knowledge biased associative retrieval even when participants were asked to ignore that knowledge. This suggests that any consistency between the initial survey and responses on the post-experimental questionnaire should be interpreted with caution. The questionnaire was programed and conducted using the Qualtrics platform and was identical to the initial survey discussed above, with the exception that only the 18 food items retained for the experiment were included.

RESULTS

Bayesian analyses were conducted with the "BayesFactor" package (Morey and Rouder, 2018) for R (R Core Team, 2019).

Training

Figure 1 shows the mean proportion correct predictions in the training phase as a function of cue type and instruction condition. Among participants that met the training criteria, mean accuracy in the final block of training was above 90% in both instruction groups, indicating that both groups were able to learn the contingencies. To investigate whether there were any differences between groups in the strength of acquisition of the compounds of interest, we conducted a three-way ANOVA on training accuracy with factors of compound (high consistent + moderate vs. high inconsistent + moderate vs. low + moderate), training block (1–4), and instruction group (ignore vs. use). As a complement to this analysis, we conducted an equivalent Bayesian repeated measures ANOVA with default priors for the effects (r = 0.5). For the interaction, we report the Bayes factor inclusion across matched models, which provide an estimate of evidence for the effect by comparing the model with the interaction effect against equivalent models stripped of the effect (denoted $BF_{Inclusion}$; Rouder et al., 2017). There was a significant effect of block, $F(2.56, 312.84)$[1] = 627.58, $p < 0.001$, $\eta_p^2 = 0.846$, $BF_{10} = 1.33 \times 10^{259}$, indicating that performance improved with training. There was also however a significant main effect of compound, suggesting that there were some differences in accuracy for the different compounds, $F(1.98, 241.13) = 16.10$, $p < 0.001$, $\eta_p^2 = 0.117$, $BF_{10} = 252$. However, as the interaction with block implies, these differences were significantly diminished by the end of training, $F(5.06, 617.66) = 4.46$, $p < 0.001$, $\eta_p^2 = 0.035$, $BF_{Inclusion} = 2.47$. Neither the main effect of instruction nor any other interactions reached significance, the largest $F = 1.47$ for the compound by instruction interaction.

[1]Mauchly's test of sphericity indicated that the assumption of sphericity was violated. We therefore report the Greenhouse-Geisser corrected degrees of freedom for this ANOVA.

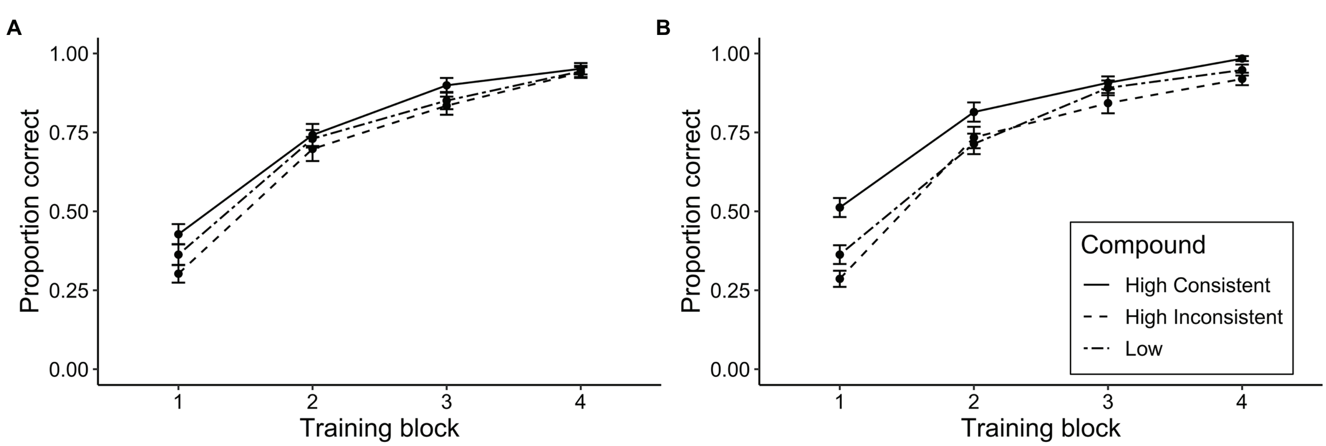

FIGURE 1 | Mean proportion correct during training for **(A)** the ignore group and **(B)** the use group as a function of compound and training block. Compound names refer to the status of the competing cue. For example, "High Consistent" summarizes accuracy data for compounds with one highly allergenic cue that were followed by a symptom consistent with expectancies derived from the initial survey (AR and DU from **Table 1**). For simplicity, we have not included data from the filler compounds paired with "no allergic reaction," but we report this training data in the **Supplementary Material**. Error bars represent standard error of the mean.

Test

Learning Scores

Choice accuracy and confidence ratings from the memory test were converted to a learning score reflecting the strength of associative recall for each cue. Learning scores were calculated by multiplying choice accuracy (coded 1 for a correct response and −1 for an incorrect response) by confidence rating (ranging from 0 to 100). Thus learning scores could range from −100, indicating strong memory for an outcome that was not paired with the cue during training, to 100, reflecting strong recall of the correct outcome. Of primary interest were the learning scores for the moderate cues that were presented with either a high or low allergenic food during training. **Figure 2A** shows the individual and mean learning scores for the moderate cues (R/U/S/V/X/Y) as a function of instruction group. Learning scores were analyzed by means of a set of two planned orthogonal contrasts that answered our specific hypotheses. The first compared the scores for moderate cues paired with high allergenic foods (regardless of the consistency of outcome) with scores for moderate cues paired with low allergenic foods (high vs. low).[2]

[2]The high vs. low comparison involves comparing the average of four cues (from the high conditions) against the average of two (from the low conditions). In principle, it might be possible that this difference alone introduces a bias in the results (*via* regression to the mean, for instance). To allay concerns about this, we conducted a bootstrapping exercise that randomly shuffled scores into four vs. two conditions separately for each instruction group and each test measure (learning scores and causal ratings). If there were bias as a result of comparing the mean of four cues to the mean of two, the mean of the differences from the randomly shuffled distributions of four and two cues should differ significantly from zero. The mean differences for learning scores and causal ratings for each instruction group ranged from −0.082 to 0.024. None of these differed significantly from zero, the largest $t(9999) = 1.549$, $p = 0.121$, and Bayes factors all indicated a strong support for the null hypothesis (smallest $BF_{01} = 26.73$). The low − high learning score effect illustrated in **Figure 2B** was more extreme than 99.56% of the bootstrapped values, and the low − high causal rating effect for the use group illustrated in **Figure 3B** was more extreme than 100% of the bootstrapped values.

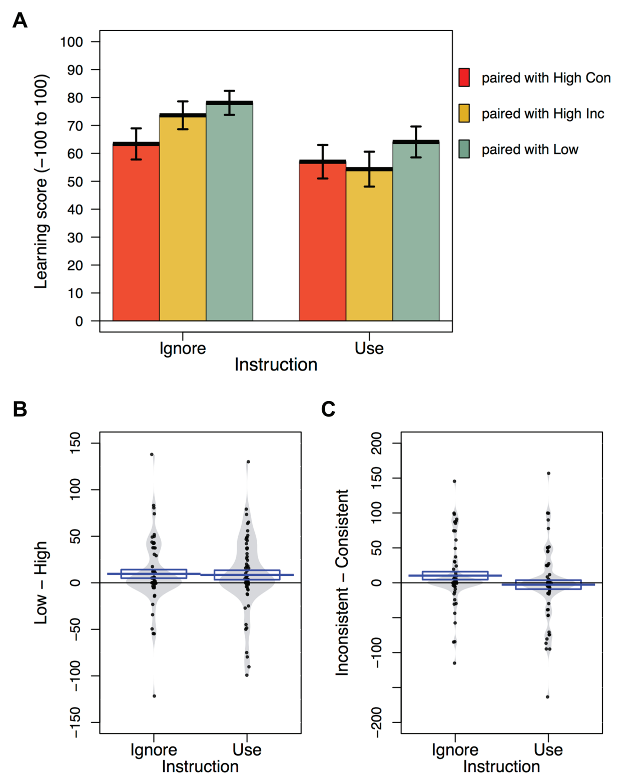

FIGURE 2 | Learning scores as a function of cue type and instruction group. **(A)** Mean, and standard error of learning scores for each cue type as a function of instruction group. **(B)** Violin plots showing the distribution of learning score differences between moderate cues paired with low and high allergenic cues. **(C)** Violin plots showing the distribution of difference scores between cues paired with high consistent and high inconsistent foods. Box around mean line represents standard error of the mean.

The second, compared scores for moderate cues paired with high consistent with scores for moderate cues paired with high inconsistent cues (consistent vs. inconsistent). These comparisons are represented in **Figures 2B,C**, which show individual and mean difference scores for each contrast (low – high and inconsistent – consistent, respectively) as a function of instruction group. These contrasts were complemented with Bayesian *t*-tests, comparing the evidence for the comparisons against a null hypothesis. For each of these tests, we specified a non-directional alternative assigned a Cauchy distribution with default scaling $r = 0.707$.

Learning scores were on average lower for moderate cues paired with high allergenic foods (R/U/S/V) than for moderate cues paired with low allergenic foods (X/Y), $t(122) = 2.63$, $p = 0.010$, $\eta_p^2 = 0.054$, $BF_{10} = 2.78$. This pseudo-blocking effect in associative memory did not appear to differ in magnitude across instruction groups, $t(122) = 0.17$, $p = 0.865$, $\eta_p^2 < 0.001$, $BF_{01} = 5.15$ in favor of the null. For moderate cues paired with high allergenic foods, there was no significant difference in learning scores for those followed by consistent (R/U) vs. inconsistent outcomes (S/V), $t(122) = 0.78$, $p = 0.379$, $\eta_p^2 = 0.006$, $BF_{01} = 6.88$ in favor of the null, and no significant interaction with group, $t(122) = 0.92$, $p = 0.136$, $\eta_p^2 = 0.018$, $BF_{01} = 1.89$ in favor of the null.

For completeness, we also analyzed the scores and ratings for the pseudo-blocking cues A–F; however, we note that the interpretation of these analyses is complicated for two reasons. First, it was not possible to randomly allocate the foods to high or low allergenic categories as it was for the moderate cues so this comparison is not properly counterbalanced. Secondly, these ratings do not inform our hypotheses. That is, whether or not we can ignore prior beliefs about food allergies when making judgments about them is a different question to whether these beliefs have an impact on learning about other cues with which they are competing. Associative memory for the pseudo-blocking cues A–F that had either a strong or weak prior association with allergy was overall quite strong. We ran the same set of contrasts for the pseudo-blocking cues, as for the moderate cues of interest. On average, learning scores for highly allergenic foods (A/B/C/D; $M = 63.30$, $SE = 3.34$) did not differ significantly from those for low allergenic foods (E/F; $M = 62.49$, $SE = 4.23$), $t(122) = 0.190$, $p = 0.849$, $\eta_p^2 < 0.001$, $BF_{01} = 9.85$ in favor of the null. This was true regardless of whether or not participants were instructed to use or ignore this information as there was no significant interaction with instruction group, $t(122) = 1.15$, $p = 0.252$, $\eta_p^2 = 0.011$, $BF_{01} = 2.89$ (mean difference high – low was −4.09 ($SE = 5.79$) in the ignore group and 5.71 ($SE = 6.23$) in the use group). Similarly, there was no main effect of consistency on memory for the high allergenic foods, $t(122) = 0.17$, $p = 0.865$, $\eta_p^2 < 0.001$, $BF_{01} = 2.59$ (high consistent $M = 67.76$, $SE = 4.09$; high inconsistent $M = 58.85$, $SE = 4.44$) and no significant interaction with instruction group, $t(122) = 1.51$, $p = 0.135$, $\eta_p^2 = 0.018$, $BF_{01} = 1.88$ (the mean difference consistent – inconsistent was 0.94 ($SE = 6.99$) and 16.89 ($SE = 7.96$) for the ignore and use groups, respectively).

Causal Ratings

Causal ratings for the critical cues are illustrated in **Figure 3**. Causal ratings were analyzed in the same way as learning scores. Consistent with the learning scores, there was evidence of cue competition with causal ratings for the moderate cues paired with low allergenic foods being significantly higher on average than those paired with high allergenic foods (high vs. low), $t(122) = 3.33$, $p = 0.001$, $\eta_p^2 = 0.083$, $BF_{10} = 15$. However, the magnitude of pseudo-blocking was significantly larger following instructions to use prior knowledge than to ignore it, $t(122) = 2.35$, $p = 0.021$, $\eta_p^2 = 0.043$, $BF_{10} = 2.25$. Bayesian *t*-tests for each instruction group confirmed that participants instructed to use their prior knowledge gave significantly higher causal ratings to moderate cues that were paired with low allergenic foods (X/Y) than with high allergenic foods (R/U/S/V), $BF_{10} = 285$, $t(61) = 4.25$, $p < 0.001$, $d = 0.54$. Whereas those instructed to ignore their prior knowledge causal ratings for cues paired with high allergenic foods did not differ significantly from those for cues paired with low allergenic foods, $BF_{01} = 5.843$ in favor of the null, $t(61) = 0.66$, $p = 0.513$, $d = 0.084$. The second contrast, comparing ratings for moderate

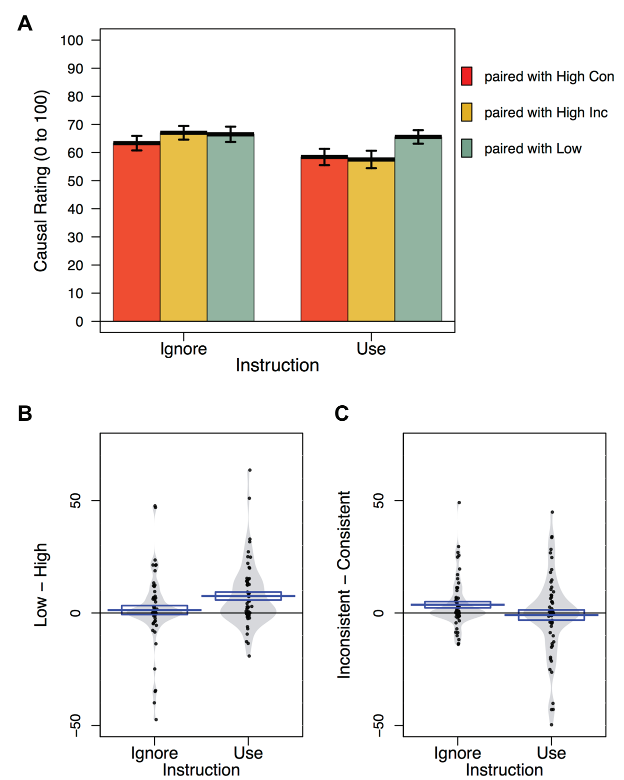

FIGURE 3 | Causal ratings as a function of cue type and instruction. **(A)** Mean, and standard error of causal ratings for each cue type as a function of instruction group. **(B)** Violin plots showing the distribution of causal rating differences between moderate cues paired with low and high allergenic cues for each instruction group. **(C)** Violin plots showing the distribution of differences between causal ratings for cues paired with high consistent and high inconsistent foods. The box around mean line represents the standard error of the mean.

cues paired with high consistent with high inconsistent competitors did not reach significance, $t(122) = 1.06$, $p = 0.291$, $\eta_p^2 = 0.009$, $BF_{01} = 5.84$ in favor of the null, nor was there a significant interaction with group, $t(122) = 1.72$, $p = 0.088$, $\eta_p^2 = 0.024$, $BF_{01} = 1.38$ in favor of the null.

For the pseudo-blocking cues A–F, we found that the high allergenic foods (A/B/C/D; $M = 69.18$, $SE = 1.59$) were on average given significantly higher causal ratings than the low allergenic foods (E/F; $M = 61.84$, $SE = 1.89$), $t(122) = 4.23$, $p < 0.001$, $\eta_p^2 = 0.128$, $BF_{10} = 337$. However, there was no significant interaction with instruction group ($t(122) = 1.46$, $p = 0.146$, $\eta_p^2 = 0.017$, $BF_{01} = 1.99$), indicating that even when participants were instructed to ignore their prior knowledge low allergenic cues were considered to be less likely to be causing an allergic reaction than the high allergenic cues [MD (high – low) = 4.80; $SE = 1.71$ in group ignore; $MD = 9.88$, $SE = 3.02$ in group use]. Comparing the high allergenic cues on consistency of outcome, high consistent cues (A/C; $M = 60.87$, $SE = 1.95$) were given higher mean causal ratings than high inconsistent cues (B/D; $M = 62.28$, $SE = 2.01$), $t(122) = 3.01$, $p = 0.003$, $\eta_p^2 = 0.069$, $BF_{10} = 7.37$. This was not affected by the critical instructions, $t(122) = 0.79$, $p = 0.433$, $\eta_p^2 = 0.005$, $BF_{01} = 3.94$ [MD (consistent – inconsistent) = 3.15, $SE = 1.61$ in group ignore, and $MD = 5.38$, $SE = 2.33$ in group use].

Post-experimental Questionnaire

Results from the post-experimental questionnaire assessing the strength of association with allergy for each food at the beginning of the experiment were consistent with those drawn from the survey used to create the design. **Figure 4** shows the mean association with allergy for each food item in the experiment as a function of instruction group. The ratings for each cue were collapsed across categories identified in the initial survey (high, moderate, and low) and subjected to a cue category by instruction group ANOVA. There was a significant main effect of cue category, $F(1,366) = 366.57$, $p < 0.001$, that did not interact with instruction group, $F < 1$.

DISCUSSION

This study tested the assumption that participants can successfully follow instructions to set aside existing beliefs or knowledge about causal relationships and learn new cue-outcome relationships in an unbiased way. Using a cue competition design in the allergist task, we demonstrated a pseudo-blocking effect in both associative recall and causal judgments. That is, foods commonly held to be highly associated with allergies produced less prediction error and, therefore, greater overshadowing, than foods rarely associated with allergies. Critically however, causal judgments reflected some level of instructed control over this process. Those explicitly instructed to ignore rather than use their prior knowledge of food allergies when completing the task showed no evidence of differential competition for high and low allergenic foods in their judgments of causality. This was not true however of associative recall, high allergenic cues produced greater overshadowing than low allergenic cues regardless of whether or not they were instructed to take their prior knowledge into consideration when completing the task.

It is not uncommon in causal learning research to manipulate instructions in an attempt to control the relevance of previous information to the current situation. Sometimes this takes the form of a standalone manipulation aimed at encouraging participants to attend or attribute causation to cues in a different way to what they have learned previously (e.g., Mitchell and Lovibond, 2002; Mitchell et al., 2012; Don and Livesey, 2015; Shone et al., 2015; López et al., 2016). In other experiments, such instructions complement demonstrations made *via* cue-outcome pre-training trials (e.g., Lovibond et al., 2003; Livesey and Boakes, 2004; Beckers et al., 2005a). Many of these results have shown that people's causal judgments are shifted in rational ways by such instructions. Here, we demonstrated the same general sensitivity, at least for causal ratings. When judging the ambiguous causal connections between food cues and allergic reaction outcomes, people told to ignore what they knew about food allergies treated the cues in this task as if they were all equally likely to be allergenic.

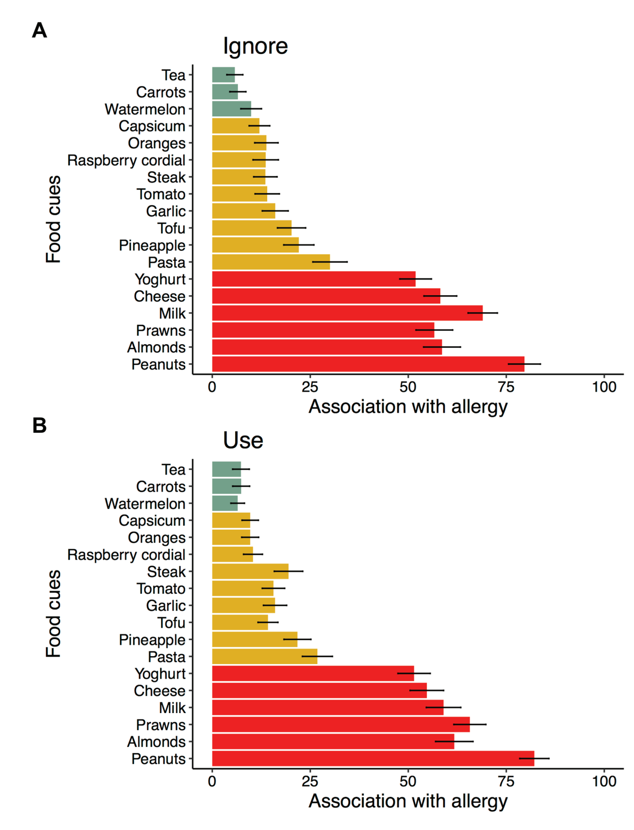

FIGURE 4 | Mean ratings of association with allergy from the post-experiment questionnaire for those instructed to ignore **(A)** or use **(B)** their knowledge of food allergies during the allergist task. Colors represent the categories low (green), moderate (yellow), and high (red) allergenic as identified from the previous survey of a separate sample of participants. Error bars represent standard error of the mean.

However, the results also indicate that the strength of associative memory may be less affected by such instructions.

What does this mean? Thorwart and Livesey (2016) suggested that even if it was assumed that prediction-error driven association formation comprised a core memory system on which causal inferences might be based then there were at least three ways in which other types of knowledge (for instance, inferences and assumptions drawn from instructions) might impact on the learning that takes place within that associative system. Perhaps the simplest of these would be to assume that the instructions modify the way associative memories are translated to explicit decisions and judgments, without strongly influencing the way those memories are laid down in the first place. In other words, we could assume that the instruction to ignore prior knowledge leads participants to give a similar causal rating to all ambiguous cues but has little impact on the way participants learn about them. It should be noted that this is a possible explanation for many of the other demonstrations of instruction-based manipulations of learning.

If this were the case then we could hypothesize which aspects of memory encoding are resistant to being modified by relevant instructions, like the instruction to ignore prior knowledge. One possibility is that the encoding of the cues, their initial sampling and the distribution of selective attention are unaffected by these instructions. That is, highly allergenic foods might attract more attention than less allergenic foods, regardless of whether participants are told to use or ignore their prior knowledge. Another possibility is that predictions made during memory encoding (i.e., those relevant to prediction-error based learning) are resistant to this instruction manipulation. Predictions about allergic reaction outcomes might be automatically retrieved based on prior memories in a way that impacts competitive learning even when the participant has been told to ignore this information.

The lack of any strong difference between the consistent and inconsistent cue-outcome pairing conditions is more consistent with the first of these possibilities. In the inconsistent condition, prior knowledge of the outcomes typically associated with highly allergenic foods would lead to the prediction of an incorrect outcome based on the highly allergenic food in the compound. Although the participant would be correct in anticipating an allergic outcome of any type, there would presumably still be greater prediction error in this condition. According to associative learning algorithms that assume a summed error term, this should drive stronger learning about the moderate cue presented in this compound. In contrast, if there were a persistent bias toward the highly allergenic cue during learning then this might impact learning of the moderate cue regardless of whether the outcome presented was consistent or inconsistent with the participant's prior beliefs. We tentatively suggest then that the results reflect a persistent bias in cue encoding in particular, though we cannot rule out the possibility that the instructions failed to influence other aspects of learning and memory also.

A selection bias toward the highly allergenic cues is consistent with an attentional account of cue competition effects like blocking (Mackintosh, 1975). There is empirical support this explanation of blocking, as blocked cues are slower to enter into new learning, consistent with a decrease in associability (Kruschke and Blair, 2000; Le Pelley et al., 2007). Further, there is evidence that these changes occur very rapidly. Luque et al. (2018) used a dot probe task to show that a blocking procedure produces very early shifts in attention away from the blocked cue consistent with a learned reduction in the perceived salience of the blocked cue. While we do not measure attention directly here, our data are certainly compatible with such an account if prior knowledge can increase cue salience in a similar manner to pretraining. A corollary of the idea that cue competition entails very rapid and perhaps automatic changes in attention is that these selection biases are likely to be somewhat resistant to control by instructions. While there is some evidence that learned attentional biases are under voluntary control as they can be completely reversed by instructions alone (Mitchell et al., 2012), partial resistance to instructed changes in attention have been documented in other contexts involving causal learning (e.g., Don and Livesey, 2015; Shone et al., 2015). Perhaps the most convincing evidence of such resistance comes from a recent study by Cobos et al. (2018), in which they demonstrated that the selection history of a cue, its previous predictive value, produces a very rapid attentional shift that is resistant to instructed control. However, it should be noted that even in these studies there was still substantial evidence that instructions modified both causal ratings and associative memory ratings.

The observed dissociation between associative memory and causal judgments raises another question about the relationship between these judgments. As noted earlier, there are clear parallels between associative and causal learning, both are subject to cue competition, for instance, that have led some to conclude that the same processes inform both learning and causal judgments. However, it is clear that associative models cannot provide a full account of human causal judgments and the evidence here is consistent with previous findings that causal judgments are sensitive to factors that fall outside the scope of associative models of learning (Waldmann and Holyoak, 1992; Waldmann, 2001; Beckers et al., 2005b). However, this does not preclude the notion that the strength of associative memory may serve as a useful heuristic for establishing causality in the absence of other information (Thorwart and Livesey, 2016; Le Pelley et al., 2017).

Cues were always trained in a compound of two cues such that, in the absence of any prior knowledge about the allergenic properties of cues, the causal status of *all* cues (aside from those never paired with an allergic reaction) was ambiguous. Thus, even if associative memory was weaker for those moderate cues paired with highly allergenic competitors, in reflecting upon the instruction to ignore their prior knowledge, participants could infer that all cues have the same relationship with the outcome and adjust their causal ratings accordingly. That is, causal ratings could be based on the strength of associative recall, as they appeared to be when participants were told to use prior knowledge, but participants may also be able to reflect upon the validity of this evidence when making these judgments at test. It appears that prior knowledge of causal or predictive relationships, regardless of how it is acquired, influences competition for associative memory and that this process is resistant to control by instructions. The question is whether this is also true of previous learning. In other words, are direct experiences of cue-outcome relationships gained within the same experimental context similarly resistant to instructed control, and if so, does this affect associative memory and causal judgments equally? Future research could address this question by using a combination of pretraining and instructions.

In summary, we have demonstrated that associative memory is relatively insensitive to instructions manipulating the relevance of prior knowledge. We observed that while participants could successfully set aside their prior knowledge when making causal judgments about the relationships in the task, instructions to ignore prior knowledge did not affect competition for associative memory within the task. That is, prevailing beliefs about food allergies affected learning, perhaps by influencing selective attention to cues, even when participants were told to disregard such knowledge, and clearly successfully achieved this when judging causal relationships. These findings imply that instructing participants to ignore prior knowledge in a familiar scenario may not be entirely effective, learning is biased by prior knowledge even when given instructions to disregard it, thus researchers must consider the impact of pre-existing or commonly held beliefs on the relationships in the task.

DATA AVAILABILITY STATEMENT

The datasets presented in this study can be found in the Open Science Framework at: https://osf.io/t68rk/.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the Human Research Ethics Committee of the University of Sydney. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

JG and EL contributed to the conception and design of the study and the drafting and revising of the manuscript. JG collected and analyzed the data. Both the authors contributed to the article and approved the submitted version.

SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2020.578775/full#supplementary-material

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Overconfidence in Understanding of How Electronic Gaming Machines Work Is Related to Positive Attitudes

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Previous research has demonstrated that attitudes are a primary determinant of intention to gamble on electronic gaming machines (EGMs) consistent with the Theory of Reasoned Action. This paper aims to address how biases in judgment can contribute to attitudes and subsequently behavior, including maladaptive problematic gambling behavior. We take a novel approach by viewing overconfidence in one's understanding of how outcomes are determined on EGMs as an indication of cognitive distortions. The novelty of this paper is further increased as we compare attitudes to existing EGMs with novel EGMs which include a skill component, referred to as skill-based gaming machines (SGMs), which enables a better controlled comparison between actual and perceived skill. In Study 1, 232 US-based participants were recruited online who were shown various slot machines and SGMs and asked a series of questions about perceived skill and chance in determining outcomes to assess their understanding, then were asked their confidence in their understanding, attitudes toward the machines and they completed the Problem Gambling Severity Index. In Study 2, 246 Australian participants were recruited through community and university student samples; they attended a laboratory where they were randomly allocated to play a real EGM or SGM without money and completed the same measures as in Study 1. In Study 2, participants were randomly told that the outcomes on the machine they would play were determined entirely by chance, skill, or a mixture of both. In both studies, our findings suggest that there are more extreme values in overconfidence in how EGMs work, whereas individuals are more similar in their confidence in understanding SGMs. We also find a relationship between overconfidence in EGM understanding and positive attitudes toward EGMs, but no such relationship with SGMs. There was no impact from controlling for demographics, problem gambling severity, or labeling of machines on these relationships.

Keywords: electronic gaming machines, skill, attitudes, illusions of control, erroneous beliefs, cognitive distortions, misunderstanding, gambling

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INTRODUCTION

Gambling is a popular activity internationally, with past-year participation rates varying from 58 to 83% (Dowling et al., 2016; Castrén et al., 2018; Salonen et al., 2018; Volberg et al., 2018). Research often focuses on what contributes to excessive gambling, but there is a significant research gap to inform why people gamble, including uptake of novel gambling activities. There is evidence to support the Theory of Reasoned Action (TRA, Fishbein and Ajzen, 1975) to explain intentions to gamble, including on electronic gaming machines (EGMs), i.e., slots, pokies, fixed odds betting terminals (Thrasher et al., 2011; Lee, 2013; Shin and Montalto, 2015; Gainsbury et al., 2019a). According to the TRA, intent to gamble is predicated on both positive attitudes toward the activity and perceived social norms. However, there is an absence of research to understand what contributes to positive attitudes toward various gambling activities.

Despite policies mandating gambling product information disclosure, erroneous beliefs about gambling and erroneous understandings of how outcomes are determined are widely held by gambling consumers (Goodie et al., 2019). Due to the potential for substantial money spending in the case of gambling, erroneous beliefs about gambling may lead to serious harms (Goodie et al., 2019).

One of the empirical challenges in understanding the link between electronic gaming machines (EGMs), erroneous beliefs, and attitudes toward gambling, relates to the role of user actions in generating the random outcomes. A new gambling product has been developed, similar to EGMs but with notable differences in how outcomes are determined through the inclusion of skill-based outcomes, which are referred to here as skill gaming machines (SGMs). This development provides an opportunity to examine the relationship between attitudes toward a gambling product and erroneous understanding of how outcomes are determined, in the form of overconfidence in understanding of the machines. This paper considers the extent to which consumers understand the newer SGMs in comparison to EGMs, the impact of labels explaining how outcomes are determined, and the role of actual and subjective understanding of the differing machines on positive product attitudes.

LITERATURE REVIEW

Gambling-related erroneous beliefs tend to be based on a misunderstanding of how outcomes are determined and can impact gambling, and can contribute to the development of gambling problems (Goodie et al., 2019). Overestimation of control over outcomes strengthens individuals' win expectations, leading players to place higher bets and persist in betting (Kwak, 2016). Distorted cognitions can moderate the relationship between risky gambling practices and spending (Miller and Currie, 2008) and erroneous beliefs are an independent predictor of problem gambling severity (MacKay and Hodgins, 2012). Illusions of control and other erroneous beliefs, such as a belief in luck, may be more common in relation to gambling activities that include a skill element or structural characteristics that encourage the perception of skill. Individuals who prefer gambling activities that contain elements of skill have a greater illusion of control over outcomes (Myrseth et al., 2010). A study of online poker players found those who did not overestimate their skill were more successful at avoiding developing gambling problems (Griffiths et al., 2010), suggesting that avoiding cognitive distortions may be protective against problematic gambling. Given the potential influence of erroneous beliefs on behavior, it is important to identify the relationship between these and attitudes toward gambling activities.

One limitation of erroneous belief measures is that they generally attempt to estimate fallacies across multiple activities or the entire set of gambling activities (Goodie and Fortune, 2013; Goodie et al., 2019). These measures provide limited insight as to how attitudes toward an individual game or game type are shaped, which is an important consideration for policy and lower-risk game design. While there are likely to be global effects of cognitive distortions that impact individuals behavior across all gambling variants, we contend that attitudes toward any particular game can be explained, in part, by the degree of overconfidence that the individual has toward that game, in their ability to control outcomes. There is minimal research on the role of overconfidence in how outcomes are determined in the gambling field. One study found that individuals with gambling problems are more likely to be overconfident in tasks involving skills and were more likely to wager that they were correct in their performance of skilled activities in a simulated gambling task compared to individuals without gambling problems (Goodie, 2005). In this paper we take a novel approach in viewing overconfidence as a potential cognitive distortion; if an individual has a poor understanding, but their subjective assessment of their understanding matches that uncertainty, then there are no distortions. We expect individuals could accurately calibrate their risk taking, based on their matched understanding and subjective uncertainty.

EGMs are of central interest to gambling regulators and researchers, given their propensity to be related to gambling problems (Delfabbro et al., 2020b). EGMs are randomly determined, however, the products often include redundant features that reinforce an illusion of control, such as bonus rounds or stop buttons, which appear to tie user actions to outcomes (Harrigan et al., 2014; Gainsbury et al., 2018). Until recently, there were no suitable control devices that facilitated a comparison to the false appearance of skill in EGMs. However, SGMs have been developed which incorporate skill elements into the randomly determined payout schedules of EGMs (for a review please see Delfabbro et al., 2020a; Pickering et al., 2020). These machines allow for player skill to impact the long-run house advantage such that all players have the possibility of winning, including jackpots, but players with higher levels of skill increase their likelihood of winning small to medium monetary payouts.

This is similar to other gambling activities, for example casino-based poker or blackjack, whereby greater skill provides the player with an advantage such that skill players are more likely to win than unskilled players, but the house retains an advantage to ensure that most players will lose in the long-run. Unlike poker and blackjack, the skill mechanics within SGMs are modeled on video and mobile games (which are not classified as gambling as they do not provide monetary payouts) and may include pattern matching, fighter, or sports-based player actions. SGMs are often physically different from EGMs and include touchscreen or video-game-style controllers and considerable interactivity and decision-making. Several, predominantly US, jurisdictions have enacted legislation permitting SGMs and other jurisdictions are permitting SGMs within existing EGM regulation frameworks. However, policy makers and stakeholders have expressed concerns regarding the extent to which SGMs may exacerbate gambling harms (Hoskins and Hoskins, 2019). One concern is that consumers may not understand the extent to which skill impacts SGM outcomes in relation to chance and that subsequently, consumers will develop erroneous beliefs which may increase risky gambling including excessive play and chasing losses and lead to harms.

There is limited empirical research on SGMs related to their impact or the extent to which players understand these devices. The current paper builds on published results by the authors, which found that after reading a description of SGMs and viewing a brief video of examples, participants recruited from a US-based online sample understood that SGMs involved more skill than EGMs, but they were not confident that they understood how the machines worked (Gainsbury et al., 2020). Further, compared to participants with no prior SGM experience, participants who had experience gambling on SGMs had a poorer understanding of how outcomes were determined for EGMs but not SGMs. They also had higher rates of gambling-related cognitive distortions in general and higher problem gambling severity scores. This indicates that experience playing SGMs did not significantly enhance understanding of SGMs and that individuals already involved in gambling and with existing cognitive distortions and gambling problems are likely to play these new products. Similar results were found in a related study of casino patrons who played a SGM (Gainsbury et al., 2019b). Following play, participants did not have a good understanding of how outcomes were determined for SGMs, and this did not differ based on prior use of SGMs in casinos. Participants with greater gambling-related erroneous beliefs, including illusions of control, and gambling problems were more likely to have played and report interest in playing SGMs.

Many harm prevention strategies involve the provision of information intended to educate consumers about game play for example through messages on products, or signs and brochures in venues. These strategies persist despite repeated research showing that knowledge of gambling odds and information about gambling is unlikely to impact gambling behaviors (Monaghan and Blaszczynski, 2009; Parke et al., 2014; Ginley et al., 2017). A common legislative requirement across jurisdictions that were early adopters in permitting skill-based gambling, is clear labeling of SGMs as containing skill-elements (Larche et al., 2016). However, given the tendency for EGM play information to fail to influence behavior, it is possible that similar signage for SGMs is ineffective in impacting cognitions.

The TRA is a well-established social cognitive model which posits that behavior is determined by an individual's intent to perform that behavior, which is in turn predicted by attitudes toward the behavior and perceived social norms toward the behavior (i.e., a perception of how others perceive the behavior). The TRA has been previously applied to gambling with evidence supporting this theoretical framework (Cummings and Corney, 1987; Moore and Ohtsuka, 1997; Thrasher et al., 2011; Lee, 2013; Shin and Montalto, 2015). Preliminary evidence supports the TRA as an explanatory model for understanding intent to gamble on EGMs and SGMs which found more positive attitudes and stronger subjective norms predicted a stronger intention to gamble, and this finding was stronger for SGMs than EGMs (Gainsbury et al., 2019a). The current investigation uses the data from this study and aims to extend the published results by examining an antecedent of attitudes (Study 1) and addresses the MTurk study data limitations with a subsequent lab-based study (Study 2). As the TRA is a relevant conceptual model to understand gambling behavioral intention and subsequent action it is important to identify what factors influence the sub-components, including personal attitudes toward specific gambling activities and products, given the role of these in determining behavior.

Current Study

The current research aimed to examine the role of overconfidence in EGM and SGM game understanding, testing whether overconfidence predicted attitudes toward the game types, and testing whether simple labels providing information about how outcomes are determined will impact this relationship. Study One presents further analyses from the previously mentioned US-online dataset (Gainsbury et al., 2020) and Study Two presents analyses from a subsequent Australian lab-based study, which aimed to overcome some of the limitations in Study One. Specifically, compared to Study One, Study Two recruited a broader sample from a different geographic location with no previous exposure to SGMs and provided participants with a notice of how outcomes were determined. Study Two randomly allocated participants to either play and respond regarding SGMs or EGMs, unlike Study One, which asked about both types of machines without random allocation. The outcomes of this research make an important Contribution To The Field by advancing understanding of whether cognitive distortions measured by overconfidence in an individual's understanding of how outcomes are determined, predicts attitudes toward EGMs and newly introduced SGMs, which introduce an element of skill to EGMs. The implications of this research are relevant for policy makers in determining whether interventions which enhance accuracy of understanding how outcomes are determined for existing and novel gambling activities are an appropriate harm minimization measure.

We hypothesize that game understanding is important and systematically misunderstood by EGM players. In SGMs, we expect similar distributions of game understanding but given that the skill component is more obvious, we expect individuals to project their subjective skill level more correctly.

STUDY 1

Study 1 was a secondary analysis of an observational study administered through an online survey. The study included several questions about the nature of skill and chance in SGMs and EGMs, along with self-reported measures. Ethics clearance for Study 1 was provided by University of Sydney Human Research Ethics Committee 2017-890. The study was not pre-registered, but the measures used and data analyzed are freely available at: https://osf.io/utf9z/.

Materials and Methods

Participants

A sample was recruited in November 2017 using Amazon Mechanical Turk (MTurk), an online platform for tasks. Participants were restricted to legal gambling age (21 years of age or older) individuals with an MTurk approval rating of at least 95% (Goodman et al., 2013), who speak English, and who live in North America. Respondents must also had resided in or visited a jurisdiction that contained the games shown in the study. A total of 232 respondents were recruited and 48 were removed from analysis due to failing at least one of two attention checks or not completing the survey.

Procedure

Participants were shown brief videos[1] within the survey depicting various SGMs and EGMs and were asked a series of questions about perceived skill and chance in gambling and non-gambling games.

Measures

Game Understanding

Respondents were asked similar, but separate, game understanding questions about EGMs and SGMs. Consistent with regional terminology, EGMs were described as "slot machines." The questions were then scored differently to reflect accuracy of the responses (measured game understanding). Items were coded on a five-point Likert scale (1, *strongly disagree*; 5, *strongly agree*) and a summative accuracy score from 4 to 20 was computed. The questions were:

- *A player of greater skill is more likely to win money on (slot machines/skill-based gambling machines) over 1 h of play, compared to a player of lesser skill.* (Reverse scored for slot machines).
- *Over the long term, all players will lose money on (slot machines/skill-based gambling machines).*
- *The outcomes of (slot machines/skill-based gambling machines) are random no matter what a player does.* (Reverse scored for SGMs).
- *With practice a player can improve their outcomes on (slot machines/skill-based gambling machines) over time.* (Reverse scored for slot machines).

Across all individuals, the difference in average scores for SGM understanding (M = 15.27, SD = 2.22) and slot machine understanding (M = 16.04, SD = 3.43) was small but statistically significant using Welch's unequal variances t-test, $t(314) = -2.54$, $p = 0.01$.

Self-Reported Game Understanding

Self-reported game understanding was coded on a five-point Likert scale (1, *strongly disagree*; 5, *strongly agree*). The question was:

- *I understand how a player's skill impacts the outcomes of (slot machines/skill-based gambling machines).*

The difference in average scores for self-reported SGM understanding (M = 3.99, SD = 0.93) and self-reported slot understanding (M = 2.91, SD = 1.42) was over one unit on the scale and statistically significant using Welch's unequal variances t-test, $t(315) = 8.64$, $p < 0.001$.

Game Attitudes

Three items assessed the appeal, excitement, and enjoyableness of *(slot machines/skill-based gambling machines)*. These measures have previously been used to assess attitudes and were found to be predictive of future intent to gamble on machines (Gainsbury et al., 2019a). Items were assessed on a 5-point Likert scale (1, very unenjoyable; 5, very enjoyable). Higher scores indicate a more positive attitude. The items showed adequate consistency using Cronbach's alpha (α) in slot machines, $\alpha = 0.92$, and SGMs, $\alpha = 0.90$.

Problem Gambling Severity Index

Respondents were asked questions from the problem gambling severity index (PGSI) (Ferris and Wynne, 2001), as gambling problems are potentially related to both attitudes and overconfidence. We score the index using categories suggested by Currie et al. (2013), as these are the points that have been found as valid for inference. The sample classified participants into one of four groups: non-problem/non-gambling (n = 83; 45.11%), low-risk gambling (n = 48; 26.09%), moderate-risk gambling (n = 13; 7.07%), and problem gambling (n = 40; 21.74%).

Demographic Variables

Respondents were asked to provide their age (M = 34.02, SD = 9.29), gender (32.07% female, 67.93% male), employment status (full-time = 78.26%, part-time = 9.24%, unemployed = 4.89%, other = 7.61%), and household income band (median band = USD 50,000–70,000).

Analysis

To test for the relation between overconfidence and attitudes toward the electronic gaming machines, we regress measures of overconfidence onto the attitude factor variables in a series of ordinary least square models with sensitivity analysis to demonstrate the robustness of the results (Leamer, 1985). Our sensitivity analysis procedure includes first estimating a simple regression model, then estimating a second model that includes potentially confounding PGSI categories, and finally a fully specified model that adds demographic controls.

[1] Available upon reasonable request to the author.

Overconfidence Measures

Measures of overconfidence were computed as the difference between standardized measured game understanding and standardized self-reported game understanding, for both game variants, respectively. That is,

$$Over\ confident_i^j = (Self\ \ reported\ unders\tan ding_i^j - measuredunders\tan ding_i^j) \quad (1)$$

Where, the respondent is denoted by *"i"* and the activity (Slots or SGMs) is denoted by *"j."* Intuitively, we measure whether respondents self-reported level of game understanding is near our measured level, with higher values denoting greater overconfidence and thus greater misunderstanding of the nature of the game. Prior to computing overconfidence, we standardize our measures of understanding with z-scores, to place them on similar scales.

Overconfidence can be viewed as an overestimation of understanding of how games work or an overestimation of control over outcomes[2]. As our measures of understanding do not distinguish between these concepts, we treat our overconfidence variable as a more general measure of these specific phenomena. **Table 1** includes summary statistics of the attitude, overconfidence, and standardized understanding variables.

Figure 1 illustrates the Kernel density plots of understanding and overconfidence variables. SGM understanding appears relatively normally distributed, but there is left-skewness in the distribution of slot machine understanding. This skewness contributes to a fat-tail in the distribution of overconfidence. A larger share of respondents have a high level of overconfidence about their understanding of slot machines, as compared to SGMs.

RESULTS

We found evidence suggesting that overconfidence is positively related to attitudes toward slots. As shown in **Table 2**, computed estimates suggest that each unit in the slot overconfidence scale is related to a 1.28–1.67 increase in the slot attitude factor scores, depending on the model specification. We do not find a significant relation of SGM overconfidence to SGM attitudes. These findings are robust to inclusion of control variables for PGSI categories, age categories, gender, employment status, and household income. We also find that individuals in the low and moderate PGSI categories have more positive attitudes toward slots and SGMs than gamblers in the non-problem category. The relationship is less robust for individuals in the PGSI problem category, where we do not find a significant effect in either of our fully specified models.

[2]We thank our editor for this insight.

STUDY 2

Study 1 demonstrated a different relation between EGM and SGM overconfidence and player attitudes, but there may have been confounds from comparisons made by respondents, as they were asked to rate both machines simultaneously. It is also unclear if the media fully illustrated the user experience on these gaming machines. To address these deficiencies, Study 2 was a laboratory study, involving play by subjects on actual SGMs and EGMs in a demonstration mode. The study included several questions about the nature of skill and chance in SGMs and EGMs, but we address the potential confounding issues in Study 1 by randomly assigning subjects to either SGMs or EGMs. Ethics clearance for study 2 was provided by the University of Sydney Human Research Ethics Committee (2010-738).

Given the propensity for individuals to hold cognitive biases, our pre-registered hypothesis was that participants who played machines with skill labels will have a less accurate understanding of the outcomes of machines than those who played machines labeled as determined by chance or with no label at all. As there is minimal literature to guide expectations, our pre-registered exploratory analysis was to investigate the impact of framing on understanding of SGM/EGMs and irrational beliefs. Finally, we explored whether labeling impacted variables that may be indicative of gambling-related harm, including future intent to play, immersion, craving, perceptions of skill vs. chance and irrational beliefs. In addition, we conducted a non-pre-registered exploration of the impact of mis-labeling machines on erroneous beliefs and game understanding. The pre-registration details, measures, and data are available at https://osf.io/ba5n2/.

Materials and Methods

Participants

The study aggregated subjects sampled from three target groups within the Australian population: (i) young adults (aged 18–39 years), since this population is a potential target audience for SGMs (Delfabbro et al., 2020a; Pickering et al., 2020) and have relatively high rates of gambling and gaming-related problems (Gainsbury et al., 2014; Welte et al., 2015); (ii) regular EGM users, since such individuals would likely encounter SGMs if they were made available in gambling venues; and (iii) community members, since it is important to understand the potential appeal and impact of SGMs on individuals who may not regularly attend licensed gambling venues but may be interested in skill-related games. The use of multiple sampling strategies improves our study validity, as convenience samples like college students may not generalize well when attitudinal variables are used (Hanel and Vione, 2016).

To participate, respondents had to be at least 18 years of age, an Australian resident, and fluent in English. Young adults were recruited via an online research participant recruitment platform hosted by the (University redacted). This platform allows students to sign up to participate in research studies as part of a voluntary research participation assessment component in exchange for course credit. Students outside of the research participation assessment scheme can also sign up to participate in

TABLE 1 | Summary Statistics.

	Count	Mean	SD	Min	Max
Self-reported slot understanding (z-score)	184	0.000	1.000	−1.344	1.466
Self-reported SGM understanding (z-score)	184	0.000	1.000	−3.233	1.086
Measured slot understanding (z-score)	184	0.000	1.000	−2.049	1.154
Measured SGM understanding (z-score)	184	0.000	1.000	−3.729	2.132
Slot attitude factor	184	0.000	0.947	−2.524	1.200
SGM attitude factor	184	0.000	0.926	−3.860	1.006
Slot overconfidence	184	0.000	1.658	−2.498	3.225
SGM overconfidence	184	0.000	1.010	−3.834	3.462

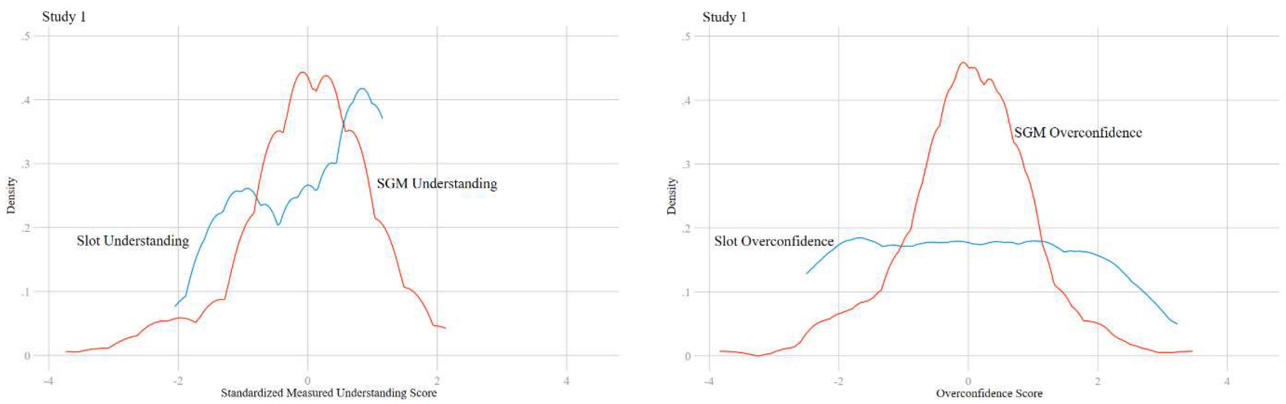

FIGURE 1 | Kernel density plots of measured understanding and overconfidence scores by machine type for study 1. The distribution of overconfidence shows large fat-tail effects in slot machines but not SGMs.

TABLE 2 | Ordinary least squares regression of overconfidence variables onto attitude factor variables (online survey).

	Slots	Slots	Slots	SGMs	SGMs	SGMs
Slots-	0.167***	0.128*	0.132*			
Overconfidence	(0.041)	(0.053)	(0.059)			
SGM-				−0.019	0.018	0.034
Overconfidence				(0.088)	(0.089)	(0.091)
PGSI low		0.442*	0.387*		0.556***	0.524**
		(0.170)	(0.181)		(0.154)	(0.164)
PGSI moderate		0.866***	0.728***		0.765***	0.739**
		(0.186)	(0.201)		(0.189)	(0.223)
PGSI problem		0.407*	0.362		0.060	0.002
		(0.160)	(0.202)		(0.163)	(0.214)
Constant	0.000	−0.265*	−0.202	−0.000	−0.212	−0.228
	(0.067)	(0.113)	(0.223)	(0.068)	(0.127)	(0.236)
Age categories	No	No	Yes	No	No	Yes
Gender	No	No	Yes	No	No	Yes
Employment	No	No	Yes	No	No	Yes
Household income	No	No	Yes	No	No	Yes
N	184	184	184	184	184	184
R^2	0.086	0.159	0.252	0.000	0.090	0.204

$^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Heteroskedasticity robust standard errors shown in brackets. PGSI measures are binary variables representing categories in the problem gambling severity index, with non-problem/non-gambling excluded as the base category.

studies and are offered a monetary reimbursement for their time. Regular EGM users were recruited by distributing 500 leaflets in a local gambling venue and by posting a recruitment notice in an e-newsletter distributed to club members. Additional participants who reported playing EGMs at least monthly were recruited through a recruitment agency. Community members were recruited through word-of-mouth and social media posts, and via a recruitment agency. A total of 133 student respondents and 113 community members were recruited in November/December 2019, for an aggregate sample of 246 individuals.

Procedures

Upon arrival, consenting participants completed *pre-test* questionnaires using tablet devices. Once all participants had completed the *pre-test* questionnaire, they were taken by a researcher to a room housing three EGMs and three SGMs. Up to six participants were included in each session. Machines were pre-loaded with credit. The researcher instructed each participant to sit at the specific gaming machine corresponding to the experimental condition to which they had been randomly assigned.

Each machine additionally had a labeling condition to which participants were randomly assigned. The appearance of a machine as having a skill component through design elements, such as a video game style controller, may have an impact on the individual's thoughts and behaviors. To control for the effects of framing gaming machine outcomes as being influenced by skill or chance, the description of the machines provided to participants in the *pre-test* questionnaire differed across three categories: (i) "outcomes on the machine you are going to play are determined by a mix of skill and chance" ("skill label"); (ii) "outcomes on the machine you are going to play are determined completely by chance" ("chance label"); or (iii) no reference was made to the role of skill or chance ("no label"). We summarize the conditions in **Table 3**, and use these labeling conditions as additional explanatory variables.

All machines were set to "demo mode" such that no real money was involved in playing the machines, however, the machines operated as they would if they were in a licensed venue. The EGM was a standard reel-based game which included bonus rounds in which participants were shown a deck of cards and could select "red/black" for the next card to be drawn to win an additional prize. The SGM had two play components, the "chance" component consisted of reel-spins as in regular EGMs and the "skill" component was a bonus feature in which participants entered a battle scene and used the video-game controller to fight monsters while acting as a Knight-style avatar[3]. No credits were bet in the skill gaming component, however, superior performance in this component may result in a "win" for participants and increase their credit total. Participants were instructed to play the machines for 20 min. Participants were then asked to complete the *post-test* questionnaire using tablet devices. Once all participants had completed the *post-test* questionnaire, the researcher provided a verbal debrief to ensure that participants understood the experimental protocol, and the role of skill and chance in determining outcomes in each machine type. Participants were able to ask any questions about their experience playing the machines. Participants were awarded course credit or offered a monetary reimbursement for their time.

[3]For a demonstration of the specific SGM used please see https://www.youtube.com/watch?v=9ga7UC6zL_s

TABLE 3 | Experimental conditions.

Machine type	Framing label: Skill label	Framing label: Chance label	Framing label: No label
EGM	Condition 1	Condition 2	Condition 3
SGM	Condition 4	Condition 5	Condition 6

Measures

A similar set of variables to those used in Study 1 appeared in the survey instrument from Study 2, including game understanding, self-reported game understanding, game attitudes, game intentions, and SGM/EGM overconfidence. Household income values were collected as Australian dollars, and age was collected as a non-categorical variable to allow for its use as a continuous regression input. We use two additional variable, "told skill," which is a dummy variable equal to "1" if their machine had a skill label and "0" otherwise; and "told chance," which is a dummy variable equal to "1" if their machine had a chance label and "0" otherwise. A total of 75 participants received the skill label and 93 received the chance label. The average age was 34.04 (SD = 17.32), gender (female = 56.91%, male = 42.68%, other = 0.41%), employment status (student = 39.43%, full-time = 24.39%, part-time = 22.36%, unemployed = 3.25%, other = 10.57%), and household income band (median band = AUD 65,000–77,999 per year).

Across all respondents, there were 105 non-gamblers/non-problem gamblers (41.50%), 87 low-risk gamblers (34.39%), 31 moderate-risk gamblers (12.25%), and 30 problem gamblers (11.86%). There were 140 females (56.91%), 105 males (42.68%), and 1 non-binary (0.41%) subjects. There were 115 respondents reporting as working in a paid job (46.75%), 97 as students (39.43%), and 34 in other employment circumstances (13.82%).

Between groups, the difference in average scores for SGM understanding (M = 12.40, SD = 2.92) and EGM understanding (M = 15.49, SD = 3.47) was significant, using Welch's unequal variances t-test, $t(237) = -7.55$, $p < 0.001$. Self-reported SGM understanding (M = 3.10, SD = 1.23) was higher than self-reported EGM understanding (M = 2.43, SD = 1.34) using Welch's unequal variances t-test, $t(243) = 4.10$, $p < 0.001$.

Table 4 summarizes non-categorical variables used in the study, including standardized versions of the understanding measures.

Figure 2 illustrates the Kernel density plots of the understanding variables, and the overconfidence variables. We observe that SGM understanding appears relatively normally distributed, but there is left-skewness in the distribution of EGM understanding. This skewness contributes to a fat-tail in the distribution of overconfidence. A larger share of respondents have a high level of overconfidence about their understanding of EGMs, as compared to SGMs.

Analysis

To test for the relationship between overconfidence and attitudes toward the gambling machines, we regress overconfidence onto the factor variables. We then fit multivariate models to assess the robustness of the findings, which includes

TABLE 4 | Summary statistics.

	Count	Mean	*SD*	Min	Max
Self-reported EGM understanding (z-score)	122	0.000	1.000	−1.074	1.920
Self-reported SGM understanding (z-score)	124	0.000	1.000	−1.713	1.543
Measured EGM understanding (z-score)	122	0.000	1.000	−3.024	1.299
Measured SGM understanding (z-score)	124	0.000	1.000	−2.192	2.600
EGM overconfidence	122	0.000	1.787	−2.373	4.656
SGM overconfidence	124	0.000	1.107	−3.287	3.050
EGM attitudes factor	122	0.000	1.000	−2.241	1.697
SGM attitudes factor	124	0.000	1.000	−1.938	1.862
Age	246	34.044	17.324	18	75

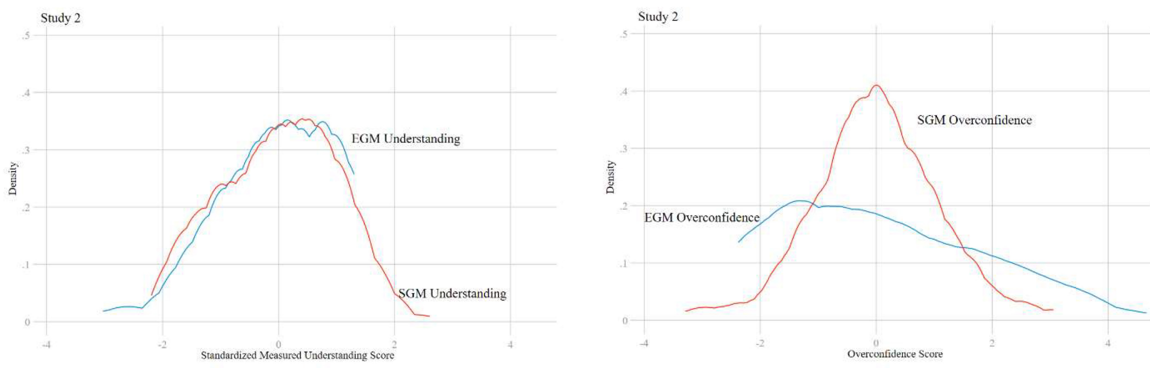

FIGURE 2 | Kernel density plots of measured understanding and overconfidence scores by machine type for study 2. The distribution of overconfidence shows large fat-tail effects in EGMs but not SGMs.

the mislabelled machine condition. Our sensitivity analysis procedure consolidates the PGSI and demographic control addition step used in Study 1 into a single model.

RESULTS

We find similar results as in Study 1. Overconfidence is related positively toward EGM attitudes but is unrelated to SGM attitudes. We produce a similar effect size as in Study 1. As shown in **Table 5**, our estimates of the EGM overconfidence scale imply a 0.172–0.190 increase in attitude factor scores, for each unit increase in overconfidence. Consistent with prior literature that shows a limited effect of messaging, we find no statistically significant impact of the chance or skill labeling conditions. We also find that individuals in the low and moderate PGSI categories have more positive attitudes toward EGMs and SGMs. In this study, we found individuals in the low and problem categories had significantly higher attitudes toward EGMs than individuals in the non-problem category. However, we found individuals in problem category had significantly lower attitudes toward SGMs than non-problem gamblers.

DISCUSSION

The advent of a new form of machine gambling, which involves a skill component, provided an opportunity to examine the role of game (mis)understanding in consumer attitudes toward EGMs. Our hypothesis was supported, as our results demonstrated that EGMs are systematically misunderstood by individuals, resulting in cognitive biases that relate strongly to attitudes. These findings highlight the importance of overconfidence as maladaptive thought that relates to positive affect toward EGMs. Results from both studies showed a non-normal distribution of overconfidence toward EGM understanding, and that as overconfidence increased, participant's positive attitudes toward playing EGMs also increased. This finding was robust and did not change in relation to personal characteristics, level of problem gambling severity, or whether participants were provided with accurate or inaccurate information about how EGMs worked. The same relationship was not found for SGMs, which may be related to the absence of a relationship or to a much lower frequency of influential extreme values than was observed in the case of EGMs.

The provision of accurate and inaccurate information about how outcomes were determined did not influence attitudes. As positive attitudes have been shown to predict intent to play EGMs (Gainsbury et al., 2019a), individuals who are overconfident that they understand EGMs may be more likely to play the devices. This may lead to negative outcomes as previous research shows inaccurate understanding of EGMs and erroneous beliefs is related to gambling problems (Goodie et al., 2019). The current research suggests that the erroneous beliefs may influence behavior due to their influence on attitudes, which is consistent

TABLE 5 | Ordinary least squares regression of overconfidence variables onto attitude factor variables (experiment).

	EGMs	EGMs	EGMs	SGMs	SGMs	SGMs
EGM-Overconfidence	0.190***	0.187***	0.172**			
	(0.041)	(0.043)	(0.053)			
SGM-Overconfidence				0.154	0.152	0.085
				(0.081)	(0.084)	(0.101)
Told chance		−0.012	−0.154		0.240	0.334
		(0.198)	(0.213)		(0.231)	(0.219)
Told skill		−0.083	−0.219		0.054	0.280
		(0.231)	(0.240)		(0.202)	(0.205)
PGSI low			0.579**			0.151
			(0.200)			(0.198)
PGSI moderate			0.444			0.118
			(0.326)			(0.303)
PGSI problem			0.729*			−0.876**
			(0.312)			(0.326)
Age			0.015			−0.018*
			(0.008)			(0.008)
Constant	−0.000	0.030	−0.518	0.000	−0.099	−0.938
	(0.085)	(0.150)	(0.556)	(0.089)	(0.162)	(0.742)
Gender	No	No	Yes	No	No	Yes
Employment	No	No	Yes	No	No	Yes
Household income	No	No	Yes	No	No	Yes
N	122	122	122	124	124	117
R^2	0.116	0.117	0.443	0.029	0.040	0.372

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Heteroskedasticity robust standard errors shown in brackets. PGSI measures are binary variables representing categories in the problem gambling severity index, with non-problem/non-gambling excluded as the base category.*

with the learning and cognitive pathway mentioned in several psych-social models of gambling behavior (e.g., Blaszczynski and Nower (2002). This finding did not hold for SGMs and participants had a more normal distribution of overconfidence values in their understanding of SGMs.

Despite the appearance of more extreme values of EGMs overconfidence, both studies showed participants had higher measured understanding and lower self-reported understanding of EGMs than SGMs. While this may relate to the distribution of the underlying measures, it may also suggest that there is a subset of individuals with a highly distorted view of their EGM understanding and warrants further study.

Based on our results, it may be expected that SGM players would be more effective at moderating their risk taking behaviors, based on their understanding or lack of certainty of how outcomes are determined, in comparison to EGMs. That is, relative to EGMs, SGM players recognize the limits of their understanding, and have attitudes that are uncorrelated with the degree of overconfidence. The findings may indicate that when faced with a novel activity, individuals have less well-developed biases. SGMs were entirely new to Study Two participants and Study One participants had limited experience with the devices.

Alternatively, the increased interactivity and complexity of SGMs may be less likely to lead to cognitive biases than randomly determined games, given human biases to look for patterns (Kahneman and Tversky, 2013). Social and cognitive psychology research suggests that when performing challenging tasks with a focus on acquiring an incremental skill that can be enhanced through effort, people focus on learning, use analytical strategies, and have high self-efficacy (Wood and Bandura, 1989). The greater perceived role of skill in SGMs may keep individuals focused on the activity and how their actions influence outcomes, with the constant feedback and shifting efforts reducing a sense of overconfidence developing that influences attitudes. We note that in Study 2, we observed that individuals in the PGSI problem gambling category had significantly higher attitudes toward EGMs than non-problem gamblers, but significantly lower attitudes toward SGMs. Given the lack of research on SGMs, the interpretation of these findings are hypothetical and warrant further research.

The research outcomes are somewhat surprising as EGMs should be easier to understand in terms of how outcomes are determined than SGMs, yet participants had higher rated understanding of SGMs. Outcomes are completely chance based for EGMs, with no influence of any external or personal factors. In contrast, SGMs include many different formats, skill plays a differing and inconsistent role, and chance is still the predominant factor but to an undefined extent. As such, it is rational that participants indicated uncertainty in their understanding of SGMs to a greater extent than EGMs. The findings suggest that this awareness of the lack of understanding may play a protective factor as it was not related to attitudes to play, which is related to intention in the literature. Previous research has found that individuals with greater gambling-related irrational beliefs are more likely to play SGMs, but that understanding did not influence intent to play (deidentified). Study Two supports this finding in a new sample and based on random allocation to exposure to SGMs.

Given previous findings that positive attitudes predict intention to play EGMs (Gainsbury et al., 2019a), our results are similar to Goodie (2005), who found that individuals with greater confidence in their understanding of how the outcomes are determined were more likely to engage in EGM gambling activity. Our findings suggest that decision-making underlying gambling behavior may be related to individual cognitive processes and decision strategies. Therefore, effective policies and practices to influence cognitive processes and decision-making capabilities could influence EGM gambling behavior. However, there was no impact of the labeling of machines as being based on either chance or skill, including when this information was accurate or inaccurate. This is consistent with previous research that messages providing information to inform EGM play is ineffective in altering cognitions or behaviors (Monaghan and Blaszczynski, 2010). The labeling may have been too subtle to influence attitudes given the highly impactful stimulus of SGMs and EGMs. The results support previous findings suggesting that messages need to attract attention to have any impact, such as being presented on machine screens during a break in play (Gainsbury et al., 2015; Landon et al., 2016; Ginley et al., 2017; Harris et al., 2018; Critchlow et al., 2020).

SGM understanding was higher among participants in Study One than in Study Two. Given that 43% of participants in Study One had some prior experience with SGMs, this may indicate that previous gambling opportunities shapes understanding, although Study Two participants also had a chance to play the SGM, albeit in a simulated environment. Given the differences between the participant groups, further research is needed to assess the impact of SGM play on consumer understanding of how outcomes are determined.

SGM understanding was not impacted by framing in the current research. The framing was relatively minor, but the results suggest that play experience or perception of machines, not how these are describes shapes attitudes. The lack of attention and comprehension of information provided about how outcomes are determined is consistent with research about informative messaging (Monaghan and Blaszczynski, 2009; Ginley et al., 2017) and may account for misunderstanding of EGMs. Although the findings are preliminary, the implications are that labeling needs to be much stronger and more persuasive to change beliefs and misconceptions. Further research is required regarding the optimal timing for messages to shape attitudes as messages may be more impactful after initial engagement given that messages prior to play had no impact, but these need to be strong enough to counter experience.

Limitations and Future Research

The results from these studies need to be considered in terms of the methodological limitations of the research. Both samples were self-recruited and were non-probabilistic or representative of the broader population of consumers likely to play EGMs and SGMs. The results are based on self-report which may be biased and not accurately capture understanding of the machines or true attitudes. To reduce the potential bias in self-report responses we asked participants on feedback of both EGMs and SGMs and used the PGSI to control for prior gambling exposure and experience of harms. There was no gambling behavior measured in either study. Nonetheless, given the precautionary principle (O'Riordan and Cameron, 1994; Raffensperger and Tickner, 1999), it is important for research to examine SGMs in jurisdictions before these are considered for regulatory approval. The benefit of laboratory-based research enables random allocation to be exposed to EGMs or SGMs, which would not be possible in a real-world trial. Given the inherent limitations associated with laboratory-based research, we recommend real-world trials in controlled venues to investigate engagement amongst a broader cohort with SGMs including those familiar with gaming without gambling experience as well as those already highly engaged with other gambling activities to assess the impact on real gambling behavior and outcomes.

Implications

This study makes an important Contribution To The Field. It furthers the available literature regarding SGMs and builds on wider research demonstrating connections in understanding, cognitive biases, and attitudes between populations. These findings suggest that in contrast to a tendency for individuals to over-estimate their knowledge of how EGMs work, which leads to positive attitudes with a subsequent impact on decision-making and behavior, individuals are cautious in their accuracy of understanding SGMs and there is no observed biases which influence attitudes. This is an important outcome as we found no evidence to suggest that when exposed to SGMs, individuals will over-emphasize the role of skill and this will influence their attitudes toward SGMs and subsequent intent to play these machines. Even considering the limitations of the research, this is an important finding for regulators considering the impact of SGMs. Further research is needed to examine the impact of SGMs in a licensed gambling environment and to consider differences between cohorts in terms of overconfidence in understanding SGMs and the influence of this on attitudes and subsequent intention and behavior.

DATA AVAILABILITY STATEMENT

The datasets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found below: https://osf.io/ba5n2/.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the University of Sydney Human Research Ethics Committee. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

Both authors listed have made a substantial, direct and intellectual contribution to the work, and approved it for publication.

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BRIEF RESEARCH REPORT
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Repeat Traffic Offenders Improve Their Performance in Risky Driving Situations and Have Fewer Accidents Following a Mindfulness-Based Intervention

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Risky decision-making is highly influenced by emotions and can lead to fatal consequences. Attempts to reduce risk-taking include the use of mindfulness-based interventions (MBI), which have shown promising results for both emotion regulation (ER) and risk-taking. However, it is still unclear whether improved emotion regulation is the mechanism responsible for reduced risk-taking. In the present study, we explore the effect of a 5-week MBI on risky driving in a group of repeat traffic offenders by comparing them with non-repeat offenders and repeat offenders without training. We evaluated the driving behavior of the participants through a driving simulation, and self-reported emotion regulation, both before and after the intervention. At baseline, poor emotion regulation was related to a more unstable driving behavior, and speeding. The group that received mindfulness training showed improved performance during risky driving situations and had fewer accidents, although their overall driving behavior remained largely unchanged. The observed trend toward improved emotion regulation was not significant. We discuss whether other effects of MBI – such as self-regulation of attention – could underlie the observed reduction in risky driving in the initial stages. Nonetheless, our findings still confirm the close relationship between emotion regulation skills and risky driving.

Keywords: mindfulness, risk-taking, repeat traffic offender, emotion regulation, attention regulation

INTRODUCTION

Daily life involves constant decision-making with regard to what actions to take and some situations can lead us to take certain risks, e.g., when we are in a rush or in a bad or even euphoric mood. In fact, the factors that modulate the process of risky decision making include emotion (Angie et al., 2011; Engelmann and Hare, 2018), impulsivity (Nagin and Pogarsky, 2003), and self-regulation (Kelley et al., 2015). Driving is an example of a typical everyday risk-taking scenario, where the most extreme consequences are fatal accidents, estimated at 1.35 million deaths each year (World Health Organization, 2018). Careless driving behavior has been identified as one of the reasons why people take more risks at the wheel and suffer more road accidents (Taubman – Ben-Ari et al., 2016), while speeding has also been linked to higher accident and fatality rates (OECD, 2018).

In driving environments in particular, risky driving and low perception of risk have been found to be influenced by emotions (Nesbit et al., 2007; Megías et al., 2011, 2014; Jeon and Zhang, 2013) and emotion regulation (ER; Navon and Taubman – Ben-Ari, 2019), where ER represents the ability to recognize one's own emotions and to know how to express and experience them in an adaptive and flexible way (Gratz and Roemer, 2004). Several studies have found that the use of appropriate ER strategies is associated with a safer driving behavior, while difficulties in ER, such as not being aware or able to control impulsive behavior or emotional responses, has been linked to risky driving behavior and traffic violations, for instance exceeding speed limits and mobile use while driving (Hancock et al., 2012; Trógolo et al., 2014; Sani et al., 2017; Šeibokaitė et al., 2017; Parlangeli et al., 2018; Navon and Taubman – Ben-Ari, 2019).

Due to the influence of ER on risky driving, one promising strategy for reducing road fatalities could be the use of interventions that, through the improvement of ER skills, lead to safer driving behavior (Feldman et al., 2011; Koppel et al., 2019). One way of enhancing these skills could be the use of mindfulness-based interventions (MBI), which have been found to produce an improvement in ER (Eberth and Sedlmeier, 2012), even in clinical populations with ER difficulties (Garland et al., 2017; Vanzhula and Levinson, 2020). Mindfulness can be defined as the act of deliberately paying attention to the present moment, with acceptance, openness, and without judgment (Kabat-Zinn, 2005). This deliberate attention involves self-regulation of attention (Tang et al., 2007, 2015; Gil-Jardiné et al., 2017), a fundamental process for the adaptive execution of driving, where attention regulation is essential (Groeger, 2001).

Mindfulness, understood as a trait, is the natural mindful tendency of each individual (Brown and Ryan, 2003) and has been negatively related to risky driving (Feldman et al., 2011; Panek et al., 2015; Koppel et al., 2018; Murphy and Matvienko-Sikar, 2019). In a review of effective interventions for reducing driving anger, Deffenbacher (2016) concluded that MBI reduced anger and facilitated more adaptive expression of anger in drivers (Diebold, 2003; Kazemeini et al., 2013). Some studies have also found that MBIs improve performance on driving simulators, although methodological limitations, such as a very low number of participants and the lack of baseline measurements as in Kass et al. (2011), or the application of only a 10 min mindfulness meditation as in Reynaud and Navarro (2019), and the scarcity of research in this field mean that no firm conclusions can yet be drawn on this issue (for a review, see Koppel et al., 2019).

Thus, in the light of these findings, we aimed to test the effectiveness of a MBI in reducing risky driving behavior in a group of repeat traffic offenders, measuring behavioral change through a driving simulation. This type of measurement has been used to study real driving behavior, with a correspondence between real and simulated driving (Meuleners and Fraser, 2015; Branzi et al., 2017). Specifically, the Honda Riding Trainer (HRT) simulator, used in the present work, has been used to study the processes underlying driving skills (Di Stasi et al., 2010, 2011) and for training in safer driving (Tagliabue et al., 2019). Furthermore, we hypothesized, based on previous research (Koppel et al., 2019), that ER skills would improve and that safer driving is encouraged through improved ER.

MATERIALS AND METHODS

Participants

Our sample was composed of 89 participants (29 women; age range between 18 and 63 years, $M = 34.39$, $SD = 14.57$) recruited in an online survey from the University of Granada (students, teachers, and administration staff), as well as during a rehabilitation course run by a driving school, where drivers recover their points lost because of traffic rule violations. All participants were drivers with a valid driving license. The greater number of men represents the gender differences in driving violations present in the population (Health and Safety Executive, 2002; World Health Organization, 2020).

To group drivers into repeat and non-repeat offenders, we used the following self-reported traffic violations as criteria: attendance of a rehabilitation course for drivers at least once, a loss of points according to the Spanish penalty system for traffic rule violations, being fined at least twice for risky driving behavior (alcohol or drug use, not using a seat belt, or exceeding speed limits), or reporting as having usually exceeded speed limits by more than 20% of the permitted speed. Sixty repeat offenders, meeting at least one of these criteria, and 29 non-repeat offenders, meeting none of these criteria, completed the baseline and post-intervention evaluations.

Half of the risky drivers were selected for a 5-week MBI program dependent on their weekly availability, which was established prior to testing, to gather the greatest number of participants for the intervention groups. At four different time points along the 2-year period of data collection, we grouped the participants who coincided at the same weekday availability, resulting in a quasi-randomized controlled trial. The drop-out rate of the mindfulness training following the second session was 6 out of the 30 participants.

Thus, in the current study, we compared the following three groups: non-repeat offenders (NR, $N = 29$), repeat offenders (R, $N = 30$), and repeat offenders who received mindfulness training (R-M, $N = 30$; see **Table 1** for more details).

Questionnaires

We used two complementary questionnaires, focusing on the awareness of emotion and its regulation and different types of ER strategies, respectively.

Self-Reported Traffic Violation

To group participants into repeat and non-repeat offenders they reported on demographic variables (sex and age), km

TABLE 1 | Demographic variables and driving experience (mean and standard deviation).

	NR	R	R-M
Age	32.38 (14.6)	35.03 (14.66)	35.7 (14.75)
Sex (women/men)	11/18	6/24	12/18
Education level	3.55*(0.51)	3.67*(0.61)	3.4*(0.62)
Estimated km driven in life	228867.24 (424848.94)	324144.83 (401172.55)	373083.33 (606785.13)
Interval of estimated km driven per year by car	5.1**(2.82)	6.59**(3.21)	5.83**(3)

*Education ranged from 2 (Primary studies) to 4 (Superior level studies).
**Km driven/years is measured in estimated intervals, with 5 = 6,000-9,000; 6 = 9,000-12,000; and 7 = 12,000–15,000 km.

driven per year and in life, months of holding a driver license, number of rehabilitation courses, number of lost points, number of traffic fines, and frequency of exceeding speed limits.

Difficulties of Emotion Regulation Scale

The Spanish version of the Difficulties Emotion Regulation Scale (DERS; Gratz and Roemer, 2004; Gómez-Simón et al., 2014) measures different negative aspects of emotion recognition, control, and regulation strategies. The 36-item questionnaire consists of six subscales, using a five-point Likert scale from 1 (Almost never, 0–10%) to 5 (Almost always, 91–100%) evaluates the following: *Lack of emotional awareness* (six items), *Impulse control difficulties* (six items), *Non-acceptance of emotional response* (seven items), *Difficulties engaging in goal-directed behavior* (five items), *Lack of emotional clarity* (five items), and *Limited access to emotion regulation strategies* (seven items). The internal consistency of the scale is adequate (Cronbach's $\alpha = 0.88$).

Cognitive Emotion Regulation Questionnaire

The Spanish version of the Cognitive Emotion Regulation Questionnaire (CERQ; Garnefski et al., 2001; Domínguez-Sánchez et al., 2013) measures different types of ER strategies. The 36-item questionnaire consists of nine subscales with four items each, using the same five-point Likert as the DERS: *Self-Blame* (Cronbach's $\alpha = 0.61$), *Rumination* (Cronbach's $\alpha = 0.74$), *Catastrophizing* (Cronbach's $\alpha = 0.72$), *Other-Blame* (Cronbach's $\alpha = 0.79$), *Acceptance* (Cronbach's $\alpha = 0.64$), *Positive reinforcing* (Cronbach's $\alpha = 0.89$), *Refocus on planning* (Cronbach's $\alpha = 0.83$), *Positive reappraisal* (Cronbach's $\alpha = 0.8$), and *Putting into perspective* (Cronbach's $\alpha = 0.83$). The first four and the remaining five subscales were grouped into negative and positive ER strategies (Cronbach's $\alpha = 0.89/0.79$, respectively), respectively, as suggested by the original authors (Garnefski et al., 2001).

Driving Simulation

For the driving simulation, we used the HRT motorcycle simulator, which consists of a seat, handlebar, pedals, accelerator, brakes, turn indicators, and claxon (see Di Stasi et al., 2009; Megías et al., 2017, for more details). All participants rode through the same three different traffic scenarios in a counterbalanced order to measure driving behavior in different contexts: two urban scenarios, one by day and the other by night, and a mountain road scenario. Each of these scenarios contained a total of eight risk situations, such as crossing pedestrians or obstacles on the road, and was approximately 5–10 min long depending on the type of scenario, speed, crashes, and variability of the course taken by the participant. They were projected on the wall in front of the participants seated on the motorcycle simulator at a distance of 185 cm on a 110 × 180 cm screen, with a refresh rate of 30 Hz and a resolution of 1,024 × 768 pixels.

Indices calculated from data recorded by the HRT included: average and variance of speed (km/h), of speed in a risk situation (km/h), and of exceeded speed limits (km/h), length of time spent exceeding speed limits (sec), average throttle rotation (%), variance of steering wheel (rad), front and rear brake force (kg), number of accidents, and the average rating of performance in each risk situation, calculated by the HRT, ranging from A (good performance) to D (accident), taking into account variables, such as speed when entering the risk situation and distance to crash with an object. This last value is coded from 1 to 4 with lower values indicating greater risk-taking and worse performance in a risk situation. Exceeded speed limits, speed in risk situations, and performance ratings are calculated for the two urban courses only, since the HRT software does not register measures of speed limits or risk situations in the mountain road scenario.

Mindfulness-Based Training

The mindfulness-based training was adapted from the Mindfulness-Based Stress Reduction (MBSR) program widely used in research (Kabat-Zinn, 2003). The program length was reduced to 5 weeks with 3-h weekly sessions due to the availability of the participants. The sessions were prepared by an instructor with extensive experience in mindfulness-based training and were delivered by the instructor herself (CVL) and another instructor (ECV) with experience in the MBSR program. To avoid the influence of researcher bias, neither of the instructors was involved in the data collection. The sessions were designed to enhance situation awareness and included meditation and yoga practice (attention to breathing, body scanning, yoga, and guided meditation), group discussions, and training in ER, as well as the importance of focusing on what happens in the present moment both inside and outside, and pausing to take a breath before observing and finally selecting the appropriate response.

Procedure

The participants were selected according to their self-reported traffic violations, which were requested by means of an online survey (see participants section). The baseline and post-intervention evaluation was the same. Participants came to the research center and, as a part of a broader project, filled in the questionnaires and completed the driving simulation, with the order based on the availability of the facilities (HRT and computer room) and participants' temporal availability. The average interval between both evaluations was approximately 4 months (Mean = 142.26 days, $SD = 69.15$) and varied across the participants due to their availability. Therefore, the length of this interval was included as a factor in the data analysis.

Data Analysis

As in the literature recommended, we used an intention-to-treat approach (Gupta, 2011), analyzing participants who dropped-out in the R-M group.

JASP statistical software (Version 0.11.1, JASP Team, 2020, freely available at https://jasp-stats.org/) and R Studio (RStudio: Integrated Development for RStudio, Inc., Boston, MA, 2016, freely available at http://www.rstudio.com/) were used for analyses, along with p value null hypothesis statistical testing (NHST) and Bayesian methods.

We found no differences between the three groups in terms of gender (Pearson's $\chi^2 = 3.291$, $df = 2$, $p = 0.193$), education level (Kruskal Wallis $H = 0.832$, $df = 2$, $p = 0.66$), age [$F(2, 86) = 0.421$, $p = 0.658$], or driving experience [km driven in life: $F(2, 85) = 0.664$, $p = 0.518$; km/year by car: $F(2, 85) = 1.751$, $p = 0.18$].

Mixed-factor ANCOVAS were conducted to analyze the effect of the intervention on the driving behavior indices and the ER strategies. The experimental design includes time (baseline and post-intervention evaluation) and subscales as within-subject factors, and group (NR, R, and R-M) as between-subject factors, using age and interval between evaluations as covariates. To support our hypothesis with Bayesian methods, the Bayes Factor (BF_{10}) was calculated for all possible models compared to the null model, as well as the $BF_{Inclusion}$ for each predictor (Wagenmakers et al., 2018; Bergh et al., 2020), estimated across all matched models following Sebastiaan Mathôd (Bergh et al., 2020).

For variables with significant time × group interactions, mediation analysis was conducted. To explore the magnitude of the intervention effect, an ANCOVA was carried out on the Behavior Shift Index (BSI; Li et al., 2018), defined as the magnitude of change between baseline and post-training evaluation [$(Value_{baseline} - Value_{post\text{-}training})/(Value_{baseline} + Value_{post\text{-}training})$] with group as the between-subjects factor and age and interval between evaluations as covariates.

As accident rate is not a continuous variable, these rates are analyzed by categorizing the difference between baseline and post-intervention evaluation into improved (difference > 0), unchanged (difference = 0), and worse performance (difference < 0). A multinomial regression was conducted, and risk ratios were estimated with group (NR, R, and R-M) as the between-subjects factor and age and interval between evaluations as covariates.

Finally, we explored whether ER and driving behavior are related, conducting a partial correlation analysis between the BSI values of all questionnaire subscales and the driving behavior indices, using age as a covariate. We also confirmed a general relationship between ER and the driving behavior indices at baseline in the whole sample (**Supplementary Material**).

RESULTS

Effect of the Intervention on Emotion Regulation and Driving Behavior

Analysis of the driving performance ratings revealed a time × group interaction [$F(2, 84) = 3.919$, $p = 0.024$, $\eta^2 = 0.085$, $BF_{Inclusion} = 2.255$], as well as a main effect of age [$F(1, 84) = 24.798$, $p < 0.001$, $\eta^2 = 0.228$, $BF_{Inclusion} = 4166.733$, best model $BF_{10} = 4503.795$; **Figure 1A**]. Mediation analysis revealed differences between baseline and post-intervention evaluation in the R-M group [$F(1) = 10.642$, $p = 0.003$] and no differences between the two control groups ($p > 0.4$). The ANCOVA on BSI scores, indicating the magnitude of changes, revealed a main effect of group [$F(2, 84) = 4.062$, $p = 0.021$, $\eta^2 = 0.088$, $BF_{Inclusion} = 1.993$] and no main effect of age [$F(2, 84) = 2.869$, $p = 0.094$, $\eta^2 = 0.033$, $BF_{Inclusion} = 0.57$; **Figure 1B**].

No significant time × group interaction effects ($p > 0.1$) were found for the remaining indices, although a significant main effect of age was found ($p < 0.001$). However, since the performance rating is based on other indices during the risk situations, we observed strong associations between all of these indices and the baseline performance ratings (Pearson's r: min = −0.338, $p < 0.001$, max = −0.847, $p < 0.001$; **Table 2**), while the improvement in performance rating measured with the BSI is also associated with the BSI of the other indices (Pearson's r: min = −0.253, $p < 0.05$, max = −0.624, $p < 0.001$; **Table 2**), which is even stronger and more consistent in the R-M group (Pearson's r: min = −0.407, $p < 0.05$, max = −0.835, $p < 0.001$; **Table 2**).

The multinomial regression analysis of the differences in accident numbers between baseline and post-intervention evaluations revealed differences between the R-M and both control groups in the comparison between worse and better performance ($AIC = 196.67$, R-M vs. R: 1.851, $p = 0.016$, 95% CI = 0.347–3.355; R-M vs. NR: 1.464, $p = 0.046$, 95% CI = 0.029–2.9), while the control groups did not differ from each other (R vs. NR: −0.387, $p = 0.58$, 95% CI = −1.756–0.982; **Figure 1C**). Comparing the outcomes of more and fewer accidents using the relative risk ratios, R are 6.368 times, and NR are 4.325 times, more likely to have more accidents compared with the M-R group.

No time × group interaction effect was found for ER [DERS: $F(2, 84) = 2.264$, $p = 0.11$, $\eta^2 = 0.051$, $BF_{Inclusion} = 0.396$; CERQ positive: $F(2, 84) = 0.788$, $p = 0.458$, $\eta^2 = 0.018$, $BF_{Inclusion} = 0.07$; and CERQ negative: $F(2, 84) = 1.451$, $p = 0.24$, $\eta^2 = 0.033$, $BF_{Inclusion} = 0.091$]. However, there was a tendency for the R-M group to show improvement in the DERS and the negative scales of the CERQ, which can be observed in **Figure 2**.

In summary, participants who were trained in mindfulness do not show differences in ER but showed improved performance in risk situations and had fewer accidents in comparison with both control groups. It is also worth noting that while age is an important factor in the prediction of driving behavior, this factor has almost no influence on the magnitude of improvement observed as a consequence of the intervention.

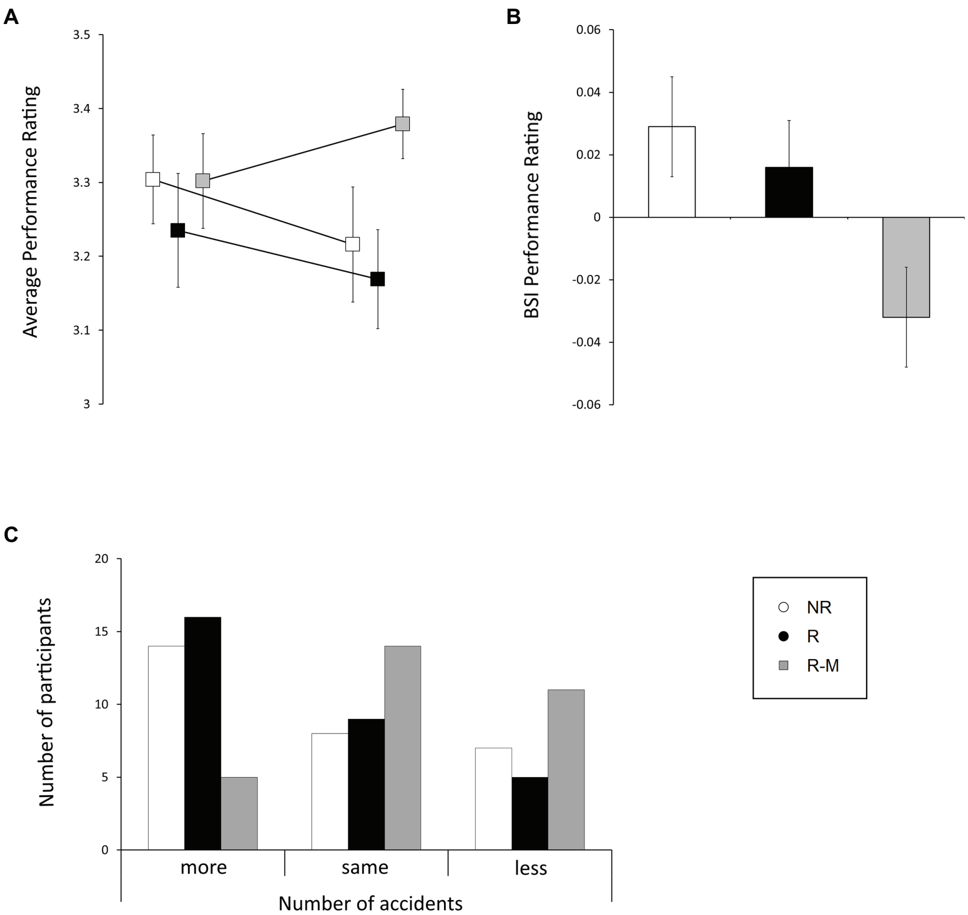

FIGURE 1 | Effect of the intervention on the behavioral indices of the driving simulation. **(A)** represents the differences in average evaluation between baseline and post-intervention, **(B)** indicates the magnitude of behavioral changes in the average performance ratings in risk situations in each group, and **(C)** shows the differences in the number of accidents between baseline and post-intervention.

DISCUSSION

In the current study, we explored the effect of MBI on driving behavior and ER. We evaluated the performance on a driving simulation and self-reported ER scores of a group of repeat offenders trained in mindfulness and compared these measures with those of two control groups, one of repeat offenders and another of non-repeat offenders. We found that the intervention had an effect on accidents and evaluation of performance in risk situations, but no effect on ER and most of the behavioral indices. However, driving indices were closely related to the performance ratings at baseline, while the magnitude of change was related to the one of performance ratings, being greatest in the mindfulness trained group.

Effect of Intervention on Driving Behavior

The R-M group of our study had fewer accidents and performed better in a risk situation, although no differences were found in terms of the other driving indices, such as speed, acceleration, and driving direction. However, these indices are closely related to performance ratings in risk situations, and the magnitude of change observed in these measures is strongly associated with the change in performance ratings in the R-M group, pointing to the possibility that most of the indices are enhanced in a similar way. As mentioned earlier, the length of the intervention could have played a role in the non-significance of some of these effects, which might be greater in a follow-up study.

Previous research on the effect of mindfulness training on driving behavior is still scarce. In fact, there is only one study exploring changes in driving simulation, which found a (non-significant) reduction in traffic violations in students enrolled in a Buddhism course (Kass et al., 2011). Since these authors reported high correlations between situation awareness of the driving simulation and the scores in mindfulness and concentration, the mechanism of driving improvements through meditation training might be due to the greater attention paid to risk factors on the road (Kass et al., 2011), which would also be consistent with our results.

TABLE 2 | Relationship between evaluation of performance in risk situations, as calculated by the Honda Riding Trainer (HRT), and other HRT indices.

	Average speed (km/h)	Variance speed (km/h)	Average speed risk situations (km/h)	Variance speed risk situations (km/h)	Average exceeded speed limits (km/h)	Variance exceeded speed limits (km/h)	Length of time spent exceeding speed (s)	Mean throttle (%)	Front brake (kg)	Variance steering wheel (rad)	Accidents (sum)
Evaluation of performance in risk situation (1–4)	−0.752**	−0.549**	−0.847**	−0.611**	−0.642**	−0.528**	−0.762**	−0.563**	−0.338**	−0.691**	−0.473**
Correlations of BSI values	−0.464**	−0.26*	−0.624**	−0.313*	−0.184	−0.219*	−0.253*	−0.273*	−0.082	−0.434**	—
Correlations of BSI in the R-M group	−0.685**	−0.453*	−0.835**	−0.542**	−0.231	−0.317	−0.438*	−0.407*	−0.322	−0.528**	—

**$p < 0.001$; *$p < 0.05$.

Rear Brake Index did not show any significant relationship. Behavior Shift Index (BSI) for accident rate could not be calculated as variable is not continues. Partial correlation coefficient reported represent Pearson's r, except for accident rates where Spearman's rho is reported.

Furthermore, our findings showed that age is one of the most important predictors of risky driving behavior on the simulator, characterized by speeding, an instable direction, and low evaluations in the performance in risk situations, which is in line with previous research (Jonah, 1990; Rhodes and Pivik, 2011). However, we did not find an effect of age on the magnitude of change produced by the intervention, which suggests that MBI is equally beneficial for all age groups. This could have practical implications for the design of new intervention programs that are primarily aimed at youths and novice drivers, who are the groups that suffer the most fatalities (World Health Organization, 2018).

Effect of the Intervention on Emotion Regulation

In the present study, we found no differences in ER following the 5-week MBI, although the pattern of results points to less ER difficulties and a reduction of negative ER strategies, such as ruminations, catastrophizing, and blaming oneself or others. Studies with a longer intervention – usually 8 weeks – have found enhanced ER (for a review, see Roemer et al., 2015). Thus, the lack of significant findings reported here could be due to the length of the intervention, since, even after 5 weeks, we were already able to observe some effects of the intervention.

In the driving context, research has focused on driving anger and aggressive driving, applying a wide variety of approaches, including behavioral, cognitive, and relaxation techniques (for a review, see Deffenbacher, 2016). The first studies to apply MBI found improvements in driving anger (Diebold, 2003; Kazemeini et al., 2013). However, methodological limitations, such as small sample sizes and the sole use of questionnaires make it difficult to draw any firm conclusions.

Moreover, training in mindfulness may not only reduce driving anger, but might also produce other changes in ER strategies that affect aberrant driving behavior. Thus, it is important to measure changes in emotion regulation or expression in general. This issue was addressed in a study with Chinese bus drivers, where cognitive therapy, using the same type of ER instruction as that used in the present intervention, resulted in the greater use of positive ER strategies (Feng et al., 2018). These findings may help to explain the differences we found in the observed changes associated with the distinct ER strategies. Positive regulation strategies, such as positive reappraisal, may be enhanced by the components of cognitive therapy used in the MBI. On the other hand, mindfulness meditation itself could enhance ER processes, such as emotional awareness and clarity, impulsive control, and acceptance of emotional responses, as well as reduce negative ER strategies. This would be in line with neuroscientific approaches, where two different mechanisms have been suggested for the enhancement of ER through mindfulness top-down and bottom-up processes (Chiesa et al., 2013; Guendelman et al., 2017).

Emotion Regulation as a Mechanism of Improvement

Taken together, our results provide first evidence of a behavioral change following MBI in repeat offenders, a high-risk group for road accidents and fatalities. Since in the current study, behavior is measured in a simulated traffic environment, and not only with questionnaires or decision-making tasks, the results are promising and suggestive of real-life behavior. Additionally, it should be noted that, even though a motorcycle simulator was used, these results may indicate safer driving behavior in general, as well in other vehicles such as cars and bikes.

Although research has pointed to ER as the mechanism underlying safer driving behavior (Feldman et al., 2011; Koppel et al., 2019), our results only indicate a (non-significant) tendency for less ER difficulties and the use of fewer negative ER strategies, such as rumination, catastrophizing, and self- and other-blame.

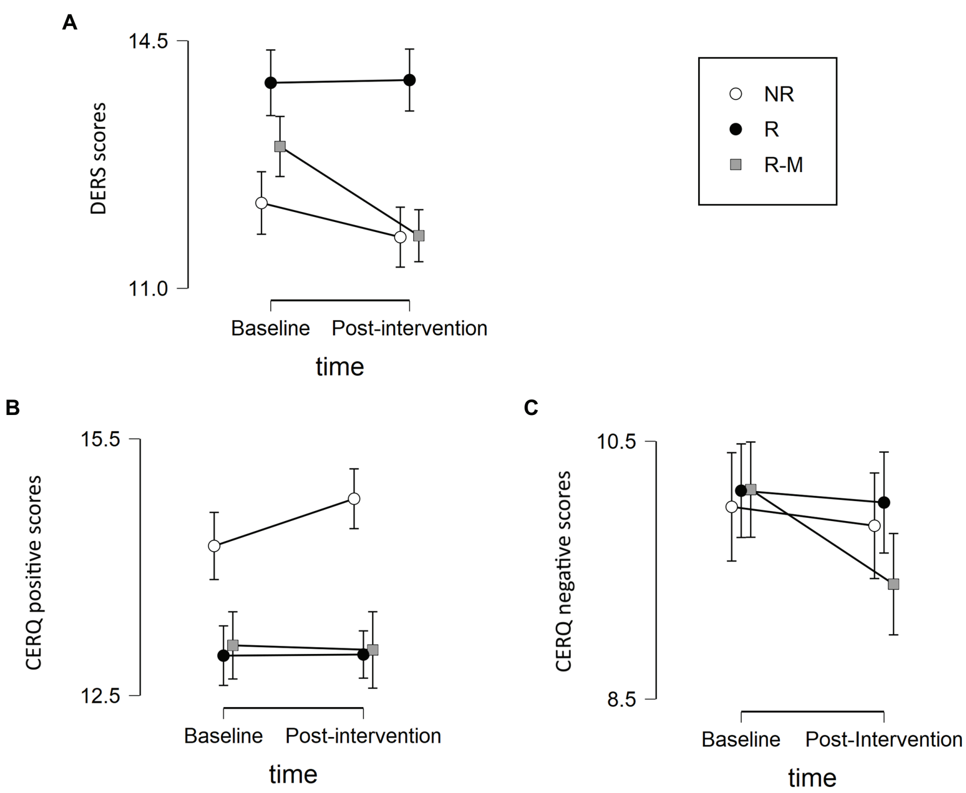

FIGURE 2 | Differences in the emotion regulation (ER) scales between the baseline and post-intervention for each group. Total scores are presented for the Difficulties Emotion Regulation Scale (DERS; **A**) and the positive **(B)** and negative **(C)** scales of the Cognitive Emotion Regulation Questionnaire (CERQ).

We hypothesize that the first behavioral changes may be faster and easier to measure than differences in ER, which may be more stable over time. The improvements in attentional control, which are enhanced by MBI (Tang et al., 2007; Malinowski, 2013), might be greater in these first weeks, generating better performance in risk situations, and thereby leading to fewer accidents. By paying more attention to road signals, conditions, and signs of risk, they may have improved risk perception, and thus, drive safely (Kass et al., 2011). Changes in ER could require more practice and thus may not be directly responsible for the behavioral changes we found in the present study. Nonetheless, our baseline correlations between ER and driving indices of the driving simulation (**Supplementary Table 1**) suggest an association between ER and driving behavior. Therefore, more research is needed to identify the precise mechanism by which mindfulness training can enhance safe driving behavior.

Limitations

Although our findings indicate that MBI lead to a safer performance in risk situation, more research is needed to confirm our results and to study long-term effect. Since our sampling was based on the temporal availability of the participants, complete randomized trials are needed with a greater number of participants, as well as studies using longer MBI programs, to explore whether longer training improves ER and other indices of driving behavior.

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation, to any qualified researcher.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by the Human Research Ethics Committee of the University of Granada (n° 204/CEIH/2016). The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

ACt, ACn, AM, and SB designed the experiment. LM-C and SB carried out the testing of participants. CV-L and EC-V designed and performed the intervention. SB and ACt analyzed the data. SB and LM-C drafted the manuscript. All authors contributed to the critical revision of the manuscript and approved the final version.

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SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2020.567278/full#supplementary-material

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Gambling-Specific Cognitions Are Not Associated With Either Abstract or Probabilistic Reasoning: A Dual Frequentist-Bayesian Analysis of Individuals With and Without Gambling Disorder

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Background: Distorted gambling-related cognitions are tightly related to gambling problems, and are one of the main targets of treatment for disordered gambling, but their etiology remains uncertain. Although folk wisdom and some theoretical approaches have linked them to lower domain-general reasoning abilities, evidence regarding that relationship remains unconvincing.

Method: In the present cross-sectional study, the relationship between probabilistic/abstract reasoning, as measured by the Berlin Numeracy Test (BNT), and the Matrices Test, respectively, and the five dimensions of the Gambling-Related Cognitions Scale (GRCS), was tested in a sample of 77 patients with gambling disorder and 58 individuals without gambling problems.

Results and interpretation: Neither BNT nor matrices scores were significantly related to gambling-related cognitions, according to frequentist (MANCOVA/ANCOVA) analyses, performed both considering and disregarding group (patients, non-patients) in the models. Correlation Bayesian analyses (bidirectional BF_{10}) largely supported the null hypothesis, i.e., the absence of relationships between the measures of interest. This pattern or results reinforces the idea that distorted cognitions do not originate in a general lack of understanding of probability or low fluid intelligence, but probably result from motivated reasoning.

Keywords: gambling-related cognitions, abstract reasoning, probabilistic reasoning, intelligence, motivated reasoning, gambling disorder

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INTRODUCTION

Gambling is a leisure activity, practised non-problematically by a large share of the population, but that can generate substantial harm to the community (Shannon et al., 2017). The severity of potentially problematic gambling lies on a continuum in which gambling disorder is placed at its highest end (Shaffer and Martin, 2011; Rai et al., 2014). However, from a public health perspective, gambling-related harms go beyond the individual, and are not exclusively driven by the severity of disordered gambling (Wardle et al., 2019).

Understanding the factors that foster gambling involvement is thus important at the individual, social, and policy levels, regardless of clinical status. And, among these factors, distorted gambling-related cognitions play a central role (Fortune and Goodie, 2012; Lindberg et al., 2014a; Goodie et al., 2019; Brooks et al., 2020). These cognitions are frequently targeted by commercial advertising (Lopez-Gonzalez et al., 2018), and are among the main therapeutic targets in cognitive-behavioral therapy of gambling disorder (Rash and Petry, 2014; Choi et al., 2017; Menchon et al., 2018). Indeed, they are present to some degree in virtually all gamblers, play a key role in maintaining gambling behavior [see (Goodie and Fortune, 2013), for a review], and their strength varies as a function of severity (Emond and Marmurek, 2010; Del Prete et al., 2017; Jara-Rizzo et al., 2019) and is modulated by the effectiveness of therapy (Breen et al., 2001; Doiron and Nicki, 2007; Toneatto and Gunaratne, 2009; Donati et al., 2018).

The most comprehensive and widely used model of gambling-related cognitions [the Gambling-Related Cognitions Scale, GRCS (Raylu and Oei, 2004)], encompasses five different domains, namely, *inability to stop, expectancies, predictive control, illusion of control*, and *interpretative bias*. The first two are common dysfunctional (but not necessarily "erroneous") beliefs present in a range of potentially addictive behavior patterns. Specifically, inability to stop refers to a lack of self-efficacy in controlling gambling behavior and overcoming urges, and expectancies allude to expected outcomes than can work as motives to gamble, such as winnings or curbing negative affect. The other three can be strictly considered cognitive biases at making causal inferences. Illusion of control and predictive control are beliefs about the possibility to control and predict gambling outcomes, respectively. Interpretative bias is the tendency to attribute positive and negative gambling outcomes to internal and external causes, respectively, that is, to reformulate wins as due to skills, and losses as due to bad luck (Oei and Burrow, 2000; Oei and Raylu, 2004).

There are at least two mechanisms by means of which better domain-general reasoning abilities could protect individuals from distorted gambling cognitions, and thus, indirectly, from developing gambling problems. The first one is more specific: given the evident overlap between poor understanding of probability and randomness, and causal biases (Gilovich et al., 1985; Ladouceur et al., 1996; Clark, 2017), it seems reasonable to assume that people with lower scores in probabilistic reasoning will transfer that disadvantage to gambling activities, where, as mentioned earlier, causal misattribution plays a key role. Or the other way round, good domain-general probabilistic reasoning could potentially prevent the development of at least some types of distorted gambling-related cognitions.

The second mechanism is more general, and regards the potential role of general fluid intelligence and abstract reasoning. These two largely overlapping constructs refer to the capacity to think logically, solve novel problems and operate abstract symbols with minimal dependence on previously acquired knowledge (Carpenter et al., 1990; Santarnecchi et al., 2017; Gómez-Veiga et al., 2018). Gambling devices and the rules under which they operate can be mathematically complex and opaque, so, in principle, fluid intelligence could contribute to a better understanding of how gambling devices work, and thus to override cognitive biases (Evans and Over, 2010). Complementarily, fluid intelligence could foster a more reflective reasoning style (Barrouillet, 2011), and thus preclude the tendency to rely on the device-triggered intuitions and heuristics from which gambling-related cognitions seem to originate.

Nonetheless, the possibility that gambling-related cognitions (and specifically gambling-related biases) could be disconnected from general reasoning abilities has also been theoretically articulated. In some previous work, it has been shown that dysfunctional gambling-related cognitions, and especially gambling-related causal biases and misattributions, as measured by the GRCS, are more prevalent in individuals playing skill-based games, who, in turn, tend to be younger and better educated, relative to individuals who mostly practice pure chance games (Griffiths et al., 2009; Myrseth et al., 2010; Wood and Williams, 2011). In the context of the Gambling Space Model [GSM, (Jara-Rizzo et al., 2019; Navas et al., 2019; Ruiz de Lara et al., 2019)], more dysfunctional cognitions and stronger gambling-related biases are not hypothesized to originate in weaker domain-general reasoning processes, but in domain-specific motivated reasoning. This kind of reasoning (Kunda, 1990) is driven by ego-protection, that is, it is used by the individual to disguise the real (and potentially ego-damaging) reasons that drive gambling, to make gambling more acceptable, and to reappraise aversive gambling outcomes. In other words, the underpinnings of gambling cognitions would not be mainly intellectual, but affective (Navas et al., 2016, 2017b).

A Brief Review of the Literature on the Link Between Domain-General Reasoning and Gambling Cognitions

Studies on domain-general reasoning skills in gamblers fall into three broad categories. In the first one, intelligence or domain-general reasoning is recorded only for control purposes, in case-control designs with problematic vs. non-problematic gambling (so that domain-general reasoning measures were not the main variables of interest). This category is heterogeneous and the studies in it do not systematically report associations between domain-general reasoning and gambling cognitions. With regard to the association between domain general reasoning and gambling disorder symptoms or diagnosis, results are mixed: in some studies, the group with disordered or problematic gambling obtained lower scores than controls in domain-general reasoning constructs (Martínez-Pina et al., 1991; Toplak et al., 2007; Forbush et al., 2008), whereas, in others, the groups did not show significant differences (Brevers et al., 2012). It is important to take into account, however, that in part of these studies, domain-general reasoning scores were intentionally matched across groups (groups were sampled *a priori* to show no differences in general reasoning ability), so the absence of differences in reasoning abilities between groups is not always informative. For that reason, studies in which matching in general reasoning measures was forced are not included in this review.

A second category of studies has intentionally investigated the putative associations between gambling severity (or presence of gambling disorder/problem gambling) and domain-general reasoning (Templer et al., 1993; Fernández-Montalvo et al., 1999; Delfabbro et al., 2006; Lambos and Delfabbro, 2007; Kaare et al., 2009; Hodgins et al., 2012; Rai et al., 2014; Primi et al., 2017) in broad community or convenience samples, using regression or correlation techniques. These show that individuals with low domain-general reasoning abilities show more severe gambling problems or are in a higher risk of presenting disordered or problematic gambling, with few exceptions [(Fernández-Montalvo et al., 1999); in Primi et al. (2017), gambling problems' severity was found to correlate positively with fluid intelligence, but negatively with probabilistic reasoning]. Again, however, gambling-specific cognitions were not central variables of interest. With the exception of Lambos and Delfabbro (2007), the moderating role of gambling-related cognitions in the association between general reasoning and gambling problems was not assessed either.

Studies of these two categories, primarily or supplementarily estimating the association between domain-general reasoning abilities and presence or severity of gambling problems, are summarized in **Table 1**.

A third category of studies, more directly relevant to the aims of the present study, has directly investigated whether gambling-related cognitions are underpinned in some way by

TABLE 1 | Characteristics and summary of results of the revised studies on the relationship between domain-general reasoning abilities and gambling symptoms' severity.

Study	*N*	Participants	Severity index	Domain-general reasoning task	Main findings regarding severity/diagnosis of gambling disorder and domain-general reasoning
Brevers et al. (2012)*	100	27 PG 38 PrG 35 HC	SOGS	WAIS Vocabulary and WAIS Block Design	PGs, PrGs and controls were similar in estimated IQ. Groups were not intendedly matched in IQ *a priori*
Delfabbro et al. (2006)†	926	Approximately, 5% of the sample were PrG. The rest were non-PrG	DSM-IV-J criteria for PG in children and VGS	Five questions about understanding of odds and probabilistic concepts	Little evidence that PrGs had a poorer understanding of the objective odds of gambling activities. PrGs were more accurate than non-PrG on one question concerning binary odds
Fernández-Montalvo et al. (1999)†	69	69 PG	SOGS	Raven's Standard Progressive Matrices	Non-significant negative correlation between fluid intelligence and SOGS
Forbush et al. (2008)*	59	25 PG 34 HC	SOGS	WAIS Letter-Number Sequencing and WAIS Picture Completion	PGs performed significantly worse than controls on the two WAIS subtests
Hodgins et al. (2012)†	136	60 PrG 76 non-PrG	CPGI (frequency). PGSI and CIDI (severity)	WASI Vocabulary and WASI Matrix reasoning	PrGs performed significantly worse than non-PrGs on intelligence subtests
Kaare et al. (2009)†	75	33 PG 42 NG	SOGS (compared with DSM-IV criteria for PG)	Raven's Standard Progressive Matrices	PGs had significantly lower total scores than controls in fluid intelligence. Low cognitive ability was among the main predictors of pathological gambling, they but remained non-correlated with gambling-related irrational beliefs
Lambos and Delfabbro (2007)†	135	44 PG 46 RG 45 IG	SOGS	Numerical Reasoning Ability and five questions about understanding of odds	There was no significant difference between the groups for their knowledge of gambling odds. PGs and RGs had significantly lower total scores than IGs for numerical reasoning ability
Martínez-Pina et al. (1991)*	172	57 PG 115 HC	SOGS	WAIS	Intelligence was lower in PGs than in controls
Primi et al. (2017)†	822	822 students	SOGS	Advanced Progressive Matrices and PRS	Significantly positive correlation between SOGS and fluid intelligence, and significantly negative correlation between SOGS and probabilistic reasoning
Rai et al. (2014) †	7461	36 PrG 4557 non-PrG 2234 NG	DSM-IV diagnostic criteria for PG	NART Verbal IQ	PrGs had a significantly lower estimated verbal IQ than non-PrGs and non-gamblers. The odds of PrG nearly doubled with each 1 SD drop in IQ
Templer et al. (1993)†	136	136 men convicted	SOGS	Raven's Standard Progressive Matrices	Higher gambling scores were associated with more unfavorable scores on fluid intelligence
Toplak et al. (2007)*	107	24 PG 26 risk non-PG 57 non-PrG	SOGS and DSM-IV diagnostic criteria	WAIS-R Vocabulary and WAIS-R Block Design	PGs and subclinical gamblers tended to have significantly lower WAIS-R scores than non-PrGs

PG,Individuals with pathological gambling; PrG,Individuals with problem gambling; IG,Infrequent gamblers; RG,Regular Gamblers; HC,Healthy controls; NG,Non-gambling individuals; SOGS,South Oaks Gambling Screen; CPGI,Canadian Problem Gambling Index; VGS,Victorian Gambling Screen; PGSI,Problem Gambling Severity Index; WAIS,Wechsler Adult Intelligence Scale; WASI,Wechsler Abbreviated Scale of Intelligence; PRS,Probabilistic Reasoning Scale; NART,National Adult Reading Test.
**Studies in which domain-general reasoning was recorded only for control purposes, in between-participants designs with problematic vs. non-problematic gambling.*
†Studies primarily investigating the associations between gambling severity (or presence of gambling disorder/problem gambling) and domain-general reasoning (see section "A Brief Review of the Literature on the Link Between Domain-General Reasoning and Gambling Cognitions"). None of the studies from our research team is included in this table, due to partial sample overlapping with the present one.

domain-general reasoning processes. Most of the studies in this category are also observational or correlational, but they do straightforwardly focus on the relationship between domain-general and gambling-related reasoning. For instance, using a card-guessing task, Xue et al. (2012a) found that students with higher cognitive abilities (intelligence and executive function) were more prone to show the gambler's fallacy. i.e., the erroneous belief that streaks of bad luck are bound to end in a win. In a similar vein, Perales et al. (2017) found gamblers with stronger biases to perform better than gamblers with weaker biases on non-gambling related causal learning tasks [for a different, although compatible, result, see Orgaz et al. (2013)]. The abovementioned study by Lambos and Delfabbro (2007), beyond the association between gambling problems and general understanding of odds, also found such a measure of odds understanding to be unpredictive of gambling-related irrational beliefs. However, in a recent study by Delfabbro et al. (2020), participants who reported greater illusory control in non-gambling-related everyday tasks (in a self-report questionnaire) scored higher on standardized measures of gambling-specific illusory control.

To our knowledge, only one study in this last category has directly intervened on general-domain reasoning abilities in an attempt to reduce gambling-related biases. Donati et al. (2018) showed that a preventive intervention to modify erroneous cognitions by shaping probabilistic and superstitious thinking in adolescents, reduced their erroneous gambling-related cognitions, suggesting that gambling-related cognitions could related to domain-general reasoning.

Present Study

The present study is aimed at directly testing the association between domain-general reasoning abilities and gambling cognitions, in two samples of (a) individuals from the community who present a detectable level of gambling but do not present gambling problems (henceforth, individuals with non-problematic gambling, NPG), and (b) treatment-seeking patients with gambling disorder (PGD).

Reasoning abilities (i.e., the independent variables in our study) were assessed using the matrices task of the WAIS-IV intelligence scale (Wechsler, 2008), and the Berlin Numeracy Test [BNT (Cokely et al., 2012)], for abstract and probabilistic reasoning, respectively, mirroring the two mechanisms described earlier. These two measures have good validity and reliability. The BNT is a sound index of probabilistic reasoning in practice (Cokely et al., 2018), namely individuals' easiness to deal with basic probabilistic operations from real-life problems (Lipkus and Peters, 2009; Cokely et al., 2012). The matrix reasoning subtest of the WAIS-IV assesses non-verbal perceptual reasoning abilities, and is considered to be a reliable measure of fluid intelligence (Bugg et al., 2006; Wechsler, 2008; Stephenson and Halpern, 2013; Gignac, 2014; Green et al., 2017; Kim and Park, 2018). This mostly overlaps with the g-factor (Spearman, 1927; Tranel et al., 2008; Jaeggi et al., 2010).

On the side of dependent measures, gambling-related cognitions were assessed using the GRCS, described earlier. Complementarily, severity of potentially disordered gambling was assessed with the South Oaks Gambling Screen [SOGS, Spanish version (Echeburúa et al., 1994)].

In view of the evidence briefly reviewed earlier, we expect participants in the PGD sample to present a small-to-moderate disadvantage in the matrices and BNT tests, and much stronger dysfunctional/distorted gambling-related cognitions, relative to participants in the NGD sample. Yet, our main hypotheses, specifically regarding the relationships between BNT/matrices scores and gambling-related cognitions, remain open. Firstly, across samples, we will estimate the independent contribution of domain-general reasoning scores to the five domains of gambling-related cognition. Secondly, associations (or their absence) between reasoning and gambling-related cognitions will be tested in the two samples separately. Support for the existence (H_1) or inexistence (H_0) of such links will be assessed using Bayes factors.

MATERIALS AND METHODS

Participants

The study sample comprised 135 participants, divided in 77 treatment-seeking patients with gambling disorder (PGD) and 58 participants with non-problematic gambling involvement (NPG). Characteristics of the two samples are reported in **Table 2**. Participants in the PGD group had a diagnosis of gambling disorder, as established by their therapist based on DSM5 criteria, and they had abstained from gambling for 15 days or more. The NPG group consisted of individuals with different degrees of involvement in gambling activities (with the minimum being "having ever gambled"). A specific exclusion criterion for NPG was presenting a gambling pattern severe enough to be classified as a disordered gambler [i.e., $\geq$5 in SOGS; (Stinchfield, 2002)]. The rest of exclusion criteria were similar for both groups, i.e., having ever been diagnosed or treated for any psychopathology (beyond gambling disorder in the case of PGD), and any history of neurological disease or brain trauma causing unconsciousness for 10 min or longer. Common exclusion criteria were assessed with a semi-structured interview.

Procedure

Patients with gambling disorder were recruited from different associations of rehabilitated gamblers in Andalucía (Spain), whereas NPG were recruited using convenience and snowball sampling methods among researchers' and patients' acquaintances, and using advertisements.

All participants were recruited across different phases of a more ambitious multi-stage research project (GBrain, and GBrain-2, see section "Funding"), with the different stages having slightly different aims and assessment protocols (with some measures being common to all phases and others present in only some of them). The participants included in the present study were the ones from all the phases of the project that were assessed with both the Matrices test for abstract reasoning, and the BNT for probabilistic reasoning (i.e., the two main independent variables involved in the hypotheses articulated earlier).

TABLE 2 | Descriptive statistics of all measured variables, and Bayes factors (based on the non-parametric Mann-Whitney statistic for quantitative variables and a Bayesian contingency table test for gender) and *p*-values (Welch's *t*-tests for quantitative variables, and χ^2-test for gender) for the differences between samples (patients with gambling disorder vs. individuals with non-problematic gambling).

	Sample	Mean	SD	Min.	Max.	BF_{10}	p
Gender	PGD	2F/75M				0.60	0.733
	NGD	1F/57M					
Age	PGD	36.18	11.42	19	61	0.29	0.142
	NGD	33.62	8.75	20	61		
Education ys.	PGD	12.34	3.92	5	24	0.95	0.064
	NGD	13.48	3.19	7	20		
Matrices	PGD	97.08	16.31	65	130	3.79	0.008
	NGD	104.31	14.61	75	140		
BNT	PGD	0.82	0.96	0	3	1.64	0.011
	NGD	1.26	1.00	0	3		
Expectancy	PGD	3.95	1.68	1	7	>100	<0.001
	NGD	1.49	0.71	1	4		
Inability to stop	PGD	4.26	1.66	1	7	>100	<0.001
	NGD	1.19	0.51	1	4		
Control illusion	PGD	2.59	1.40	1	7	>100	<0.001
	NGD	1.25	0.52	1	4		
Predictive control	PGD	3.75	1.53	1	7	>100	<0.001
	NGD	1.48	0.64	1	4		
Interpretative bias	PGD	4.75	1.79	1	7	>100	<0.001
	NGD	1.50	0.86	1	5		
SOGS	PGD	10.35	2.99	3	17	>100	<0.001
	NGD	0.62	0.93	0	3		

Across phases, PGD and NPG participants were sampled from similar social milieus, and groups were intendedly matched in sociodemographics, including gender, age and education years (but not psychological/cognitive characteristics; please see complementary information about matching in the section "Preliminary Analyses").

In all phases, the protocol consisted of a set of questionnaires and neuropsychological tasks, administered in a quasi-randomized order, in a single session that lasted approximately 2 h. Some participants were invited to participate in an extra session in a different day, in which psychophysiological or neuroimaging measures were recorded. There is thus some overlap between the current sample and the one in other studies of our research group: Megías et al. (2018), 33.3%; Navas et al. (2016, 2017b), 60%; Perales et al. (2017), 47.4%; Perandrés-Gómez et al. (2020), 97%; Ruiz de Lara et al. (2018), 34.1%; and Navas et al. (2017a), 52.6%.

Participants were debriefed about study aims and signed an informed consent prior to their participation, and received a €10/hour compensation. In the case of patients, the compensation was paid via an authorized relative. The study was approved by the Ethic Committee of the University of Granada and complied with the Helsinki Declaration.

Instruments

Matrix Reasoning Task [WAIS-IV (Wechsler, 2008)]

This instrument consists of 26 sequences of geometric figures, with each one following a unique organizational pattern, and a blank cell. Participants are asked to guess the underlying logic in the sequence, and to fill the blank cell with the option that best fits among the five possible alternatives. This is a standardized task that has excellent psychometric properties and is adapted for Spanish populations (Wechsler, 2012).

Berlin Numeracy Test [BNT (Cokely et al., 2012)]

This is a paper-and-pencil test in which participants are asked to answer 4 different questions on probability in ascending order of difficulty [e.g., *Imagine we are throwing a five-sided die 50 times. On average, out of these 50 throws, how many times would this five-sided die show an odd number (1, 3 or 5)*?]. A final score of numeracy skills is calculated as the sum of correct answers.

Gambling-Related Cognitions Scale [GRCS; Raylu and Oei, 2004; Spanish version: Del Prete et al., 2017)]

This is a self-reported measure of gambling-related cognition based on Raylu and Oei's model. It consists of 23 items to be answered using a five-point Likert scale that assess five cognitive distortions: inability to stop gambling (e.g., *My desire to gamble is so overpowering*), gambling expectancies (e.g., *Gambling makes things seem better*), predictive control (e.g., *Losses when gambling, are bound to be followed by a series of wins*), illusion of control (e.g., *I have specific rituals and behaviors that increase my chances of winning*), and interpretative bias (e.g., *Relating my winnings to my skill and ability makes me continue gambling*). Given that individuals in the PGD group had been in therapy for some time (from 15 days to 6 months), these participants were specifically instructed to refer their answers to the GRCS items to the time when they initiated treatment [see also (Navas et al., 2017a)].

This scale has shown good psychometric properties (Del Prete et al., 2017). In the present study, internal consistency values (Cronbach's α) were 0.866, 0.914, 0.709, 0.826, and 0.920 for gambling expectancies, inability to stop, illusion of control, predictive control and interpretive bias, respectively, and 0.963 for the total scale.

South Oaks Gambling Screen [SOGS (Lesieur and Blume, 1987); Spanish Version (Echeburúa et al., 1994)]

This instrument was used to assess disordered gambling symptoms' severity. The Spanish version has shown good psychometric properties. For this study, SOGS showed an excellent level of internal consistency (Cronbach's α = 0.929).

Statistical Analyses

Descriptive statistics are provided for age, education years, gender composition, WAIS-IV matrices scores, BNT scores, SOGS total severity scores, and the five dimensions of the GRCS questionnaire (gambling expectancies, inability to stop, control illusion, predictive control, and interpretative bias). For quantitative or quasi-quantitative variables, these descriptives include mean, standard deviation, and maximum and minimum values. These descriptives are complemented with Bayesian and frequentist tests to check for differences between participants showing non-problematic gambling involvement (NPG) and patients with gambling disorder (PGD). Scores in the five dimensions of the GRCS are submitted to a first multivariate analysis of covariance (MANCOVA), with group (sample: PGD, NPG) as a between-participant factor, and WAIS-IV matrices score as a continuous predictor. These are followed by GRCS dimension-by-dimension analyses of covariance (ANCOVA), with the same independent variables. The same analyses will be performed with BNT (instead of matrices) scores as continuous predictor.

Given the nature of the dependent variables involved, these analyses are likely to be affected by two limitations: (a) violation of homogeneity of covariance matrices and multivariate normality assumptions, and (b) the unsuitability of null-hypothesis significance testing (NHST) to provide evidence in favor of the null hypothesis. In view of that, non-parametric correlations (Kendall's τ) will be computed for correlations of each GRCS subscore with matrices and BNT scores. These correlations will be interpreted using bidirectional Bayes factors (BF_{10}) instead of NHST.

Regarding these statistical analyses, there are two important points that require further consideration. First, we did not use stratified sampling (or any other method to ensure populational representativity; see section "Limitations and Final Remarks"), but the sampling strategy and the inclusion/exclusion criteria were very similar for the two groups, and we did not force matching on psychological/cognitive variables [please see Perandrés-Gómez et al. (2020), for a discussion on the consequences of IQ non-matching in cross-sectional analyses of a sample largely overlapping with the present one]. Using convenience samples of gamblers with and without gambling problems is quite a standard practice in correlational research in the field (Barrada et al., 2019). Still, and in order to surpass the problems that this sample composition may cause, we ran analyses with the whole sample, while controlling for group (first part of the section "Main Analyses"), with the whole sample without controlling for group (**Supplementary Materials**), and with the two groups separately (second part of the section "Main Analyses"). As detailed below, results were robust across statistical approaches.

And second, please note that frequentist tests are aimed at checking for statistical significance of effects (i.e., whether the observed test statistic is extreme enough for the null hypothesis to be rejected), so null results can be explained as resulting from either the absence of an effect or the lack of power of the test. That implies that frequentist tests cannot distinguish between evidence of absence and absence of evidence (Altman and Bland, 1995). In the present study, however, we are as much interested on the possible inexistence of certain relations as we are in their existence. Bayesian tests expressed in the form of Bayes factors (BF_{10}) are aimed at comparing two models of the world, one in which the effect of interest is zero, and another one in which it is non-zero (with a given probability density distribution over the populational effect size). These two models representing the null and the alternative hypothesis are treated symmetrically, in such a way that $BF_{10} < 1$ is interpreted as supporting the null, whereas $BF_{10} > 1$ is interpreted as supporting the alternative. The arbitrary thresholds to consider evidence in favor of one or the other substantial enough vary across reference guidelines, so BFs will be interpreted here as strictly continuous measures of evidence (Dienes, 2014). For a discussion on equivalence tests and Bayes factors as tools to establish evidence for the null, see Lakens et al. (2020).

Data and reproducible analysis files are fully available in the OSF framework[1].

RESULTS

Preliminary Analyses

Table 2 shows group means, maximum, and minimum values, and standard deviations for age, education years, matrices, BNT, SOGS severity, and GRCS dimensions scores; proportions for gender; as well as Bayes factors and *p*-values for differences between groups in all variables. Detailed distributions for all these variables across groups are reported in the **Supplementary Materials**.

As expected, the two groups differed in SOGS and GRCS scores, and were closely matched in gender composition and mean age. Although education years was also controlled across phases of the project, the pooling of samples across phases made the difference between groups in this variable to get close to the significance threshold (p = 0.064), and to yield a virtually uninformative BF ($BF_{10} \approx 1$).

[1] https://osf.io/8ksxa/

The two groups, however, differed in both Matrices and BNT scores. In other words, differences in reasoning abilities remained in spite of control of sociodemographic variables. Actually, a MANCOVA with BNT and matrices scores as dependent variables, group as independent variable, and sociodemographics (age, gender, and education years) as covariates yielded significant effects for both the multivariate effect (Wilks' λ = 0.910, p = 0.002), and the univariate effects [F (1, 130) = 8.109, p = 0.005; and F (1, 130) = 8.335, p = 0.005, for matrices and BNT scores, respectively]. In other words, despite sociodemographic matching, general reasoning scores remained associated with GD, which is in line with the abovementioned evidence of links between reasoning abilities and risk of being diagnosed with GD.

Main Analyses

The MANCOVA with group as between-participants factor, matrices score as continuous predictor, and GRCS subscores as dependent variables, yielded a significant effect for group, Wilks' λ = 0.378, F (5, 128) = 42.181, p = 0.001, but not for the matrices score, Wilks' λ = 0.991, F (5, 128) = 0.231, p = 0.948. **Table 3** (left panel) shows the results of separate ANCOVAs for the five GRCS dimensions. In accordance with the global MANCOVA, all dependent variables showed significant effects of group, but not of matrices score.

Similarly, the MANCOVA with group as between-participants factor, BNT score as continuous covariate, and GRCS subscores as dependent variables yielded a significant effect for group, Wilks' λ = 0.374, F (5, 128) = 42.884, $p < 0.001$, but not for the BNT score, Wilks' λ = 0.977, F (5, 128) = 0.607, p = 0.695. **Table 3** (right panel) shows the results of separate ANCOVAs for the five GRCS dimensions. In accordance with the global MANCOVA, all dependent variables showed significant effects of group, but not of BNT score[2].

The Box's test [χ^2 (15) = 201, $p < 0.001$], and the Shapiro-Wilks' test [$W = 0.875$, $p < 0.001$], showed clear violations of the homogeneity of covariance matrices and multivariate normality assumptions, respectively. In view of that, we computed non-parametric correlations (Kendall's τ) between reasoning abilities and GRCS dimensions for the two groups separately, and interpreted the evidence portrayed by them using bidirectional Bayes factors (BF_{10}), computed with the default settings in JASP software (JASP Team, 2019). **Figure 1** and **Table 4** show the results of these analyses for the PGD and the NPG group, respectively.

As expected, in both groups, substantial correlations were found between the different subdimensions of GRCS. In the NPG group, the SOGS score correlated positively with all GRCS dimensions, with the strength of evidence for H_1 ranging from BF_{10} = 2.36 to $BF_{10} > 100$. Correlations between SOGS and GRCS were weaker in the PGD group, with only three BFs above 1, i.e., for inability to stop (BF_{10} = 7.59), interpretative bias (BF_{10} = 6.83), and predictive control (BF_{10} = 1.93, anecdotal)[3]. BNT and matrices also correlated positively between them, and with education years, and negatively with age.

Most importantly, BFs for correlation coefficients between reasoning abilities (matrices and BNT) and GRCS scores mostly provided moderate ($BF_{10} < 0.33$) evidence in favor of the null hypothesis. The only exceptions (i.e., $BF_{10} > 1$) were the BF_{10} = 15.04, Kendall's τ = 0.27 between BNT and gambling expectancies, the BF_{10} = 3.41, Kendall's τ = 0.22 between BNT

[2]These MANCOVAs were re-run including either education years or age as covariates, and allowing both covariates to interact with either BNT or Matrices (i.e., estimating the possible dependence of the effect of reasoning abilities on age or education years). Results from those analyses are reported in the second section, **Supplementary Table 1** and **Supplementary Figure 2**. The potential relevance of these results is detailed in the section "Discussion."

[3]The different size of correlations in the two groups could be due to range restriction in the SOGS scale. As shown in **Supplementary Figure 1**, GRCS scores showed a large range of variation in the PGD group, but SOGS scores in that group were more tightly concentrated around the mean (M = 10.35, SD = 2.99), which results from the fact that PGD in our study were severe enough to have sought treatment. Still, results in this group are also consistent with the fact that inability to stop and interpretative biases are the best GRCS indicators of severity at pathological levels (Michalczuk et al., 2011; Del Prete et al., 2017).

TABLE 3 | Results of separate ANCOVAs for GRCS dimensions as dependent variables, and Group and continuous predictors (left: WAIS matrices, right: BNT) as independent variables.

		WAIS matrices			*BNT*		
IV	*DV*	*MSE*	*F (1, 132)*	*P*	*MSE*	*F (1, 132)*	*p*
Group	*EXP*	1.836	108.850	< 0.001	1.812	110.305	< 0.001
	IS	1.707	182.762	< 0.001	1.691	184.538	< 0.001
	CI	1.245	47.697	< 0.001	1.239	47.950	< 0.001
	PC	1.517	112.572	< 0.001	1.506	113.375	< 0.001
	IB	2.160	161.027	< 0.001	2.119	164.100	< 0.001
Covariate	*EXP*	1.836	0.234	0.629	1.812	2.001	0.160
(Matrices/BNT)	*IS*	1.707	0.006	0.938	1.691	1.289	0.258
	CI	1.245	0.072	0.789	1.239	0.772	0.381
	PC	1.517	0.085	0.772	1.506	1.027	0.313
	IB	2.160	0.124	0.725	2.119	2.645	0.106

IV, Independent variable; DV, Dependent Variable; EXP, Gambling Expectancies, IS, Inability to Stop, CI, Control Illusion, PC, Predictive Control, IB, Interpretative Bias, BNT, Berlin Numeracy Test.

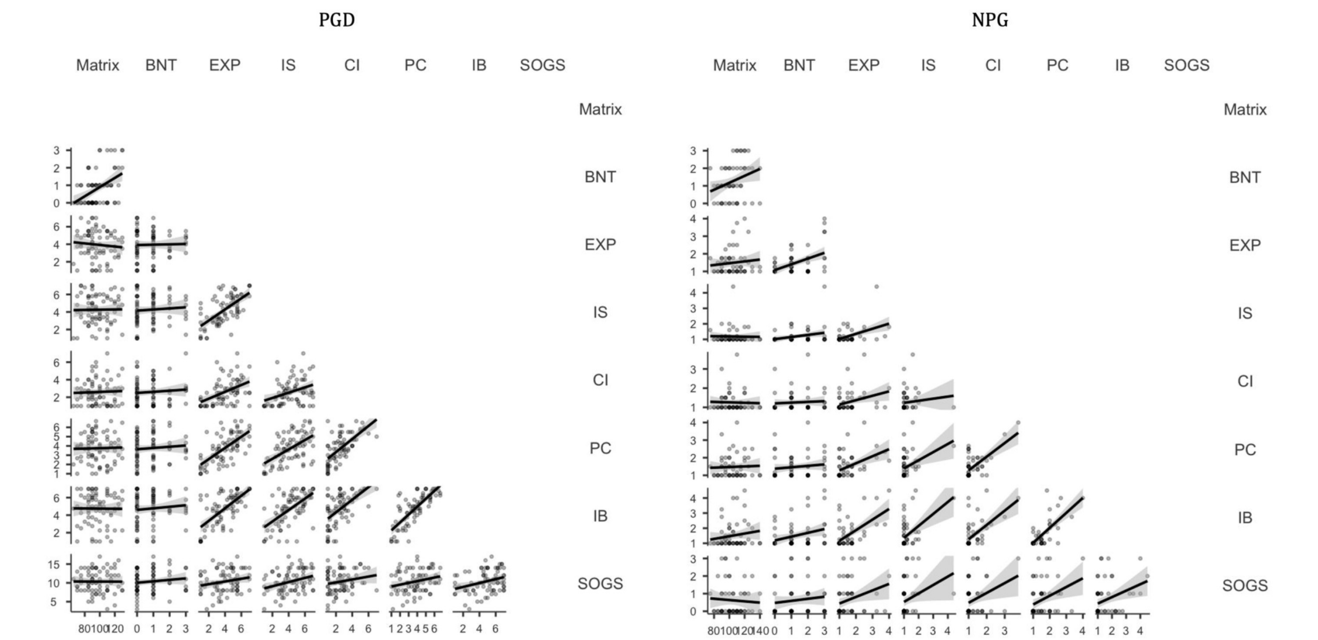

FIGURE 1 | Graphic depiction of the correlation matrix for all variables of interest across groups (PGD, patients with gambling disorder; NPG, Individuals in non-problematic gambling; Matrix, WAIS matrices scores; BNT, Berlin Numeracy Test; EXP, Gambling Expectancies; IS, Inability to Stop; CI, Control Illusion; PC, Predictive Control; IB, Interpretative Bias; SOGS, Gambling severity).

and inability to stop, and the $BF_{10} = 1.64$ (anecdotal), Kendall's $\tau = 0.19$ between BNT and interpretative bias, in the NPG group. In other words, there is some weak evidence of a direct link between BNT and some gambling-related cognitions (mainly excluding gambling biases) in the NPG group, with stronger cognitions in individuals with higher BNT scores. There were not any cases in which evidence supported an inverse relationship between reasoning abilities and gambling-related cognitions.

DISCUSSION

The goal of the present study was to explore the relationships between domain-general reasoning abilities and gambling-related cognitions in non-problematic gamblers (NPG) and patients with gambling disorder (PGD). Results from NHST (MANCOVAs on the association between BNT/Matrices and gambling-related cognitions, and subsequent dimension-by-dimension ANCOVAs) did not yield any significant associations. This result holds regardless of whether group (PGD, NPG) was included in the model or not. Subsequent Bayesian analyses yielded consistent support for the null hypothesis, i.e., no association between BNT/Matrices and gambling-related cognitions, except for anecdotal-to-substantial support for positive associations in the NPG subsample between BNT, on the one side, and gambling expectancies, inability to stop, and interpretative bias, on the other.

These results converge with the ones of some previous works. For instance, Perales et al. (2017) found gamblers with stronger biases to perform better in a causal learning task than those with weaker biases. This result was interpreted as originating in the fact that gambling-related cognitive distortions are significantly more intense in gamblers preferring skill-based games (i.e., sports betting, casino and card games) than in those preferring chance games (i.e., slots, bingo, or lottery) [see also (Myrseth et al., 2010; Navas et al., 2017b; Mallorquí-Bagué et al., 2019)]. Individuals preferring skill-based games are, on average, younger, better educated, and more sensitive to reward (Navas et al., 2017b), so that their distorted beliefs about gambling are unlikely to be originated in any general-domain reasoning disadvantage. Relatedly, Xue et al. (2012a) found students with higher cognitive abilities (intelligence and executive function) to be more prone to show the gambler's fallacy. And in Lambos and Delfabbro (2007) disordered gamblers were found to be more susceptible to cognitive biases than non-gamblers and non-disordered gamblers, but no significant differences were observed between the three groups for their knowledge of gambling odds [see also (Benhsain et al., 2004)].

This lack of substantial inverse relationships between domain-general reasoning abilities and gambling-related cognitions renders two theoretical puzzles unresolved. First, to describe the mechanisms responsible for bias generation and their activation during and between gambling sessions; and, second, accounting for the seemingly robust link between domain-general cognitive abilities and the risk developing gambling problems, *without* the mediation of gambling-related distorted cognitions.

With regard to the first question, a possible solution arises from the cognitive switching (Sévigny and Ladouceur, 2003) hypothesis. According to this hypothesis, individuals with disordered gambling "switch off" their rational beliefs during

TABLE 4 | Bayesian correlation tests (bidirectional Bayes factors for Kendall's τ) between variables of interests in PGD and NPG samples.

		Age	*Education*	*Matrices*	*BNT*	*EXP*	*IS*	*CI*	*PC*	*IB*
PGD										
Education	τ	−0.31								
	BF_{10}	>100								
Matrices		−0.07	0.40							
		0.22	>100							
BNT		−0.27	0.41	0.34						
		48.47	>100	>100						
EXP		−0.03	−0.04	−0.08	−0.02					
		0.16	0.18	0.27	0.15					
IS		0.00	−0.09	−0.02	0.07	0.46				
		0.15	0.28	0.16	0.22	>100				
CI		−0.08	−0.04	0.00	0.05	0.38	0.27			
		0.24	0.16	0.15	0.19	>100	67.17			
PC		−0.14	0.05	0.00	0.07	0.49	0.40	0.51		
		0.71	0.18	0.15	0.22	>100	>100	>100		
IB		−0.10	0.00	−0.02	0.05	0.50	0.47	0.42	0.61	
		0.35	0.15	0.16	0.18	>100	>100	>100	>100	
SOGS		−0.06	−0.05	0.00	0.11	0.13	0.22	0.11	0.18	0.22
		0.20	0.19	0.15	0.38	0.53	7.59	0.42	1.93	6.83
NPG										
Education	τ	−0.18								
	BF_{10}	1.20								
Matrices		0.23	0.11							
		4.24	0.36							
BNT		0.05	0.29	0.25						
		0.19	26.54	7.42						
EXP		−0.10	0.05	0.05	0.27					
		0.30	0.19	0.20	15.04					
IS		0.03	0.03	−0.04	0.22	0.35				
		0.18	0.18	0.19	3.42	>100				
CI		0.03	−0.04	−0.02	0.12	0.33	0.32			
		0.18	0.19	0.18	0.39	>100	76.20			
PC		−0.16	0.07	0.02	0.13	0.44	0.38	0.40		
		0.82	0.23	0.17	0.49	>100	>100	>100		
IB		−0.13	0.08	0.11	0.19	0.42	0.37	0.55	0.62	
		0.48	0.24	0.35	1.64	>100	>100	>100	>100	
SOGS		−0.02	−0.15	−0.03	0.16	0.36	0.37	0.21	0.30	0.32
		0.17	0.69	0.18	0.76	>100	>100	2.36	45.14	81.42

EXP, Gambling Expectancies; IS, Inability to Stop; CI, Control Illusion; PC, Predictive Control; IB, Interpretative Bias; BNT, Berlin Numeracy Test; SOGS, Gambling symptoms' severity.

gambling, so that their behavior becomes governed by features of the game or the gambling device, and "switch them on" again when they finish. In other words, in-game behavior and cognitions remain impermeable to general-domain reasoning.

The cognitive switching hypothesis is inspired by dual-process models of cognition, according to which two competing systems, the intuitive and the analytic, filter the information necessary to control action. The intuitive system is regarded as fast, efficient, and heuristic-based, whereas the analytic system is slower and more effortful, but also more rational (Armstrong et al., 2020). The term *cognitive reflection* has been coined to denote the degree to which an individual is more or less willing to invest the necessary cognitive resources to engage in analytic thinking [see (Stange et al., 2018), for a discussion of its potential link with gambling]. Importantly, being less prone to cognitive reflection, especially under certain environmental and affective circumstances, does not imply having poorer reasoning abilities, but somehow eschewing the effort to use them, especially when motivated to do so. In words of Armstrong et al. (2019), "gamblers are often unlikely or unwilling to reflect on the veracity of beliefs as they are often used to justify gambling behaviors" (p. 183) [see also (Emond and Marmurek, 2010; Armstrong et al., 2019; Cosenza et al., 2019)]. This mechanism reminds of the "tilt" phenomenon in poker (Barrault et al., 2014), and some recent studies using functional magnetic resonance imaging (fMRI) (Xue et al., 2011), and transcranial direct current stimulation (tDCS) (Xue et al., 2012b) also indirectly support it.

A second, non-exclusive possibility is that some gamblers do remain reflective during gambling episodes, but they invest their cognitive resources in trying to "outsmart" the gambling device, and to find causal patterns where there are not any. Indirect evidence supporting this mechanism comes from the abovementioned reports that, especially in some sociodemographic sectors, individuals with preserved –or even superior– cognitive skills are more vulnerable to certain gambling-related fallacies. To our knowledge, there is no direct evidence of this mechanism, although the deleterious effects of trying to outsmart random devices on judgment and decision-making are well known [see (Gaissmaier and Schooler, 2008)].

That connects with a third possibility, emerging from the putative interaction of domain-general reasoning skills with age and/or education. Actually, when matrices scores were allowed to interact with age and education years (see **Supplementary Materials**, second section), some non-significant trends suggested that, in younger and more educated individuals, matrices scores were positively associated with GRCS scores, whereas in older and less educated individuals the association was non-existing or in the opposite direction. It is definitely premature to make any inferences from these trends, but they open the possibility that in younger, more educated people, distorted gambling cognitions were fueled by domain-general reasoning skills, whereas in older, less educated gamblers, poorer reasoning skills were a risk factor for developing gambling-related biases. Additionally, this interaction would explain why some studies have found no associations whatsoever between reasoning skills and gambling-related biases, whereas others have found a direct link (Xue et al., 2012a; Perales et al., 2017).

In summary, low domain-general reasoning skills are not necessary to develop gambling-related distorted beliefs, which reinforces the idea that, at least in some gamblers, in- or about-game emotion-laden states (e.g., urges triggered by conditioned cues, or negative affect caused by losses) can take control over gambling-related cognition, and probably motivate the individual to stick to irrational cognitions. Such possibility is one of the main tenets of the GSM, according to which the main source of gambling-related cognitive distortions is motivated reasoning, that is, the individual's tendency to regulate affect by overestimating their degree of control or reinterpreting gambling outcomes in a more favorable, ego-protecting light (Navas et al., 2017b, 2019; Ruiz de Lara et al., 2019). Whether this motivated reasoning mechanism is specific to some gamblers (more educated, younger ones) or generalizes to a wider range of individuals remains an open question for future research.

The second puzzle, namely the moderate but seemingly robust relationship of intelligence and abstract reasoning with gambling problems without the mediation of gambling related cognitions, seems more difficult to address. In our sample, this link held for GD diagnosis across groups, but not for severity of gambling problems within groups, and its interpretation is limited by features of the design. This result resonates with the one from Rai et al. (2014), in which a link between IQ and gambling problems was also corroborated at the populational level, but no association was found between IQ and non-problematic gambling. Unfortunately, none of the possible explanations for this link has been explored in detail. Tentatively, the association between poorer reasoning abilities and a higher risk of developing gambling problems can be partially accounted for by the overlap between these abilities and aspects of executive function as self-control and top-down regulation of impulses (Meldrum et al., 2017). A detailed review of the role of executive functions related to cognitive control in gambling problems, and its neurobiological correlates, can be found at Moccia et al. (2017).

Clinical implications of our results, and the abovementioned related ones, are far-reaching. Gambling-related cognitions are hard to restructure, and the efficacy of cognitive therapy, although well-established, remains modest (Petry et al., 2017). Furthermore, individuals with problematic gambling are normally reluctant to change their beliefs when faced with disconfirming evidence, and often counterargument it (Delfabbro et al., 2006). In a variety of domains, this sort of reluctance has been related to the fact that, when motivated to maintain a given belief, individuals perceive information disconfirming it as confronting or uncomfortable (Gilbert et al., 1990; Mezirow, 1990; Stange et al., 2018). In consequence, altering beliefs will not only require more (or more accurate) information, but an increased degree of metacognition about how motives to gamble and to regulate emotions derived from gambling (and its consequences) relate to one's beliefs (Wells, 2009; Lindberg et al., 2014b; Caselli and Spada, 2016).

Limitations and Final Remarks

Results of our study should also be understood considering at least five main limitations. First, we cannot establish causal associations between the variables examined, since this is a cross-sectional study. Second, since the majority of the participants are male, generalizability to the entire population of gamblers should not be taken for granted. Third, assessing psychological constructs using self-report questionnaires may not fully represent the cognitive processes involved, and social desirability effects are possible. Fourth, no power analysis was performed *a priori* to determine sample size. As noted earlier, participants in this study were the ones in a larger project who had been assessed with all the measurements of current interest. This problem is, however, partially palliated by the use of Bayes factors, that provide evidence in support of the null or the alternative hypothesis in a continuous fashion, so that no dichotomous decisions leading to type I or type II errors are made. And fifth, we did not use stratified sampling (or any other method to ensure populational representativity), which means that the sampling strategy and the inclusion/exclusion criteria were very similar for the two groups, and we did not force matching on psychological/cognitive variables. That implies that the proportion of PGD in our sample is much larger than in the general population, but there are no reasons to expect substantial alterations of the correlations between psychological variables. Given that there is an association between gambling problems, on the one hand, and both stronger gambling-related biases and lower reasoning skills, on the other, the overrepresentation of PGD could have artificially inflated correlations between the latter when group was not controlled for (supplementary analyses). Despite this risk of inflation, gambling-related cognitions and domain-general reasoning remained mostly disconnected.

On the side of strengths, although some previous studies had explored the relationship between reasoning abilities and gambling-related beliefs, to our knowledge, this is the first one simultaneously assessing two core constructs of domain-general reasoning directly relevant to gambling (abstract and probabilistic reasoning), and their relationship with different dimensions of gambling-related cognitions in individuals without problem gambling and patients with gambling disorder. Additionally, the inclusion of Bayesian analyses allows to symmetrically assess the evidential support in favor of the null or the alternative hypothesis. Our results evidence that probabilistic and abstract reasoning abilities are mostly unrelated to the intensity of distorted gambling-related beliefs, and are thus unlikely to protect gamblers from them. This pattern or results reinforces the idea that distorted cognitions do not originate in a general lack of understanding of probability or low fluid intelligence, but probably result from motivated reasoning.

DATA AVAILABILITY STATEMENT

The datasets presented in this study can be found in the OSF repository: https://osf.io/8ksxa/.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by Ethic Committee of the University of Granada. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

JP, JN, and IM: conceptualization and writing–review and editing. JP: formal analysis. JP and JN: methodology. IM and JN: writing – original draft. All authors contributed to the article and approved the submitted version.

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SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2020.611784/full#supplementary-material

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Fear From Afar, Not So Risky After All: Distancing Moderates the Relationship Between Fear and Risk Taking

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A growing line of research has shown that individuals can regulate emotional biases in risky judgment and decision-making processes through cognitive reappraisal. In the present study, we focus on a specific tactic of reappraisal known as *distancing*. Drawing on appraisal theories of emotion and the emotion regulation literature, we examine how distancing moderates the relationship between fear and risk taking and anger and risk taking. In three pre-registered studies (N_{total} = 1,483), participants completed various risky judgment and decision-making tasks. Replicating previous results, Study 1 revealed a negative relationship between fear and risk taking and a positive relationship between anger and risk taking at low levels of distancing. Study 2 replicated the interaction between fear and distancing but found no interaction between anger and distancing. Interestingly, at high levels of distancing, we observed a reversal of the relationship between fear and risk taking in both Study 1 and 2. Study 3 manipulated emotion and distancing by asking participants to reflect on current fear-related and anger-related stressors from an immersed or distanced perspective. Study 3 found no main effect of emotion nor any evidence of a moderating role of distancing. However, exploratory analysis revealed a main effect of distancing on optimistic risk estimation, which was mediated by a reduction in self-reported fear. Overall, the findings suggest that distancing can help regulate the influence of incidental fear on risk taking and risk estimation. We discuss implications and suggestions for future research.

Keywords: judgment and decision making, emotion regulation, psychological distance, cognitive reappraisal, incidental emotions, risk taking, self-distancing

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INTRODUCTION

Studies in the last couple of decades have provided significant insight into the complex ways in which emotions influence judgments and decisions. Although emotions serve as sources of information that help individuals navigate through uncertainty, emotions can also "carry over" and influence judgments and decisions in a biasing way (Lerner et al., 2015). As a result, scientists have increasingly recognized the importance of identifying specific ways to minimize such biases (Lerner et al., 2015). While still in its infancy, an emerging and promising line of research has explored how various emotion regulation strategies influence risky decision making (Sokol-Hessner et al., 2009, 2013; Heilman et al., 2010; Miu and Crişan, 2011; Panno et al., 2013). The present study seeks to contribute to this developing line of research in several ways.

First and foremost, we examine a specific emotion regulation tactic that has received relatively little attention in judgment and decision-making research, namely, *distancing*. This tactic involves mentally changing the psychological distance of a stimulus to reduce its emotional impact (see Powers and LaBar, 2019). It has been associated with a range of emotional (Kross et al., 2014; Bruehlman-Senecal and Ayduk, 2015; Nook et al., 2017, 2020; Ahmed et al., 2018; Powers and LaBar, 2019; White et al., 2019) and cognitive benefits (Kross and Grossmann, 2012; Grossmann and Kross, 2014; Sun et al., 2018). Studies suggest that distancing requires less effort than other tactics and strategies, rendering it a promising tool in practical settings (Powers and LaBar, 2019). Second, the present study examines how distancing moderates the relationship between *incidental emotions*—emotions that are elicited from unrelated situations—and risk taking. Finally, we focus on specific emotions that can be expected to lead to opposite effects on risk; n, fear and anger (Lerner and Keltner, 2000, 2001; Lerner et al., 2015). It is worth emphasizing at the outset that in some situations, emotions can be highly adaptive. However, individuals might wish to down-regulate emotions where they can be expected to lead to judgments and decisions that are inconsistent with one's goals or values. Moreover, whether risk taking is beneficial or detrimental is not a question that we can answer in this study.

THEORY AND HYPOTHESES

Incidental Fear and Anger

As noted by Lerner et al. (2015), the majority of research on emotion and risky decision making has focused on valence (i.e., subjective feelings of pleasantness/unpleasantness). Valence-based models posit that emotions of the same valence (i.e., positive vs. negative emotions) have similar effects on risk perception. Appraisal theories, on the other hand, posit that emotions of the same valence can have opposite effects on judgments and decisions. Moving beyond dimensions of valence, the Appraisal Tendency Framework (ATF; Lerner and Keltner, 2000, 2001) focuses on distinct emotions (e.g., fear, anger, sadness, happiness) and their associated appraisals (i.e., evaluations of events and situations). Lerner and Keltner (2001) demonstrated that fear and anger, both of which are negative valence and high arousal (i.e., intense) emotions, have opposite effects on risky judgments and decisions due to their distinct underlying appraisals of certainty and control (Lerner and Keltner, 2001; Lerner et al., 2003; Habib et al., 2015; Ferrer et al., 2017; Wake et al., 2020). Fear reduces risk taking due to its appraisals of uncertainty and low personal control. In contrast, anger increases risk taking due to its appraisals of certainty and personal control (Lerner and Keltner, 2001).

Finally, studies that examine the influence of specific emotions like fear and anger on judgments and decisions usually adopt an *incidental emotion* approach. In contrast to integral emotions, which are elicited by the decision task at hand, incidental emotions are elicited by unrelated events that carry over to the decision-making process (for an in-depth distinction, see Västfjäll et al., 2016). For instance, anger triggered in one situation (e.g., anger stemming from bad traffic while driving to work) can carry over to influence judgments and decisions in unrelated settings (e.g., deciding to invest in a risky project without giving the decision sufficient thought). Unlike integral emotions which are "normatively defensible input to judgment and decision making" (Lerner et al., 2015, p. 803), incidental emotional influences are often unwanted.

Psychological Distance and Emotion Regulation

Trope and Liberman (2010) define psychological distance as "the subjective experience that something is close or far away from self, here and now" (p. 440). Psychological distance has been found to decrease emotional intensity (van Boven et al., 2010), and appears to be particularly effective in regulating basic emotions such as fear and anger (Katzir and Eyal, 2013). In a study by Davis et al. (2011), participants who imagined that aversive images presented on a screen were moving further away from them exhibited lower negative affect and physiological responses. Adopting a temporally distant perspective from future stressors has been associated with lower levels of anxiety and image vividness (White et al., 2019). Supporting these findings, Nook et al. (2017) demonstrated that participants who wrote about negative images using psychologically distant (vs. close) language in physical, social, and temporal domains exhibited lower negative affect. Bruehlman-Senecal and Ayduk (2015) found that participants who reflected on how they would feel about recent stressors in the distant future showed significantly lower emotional distress. Moreover, the authors found that an impermanence focus (e.g., focusing on how one's feelings might change with time) mediated this effect. Similar results have been found in studies examining individual differences in temporal distancing (Bruehlman-Senecal et al., 2016). Not only do these findings support folk sayings like "time heals all wounds," but they show that people can mentally project themselves into the future to reduce stressors in the here and now. Other studies have shown that distancing is also associated with cognitive benefits, such as wise reasoning (e.g., realizing the limits of one's knowledge and recognizing diverse perspectives; Kross and Grossmann, 2012; Grossmann and Kross, 2014). According to Construal Level Theory (CLT; Trope and Liberman, 2010), psychological distance exists across various dimensions, including temporal, social, and spatial distance. In terms of its emotion-regulatory function, it means that negative emotions can be downplayed by imagining that the emotional stimulus is temporally, physically, or socially far from the self. Indeed, distancing is a specific tactic of a general emotion regulation strategy known as *reappraisal* (see a taxonomy of distancing and emotion regulation by Powers and LaBar, 2019). Reappraisal involves changing one's mental representation of an emotion-eliciting stimulus to minimize its emotional impact. This can be done through either *reinterpretation* (e.g., thinking of a lay-off as an opportunity to pursue a more desirable career) or *distancing* (e.g., adopting the perspective of a distant, uninvolved participant when dealing with a personal conflict at work). Our review, however, is restricted to studies investigating the distancing tactic. Although both tactics have been found to be effective in regulating negative emotions, some evidence suggests that distancing is more effective than reinterpretation. For instance, Denny and Ochsner (2014) compared the effects of longitudinal training in distancing and reinterpretation. Compared to those who were trained in reinterpretation, participants who were trained in distancing showed lower levels of stress in daily life and were more likely to evaluate aversive content neutrally. Moreover, distancing seems to require less effort than reinterpretation because it does not target specific features of an emotion-eliciting stimulus (Moser et al., 2017). Thus, distancing may offer regulatory benefits across a broader range of situations. Although emotion regulation studies are typically restricted to the down-regulation of negative emotions, there are situations where one's goal might be to down-regulate positive emotions or up-regulate negative emotions (e.g., Tamir and Bigman, 2014; Tamir and Ford, 2009). For example, like anger, happiness can lead to excessive risk taking (Lerner and Keltner, 2001).

Psychological Distance and Risk

Only recently have studies started to explore the role of psychological distance in risky decision making. This small set of studies has tested how psychological distance, across various dimensions, impacts risk taking (e.g., Polman, 2012; Raue et al., 2015; Sun et al., 2017; Zhang et al., 2017). For instance, social distance (i.e., choosing for socially distant others) has been associated with reduced loss aversion (Polman, 2012; Andersson et al., 2014; Sun et al., 2017; Zhang et al., 2017). In a medical scenario about a deadly virus, people who chose for others showed a greater tendency to accept the vaccine than those who chose for themselves (Zikmund-Fisher et al., 2006). Similar results have been obtained in studies examining temporal distance. Chandran and Menon (2004) showed that "every day" framing made risks appear more proximal and concrete than "every year" framing, resulting in increased risk perceptions, intentions to engage in preventive behavior, and increased anxiety about hazards. Raue et al. (2015) manipulated psychological distance by varying the temporal, social, and spatial distance in decision scenarios. Across several experiments with students, physicians, and hotel managers, psychological distance reduced framing effects. Finally, Sun et al. (2018) similarly demonstrated that self-distancing (by adopting a distant observer's perspective) reduced probability-weighting biases.

The influence of psychological distance on risk is believed to result from a reduction in emotional intensity, as distance enables individuals to "zoom out" and transcend features of the here and now (Fujita et al., 2016). This notion is consistent with studies that have linked self-distancing to enhanced wise reasoning (Kross and Grossmann, 2012; Grossmann and Kross, 2014). These findings raise an interesting question; how does psychological distance shape the role of emotions like fear in decisions and judgments involving risk? A recent line of research provides a starting point. Although, it appears that these studies have either examined the general strategy of reappraisal or reinterpretation, not distancing. A study by Heilman et al. (2010) examined incidental regulation of fear and disgust on risk taking in the Balloon Analog Risk Task (BART) and Iowa Gambling Task (IGT). Participants were instructed to either reappraise or suppress their emotions while watching a fear-inducing or disgust-inducing video. As predicted, Heilman et al. (2010) found that reappraisal effectively reduced the influence of these two incidental emotions in both tasks. Similar results have been reported in studies examining integral emotion regulation and risk taking. Sokol-Hessner et al. (2009) found that instructing participants to adopt the perspective of a trader promoted risk taking by reducing physiological arousal. Building on these findings (Panno et al., 2013) found the same pattern of results for habitual reappraisal (i.e., naturally occurring individual differences in reappraisal). Specifically, habitual reappraisal was related to increased risk taking, accompanied by decreased sensitivity to changes in probability and loss amount. Yet, no study has directly tested how the distancing tactic of reappraisal regulates the influence of incidental emotions on judgments and decisions involving risk. This might be of particular interest in light of the benefits of distancing discussed in the previous section.

PRESENT RESEARCH

Few studies have examined how psychological distance moderates the influence of incidental emotions on judgments and decisions involving risk. Some of the studies covered earlier have manipulated distance by varying the proximity to targets in risky decision-making tasks (Chandran and Menon, 2004; Raue et al., 2015; Sun et al., 2017; Zhang et al., 2017) or instructed participants to adopt a distant perspective while completing a task (Sun et al., 2018). The authors behind some of these studies speculate that the impact of psychological distance on risk occurs via a reduction in emotional intensity (e.g., Raue et al., 2015; Sun et al., 2018). The present study aims to test this hypothesis by examining how distancing moderates the relationship between incidental emotions and risky judgments and decisions. More specifically, we focus on the regulation of fear and anger. A comparison between fear and anger is of theoretical interest since both are characterized by negative valence and high arousal (Smith and Ellsworth, 1985), but differ in their underlying appraisals (i.e., mental evaluations of a situation). While fear is characterized by appraisals of uncertainty and lack of control, anger is characterized by the opposite appraisal patterns. The ATF predicts that, because of their different appraisal patterns, fear should decrease risk taking whereas anger should increase risk taking. Thus, we predict that the opposing effects of anger and fear on risk taking will be particularly strong at low levels of distancing. We believe that this approach can help provide a more nuanced understanding of the role of emotion regulation in decision making, by showing that the impact of emotion regulation on judgments and decisions might depend on the target emotion.

Taken together, our study set out to examine how distancing moderates the influence of fear and anger on risk taking. Following our pre-registered hypotheses, we hypothesized that distancing would moderate the negative relationship between fear and risk taking, and the positive relationship between anger and risk taking. We conducted three pre-registered and high-powered studies to test these hypotheses. Study 1 tested the moderating role of habitual distancing on the relationship between trait fear and anger on risk taking. Study 2 experimentally manipulated distancing to examine whether trait fear and trait anger exert stronger effects on risk taking when decision scenarios are imagined as proximal. In other words, Study 2 examined how distancing from the decision-making task regulates the influence of incidental (trait) emotions. Finally, Study 3 manipulated both emotions (fear and anger) and distancing to examine how distancing from current fear-related and anger-related stressors carries over to impact subsequent risk taking.

ETHICS AND TRANSPARENCY STATEMENT

The three studies presented in this article received ethical approval from the Norwegian Center for Research Data (NSD) before data collection. Participants in each study provided their consent to participate. We report how we determined the sample size, all data exclusions, all manipulations, and all measures collected in this study (Simmons et al., 2012). We pre-registered each study on the Open Science Framework (OSF) prior to data collection. The pre-registrations, data, code, and materials associated with this paper are available on the OSF repository.[1]

STUDY 1

Method

Participants

A total of 400 participants were recruited from Amazon's Mechanical Turk (MTurk), using the CloudResearch platform that blocks low quality participants by default (Litman et al., 2017). MTurkers were eligible to participate only if they were currently residing in the US, were native English speakers, completed a minimum of 500 surveys, and had a 95% MTurk HIT approval rating. Participants were paid $1.20 for the roughly 10-min long study. Following the pre-registered exclusion criteria, the final sample included 370 participants (198 males, 171 females, 1 other/prefer not to answer; $M_{age} = 41.58$, $SD_{\text{age}} = 11.96$). Participants were excluded if they; spent <2 min on the entire survey, indicated low English proficiency, reported not being serious about filling in the survey, failed a bot check, failed two out of three attention checks, and if they had correctly guessed the purpose of the study. We estimated the sample size by performing an a-priori power analysis (using GPower 3.1.9.4) for a hierarchical linear regression model predicting risk preference. The power analysis indicated that we needed a sample of 355 participants to detect a small effect size ($f^2 = 0.05$; based on a meta-analysis by Wake et al., 2020). We entered the effect size estimate into the power analysis with the following input parameters: $\alpha = 0.05$, power = 0.90, number of tested predictors = 6.

[1] https://osf.io/hg358/?view_only=510f9016d0fc47c39488665fda8d14ab

Design and Procedure

Participants were randomly assigned to receive the risky decision-making tasks in either the gain frame or loss frame (see description below). At the start of the survey, they read a consent form and indicated their agreement. Those who agreed received a brief cover story to dissociate the emotion measures from the risk preference measures. Specifically, we told them that different researchers had pooled together their questions for efficiency purposes and that the survey contained two different questionnaires: a "Self-Evaluation" questionnaire and a second questionnaire about "Preferences." The trait emotions and habitual distancing measures (and items) were presented first, in random order.

Measures

Habitual Distancing

Individuals' general tendency to engage in distancing to regulate negative emotions was measured using the single-factor Temporal Distancing Questionnaire, developed by Bruehlman-Senecal et al. (2016). Across eight statements, participants indicated how they typically respond to negative events by taking a broad and distant perspective (1 = "strongly disagree," 7 = "strongly agree"). Example statements included "I generally don't take a step back from the event and place it in a broader perspective" (reverse-coded), "I focus on how my feelings about the event may change with time," and "I think about how small the event is in the bigger picture of my life." The scale demonstrated strong reliability ($\alpha = 0.88$).

Trait Fear

Dispositional fear was measured using the Penn State Worry Questionnaire (PSWQ; Meyer et al., 1990). Responses were measured on a 7-point Likert scale (1 = "not at all typical of me," 7 = "very typical of me"). All items were averaged to form a single variable. Example items included "If I do not have enough time to do everything, I do not worry about it" (reverse-coded), "My worries overwhelm me," and "I have been a worrier all my life." The PSWQ has been used in previous studies examining financial risk taking (Maner et al., 2007). The scale demonstrated strong reliability ($\alpha = 0.97$). Although some theorists conceptualize worry and fear as two different (albeit very similar) emotions (Öhman, 2008), the present study follows the common, broader conceptualization of fear as an emotion that encompasses worry and anxiety (e.g., Borkovec et al., 1998). Indeed, studies on fear and risk taking typically operationalize fear using measures of anxiety and worry. Furthermore, a recent meta-analysis by Wake et al. (2020) found no differences in the relationship between emotion and risk taking between studies that referred to "fear" and those that referred to "anxiety."

Trait Anger

We measured trait anger using the State-Trait Anger Expression Inventory (STAXI-II; Spielberger, 1999). Using a 10-item scale, participants rated the extent to which various behaviors were typical of them (1 = "almost never," 4 = "almost "always"). Items were averaged to form a single trait anger variable. The STAXI-II is commonly used in studies examining emotions and risk taking

(Lerner and Keltner, 2001; Gambetti and Giusberti, 2012, 2014). The scale demonstrated strong reliability (α = 0.90).

Risky Decision-Making Tasks

Participants were presented with three different framing problems that were modeled on the classic Unusual Disease Problem (Kahneman and Tversky, 1979)[2]: The Cancer Problem (Fagley and Miller, 1987), Plant Problem (Bazerman, 1984), and the Shareholding Problem (Teigen and Nikolaisen, 2009). Half of the participants received the three risky decision-making tasks in the gain frame, while the other half received them in the loss frame. In each task, participants read a scenario and indicated the extent to which they preferred one option over the other on a 7-point Likert scale (1 = "strongly prefer option A over option B," 7 = "strongly prefer option B over A"). Option A was always the safe option, and option B the risky option. Thus, for each participant, risk preference was measured three times. A full description of these tasks can be found on the OSF repository (see text footnote 1). For example, in the Plant Problem (adapted from Bazerman, 1984), participants read:

> A large hi-tech company is experiencing serious economic troubles and needs to lay off 6,000 employees. The vice president has been exploring alternative ways to avoid this crisis and has developed two plans:
> (*gain frame*)
> Plan A: This plan will save 2,000 jobs.
> Plan B: This plan has a 1/3 probability of saving all 6,000 jobs, but a 2/3 probability of saving no jobs.
> (*loss frame*)
> Plan A: This plan will result in the loss of 4,000 jobs.
> Plan B: This plan has a 2/3 probability of resulting in the loss of all 6,000 jobs, but a 1/3 probability of losing no jobs.

Control Variables. Following the pre-registration, age and gender were included as control variables. Previous research has found that males are more likely to engage in risky behavior and to respond to anger with risk taking (Ferrer et al., 2017). Furthermore, risk taking has also been found to decrease with age (Rolison et al., 2014). We also controlled for framing condition (0 = Gain frame, 1 = Loss frame) to account for potential differences in the influence of emotions in gain and loss frames. The subsequent studies use the same control variables.[3]

Statistical Analysis

A linear hierarchical multilevel model was fitted using the lme4 (Bates et al., 2014) and the lmerTest packages implemented in RStudio (R Core Team, 2014). Risk preference was predicted by the experimental manipulation (gain vs. loss frame), dispositional fear and anger, habitual distancing, and the interaction of habitual distancing with dispositional fear and anger. Participants and decision tasks were treated as random-intercept effects. The discussion will only focus on the final, overall model (i.e., Step 3). However, mean-centered beta coefficients and model fit statistics for each step of the regression are listed in **Table 1**. The choice of a linear mixed model deviated from the pre-registration, which specified the use of hierarchical multiple regression. A linear mixed model seemed more appropriate, however, as it accounts for repeated-measures dependencies—in this case, the repeated measure of risk preference across the three risky decision-making tasks. The results remain the same regardless of the analytical approach used. Assumptions of normality of residuals, linearity, and heteroscedasticity did not seem to be violated. For this and the two subsequent experiments, one-tailed p-values and confidence intervals are reported for the pre-registered directional hypotheses (Cho and Abe, 2013).[4] For all other tests, two-tailed p-values are reported. Descriptive statistics of key variables across the three studies can be found in the online repository (see text footnote 1).

Results

Hypotheses Testing

All continuous predictors were mean centered before running the analyses (Aiken et al., 1991). Adding "subject" and "scenario" as random effects significantly improved the model fit compared to the model without the random effects, supporting the rationale for using a mixed model. The results from the hierarchical multilevel analysis are summarized in **Table 1**.[5] Risk preference was significantly higher in the loss frame, $\beta = 0.44$, $p = 0.001$ (two-tailed), 95% CI [0.17, 0.72], thus, replicating the classic framing effect. Supporting the pre-registered directional moderation hypotheses, the final model indicated that habitual distancing significantly interacted with dispositional fear, $\beta = 0.10$, $p = 0.038$ (one-tailed), 90% CI [0.01, 0.20] and anger, $\beta = -0.25$, $p = 0.029$ (one-tailed), 90% CI [−0.46, −0.03] in the predicted directions. None of the simple slopes for the interaction between fear and distancing (low distancing: $\beta = -0.07$, $p = 0.51$, high distancing: $\beta = 0.16$, $p = 0.11$) and the interaction between anger and distancing (low distancing: $\beta = 0.34$, $p = 0.05$, high distancing: $\beta = -0.23$, $p = 0.38$) were significant. Moreover, contrary to our predicted main effects of fear and anger, neither dispositional fear nor anger alone predicted risk preference (fear: $\beta = 0.05$, $p = 0.28$ (one-tailed), 90% CI = −0.08, 0.18; anger: $\beta = 0.06$, $p = 0.36$ (one-tailed), 90% CI = −0.21, 0.32).

As shown in **Figure 1**,[6] for individuals low on habitual distancing, dispositional fear is negatively related to risk preference whereas dispositional anger is positively related to risk preference (see text footnote 5). Interestingly, this pattern is reversed for individuals high on habitual distancing. Specifically, at high levels of distancing, fear is *positively* related to risk preference whereas anger is *negatively* related to risk preference.

[2]We use the more contemporary label instead of Asian Disease Problem.

[3]The pre-registrations lacked the specification that framing would be used as a control variable. Excluding framing as a control variable from the Study 1 analysis did not significantly change the interaction between distancing and anger but rendered the interaction between distancing and fear insignificant. Excluding framing from the Study 2 analysis did not significantly change any of the two interactions.

[4]Although the Study 1 preregistration included directional hypotheses—which justifies the use of one-tailed tests (Cho and Abe, 2013)—it did not specify whether one-tailed or two-tailed tests would be used. However, Study 2 and Study 3 preregistrations have specified the use of one-sided testing.

[5]Table generated using the tab_model function in the "sjPlot" in R (Lüdecke, 2021).

[6]Plot created using the interact_plot() function in the "interactions" package in R (Long, 2020).

TABLE 1 | Summary of hierarchical linear mixed model analysis predicting risk taking (Study 1).

	Model 1		Model 2		Model 3	
Predictors	**Estimates**	**CI**	**Estimates**	**CI**	**Estimates**	**CI**
Intercept	3.17**	2.73–3.61	3.18**	2.75–3.62	3.18**	2.74–3.62
Age	−0.01	−0.02–0.00	−0.02	−0.02–0.01	−0.01	−0.02–0.00
Gender	−0.14	−0.42–0.14	−0.17	−0.45–0.12	−0.16	−0.45–0.12
Framing	0.43**	0.16–0.71	0.43**	0.16–0.71	0.44**	0.17–0.72
Anger			0.17	−0.08–0.42	0.06	−0.21–0.32
Fear			0.04	−0.10–0.17	0.05	−0.08–0.18
Distancing			0.13	−0.00–0.26	0.10	−0.03–0.24
Distancing × Anger					−0.25*	−0.46 to −0.03
Distancing × Fear					0.10*	0.01–0.20
Random Effects						
σ^2	2.12		2.12		2.12	
τ_{00}	1.13_{subject}		1.11_{subject}		1.08_{subject}	
	0.11_{scenario}		0.11_{scenario}		0.11_{scenario}	
ICC	0.37		0.36		0.36	
N	369_{subject}		369_{subject}		369_{subject}	
	3_{scenario}		3_{scenario}		3_{scenario}	
Observations	1,107		1,107		1,107	
Marginal R^2/Conditional R^2	0.018/0.379		0.024/0.379		0.031/0.379	

*Continuous predictors are mean-centered. *p < 0.05, **p < 0.01. One-tailed p-values and CIs are reported for the two hypothesized relationships (fear, anger, and their interactions with distancing).*
σ^2, within-person variance; τ_{00}, between-person variance; CI, confidence interval; ICC, intraclass correlation.

Thus, not only did distancing attenuate the relationship between fear and risk preference, but even reversed the relationship. These results are discussed later in the Discussion section.

Finally, following the pre-registered exploratory analyses, we also tested whether the interactions depended on the framing condition. Accordingly, a new model was tested that included two three-way interactions (fear*distancing*frame, anger*distancing*frame). None of the three-way interactions were significant (fear*distancing*frame: $\beta = -0.11$, $p = 0.383$ (two-tailed), 95% CI = −0.34, 0.13; anger*distancing*frame: $\beta = 0.23$, $p = 0.398$ (two-tailed), 95% CI = −0.30, 0.76). This is consistent with Lerner and Keltner (2001), who argued that the opposite effects of fear on anger (i.e., fear increasing risk aversion and anger increasing risk taking) should hold regardless of framing.

Discussion

Study 1 examined whether habitual distancing (i.e., individuals' general tendency to adopt an objective and distant perspective when faced with negative events) moderates the influence of dispositional fear and anger on risk taking. Drawing on the ATF (Lerner and Keltner, 2001) and a developing line of research on emotion regulation and decision making (e.g., Heilman et al., 2010; Miu and Crişan, 2011; Panno et al., 2013), it was predicted that fear would be negatively related—and anger positively related—to risk taking, but only for individuals low on habitual distancing. Results supported both hypotheses. For individuals low on habitual distancing, fear decreased risk taking and anger increased risk taking. Interestingly, as opposed to the expected pattern of results, we found that fear *increased* risk taking whereas anger *decreased* risk taking at high levels of distancing. Although these results are difficult to interpret, one might speculate that people who naturally engage in distancing are more likely to reframe decision problems in a way that alters the influence of incidental emotions. We suggest that future studies aim to uncover underlying mechanisms. Consistent with Lerner and Keltner (2001), these results did not depend on the frame that participants received. Moreover, dispositional fear and anger alone did not predict risk taking. Their associations with risk taking were qualified by distancing. Finally, it is also worth mentioning that this study included three different domains of risk, thus accounting for possible domain-specific variations (Kühberger et al., 1999). Taken together, the results suggest that dispositional emotions and emotion regulation through distancing can predict the decisions people make. In Study 2, we used new measures of fear and anger to examine whether the null findings might be attributed to the measures.

STUDY 2

Study 2 attempted to address some of the limitations in Study 1 in two ways. First, we included new measures of dispositional fear and anger. Second, instead of measuring habitual distancing, we manipulated distancing. Because dispositional emotions may be particularly difficult to regulate (Lerner and Keltner, 2001), an interesting question is whether manipulating distancing from the risky decision-making task itself can reduce the influence of such emotions. To this end, Study 2 aimed to test whether distancing

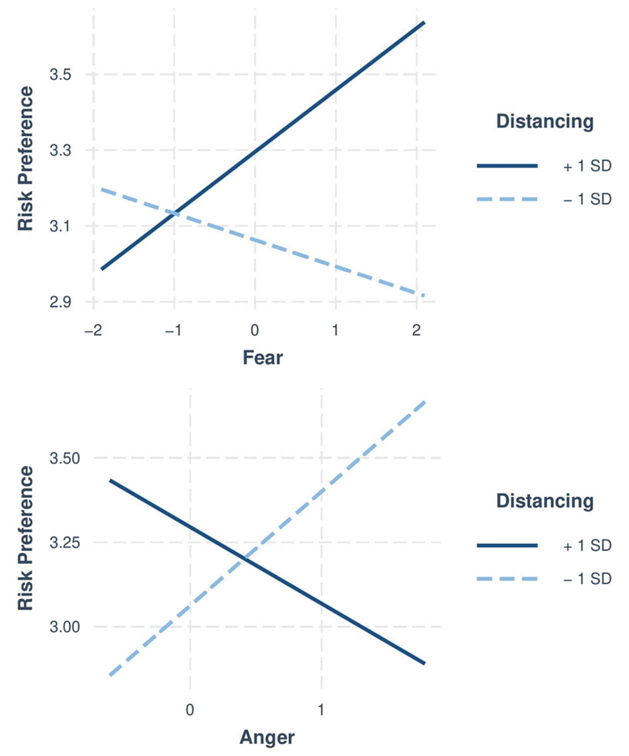

FIGURE 1 | Significant moderation by distancing in Study 1. Upper panel: negative relationship between fear and risk taking at lower levels of distancing. Lower panel: positive relationship between anger and risk taking at lowers levels of distancing. Each interaction plot presents the relationship at two levels of the moderator variable (−1SD standard deviation and +1SD standard deviation). Risk preference scored on a 1–7 scale.

moderates the relationship between (1) dispositional fear and risk taking and (2) dispositional anger and risk taking.

Method

Participants

A total of 600 participants were recruited from MTurk, using the CloudResearch platform (Litman et al., 2017). The sample size was estimated by performing an a-priori power analysis (using GPower 3.1.9.4) for a hierarchical linear regression model predicting risk preference. The power analysis indicated that we needed a sample of 550 participants to detect a small effect size ($f^2 = 0.02$; based on a meta-analysis by Wake et al., 2020). The effect size estimate was entered into the power analysis with the following input parameters: $\alpha = 0.05$, power = 0.80, number of tested predictors = 3. MTurkers were eligible to participate only if they were currently residing in the US, were native English speakers, completed a minimum of 500 surveys, and had a 95% MTurk HIT approval rating. Participants were paid $1.30 for the roughly 10-min long study. As specified in the pre-registration, participants were excluded if they; spent <2 min on the entire survey, indicated low English proficiency, reported not being serious about filling in the survey, failed a bot check, and if they correctly guessed the purpose of the study. Although not specified in the pre-registration, participants were also excluded if they spent <3 s on the page that included the self-distancing instructions. The final sample included 470 participants (235 males, 233 females, 2 other/prefer not to answer; $M_{age} = 40.55$, $SD_{age} = 12.21$). This study received ethical approval from the Norwegian Center for Research Data (NSD) before data collection.

Design and Procedure

This study used a 2 (distance: near vs. far) x 2 (frame: gain vs. loss) between-subjects design. As in Study 1, participants read a consent form and indicated their agreement. Those who agreed went on to receive a similar cover story and answered the trait emotions measurements. Again, these measures (and items) appeared in random order.

Measures

Self-Distancing Manipulation

Participants were randomly assigned to receive either a low distance or high distance prompt right before the risky decision-making tasks were presented. In the high distance condition, participants were instructed to "Imagine that the situation in the scenario happened very far from where you are now, like very long ago, very far in the future, or in another distant country." In the low distance condition, participants were instructed to "Imagine that the situation in the scenario happened very close to where you are now, like yesterday, tomorrow, or right in front of your eyes." This manipulation was adapted from van Dijke et al. (2018) (for a similar distancing manipulation, see Sun et al., 2018).

Trait Fear

Trait fear was measured using the Fear Survey Schedule-II (Geer, 1965; Bernstein and Allen, 1969). Responses were measured on a 7-point Likert scale (1 = "no fear," 7= "terror"). All items were averaged to form a single variable. Example items included "I fear being criticized," "I'm afraid of snakes," and "I'm afraid of not being a success." This scale has been widely used in previous studies examining fear and risk taking (e.g., Lerner and Keltner, 2001). The scale demonstrated strong reliability ($\alpha = 0.86$).

Trait Anger

We used two complementary measures of trait anger: the State-Trait Anger Expression Inventory (STAXI-II; Spielberger, 1999) and Lerner and Keltner's (2001) 10-item anger scale. We combined the two measures to form one single index of trait anger ($\alpha = 0.94$) Subjects rated the extent to which various behaviors were typical of them. Example items from the STAXI-II included "I am quick tempered" and "I feel infuriated when I do a good job and get a poor evaluation." Example items from the Lerner and Keltner (2001) anger scale included "I often find myself feeling angry" and "Other drivers on the road infuriate me." Responses were measured on a 7-point Likert scale (1 = "not at all true of me," 7 = "very true of me").

Risky Decision-Making Tasks

We used the same risky decision-making tasks as those in Study 1. Participants were randomly assigned to receive the tasks in either the gain frame or loss frame.

Manipulation Check

We used a single item from van Dijke et al. (2018): "How far away from the described scenarios did you feel?" (1 = "very close" to 9 = "very far"). Participants received the manipulation check after the decision-making task.

Statistical Analysis

Following our pre-registered plan, before proceeding to our main analysis of the interaction between distancing and emotions, we ran a two-way ANOVA to examine whether there was an interaction between framing and distancing in predicting risk preference. Specifically, we predicted that risk preference would be higher in loss frames and lower in the gain frame when distance is low. The ANOVA yielded a main effect of framing, $F_{(1,\,466)} = 52.51$, $p < 0.001$, $\eta_p^2 = 0.101$. However, the ANOVA yielded no main effect of distancing, $F_{(1,\,466)} = 0.71$, $p = 0.401$, $\eta_p^2 = 0.001$, and no interaction between distancing and framing, $F_{(1,\,466)} = 0.88$, $p = 0.35$, $\eta_p^2 = 0.002$.

Next, we proceed with our main analysis to examine the interaction between fear and distancing, and anger and distancing. A linear hierarchical multilevel model was fitted using the lme4 (Bates et al., 2014) and the lmerTest packages implemented in the R statistical environment (R Core Team, 2014). As in Study 1, the decision to use multilevel analysis deviated from the pre-registration, but results remain the same regardless of the analytical approach. Risk preference was predicted by framing (0 = Gain 1 = Loss), dispositional fear and anger, distancing (−0.5 = Near, +0.5 = Far), and the interactions of distancing with dispositional fear and anger. We used effect-coding (−0.5/+0.5) instead of dummy coding (1/0) to be able to interpret the lower-order main effects (Singmann and Kellen, 2019). Participants and decision scenario were treated as random-intercept effects. The discussion will focus only on the final, overall model (i.e., Step 3). Mean-centered beta coefficients and model fit statistics for each step of the regression are listed in **Table 2**. Assumptions of normality, linearity, and heteroscedasticity did not appear to be violated.

Results

Manipulation Check

An independent samples t-test revealed that participants in the far condition imagined the decision scenarios to be further away ($M = 8.13$, $SD = 1.13$) than participants in the close condition ($M = 2.24$, $SD = 1.60$), $t(468) = -46.14$, $p < 0.001$, $d = -4.27$, 95% CI [−4.58, −3.93].

Hypotheses Testing

All continuous predictors were mean-centered before running the analyses (Aiken et al., 1991). Including "subject" and "scenario" random effects significantly improved the model fit compared to the model without the random effects, supporting the rationale for using a mixed model. The results from the hierarchical multilevel analysis are summarized in **Table 2**. Risk preference was significantly higher in the loss frame, $\beta = 0.71$, $p < 0.001$, 95% CI [0.52, 0.90]. Thus, replicating the classic framing effects. Dispositional anger predicted higher risk taking, $\beta = 0.20$, $p = 0.003$ (one-tailed), 90% CI [0.07, 0.31]. Dispositional fear, on the other hand, did not significantly predict risk taking, although it was in the predicted direction, $\beta = -0.12$, $p = 0.06$ (one-tailed), 90% CI [−0.24, 0.01]. As predicted, distancing significantly interacted with fear, $\beta = 0.25$, $p = 0.007$ (one-tailed), 90% CI [0.08,0.42]. However, there was no interaction with dispositional anger, $\beta = -0.04$, $p = 0.34$ (one-tailed), 90% CI [−0.21, 0.13]. The simple slopes for the interaction between fear and distancing were not significant (low distance: $\beta = -0.12$, $p = 0.12$; high distancing: $\beta = 0.13$, $p = 0.07$).

Figure 2 illustrates a cross-over interaction between dispositional fear and distancing. In the immersed condition, dispositional fear is negatively related to risk preference. In the distanced condition, dispositional fear is positively related to risk preference.

As in Study 1, pre-registered exploratory analyses were performed to test whether the two interactions depended on the framing condition. A new model was tested that included two three-way interactions (fear*distancing*frame and anger*distancing*frame). None of the three-way interactions were significant (fear*distancing*frame: $\beta = 0.01$, $p = 0.95$, 95% CI = −0.38, 0.41; anger*distancing*frame: $\beta = -0.09$, $p = 0.66$, 95% CI = −0.49, 0.31). However, we did not calculate power for these exploratory interactions, which needs to be taken into account when interpreting the results.

Discussion

Study 2 extended Study 1 in two ways; (1) by including new measures of dispositional fear and anger, and (2) by manipulating distancing. As in Study 1, fear alone did not predict risk taking. However, anger was significantly and positively related to risk taking. This suggests that the main association between trait emotions and risk taking may depend on the specific measures used. The main hypothesis of interest was, however, the moderating role of distancing. In Study 2, we tested whether instructing individuals to distance themselves from the risky decision scenarios moderates the relationship between (1) dispositional fear and risk taking and (2) dispositional anger and risk taking. Consistent with Study 1, fear was negatively related to risk taking in the immersed condition. Interestingly, again, distancing not only attenuated this relationship but even reversed it, such that fear was *positively* related to risk-seeking in the distanced condition. Anger, on the other hand, did not interact with distancing. Finally, as in Study 1, neither interaction depended on the framing (i.e., loss vs. gain).

STUDY 3

Study 3 attempted to replicate the previous findings in an experiment by manipulating both emotions and distancing. The aim was to test whether distancing oneself moderates the influence of fear and anger on risky judgments and decisions. Specifically, participants adopted either an

TABLE 2 | Summary of hierarchical linear mixed model analysis predicting risk taking (Study 2).

	Model 1		Model 2		Model 3	
Predictors	**Estimates**	**CI**	**Estimates**	**CI**	**Estimates**	**CI**
Intercept	3.49**	3.23–3.76	3.48**	3.20–3.76	3.47**	3.20–3.75
Age	0.01	−0.00–0.01	0.01	−0.00–0.02	0.01	−0.00–0.02
Gender	−0.23*	−0.43 to −0.03	−0.24*	−0.45 to −0.04	−0.25*	−0.46 to −0.05
Framing	0.71**	0.52–0.91	0.69**	0.50–0.88	0.71**	0.52–0.90
Distance			0.07	−0.12–0.28	0.07	−0.12–0.26
Anger			0.18***	0.09–0.27	0.20**	0.08–0.32
Fear			0.01	−0.07–0.10	−0.12	−0.24–0.01
Distance × Anger					−0.04	−0.21–0.13
Distance × Fear					0.25*	0.08–0.42
Random Effects						
σ^2	2.04		2.04		2.04	
τ_{00}	$0.47_{subject}$		$0.43_{subject}$		$0.41_{subject}$	
	$0.05_{scenario}$		$0.05_{scenario}$		$0.05_{scenario}$	
ICC	0.20		0.19		0.19	
N	$468_{subject}$		$468_{subject}$		$468_{subject}$	
	$3_{scenario}$		$3_{scenario}$		$3_{scenario}$	
Observations	1,404		1,404		1,404	
Marginal R^2/Conditional R^2	0.053/0.247		0.069/0.247		0.075/0.247	

*Continuous predictors are mean-centered. *p < 0.05, **p < 0.01. One-tailed p-values and CIs are reported for the hypothesized relationships (fear, anger, and their interactions with distancing).*
σ^2, within-person variance; τ_{00}, between-person variance; CI, confidence interval; ICC, intraclass correlation.

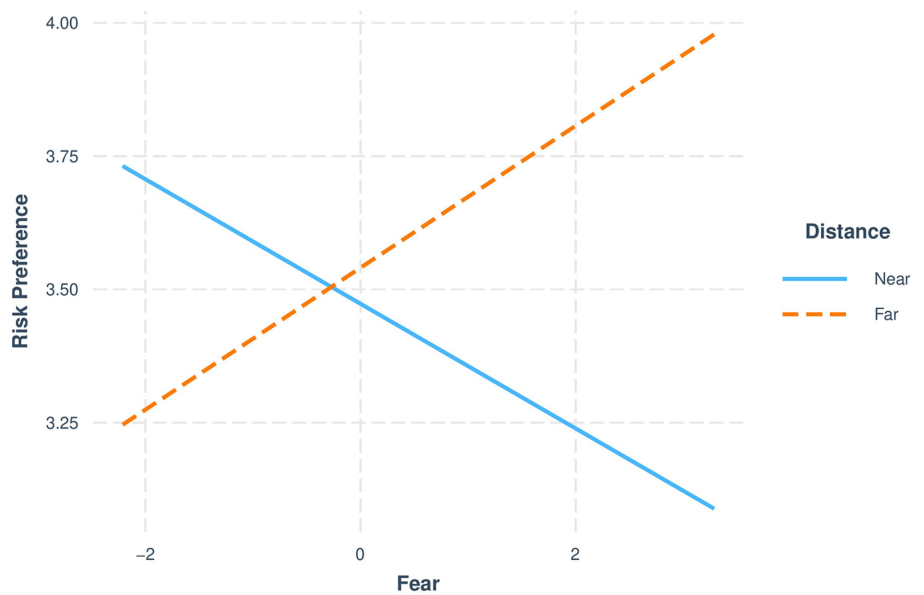

FIGURE 2 | Significant moderation by distancing in Study 2. The interaction plot presents the relationship at two levels of the moderator variable (−1SD standard deviation and +1SD standard deviation). Risk preference scored on a 1–7 scale.

immersed or distanced perspective while reflecting on fear-related and anger-related stressors before the risky judgment and decision-making tasks. Participants were not instructed to engage in distancing during the tasks as in Study 2. Rather, what we study here can be referred to as *incidental* distancing.

Method

Participants

A total of 700 participants were recruited from MTurk, using the CloudResearch platform (Litman et al., 2017). We estimated the sample size by performing an a-priori power analysis (using GPower 3.1.9.4) for a two-way between subject ANCOVA. The power analysis indicated that we needed a sample of 603 participants to detect a small effect size of $f^2 = 0.135$ (based on a meta-analysis by Wake et al., 2020). The effect size estimate was entered into the power analysis with the following input parameters: $\alpha = 0.05$, power = 0.80, number of groups = 4, number of covariates = 2. MTurkers were eligible to participate only if they were currently residing in the US, were native English speakers, completed a minimum of 500 surveys, and had a 98% MTurk HIT approval rating. Participants were paid $1.20 for the roughly 10-min long study. As specified in the pre-registration, participants were excluded if they; spent <2 min on the entire survey, indicated low English proficiency, reported not being serious about filling in the survey, failed a bot check and an attention check, and if they had correctly guessed the purpose of the study. The final sample included 643 participants (309 males, 328 females, 6 other/prefer not to answer; $M_{\text{age}} = 41.27$, $SD_{\text{age}} = 13.15$).

Procedure and Design

Study 3 used a 2 (emotion: fear vs. anger) × 2 (perspective: immersed vs. distanced) between-subjects design. Participants read a consent form first, and those who agreed proceeded to receive a similar cover story like the ones used in the previous two studies.

Emotion Induction

The emotion induction procedure was adapted from Lerner and Keltner (2001) and Lerner et al. (2003). The procedure consisted of two parts. First, they read a short story (131 words in the fear condition, 148 words in the anger condition) that described how the COVID-19 pandemic has increased unemployment and job loss (fear condition) or how the pandemic has resulted in unfair treatment of employees (anger condition). Below the paragraph were real news headlines that matched the content of the story. For instance, in the fear condition, participants saw news headlines about increased unemployment rates and job loss due to the pandemic. In the anger condition, participants saw headlines about companies that had taken advantage of the pandemic and treated employees in unethical ways. Materials are available on the OSF project page (see text footnote 1). In the second part, we asked the participants to think about a specific aspect of the pandemic that has made them most angry/afraid.

Self-Distancing Manipulation

Right after the emotion induction page, participants were asked to reflect on their thoughts and feelings about the emotional event that they identified on the previous page from an immersed or a distanced perspective (adapted from Bruehlman-Senecal and Ayduk, 2015, White et al., 2019). This manipulation focuses on the temporal dimension of psychological distance. Participants received the following instructions:

> Immersed condition:
> "Now that you've thought of a specific event related to the pandemic that makes you afraid [angry], imagine this very event unfold through your own eyes as if it was happening to you right now. As you continue to see the situation unfold in your own eyes, please take the next couple of minutes to describe your stream of thoughts about how you feel about this event that makes you afraid [angry]."
> Distanced condition:
> "Now that you've thought of a specific event related to the pandemic that makes you afraid [angry], take a few steps back and move away from the event to a point where it feels very distant from you. To help you do this, imagine what your life will be like 10 years in the future, envisioning what you might be doing and how you might be spending your time at this future time point."

We told them to take at least 3 min to describe their current thoughts and feelings (participants could not proceed to the next page until 3 min had passed).

Measures

Risky Judgment and Decision-Making Tasks

This study included two risk operationalizations; risk taking and risk estimation. We measured risk preference using the same scale as in the previous two studies. This time, as per the pre-registration, participants were given only one risky decision-making task; the Plant Problem (Bazerman, 1984), in the gain frame. Our decision to use only the gain frame was based on a recent meta-analysis by Wake et al. (2020) that suggested a stronger relationship between fear and risk in gain frames.

Risk estimation was measured with an adapted version of Lerner's shortened optimistic risk estimation scale (Lerner and Keltner, 2001; Winterich et al., 2010). Participants indicated from 1 (extremely unlikely) to 7 (extremely likely) the likelihood that each of five positive and negative events would happen to them at any point in their future life. We slightly modified the scale in this study to ensure that the items were better suited for an MTurk sample. Specifically, we excluded the items "I had a heart attack before age 50" and "I got into a prestigious internship program." These two items were replaced with an item from the original scale. The items included in this study were: 1. "I could not find a job for 6 months" (reverse-scored). 2. "I received statewide recognition in my profession." 3. "My income doubled within 10 years after my first job." 4. "I chose the wrong profession" (reverse-scored). 5. "I married someone wealthy." Items were averaged to form an optimistic risk estimates score ($\alpha = 0.56$). This indicates low reliability but is in line with previous studies (Winterich et al., 2010; Drace and Ric, 2012). As specified in our pre-registration, we included risk estimation as an additional measure to match our experiment more closely with Lerner and Keltner (2001, Study 4). Specifically, in their initial study examining trait fear and anger, they used the Unusual Disease Problem (see text footnote 2). However, in their follow-up experiment that manipulated both emotions, they used the risk estimation scale. We suspected that the influence of manipulated incidental emotions on risk taking might be weaker in decision tasks like the Plant Problem that seem somewhat more cognitively demanding. Unlike such decision tasks, the risk estimation scale concerns individuals' perceived likelihood of future events. This makes it possible for people to "guess" and rely on their intuition when estimating the likelihood of events—they simply do not have much else to base their judgments on than their gut feeling.

Manipulation Checks

To measure the effectiveness of emotion induction, participants were instructed to indicate how they felt while reflecting on the event in the writing task that they completed before the risky judgment and decision-making tasks. Participants rated the extent to which they felt fearful, worried, anxious, angry, outraged, and irritated (1= "not at all," 7 = "very much"). The first three items were averaged to form an index for fear, and the last three items were averaged to form an index for anger. The temporal distancing manipulation check was measured with a single item: "To what extent did your thoughts during the reflection period focus on the present/near future vs. distant future?" (1 = "the present/near future," 9 = "distant future"). This manipulation check was adapted from Bruehlman-Senecal and Ayduk (2015). Participants received the emotion and distance manipulation check items at the end of the survey.

Results

Manipulation Checks

To examine whether our manipulations were successful, we ran a series of ANOVAs. For perceived distance, an ANOVA revealed that participants in the distant condition focused on the distant future (M = 6.07, SD = 1.36) more than participants in the immersed condition (M = 2.02, SD = 1.23), $F_{(1, 641)}$ = 1,563.23, $p < 0.001$, $\eta^2_p = 0.710$. For self-reported fear, a two-way ANOVA revealed a significant interaction between emotion and distancing conditions, $F_{(1, 639)} = 23.94$, $p < 0.001$, $\eta^2_p = 0.040$. Tukey-adjusted pairwise t-tests indicated that participants in the immersed fear condition experienced more fear (M = 5.30, SD = 1.48) than participants in the distant fear condition (M = 3.21, SD = 1.99), $t(639) = 10.64$, $p < 0.0001$ (two-tailed), d = 1.18, 95% CI [0.94, 1.41], and the immersed anger condition (M = 3.91, SD = 1.90), $t(639) = 7.02$, $d = 0.78$, $p < 0.0001$ (two-tailed), 95% CI [0.55, 1.00]. For self-reported anger, a two-way ANOVA did not reveal a significant interaction between emotion and distancing conditions, $F_{(1, 639)} = 0.53$, $p = 0.470$, $\eta^2_p < 0.001$. Suggesting that the manipulation worked in the intended way, Tukey-adjusted pairwise t-tests indicated that participants in the immersed anger condition experienced more anger (M = 5.58, SD = 1.41) than participants in the distant anger (M = 4.22, SD = 1.99), $t(639) = 7.20$, $p < 0.0001$ (two-tailed), $d = 0.82$, 95% CI [0.58, 1.05] and the immersed fear conditions (M = 3.16, SD = 1.73), $t(639) = -13.08$, $p < 0.001$ (two-tailed), $d = -1.45$, 95% CI [−1.69, −1.20]. Overall, these results suggest that the emotion and distancing manipulations were successful.

Hypotheses Testing

Two two-way ANCOVAs were performed that examined the effects of distancing and emotion on risk preference and optimism while controlling for age and gender. First, a two-way ANCOVA was tested with risk preference (from the framing problem) as the dependent variable. The main effects of emotion, $F_{(1, 636)} = 0.00$, $p = 0.96$, $\eta^2_G < 0.001$, and distancing, $F_{(1, 636)} = 2.06$, $p = 0.15$, $\eta^2_G = 0.003$, and their interactions were not significant, $F_{(1, 636)} = 0.94$, $p = 0.33$, $\eta^2_G = 0.001$. A second two-way ANCOVA was performed with risk estimation as the dependent variable. The main effect of emotion, $F_{(1, 636)} = 0.10$, $p = 0.76$, $\eta^2_G < 0.001$, and the interaction between emotion and distance, $F_{(1, 636)} = 0.27$, $p = 0.60$, $\eta^2_G < 0.001$, were not significant. Incidental distancing, however, had a main effect on risk estimation, $F_{(1, 636)} = 7.81$, $p = 0.005$, $\eta^2_G = 0.01$. Participants in the immersed condition (M = 3.16, SD = 1.10) were less optimistic in their risk estimates than participants in the distant condition (M = 3.42, SD = 1.15), $t(638) = -2.82$, $p = 0.005$ (two-tailed), $d = -0.22$, 95% CI [−0.38, −0.07]. As per the pre-registration, we also tested the difference in risk estimation between immersed and distanced conditions in each of the two emotion conditions separately. Optimistic risk estimation was higher in the distanced fear condition (M = 3.46, SD = 1.22) compared to the immersed fear condition (M = 3.13, SD = 1.09), $t(323) = -2.22$, $p = 0.013$ (one-tailed), $d = -0.25$, 90% CI [−0.43, −0.06]. There was no statistically significant difference in risk estimation between the immersed anger and distanced anger conditions, $t(308) = -1.64$, $p = 0.10$ (two-tailed), d = −0.19, 95% CI [−0.41, 0.04]. The section below explores the main effect of distancing further by testing whether self-reported fear mediates the relationship between incidental distancing and risk estimation.

Exploratory Mediation Analysis

Given the main effect of distancing on risk estimation found earlier (section Hypotheses Testing), we performed a mediation analysis to explore whether incidental distancing increased optimistic risk estimation through reduced fear (as measured with the manipulation check). The analysis followed recommendations by Yzerbyt et al. (2018), using the JSmediation package. First, we report the results from the joint significance test of the a-component (a path) and b-component (b path) of the mediation model and conclude mediation if both are significant. Next, we report the boot-strapped estimated size of the indirect effect (ab) and its 95% confidence interval. Results indicated that reduced fear, but not anger, mediated the relationship between incidental distancing and optimistic risk estimation. Specifically, both the a and b paths were significant [a point estimate = −1.40, SE = 0.15, $t(641) = 9.59$, $p < 0.001$, b point estimate = −0.11, SE = 0.02, $t(640) = 4.77$, $p < 0.001$], as was the indirect effect (point estimate = 0.16, 95% CI [0.09, 0.23], 5,000 Monte Carlo iterations). The model is illustrated in **Figure 3**.

Discussion

In Study 3, we aimed to replicate the findings from the previous two studies by manipulating emotion and distancing. Furthermore, we adjusted our emotion manipulation to the current COVID-pandemic for a more ecologically valid manipulation. We found no support for our hypothesis regarding a moderating role of distancing, nor did we find a

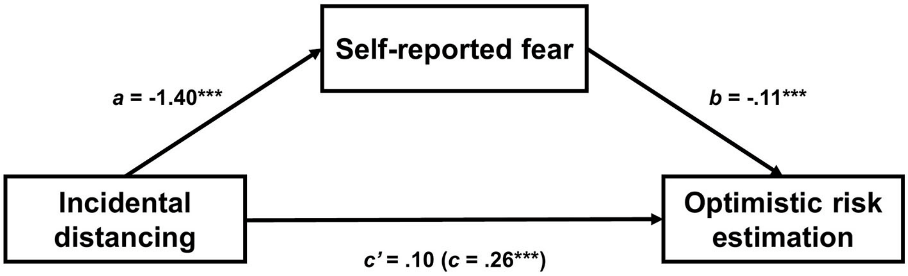

FIGURE 3 | Mediation model in Study 3. Coefficients are unstandardized regression coefficients. The unstandardized regression coefficient representing the total relationship between incidental distancing condition and risk estimation is in parentheses. ***$p < 0.001$.

main effect of emotion (i.e., fear and anger). However, we found a positive main effect of distancing on risk estimation (but not risk taking). Participants in the distanced condition showed more optimistic risk estimations in a subsequent risk judgment task than participants in the immersed condition. Further exploratory analysis indicated that the effect of distancing on optimistic risk estimation was mediated by reduced fear. In other words, adopting a distant perspective while reflecting on current stressors increased optimistic risk estimation by reducing fear. However, the lack of a control group prevents us from drawing more specific conclusions. We expand on these points in the next section.

GENERAL DISCUSSION

The current study set out to examine how psychological distancing moderates the relationship between fear and risk taking, and anger and risk taking. In Study 1, at low levels of habitual distancing, dispositional fear predicted lower risk taking, whereas dispositional anger predicted greater risk taking. These relationships (fear and risk taking, anger and risk taking) reversed among individuals higher on distancing. Study 2 manipulated distancing and used different measures of dispositional fear and anger. Distancing interacted with dispositional fear but not anger. Replicating the pattern for fear observed in Study 1, the relationship between fear and risk taking was negative for participants who adopted a distanced perspective while reading the risk scenarios, but positive for those who adopted an immersed perspective. Finally, Study 3 manipulated emotions and distancing to examine the impact of incidental distancing from fear and anger on risk preference and risk estimation. While the study found no main effect of emotion or interaction between emotion and distancing on risk preference and risk estimation, exploratory analyses revealed that incidental distancing (across both emotion conditions) increased optimistic risk estimation through a reduction in self-reported fear. This is a relevant finding, as subjective probabilities inform people on what actions they should take, and thus, may shape important life outcomes. Overall, although we find mixed results across the three studies, the results regarding fear reveal a clearer pattern. Distancing moderated the relationship between fear and risk taking the same way in both Study 1 and 2. While we did not observe a moderating effect of distancing in Study 3, distancing increased optimistic risk estimation via reduced fear.

The results contribute to the field by providing important insight into the interplay between psychological distance and emotions in risky judgment and decision making. Previous research has found that distancing is associated with a range of cognitive (Kross and Grossmann, 2012; Grossmann and Kross, 2014; Sun et al., 2018) and affective benefits (Kross et al., 2014; Bruehlman-Senecal and Ayduk, 2015; Nook et al., 2017, 2020; Ahmed et al., 2018; Powers and LaBar, 2019; White et al., 2019). With respect to its emotion-regulatory function, studies suggest that it may be even more effective than its counterpart tactic *reinterpretation* (Denny and Ochsner, 2014). The overall results of the present research provide some evidence that distancing regulates the influence of incidental fear on judgments and decisions involving risk. The influence of incidental fear (Study 1 and 2) and anger (Study 1) on risk taking was reduced and even reversed among the high distancers. More specifically, at high levels of distancing, fear *increased* risk taking. To our knowledge, this is a previously unknown effect. Since we found it in two studies, there is little reason to believe that this is an artifact. Nevertheless, future research is needed to examine how replicable this effect is (i.e., boundary conditions) and what drives it. The measures that we used did not provide much information about the process behind the effect. A previous study has shown that the relationship between fear and risk taking depends on how individuals cognitively frame the situation (Lee and Andrade, 2015). Although Lee and Andrade (2015) did not examine distancing *per se*, the results suggest that the influence of emotions on risk taking depends on how individuals interpret their emotional experiences. Future studies can try to uncover mediators behind the reversal of the relationship between fear and risk taking by using a similar approach to the one we used in Study 3. In Study 3, we observed that a decrease in fear mediated the positive effect of distancing on optimistic risk estimation. As our emotion manipulation check only tapped into fear and anger, future studies should include mediators that tap into other emotions that are typically associated with optimism, such as hope and relief. Studies can also investigate the mental and cognitive processes underlying the unexpected positive relationship between fear and risk. One example is information processing. Appraisal theories suggest that uncertainty-related emotions like fear increase systematic reasoning, whereas certainty-related emotions like anger lead to intuitive reasoning (Lerner and Keltner, 2000; Tiedens and Linton, 2001; Lerner et al., 2015). It would be interesting to examine whether the unexpected positive relationship between fear and risk taking—and the negative relationship between anger and risk taking in Study 1—is explained by a shift from systematic processing to intuitive processing and vice versa. Relatedly, it is possible that distancing regulates the appraisals underlying the predicted effects of fear and anger on risk taking (Lerner and Keltner, 2001). One could therefore test, for example, whether distancing from fear increases risk taking by reducing the level of uncertainty associated with fear.

It should be noted that the effect occurred in decision situations that were characterized by ambiguity. This is relevant since it appears reasonable to expect that reversal effects occur more often in such situations than those that are less ambiguous. Level of ambiguity might therefore constitute a boundary condition for the reversal effect. Indeed, Lerner and Keltner (2001) documented ambiguity with respect to certainty and control as a boundary condition for the predicted effects of fear and anger. Moreover, although the effects in our study were observed in controlled laboratory settings, they could be expected to exist in real-life decision-making situations (e.g., Hodgkinson et al., 1999). Overall, it remains unclear exactly what lies behind these unexpected associations. We hope that our findings will encourage steps toward a more nuanced understanding of how emotion and distancing interact in risky decision making.

Limitations and Future Research

We would like to highlight several limitations and directions for future research. Overall, we found mixed results with small effect sizes across the three studies. While habitual distancing interacted with both fear and anger (Study 1), manipulated distancing only interacted with fear (Study 2). Study 3 did not find a moderating role of distancing. One possible reason for the mixed results is that we measured and manipulated both emotion and distancing in different ways across the studies. Study 1 looked at habitual distancing from negative events, whereas Study 2 and 3 manipulated distancing. Moreover, overall, we did not find support for our predicted (based on e.g., Lerner and Keltner, 2001; Lerner et al., 2003, 2015; Habib et al., 2015) main effects of fear and anger. This may be attributed to methodological aspects in our studies, as we used slightly different measurements and manipulations. In the one instance where we used the exact measurement used by Lerner and Keltner (2001), we did find a main effect (anger in Study 2). It appears less likely that the null findings can be attributed to power or sample issues. More research is needed to test the replicability of these main effects of fear and anger, and their boundary conditions.

A key strength of this paper is in the multilevel approach used in Study 1 and 2, where participants received the risky decision-making tasks in different domains and frames. However, these tasks do not reflect decision making in real life. Decisions are often made in situations where information about outcomes is unknown. Furthermore, rather than instructing participants to explicitly engage in psychological distancing, decision scenarios can activate psychological distance indirectly by varying the distance of the targets (see Raue et al., 2015). Raue et al. (2015) showed that increasing the psychological distance in risky scenarios eliminated and even reversed the classic framing effects. They interpreted this in terms of a reduction in emotional intensity and a shift from intuitive to deliberate information processing. Our study is the first to test how distance regulates emotional biases in risky decision making. It would be interesting to test whether indirect psychological distance regulates incidental emotions in similar ways.

Moreover, unlike previous studies that have examined the general reappraisal strategy, participants in this study were not explicitly told that the goal was to down-regulate negative emotions through reappraisal. The literature suggests that distancing is an efficient but relatively effortless tactic (Moser et al., 2017) with long-term benefits such as reduced levels of stress (Denny and Ochsner, 2014). There is, however, a need for further research on how distancing impacts risky decision making in emotionally intense real-life situations.

However, studies will also need to examine conditions under which distancing may be ineffective, or even backfire. As noted by Sheppes and Levin (2013), the decision to apply an emotion regulation strategy is a difficult decision in itself. In situations where emotions are known to influence our judgments and decisions in a negative way, it should be advisable to regulate emotions. In other situations, however, it may be less advisable to regulate emotions. Despite potential downsides, we believe that the main function of distancing is not to eliminate emotions, but rather, to help individuals process them.

Finally, there is evidence suggesting that distancing may be less effective in regulating certain emotions. Construal Level Theory (CLT) distinguishes between emotions based on their underlying level of construal (i.e., level of abstractness). For instance, fear constitutes a so-called "low-level" emotion because it is concerned with immediate and visible threats (e.g., seeing a snake while hiking). Anxiety, on the other hand, is a "high-level" emotion because it is concerned with distant and ambiguous threat (e.g., feeling anxious about the possibility of losing one's job in the future). A similar distinction has been made between personal (low-level) and moral anger (high level) (Agerström et al., 2012). Because high-level emotions like anxiety and moral anger necessitate distancing, CLT predicts that distancing may in fact intensify these emotions. Doré et al. (2015) found that use of anxiety-related words following a tragic event increased over temporal and spatial distance. The opposite was found for sadness-related words. Relatedly, Bornstein et al. (2020) found that abstract processing decreased fear and intensified other high-level emotions like guilt. Agerström et al. (2012) found that greater temporal distance increased anticipated intensity of moral anger but decreased the anticipated intensity of personal anger. Although these studies did not use the same manipulations as those used in our study, the pattern of results suggests that distancing might have different effects on different emotions. Thus, future research examining emotion regulation through distancing and decision making should take into account the abstraction level of the emotion, in addition to other appraisals like certainty and control.

PRACTICAL IMPLICATIONS AND CONCLUDING THOUGHTS

The present study points to distancing as a promising tool in organizational settings. For instance, contexts that favor systematic and rule-based decision making might benefit from distancing as a simple tactic to help decision makers avoid excessive risk aversion or risk taking. The idea that a big picture focus can help improve decision making under risk is not new. In fact, in an early paper on the cognitive aspects of risk taking, Kahneman and Lovallo (1993) argued that "a broad view of decision problems is an essential requirement of rational decision making" (p. 20). They further argued that decision makers, particularly managers, tend to adopt a narrow frame of decision problems, failing to place them in broader contexts (Kahneman and Lovallo, 1993). Extending Kahneman and Lovallo's (1993) notion, we believe that one way in which a broad perspective impacts decision making is through the regulation of emotional influences. Distancing can prove effective in situations where fear might lead to excessive levels of risk aversion and where anger might lead to excessive levels of risk taking. Moreover, moving beyond self-regulation, it would be interesting to examine how leaders can regulate employees' emotions and cognitions. Anecdotal reports suggest that employees around the globe may be experiencing high levels of anxiety and pessimism brought by COVID-19 (Jacobs and Warwick-Ching, 2021). It is conceivable that leaders can regulate employees' negative emotions and perceptions by removing them from the "here and now."

DATA AVAILABILITY STATEMENT

The datasets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found in the article/supplementary material.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by NSD–Norwegian center for research data. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

LM and FB contributed to the conception and design of the study. LM collected and analyzed the data and wrote the first draft of the manuscript. Both authors contributed to the article and approved the submitted version.

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The Role of Personal Experience and Prior Beliefs in Shaping Climate Change Perceptions: A Narrative Review

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Global climate change is increasing the frequency and intensity of extreme weather events such as heatwaves, droughts, and flooding. This is the primary way many individuals experience climate change, which has led researchers to investigate the influence of personal experience on climate change concern and action. However, existing evidence is still limited and in some cases contradictory. At the same time, behavioral decision research has highlighted the importance of pre-existing values and beliefs in shaping how individuals experience changes in environmental conditions. This is in line with theories of motivated reasoning, which suggest that people interpret and process information in a biased manner to maintain their prior beliefs. Yet, the evidence for directional motivated reasoning in the context of climate change beliefs has recently been questioned. In the current paper, we critically review the literature on the interrelationships between personal experience of local weather anomalies, extreme weather events and climate change beliefs. Overall, our review shows that there is some evidence that local warming can generate climate change concern, but the capacity for personal experience to promote action may rely upon the experience first being attributed to climate change. Rare extreme weather events will likely have limited impact on judgments and decisions unless they have occurred recently. However, even recent events may have limited impact among individuals who hold strong pre-existing beliefs rejecting the reality of climate change. We identify limitations of existing research and suggest directions for future work.

Keywords: climate change, extreme weather, personal experience, prior beliefs, climate change beliefs

INTRODUCTION

Climate change is a global challenge that has already had a detrimental impact on the environment and human health, leading to increases in the magnitude and frequency of extreme weather events such as heatwaves, droughts and flooding (Seneviratne et al., 2012). Despite compelling evidence for rapid climate change and the urgency of the issue (IPCC, 2018), some individuals remain skeptical about the risk and reality of climate change (Whitmarsh, 2011).

A key factor that can shape perceptions of climate change is people's personal experience of extreme weather events and/or local weather anomalies such as temperature fluctuations (i.e., deviations from the normal seasonal temperature) (Spence et al., 2011; Zaval et al., 2014; Demski et al., 2017). Such experiences provide an opportunity for individuals to witness the otherwise abstract effects of climate change and as such make the risk more tangible and familiar (Lorenzoni and Pidgeon, 2006; Weber, 2010; Spence et al., 2011; Smith and Joffe, 2013; Reser et al., 2014). Specifically, such experiences can reduce perceived psychological distance of climate change from the self on different dimensions, including temporally, socially and geographically (McDonald et al., 2015). Furthermore, experiences with extreme weather events may also provoke negative affective responses, which can increase perceived risk (Keller et al., 2006), increase environmental concern and promote action (Demski et al., 2017; Bergquist et al., 2019).

However, personal experiences of climate change impacts may not always increase concern or motivate action. The literature on motivated reasoning suggests that people often process new information in a biased way to generate their favored conclusion and maintain their prior beliefs (Kunda, 1990; Dawson et al., 2002; Myers et al., 2013). As such, individuals' prior beliefs about climate change may influence how they interpret fluctuations in local weather conditions. This can substantially reduce the likelihood of skeptical individuals acknowledging the reality and consequences of climate change, which in turn could lower their willingness to adopt adaption (e.g., paying for flood damage insurance) and/or mitigation actions (e.g., deciding to travel by train rather than flying; Lorenzoni and Pidgeon, 2006; Weber, 2006).

In this mini review, we provide an overview of existing literature on the interrelationships between personal experience of local weather anomalies, extreme weather events and climate change beliefs. We first review studies examining whether personal experiences shape beliefs and actions, and outline moderating factors, considering relevant literature in behavioral decision research. Next, we review work examining the influence of pre-existing climate change beliefs on interpretations of weather-related experiences, in connection to work on motivated reasoning. Finally, we identify limitations of existing research and suggest directions for future research. An overview of the basic characteristics of all studies reviewed and main results for each study can be found in the online **Supplementary Materials**.

DOES PERSONAL EXPERIENCE SHAPE CLIMATE CHANGE BELIEFS AND ACTION?

There is evidence to suggest that subjective (self-reported) experiences of local weather anomalies could increase belief in and concern for climate change (Krosnick et al., 2006; Howe and Leiserowitz, 2013; Zaval et al., 2014; Howe, 2018). For example, survey respondents in Australia and the United States (U.S.) who reported warmer-than-usual temperatures on the day of study expressed greater belief in and concern about climate change (Li et al., 2011). However, these studies are limited by the cross-sectional and self-reporting nature of the data and hence a causal relationship between personal experience and climate change beliefs cannot be established.

Studies that have measured the effect of observed temperature fluctuations on climate change beliefs provide evidence that abnormally warm temperatures in the short term (Joireman et al., 2010; Egan and Mullin, 2012; Hamilton and Stampone, 2013) and the long term (Deryugina, 2013; Shao et al., 2014, 2016; Shao, 2017) are important predictors of climate change beliefs and risk perceptions. For example, three 10-year studies in the U.S. reported a positive relationship between increasing summer temperatures and belief in the immediate impacts and severity of climate change (Shao et al., 2014, 2016). Other studies, however, did not find clear associations between short and long-term local temperature fluctuations and climate change concern (Li et al., 2011). Shao et al. (2016) and Deryugina (2013) argued that this may be due to differences in measurements of short-term temperature (i.e., monthly rather than daily data) and survey questions used to assess climate change beliefs. Alternatively, Brody et al. (2008) indicated a possible misunderstanding of the risks presented by long-term temperature change on individual well-being.

A smaller number of studies have examined specifically whether experiences of extreme weather events influence perceptions of climate change. For example, early research found no differences in climate change concern between flood and non-flood victims in the United Kingdom (U.K.) (Whitmarsh, 2008), with respondents reflecting a view that flooding was a separate issue from climate change and was instead caused by local changes such as road resurfacing.

Contrasting with these initial findings, more recent studies have found that personal experience with severe storms and associated floods (Spence et al., 2011; Taylor et al., 2014; Lujala et al., 2015), hurricanes (Bergquist et al., 2019), heatwaves (Dai et al., 2015), and droughts (Carmichael and Brulle, 2017) can influence climate change beliefs and concern, at least temporarily. For example, Spence et al. (2011) found that respondents in the U.K. who reported direct flooding experience expressed a higher level of concern about climate change impacts, compared to non-flood victims. Similarly, Demski et al. (2017) documented an increase in concern about climate change, as well as heightened personal salience of climate change issues and negative emotional responses following flooding experiences. Beyond flooding experiences, Dai et al. (2015) recorded participants' experiences of several extreme weather events (e.g., heavy rainfall, heatwaves, droughts, and avalanches) in five Chinese cities and found correlations between perceived experiences (particularly heatwaves) and climate change beliefs.

There is also some evidence that personal experience of extreme weather events may motivate individuals to act on climate change (Spence et al., 2011; Broomell et al., 2017). For example, Demski et al. (2017) showed that higher levels of personal and local threat from climate change following a flooding experience can prompt actions such as changing to a green energy supplier, as well as support for climate-related policy. Similarly, a study in Vietnam illustrated that flooding experience was strongly associated with intentions to implement adaption actions (Ngo et al., 2020). Other studies, however, suggest that experience of extreme weather events is associated with perceived threat from climate change, but not with willingness to take action (Whitmarsh, 2008; Brulle et al., 2012; Carlton et al., 2016). For example, a recent survey found that self-reported flooding experiences significantly predicted perceived threat from climate change but did not influence mitigation intentions (Ogunbode et al., 2019).

FACTORS MODERATING THE IMPACT OF PERSONAL EXPERIENCE ON CLIMATE CHANGE BELIEFS AND ACTION

Overall, the empirical evidence on the relationship between personal experience of extreme weather events and climate change concern and action remains mixed. The contrasting findings may partly reflect that these experiences only generate climate change concern and action under particular circumstances that can affect memory strength for events and their impact on decisions, namely when events are: (1) relatively recent (Konisky et al., 2016); (2) linked with significant personal and/or financial damages (Lujala et al., 2015; Sisco et al., 2017); (3) experienced as abnormalities in temperature (Sisco et al., 2017); and (4) attributed to climate change (Ogunbode et al., 2019). We discuss these moderating factors in more detail below.

Firstly, more recent extreme weather events may have a significant impact on climate change concern and action, due to the ease with which they are recalled (Keller et al., 2006). In recent years, research on risky choice has investigated how personal experience affects judgment and decision making under risk. One aspect of this line of research has focused on rare (and extreme) events; This is particularly relevant in the context of climate change as most (extreme) weather events (e.g., heatwaves, flooding) have a small probability of occurrence. When making decisions based on experience such rare events are generally *underweighted* (i.e., their impact on making choices/taking action is smaller than their objective probability suggests). This is because their probability of having recently occurred is (on average) small. However, when they occur, their impact on future decisions can be larger than what is warranted by their objective probability, suggesting strong sequential dependencies and recency effects (Hertwig et al., 2004; Weber, 2010). This implies that personal experience of extreme (and rare) weather events can cause unstable effects on climate change beliefs, and that the specific nature of personal experience is a key determinant. For example, recency effects can be amplified if the event was linked with personal and financial damage (Sisco et al., 2017). Similarly, a study in Norway showed that respondents who had suffered damage from a climate-related event such as flooding or a landslide were 40% more likely to be concerned about climate change (Lujala et al., 2015). However, as it has already been mentioned [see Ogunbode et al. (2019)], experiencing extreme weather events may not necessarily lead to taking action, while at the same time may increase concern about climate change. This is similar to what Barron and Yechiam (2009) observed in a sequential risky choice task: while people's choices showed reduced sensitivity to rare events (i.e., underweighting), their probability judgments/estimations suggested overestimating the occurrence of such events. Recent research has attempted to unpack aspects of this paradoxical finding, for example whether people's choices and judgments are susceptible to biases arising from misinterpreting sequential patterns (similar to gambler's fallacy; Plonsky et al., 2015; Szollosi et al., 2019; see also Ashby et al. (2017)].

Further, as highlighted by Li et al. (2011) heightened temperatures on the day/days leading up to a study are associated with increased belief in and concern about climate change (Joireman et al., 2010; Egan and Mullin, 2012; Brooks et al., 2014). For example, in one study, belief that climate change is happening was predicted by temperature anomalies (i.e., unseasonable warm and/or cool temperatures) on the interview day and the previous day (Hamilton and Stampone, 2013). However, individuals must attribute these experiences to climate change to not only increase concern for climate change, but also to encourage action (Reser and National Climate Change Adaptation Research Facility, 2011; Akerlof et al., 2013; Myers et al., 2013). That is, when an individual experiences the impacts of climate change (i.e., extreme weather), such experience may not affect their concern or willingness to take action unless they first make a causal connection between the experience and climate change (Weber, 2010; Helgeson et al., 2012). To illustrate, individuals affected by severe flooding in the U.K. in 2013/2014, reported a greater perceived threat from climate change, but only those who attributed the event to climate change supported mitigation actions (Ogunbode et al., 2019). This points to the important role of pre-existing beliefs in shaping people's interpretations and attributions of different weather events.

DO PRIOR BELIEFS SHAPE AN INDIVIDUAL'S PERCEPTION OF CLIMATE CHANGE IMPACTS?

Given that a single extreme weather event does not necessarily reflect long-term climate change trends, individuals may rely on their pre-existing beliefs to interpret extreme weather events in terms of climate change. This implies that individuals who already believe in climate change may interpret their experiences as a confirmation of the impacts of climate change. Instead, those who are more skeptical may be less likely to attribute their experiences to climate change (Howe and Leiserowitz, 2013). Such belief-driven interpretations may reduce the likelihood of some individuals becoming concerned about climate change (Howe and Leiserowitz, 2013; Broomell et al., 2017).

The influence of prior beliefs on climate change perceptions can be explained through a process of directional motivated reasoning. In broad terms, the concept describes how individuals tend to interpret and process information in a biased way that confirms their prior beliefs (Druckman, 2015). Such processing can affect the interpretation of new climate change information and experiences and may motivate individuals to reach their preferred conclusion regardless of accuracy or credibility (Druckman, 2015). The importance of directional motivated reasoning in shaping interpretations of extreme weather events has been documented frequently in recent years (Hart and Nisbet, 2012; Dietz, 2013; Druckman, 2015; Kahan and Corbin, 2016). For example, a national U.S. survey found that respondents who lived in places that were affected by a heatwave in 2010 but were "doubtful" or "dismissive" about climate change were 27% less likely to report experiencing a warmer-than-normal summer (Howe and Leiserowitz, 2013). These findings suggest that motivated reasoning may bias recall of local climate, particularly among those who do not believe in the existence of climate change. Other studies have also indicated that prior beliefs and cultural orientations can bias people's recollections of local weather experiences (Goebbert et al., 2012; Shao, 2016). To illustrate, Shao (2016) found that individuals who believed that climate change is having impacts now were more likely to perceive a strange pattern of weather in the past. Meanwhile, results from a longitudinal survey by Myers et al. (2013) showed that highly engaged individuals (i.e., those who were either strongly convinced of the reality of climate change or strongly rejected it) were more likely than less engaged ones to interpret their personal experiences in a way that strengthened their pre-existing beliefs. It should be noted however, that this and other studies examining the effect of prior beliefs on reported experience of extreme weather events are primarily limited to regions within North America, which has, in recent decades, become deeply divided on the issue of climate change (Carmichael and Brulle, 2017).

Another useful illustration of the role of prior beliefs is research that examines associations between political indicators (i.e., ideology and party identification) and climate change perception (McCright et al., 2014; Hamilton et al., 2015; Zanocco et al., 2018; Marlon et al., 2019). For example, between 2010 and 2014, Palm et al. (2017) showed that Democrats were convinced climate change was occurring and demanded action, whereas Republicans remained skeptical about climate change. Importantly, such views were generally strengthened over time in both groups, particularly among those engaged with the news and public affairs. This finding is consistent with a process of politically motivated reasoning whereby individuals process information in a way that aligns with their party ideology. Similarly, in a survey across four Gulf Coast states, Shao and Goidel (2016) found that compared with Republicans, Democrats were more likely to believe in the existence of climate change and demonstrated greater concern for future consequences. In addition, Democrats were more likely to notice changes in local weather conditions, including an increase in the frequency and intensity of hurricanes, droughts and flooding.

There is also evidence that motivated reasoning may not affect the recall of all past experiences equally. Memories of abnormal local temperatures are more likely to be shaped by climate change beliefs than other weather types (precipitation), due to the natural link between climate change (global warming) and temperature (Leiserowitz, 2006). Evidence from two national surveys in Norway and the U.S. support this view and find that perceptions of seasonal temperature were shaped by climate change beliefs, political ideology and cultural biases (Goebbert et al., 2012; Howe, 2018). The relationship was substantially weaker for local precipitation, floods and droughts.

Finally, it should also be noted that the evidence for directional motivated reasoning in the context of climate change beliefs has recently been questioned. To illustrate, Druckman and McGrath (2019) propose that individuals may not engage in directional motivated reasoning when assessing climate change information. Instead, individuals may evaluate new evidence aiming to arrive at an accurate conclusion, independent of their prior beliefs. Still, individuals may vary in how they assess the credibility of new information. Thus, observational studies that consider the evidence of bias or political differences as motivated reasoning should be read with some caution.

DISCUSSION: FUTURE RESEARCH AND WIDER IMPLICATIONS

We have reviewed the state of knowledge on the links between personal experience, prior beliefs and climate change concern/action. Although scholars have been researching the topic for more than a decade, our review shows that the empirical evidence remains mixed. There is some evidence that local weather experiences can generate climate change concern, but the capacity for personal experience to promote action may rely upon the experience first being attributed to climate change (Ogunbode et al., 2019). Attributions may be determined by prior beliefs about climate change, political ideology and/or party identification (Howe and Leiserowitz, 2013). Additionally, the evidence for extreme weather events influencing climate change concern and actions remains limited, with a distinct focus on flooding.

Overall, our review highlights the importance of examining the role of motivational and cognitive biases to help explain how and why weather experiences and prior beliefs may shape individual's climate change perceptions. Behavioral decision research suggests that rare extreme weather events will have limited impact on judgments and decisions, unless they have occurred recently (Konisky et al., 2016). However, even recent events may have limited impact among individuals who hold strong pre-existing beliefs rejecting the reality of climate change (Myers et al., 2013). Existing evidence on the associations between climate change beliefs and perceived weather is consistent with the notion that people interpret and process personal experiences in a biased way that confirms their prior beliefs through a process of motivated reasoning (Howe and Leiserowitz, 2013). However, many of the existing studies report cross-sectional data, making it difficult to determine a causal relationship between personal experience and climate change beliefs. Therefore, as the opportunities for individuals to witness extreme weather events increase, we encourage researchers to utilize a longitudinal and/or experimental design that allow stronger assessments of causality, as studies using such designs are scarce (e.g., Myers et al., 2013; Sobkow et al., 2017). For example, experimental studies could manipulate participants' experience with extreme events in game-like settings

(Sobkow et al., 2017) or priming of heat-related cognitions (Joireman et al., 2010).

Differences in methodological approaches across studies should also be highlighted, as the mixed findings outlined may be partly due to such differences. Firstly, some studies use subjective (self-reported) experiences, whereas others examine the influence of objectively measured weather variables on climate change beliefs. Secondly, studies examine a range of independent and dependent variables, which differ in their spatial coverage, time frame, data source and values used. The phrasing of survey questions is also varied, and collectively these differences may in some cases hinder the detection of a relationship between experience and beliefs. Future work could therefore aim to synthesize research designs and methods to aid systematic comparisons, ideally using standardized measures. Finally, most research has been conducted in the U.S. and the U.K. at varying levels (e.g., community, county, state and national) with a distinct focus on local temperature anomalies and flooding. Future research examining different populations and experiences of different forms of extreme weather (e.g., heatwaves and droughts) would help to examine the generalizability and boundary conditions of the findings reviewed.

Gaining a solid understanding of the mechanism underpinning the associations between prior beliefs, personal experience, and concern and action about climate change (or lack thereof) will be critical for the development of effective risk communication strategies suitable for diverse audiences and may also help to inform debiasing interventions. While communicating about climate change risk and the importance of personal action to adapt and mitigate climate change, we suggest communications should also consider the role of non-analytical processes such emotion, the use of imagery and social/group norms to promote climate change efficacy (Hornsey et al., 2021). More work is needed examining what motivates an individual to attribute a personal experience to climate change (e.g., values, worldviews, social structures etc.) as this has been shown to be a key factor in driving mitigation responses (Ogunbode et al., 2019). The role of affective reactions should also be investigated further, as affective reactions to climate change risks may also be subject to moderation by prior attitudes and beliefs (Swim et al., 2009). Yet, intense negative affect can induce fear in some individuals, and as a result lead to avoidant behaviors and denial (Taylor et al., 2014). Finally, future work should examine in more depth the influence of event recency, personal and financial damages, local warming, and psychological and social contexts; all of which may shape how individuals perceive and interact with weather-related experiences.

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KS conducted the literature search and the literature review. KS, EK, SR, and YO have contributed equally to the finalization of the manuscript. All authors contributed to the article and approved the submitted version.

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SUPPLEMENTARY MATERIAL

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Biased Estimates of Environmental Impact in the Negative Footprint Illusion: The Nature of Individual Variation

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People consistently act in ways that harm the environment, even when believing their actions are environmentally friendly. A case in point is a biased judgment termed the *negative footprint illusion*, which arises when people believe that the addition of "eco-friendly" items (e.g., environmentally certified houses) to conventional items (e.g., standard houses), *reduces* the total carbon footprint of the whole item-set, whereas the carbon footprint is, in fact, increased because eco-friendly items still contribute to the overall carbon footprint. Previous research suggests this illusion is the manifestation of an "averaging-bias." We present two studies that explore whether people's susceptibility to the negative footprint illusion is associated with individual differences in: (i) *environment-specific* reasoning dispositions measured in terms of compensatory green beliefs and environmental concerns; or (ii) *general* analytic reasoning dispositions measured in terms of actively open-minded thinking, avoidance of impulsivity and reflective reasoning (indexed using the Cognitive Reflection Test; CRT). A negative footprint illusion was demonstrated when participants rated the carbon footprint of conventional buildings combined with eco-friendly buildings (Study 1 and 2) and conventional cars combined with eco-friendly cars (Study 2). However, the illusion was not identified in participants' ratings of the carbon footprint of apples (Study 1 and 2). In Studies 1 and 2, environment-specific dispositions were found to be unrelated to the negative footprint illusion. Regarding reflective thinking dispositions, reduced susceptibility to the negative footprint illusion was only associated with actively open-minded thinking measured on a 7-item scale (Study 1) and 17-item scale (Study 2). Our findings provide partial support for the existence of a negative footprint illusion and reveal a role of individual variation in reflective reasoning dispositions in accounting for a limited element of differential susceptibility to this illusion.

Keywords: negative footprint illusion, individual variation, reasoning, environment, actively open-minded thinking

INTRODUCTION

Climate change is one of the most significant challenges facing the modern world (Hansen et al., 2013). The Intergovernmental Panel on Climate Change (IPCC) estimates that anthropogenic greenhouse gas emissions will cause up to a 1.5 degrees centigrade global mean increase in surface air and sea temperature by approximately 2035 (Intergovernmental Panel on Climate Change [IPCC], 2020). Human activity, in the form of food production and general consumption, is directly associated with emissions of greenhouse gases (Carlsson-Kanyama, 1998) such as carbon dioxide. Whilst advances in technology have attempted to mitigate the impact of greenhouse gas emissions on the environment (Bradley, 2009), the most significant barrier to developing sustainability and mitigating climate change is psychological in nature (Gifford, 2011).

Evidence indicates that people consistently act in ways that harm the environment, even when they believe their actions to be environmentally friendly (e.g., Hope et al., 2018). It has been widely demonstrated that people incorrectly reason that the addition of "eco-friendly" (or "green") items (e.g., environmentally certified houses) to a set of conventional items (e.g., standard houses) *reduces* the carbon footprint of the combined set of items (Holmgren et al., 2018a), whereas in fact the carbon footprint of the combined set *increases*. This reasoning bias is termed the *negative footprint illusion* (Gorissen and Weijters, 2016; Holmgren et al., 2018a,b; Sörqvist et al., 2020). The illusion has now been replicated many times, and has been shown to be insensitive to scale type (Gorissen and Weijters, 2016), expertise (Holmgren et al., 2018b), framing (Holmgren et al., 2019), quantity of additional items (i.e., "quantity insensitivity"; see Kim and Schuldt, 2018), experimental design (occurring in both between- and within-participants designs; see Holmgren et al., 2018a), and different stimulus materials such as foods (Gorissen and Weijters, 2016) and buildings (Holmgren et al., 2018a).

Research suggests that a cognitive bias referred to as "averaging bias" underpins the illusion, whereby reasoners fail to estimate the total environmental impact of a set of items, as requested, but instead provide judgments based on an assessment of the average environmental impact of items (Holmgren et al., 2018a). When environmentally friendly items are added to conventional items, an averaging process would readily give rise to the negative footprint illusion (Holmgren et al., 2018a). Consistent with research from Chernev and Gal (2010), people seem to engage in a "vice-virtue" categorization of items, whereby they classify different objects in a dichotomous fashion, for example, healthy versus unhealthy, or environmentally friendly versus conventional. After people have qualitatively classified objects, they subsequently produce a quantitative judgment, which is the average of their impact rather than their summative impact (Holmgren et al., 2019). This cognitive bias appears to be highly generalizable, with evidence indicating that it is present in a variety of real-world decision-making contexts, including in criminological (Lambert and Peytcheva, 2019), marketing (Weaver et al., 2012) and economic domains (Kunz et al., 2017).

Susceptibility to the negative footprint illusion can have an adverse impact on environment-related behavior and can plausibly exacerbate climate change. For example, people might buy more green products (which still have an environmental impact) than they normally would, in an effort to compensate for their use of conventional products (Sörqvist and Langeborg, 2019). It is, therefore, important to explore ways to eliminate the negative footprint illusion and its potential impact, which in turn necessitates acquiring a deeper understanding of the mechanisms that underpin the illusion and the nature of individual variation in people's susceptibility to it. The aim of the present research was to undertake empirical studies that might shed further light on the role played by individual differences in people's susceptibility to the negative footprint illusion.

Dispositional Individual Differences and the Negative Footprint Illusion

Dispositional factors have previously been implicated in relation to people's susceptibility to the negative footprint illusion, in terms of the extent to which individuals manifest "compensatory green beliefs" (Kaklamanou et al., 2015). Compensatory green beliefs reflect people's disposition to believe that endorsing particular pro-environment behaviors (e.g., not driving a car) can serve to compensate for less environment-friendly behaviors (e.g., not recycling). Kaklamanou et al. (2015) found that the overall endorsement of compensatory green beliefs is relatively low, but is nevertheless negatively correlated with demographic and dispositional factors – with increasing age, a higher income and educational status all being related to a lower endorsement of compensatory green beliefs. Furthermore, greater endorsement of compensatory green beliefs was found to be negatively associated with pro-ecological behavior, as measured in terms of General Ecological Behaviour (GEB), a willingness to engage in pro-ecological behavior (Kaiser et al., 2003) and green identity (e.g., Whitmarsh and O'Neill, 2010). MacCutcheon et al. (2020) have recently shown that people who were more likely to endorse compensatory green beliefs are more susceptible to the negative footprint illusion compared to individuals with lower endorsement scores, and therefore this forms a potentially important element of individual variation in susceptibility to the illusion.

However, the illusion does not appear to be solely explained by individual variation in environment-related dispositions. Research suggests the illusion persists even when controlling for measures such as environmental concern (Gorissen and Weijters, 2016), ecological values (Kim and Schuldt, 2018) and green consumer values (Kusch and Fiebelkorn, 2019). Such findings raise doubts about the impact of environment-related dispositional factors in explaining susceptibility to the negative footprint illusion. Critically, there has been scarce consideration of individual variation in the illusion, arising from more general dispositional factors related to thinking and reasoning. According to Kabanshi (2020), our ability to reason about environmental impact is biased in systematic ways, and behaviors detrimental to the environment are believed to be linked to general, deep-seated cognitive and dispositional factors associated with reasoning, judgment and decision-making. It therefore seems plausible that individual variation in *general* reflective reasoning dispositions could shed further light on people's tendency to implement an averaging process of the type that appears to underpin the negative footprint illusion. Such individual variation could, for example, take the form of the adoption of particular thinking styles (e.g., Stanovich, 2009) or tendencies toward impulsivity in responding (e.g., Moeller et al., 2001).

One thinking style or disposition that has been studied extensively is referred to as "actively open-minded thinking" (e.g., Sá et al., 1999; Stanovich and West, 2007). This captures individual variation in people's motivation to engage in rational thought (Stanovich, 2009) and has been demonstrated to be independent of cognitive ability, but highly correlated with people's success in making normatively rational judgments (Sá and Stanovich, 2001). Actively open-minded thinking has also been found to be related to the objective evaluation of arguments (Stanovich and West, 1998), alongside the ability to avoid falling foul of various types of cognitive bias, including confirmation bias (Baron, 1993) and knowledge bias (Sá and Stanovich, 2001). It is typically measured on a self-report scale, of which several versions have been constructed, each involving various compositions of latent factors (see Stanovich and West, 1997, 2007; Haran et al., 2013; Svedholm-Häkkinen and Lindeman, 2018). It is noteworthy that, to date, there has been no exploration of how generic thinking styles might be related to reduced susceptibility to the negative footprint illusion.

In exploring thinking styles, a critical issue relates to the extent to which individuals engage in "intuitive" versus "reflective" thought (e.g., Evans, 2010, 2018; Evans and Stanovich, 2013a,b). According to Evans and Stanovich (2013a,b), thinking can be categorized into two qualitatively distinct types of mental processes, that is, "Type 1," intuitive, heuristic processes versus "Type 2," reflective, analytic processes. According to this "dual process" perspective on reasoning, Type 1 processes have two defining features: (i) they are relatively undemanding of working memory resources; and (ii) they are autonomous, which means that they run obligatorily to completion whenever they are activated. Type 1 processes also tend to be fast, high capacity, non-conscious and capable of operating in parallel, but these are merely correlated rather than defining features. Type 2 processes, on the other hand, are defined in terms of requiring working memory resources (Evans, 2008) and being focused on cognitive decoupling and mental simulation, which are critical for determining the viability of default judgments arising from Type 1 processes. Type 2 processes also tend to be slow, capacity limited, conscious and serial, but again, these are viewed by Evans and Stanovich (2013a,b) as being correlated features rather than defining features.

From a dual-process perspective, actively open-minded thinking can be viewed as a measure of people's disposition to engage in Type 2 reasoning, thereby potentially enabling biases to be overcome that have arisen from the operation of intuitive, Type 1 processing. This view, in which intuitive Type 1 processes provide *default* reasoning responses that can be reflected on and potentially overturned or accepted by Type 2 processes, is referred to as involving a "default-interventionist" processing structure (e.g., Evans and Stanovich, 2013a,b). It is unclear, however, whether the averaging bias that underpins the negative footprint illusion is a consequence of an autonomous, "rough-and-ready," intuitive approach to averaging happening at a Type 1 level or is a result of more reflective Type 2 reasoning that is simply inappropriate for the task at hand, which requires a summative judgment.

As both Evans (2012, 2018) and Stanovich (2018) are at pains to point out, biased reasoning is not just the preserve of Type 1 processing, as biases can also arise during Type 2 processing, for example, through applying effective reasoning processes to a misconstrued problem representation or through the application of sub-optimal, reflective processes (i.e., "defective mindware"; Stanovich, 2018). One possible explanation for what is arising in the case of individuals who fall foul of the negative footprint illusion is that they are misconstruing the problem from the outset and applying an "intuitive" averaging process based on a qualitative "vice-virtue" categorization of items. If such individuals do go on to apply Type 2 reflective reasoning, then they will only overcome the averaging bias if they appreciate that the task requires a summative judgment as opposed to one based on averaging. If Type 2 reasoning does not elicit this realization (e.g., through a careful check of the instructions), then any further Type 2 processing that is applied to the task would simply serve to *rationalize* the averaging-based judgment arising from the default, Type 1 process. This account would predict that individuals higher in actively open-minded thinking dispositions might be more likely to appreciate from the outset that the task requires summation or, alternatively, that they might be more likely to engage in a checking process at the Type 2 stage, which would reveal that an averaging response is not what is required and that a summation response is needed. Either way, increased levels of actively open-minded thinking should be associated with reduced susceptibility to the negative footprint illusion.

Another task in the reasoning literature that is claimed to capture people's dispositions to engage in reflective reasoning is the Cognitive Reflection Test (CRT; Kahneman and Frederick, 2002). This involves presenting participants with problems that lend themselves to relatively immediate and intuitive responses that seem correct, but which are, in fact, incorrect and need to be overturned by reflective thinking to attain the correct solution. Toplak et al. (2011) highlight that it is a particularly strong performance measure of the extent to which individuals can overcome intuitive errors that result from "miserly processing" by engaging in further reflective thought (but see Stupple et al., 2017, for a critical analysis of the role of miserly processing in CRT performance, and Stanovich, 2018, for counterarguments). Of relevance to the current research is the potential for the CRT to offer a measure of rational thought that is not otherwise explained by common variables such as executive functioning and intelligence, but which nevertheless captures the disposition for people to engage in reflective reasoning and their ability to do so effectively (i.e., the CRT can perhaps best be viewed as both a dispositional *and* an ability measure; Campitelli and Gerrans, 2014).

In sum, the present research aimed to advance an understanding of the role played by individual differences in people's susceptibility to the negative footprint illusion, with a key focus being placed on environment-specific reasoning dispositions as well as dispositional factors relating to the engagement of Type 2, reflective processes that may promote more accurate reasoning.

STUDY 1

In this study, we sought to determine the extent to which susceptibility to the negative footprint illusion is associated with: (i) environment-specific dispositions, measured by the endorsement of compensatory green beliefs and the use of the Environmental Concerns Questionnaire; (ii) general dispositions toward Type 2, analytic reasoning, measured in terms of an actively open-minded thinking (AOT) scale; and (iii) general dispositions toward, and abilities at, Type 2 reasoning, as measured by performance on the Cognitive Reflection Test (CRT).

We also included two further individual differences measures as exploratory variables. The first was a measure of "impulsivity" to capture impulsive personality traits (e.g., Barratt, 1959), which may play a role in incorrect responding on negative footprint illusion tasks. The second was a measure of people's inclination to make incorrect probability judgments when reasoning with problems that give rise to the so-called "conjunction fallacy," whereby people rate the conjunction of two events as being more likely than either event alone (Tversky and Kahneman, 1983; see also Fantino et al., 1997). We included this measure because it has previously been suggested that the illusion bears some conceptual similarity to fallacious reasoning about conjunctive events (Holmgren et al., 2018a). More specifically, both the conjunction fallacy and the negative footprint illusion appear to revolve around people making biased estimates regarding the conjunction of attributes or events, albeit assigning a higher value to a conjunctive probability in the case of the conjunction fallacy and assigning a lower value to a conjunctive carbon footprint in the case of the negative footprint illusion. Of course, the two biases may not necessarily be underpinned by the same cognitive operations or mechanisms (cf. Holmgren et al., 2018a), but their apparent conceptual overlap represents an issue worth investigating empirically. We finally note that this study afforded an opportunity to determine whether the well-documented negative footprint illusion holds in the case of small objects (i.e., apples) in addition to large items (i.e., buildings), which have featured more extensively in prior studies.

Method

Participants

The participants were 120 adults (72 male) with a mean age of 36 years (*SD* = 13 years). Participants were recruited via Prolific Academic and received the standard platform payment rate. The study received Ethical Clearance from the University of Gävle, Sweden.

Design

A within-participants design with two factors was employed: item type (buildings vs. apples) and carbon footprint estimation task (conventional vs. conventional plus eco-friendly "green" addition). The dependent variables were the carbon footprint rating for each task and the outcomes of each of the individual difference measures (environmental concerns, endorsement of compensatory green beliefs, actively open-minded thinking, impulsiveness, CRT performance and conjunction fallacy susceptibility).

Materials

Carbon Footprint Estimation Tasks

Rating the Carbon Footprint of Apples. Participants were presented with contextual information pertaining to the carbon footprint of apples, and a graphic depicting 10 conventional apples in a consumer's food basket. They were asked to make a judgment rating of the carbon footprint of these 10 apples on a scale from "low carbon footprint" ('1') to "high carbon footprint" ('9'). Participants were then informed that the consumer had returned to the store and added five eco-friendly apples to their basket. Participants were then required to estimate the carbon footprint of the 15 apples in the consumer's food basket on a scale from "low carbon footprint" ('1') to "high carbon footprint" ('9').

Rating the Carbon Footprint of Buildings. Participants were asked to provide a rating for the number of trees required to compensate for the carbon footprint arising from the energy used by a community of buildings (i.e., houses). Participants were presented with contextual information, alongside a graphic of 75 conventional buildings marked in orange, and information relating to greenhouse gas emissions. Participants were asked to mark the number of trees used to compensate for energy use on a scale from 1 to 100. They were then presented with the same contextual information as previously, alongside a description of the additional 25 green buildings. A graphic depicting 75 conventional buildings in orange together with the 25 new eco-friendly buildings in green was presented. They were asked once again to mark on the scale from 1 to 100 how many trees they estimated the suburb would need to compensate for its energy use per month. Please see the **Supplementary Material** for detailed carbon footprint estimation task descriptions and graphics.

Environment Specific Dispositional Measures

Biospheric, Altruistic and Egoistic Environmental Concerns Questionnaire. The Biospheric, Altruistic and Egoistic Environmental Concerns Questionnaire (see Schultz, 2001) involves 12 items, with participants being asked to rate each item on a 9-point scale (1 = not at all concerned to 9 = very concerned) in response to questions that are framed as follows: "*How concerned are you that today's environmental problems will affect.*?" The 12 presented items related to three areas of environmental concern: biospheric (e.g., animals), altruistic (e.g., future generations) and egoistic (e.g., my future).

Compensatory Green Beliefs Questionnaire. Green beliefs were measured using a 16-item questionnaire (see Kaklamanou et al., 2015). Participants were asked to rate on a 5-point scale (1 = strongly disagree to 5 = strongly agree) how closely each statement fitted their beliefs (e.g., "*Not driving a car compensates for not recycling*").

General Dispositional Measures of Reasoning

Actively Open-Minded Thinking Scale. The study utilized the shortened 7-item version of the Actively Open-Minded Thinking Scale (AOT-7), developed by Haran et al. (2013). Participants were asked to respond to seven statements (e.g., "*Allowing oneself to be convinced by an opposing argument is a sign of good character*") on a 7-point scale, ranging from 1 (completely disagree) to 7 (completely agree).

Cognitive Reflection Test. Participants were presented with the 6-item CRT developed by Primi et al. (2016) and referred to by them as the CRT-L (for CRT-Long). The 6-item version contains three items originally used by Frederick (2005) and a further three items added by Primi et al. (2016) to create the CRT-L. Please refer to the **Supplementary Material** for details of the CRT-L and the scoring method.

Other Measures

Barratt Impulsiveness Scale. The Barratt Impulsiveness Scale (Barratt, 1959; Patton et al., 1995; Stanford et al., 2009) is a 30-item questionnaire designed to measure impulsive personality traits in terms of the ways in which participants think and act (e.g., "*I am happy-go-lucky*"). Each item is rated on a 4-point scale (1 = rarely/never, 2 = occasionally, 3 = often, 4 = almost always/always).

Susceptibility to the Conjunction Fallacy. To assess people's susceptibility to the conjunction fallacy two tasks were utilized from the materials presented by Tversky and Kahneman (1983): the famous "Linda Problem" and a less well known "Die-Roll Problem." Participants were asked to select their answer from the options provided. Please refer to the **Supplementary Material** for details of these tasks and the scoring method.

Procedure

The study was deployed using Qualtrics. Participants read an information sheet and completed a consent form. A carbon footprint rating task (apples or buildings item) was completed, with item type counterbalanced across participants. For each task, participants were presented with instructions and contextual information. They were asked to make their initial carbon footprint rating for the set of conventional items, followed by a second rating for the conventional items with the addition of the eco-friendly items. On completing each rating, participants were asked to indicate on a 9-point scale, the extent to which they solved the problem via either "intuition" ('1') or "analysis" ('9'). Please refer to the **Supplementary Material** for definitions of these terms. Participants were also asked to respond on a scale from "not at all confident" ('1') to "very confident" ('9') to indicate their confidence in their given response for each of the carbon footprint rating tasks.

On completion of one of the two carbon footprint rating judgments, participants then completed the individual differences questionnaires in a fixed order, as follows: (i) Biospheric, Altruistic and Egoistic Environmental Concerns; (ii) AOT-7; (iii) Barratt Impulsiveness Scale; and (iv) Compensatory Green Beliefs Questionnaire. The CRT-L questions were then presented in a random order. Participants were given a maximum time of 3 min per question to generate their solution and write their response in the text field. The two conjunction fallacy problems were then presented, with participants permitted up to 3 min to respond to each problem from the fixed response options provided. Participants were then presented with the second carbon footprint rating task (apples or buildings), with the initial carbon footprint rating with conventional items being followed by a second rating with the addition of eco-friendly items. Finally, participants were debriefed.

Results

Rating the Carbon Footprint for the Apples and Buildings Item Types

A 2 × 2 repeated-measures analysis of variance (ANOVA) test was conducted on the carbon footprint ratings (mean proportions) to identify the presence of a negative footprint illusion. This revealed a significant main effect of item type on carbon footprint ratings, $F(1,119) = 55.29$, $p < 0.001$, $\eta_p^2 = 0.32$, with carbon footprint ratings being significantly higher for the buildings item type ($M = 0.66$, $SE = 0.02$) than for the apples item type ($M = 0.46$, $SE = 0.02$). There was no significant main effect of the carbon footprint estimation task, $F(1,119) = 0.49$, $p = 0.484$, $\eta_p^2 = 0.01$, with ratings for the conventional item estimate ($M = 0.56$, $SE = 0.01$) being similar to ratings for the conventional plus eco-friendly items estimate ($M = 0.55$, $SE = 0.02$).

There was a significant interaction between item type and carbon footprint estimation task, $F(1,119) = 14.60$, $p < 0.001$, $\eta_p^2 = 0.11$. Pairwise comparisons with a Bonferroni adjustment revealed the presence of a negative footprint illusion in the case of the buildings item type ($p = 0.004$), with estimates for the conventional buildings ($M = 0.68$, $SE = 0.02$) significantly higher than estimates for the conventional plus eco-friendly buildings ($M = 0.64$, $SE = 0.03$). However, there was no such negative footprint illusion for the apples item type, with ratings for conventional apples ($M = 0.44$, $SE = 0.02$) being significantly *lower* ($p = 0.027$) than for the ratings for conventional apples with the addition of eco-friendly apples ($M = 0.47$, $SE = 0.02$).

Ratings of Solution Confidence for the Apples and Buildings Item Types

A 2 × 2 repeated measures ANOVA revealed that participants were significantly more confident in their judgments of the carbon footprint of the apples item type ($M = 4.15$, $SE = 0.17$) than in their judgments for the buildings item type ($M = 2.84$, $SE = 0.15$), $F(1,119) = 62.92$, $p < 0.001$, $\eta_p^2 = 0.35$, seemingly indicating a degree of accurate metacognitive awareness given that greater confidence with the apples item type was also associated with normatively correct responding in relation to the carbon footprint estimation task for this item type. In contrast, less confidence arose for carbon footprint estimates associated with the buildings item type, which also gave rise to negative footprint illusion.

There was no significant main effect of carbon footprint estimation task in terms of whether the rating was for conventional items alone ($M = 3.55$, $SE = 0.14$) in comparison to conventional items with the addition of eco-friendly items

(M = 3.44, SE = 3.44), $F(1,119) = 2.46$, $p = 0.119$, $\eta_p^2 = 0.02$, although the means indicate that participants were slightly less confident in providing their second rating of the conventional plus eco-friendly items combined. There was no significant interaction between item type and carbon footprint estimation task for confidence ratings, $F(1,119) = 0.77$, $p = 0.384$, $\eta_p^2 = 0.01$.

Ratings of Solution Strategies for the Apples and Buildings Item Types

A 2 × 2 repeated measures ANOVA revealed that solutions strategies were self-reported to be significantly more intuitive in the case of carbon footprint ratings for the buildings item type (M = 3.37, SE = 0.16) in comparison to the apples item type (M = 4.45, SE = 0.18), $F(1,119) = 32.42$, $p < 0.001$, $\eta_p^2 = 0.01$. There was no significant main effect of carbon footprint estimation task, $F(1,119) = 1.02$, $p = 0.315$, $\eta_p^2 = 0.01$, with self-reported solution strategies for the first estimate (M = 4.45, SE = 0.18) not being significantly different to self-reported solution strategies for the second estimate (M = 3.37, SE = 0.16), although the mean values indicate that the second estimates were slightly more intuitive than the more analytic first estimates. There was no significant interaction between item type and carbon footprint estimation task, $F(1,119) = 0.54$, $p = 0.465$, $\eta_p^2 = 0.01$. These solution-strategy findings again suggest a degree of metacognitive awareness on the part of participants, who seem to be able to sense that their responses to the buildings item type (which gave rise to a negative footprint illusion) are more intuitive than their responses to the apples item type (which gave rise to normative responding).

Individual Differences Measures

The Biospheric, Altruistic and Egoistic Environmental Concerns Questionnaire, Compensatory Green Beliefs Questionnaire, Barratt Impulsiveness Scale (with subdivision into three second order factors; attentional, motor and non-planning impulsiveness) and the AOT-7 were all scored according to instructions, with higher scores reflecting a greater degree of each trait. The CRT-L was scored according to the process outlined by Pennycook et al. (2016). A CRT-*Reflective* score was derived to measure the ability of individuals to overcome intuitive responses and reach a normatively correct response, whilst the CRT-*Intuitive* score was derived representing a measure of solutions that spring to mind rapidly and seem plausible, but which are incorrect.

To provide a measure of an individual's susceptibility to the negative footprint illusion, a score was calculated for each item type (apples or buildings) to indicate the extent of *change* between the estimate for conventional items only and the second estimate for conventional plus eco-friendly items. The raw score for the conventional items only was subtracted from the raw score for the conventional plus eco-friendly items, resulting in either a 0 (no change), a positive score (no negative footprint illusion) or a negative score (a negative footprint illusion) for each participant. These change scores formed the key measure of susceptibility to the negative footprint illusion that was then used in computing correlations with individual differences measures. **Table 1** shows Pearson correlation coefficients for the relationship between the change scores for the carbon footprint ratings for both the apples and buildings item types and all measures of individual differences: environmental concerns, endorsement of compensatory green beliefs, impulsiveness, actively open-minded thinking, CRT-*Reflective* and CRT-*Intuitive*. Skewness and kurtosis scores were computed for each variable to determine whether scores approached a normal distribution and were deemed to fall within an acceptable range (Bulmer, 2003).

The key finding from **Table 1** is a significant positive correlation between actively open-minded thinking and the change score for the buildings item type ($r = 0.186$, $p = 0.042$, with 3.5% of the variance in the change score accounted for). Given that a positive change score is indicative of a normatively correct response to the carbon footprint estimation task, this correlation suggests that greater actively open-minded thinking is associated with a *reduction* in susceptibility to the negative footprint illusion for the buildings item type. The conjunction fallacy tasks were scored by noting a correct or an incorrect response for each participant for both the Linda Problem and the Die-Roll Problem. Point bi-serial correlations indicated there was no relationship between the ability to endorse the likelihood of one single event occurring (more so than two events happening in conjunction) and reduced susceptibility to the negative footprint illusion (all ps > 0.05).

Discussion

The findings of this study support previous research in partially replicating a negative footprint illusion (Gorissen and Weijters, 2016; Holmgren et al., 2018a,b). Estimating the number of trees necessary to offset the carbon footprint of a community of conventional buildings, along with a community of additional eco-friendly buildings, was judged to require fewer trees than a community comprised of conventional buildings alone. In contrast, no negative footprint illusion was demonstrated in the context of rating the carbon footprint of apples (participants correctly judged that a basket of 10 conventional apples and 5 eco-friendly apples would have a greater carbon footprint than a basket of 10 conventional apples alone).

Interestingly, participants' confidence ratings and strategy ratings in relation to their carbon footprint estimates indicated the presence of a metacognitive component, with confidence ratings and analytic reasoning ratings being significantly higher where there was no susceptibility to the illusion (apples task), than where susceptibility to the illusion was present (buildings task). These findings may provide some converging evidence in support of the negative footprint illusion being underpinned by Type 1 reasoning, as would arise from the operation of an intuitively applied averaging bias. We do note, however, the need for caution in this interpretation of the metacognitive data, given that the instructional context surrounding the buildings task was more complex than that surrounding the apples task. Therefore, the metacognitive judgments were potentially sensitized to the perceived differential complexity of the buildings and apples tasks, rather than to differences in the underpinning reasoning processes used to generate carbon footprint estimates.

Study 1 provides little evidence in support of an influence of either general or more environment-specific thinking

TABLE 1 | Correlation matrix showing the relationship (Pearson correlation coefficients) between individual differences measures and the carbon footprint change scores for the apples and buildings item types.

Variables	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
1. Change score for apples item type											
2. Change score for buildings item type	0.059										
3. Environmental concerns	0.062	−0.015									
4. Compensatory green beliefs	0.111	−0.057	−0.224*								
5. Impulsivity	0.007	−0.106	−0.195*	0.327**							
6. Attentional impulsivity	0.092	−0.132	−0.167	0.340**	0.797**						
7. Motor impulsivity	−0.091	0.023	−0.030	0.162	0.765**	0.416**					
8. Non-planning impulsivity	0.029	−0.148	−0.264**	0.298**	0.851**	0.583**	0.423**				
9. Actively open-minded thinking	0.107	0.186*	0.119	−0.344**	−0.424**	−0.242**	−0.375**	−0.387**			
10. CRT-*Intuitive*	−0.028	−0.065	0.118	0.141	0.250**	0.156	0.257**	0.187*	−0.202*		
11. CRT-*Reflective*	0.055	0.106	−0.204*	−0.130	−0.208*	−0.103	−0.243**	−0.149	0.221*	−0.865**	

N = 120 for each cell.
**Correlation is significant at the $p < 0.05$.*
***Correlation is significant at the $p < 0.001$.*

dispositions in susceptibility to the negative footprint illusion. With respect to environment-specific thinking dispositions, the findings failed to reveal any effect of environmental concern (Schultz, 2001) or the endorsement of compensatory green beliefs (Kaklamanou et al., 2015) in explaining susceptibility to the negative footprint illusion. This lack of evidence is at odds with previous findings reported by MacCutcheon et al. (2020), which demonstrate a link between compensatory green beliefs and susceptibility to the negative footprint illusion. We note, however, that MacCutcheon et al. (2020) demonstrated this significant association using a 9-point scale, rather than the 5-point scale adopted in the present study, which suggests that our replication failure might be attributable to a lack of sensitivity arising from a reduced range of scale.

A variety of dispositional factors that relate to general reflective reasoning ability were also explored, including actively open-minded thinking, performance on the cognitive reflection test and the conjunction fallacy and impulsivity. These questionnaires and tasks failed to predict susceptibility to the negative footprint illusion, with the single exception being that of actively open-minded thinking, as measured by the AOT-7 scale (Haran et al., 2013). Higher scores on this scale significantly predicted reduced susceptibility to the illusion on the buildings item type, albeit with a somewhat small degree of variance (3.5%). However, we note that the AOT-7 may have the potential to lead to spurious findings because of limitations in its capacity to measure actively open-minded thinking in an accurate manner, with questions being raised recently regarding its validity and reliability (e.g., Svedholm-Häkkinen and Lindeman, 2018).

STUDY 2

In Study 2, we sought to replicate and extend the findings of Study 1 whilst also increasing the test power of the study. Study 1 revealed a negative footprint illusion with buildings as the carbon producing item, yet it failed to identify the illusion in the apples task. This finding is at odds with previous research demonstrating the presence of a negative footprint illusion across various types of items, including foods (Gorissen and Weijters, 2016), buildings (Holmgren et al., 2018a), and cars (Holmgren et al., 2021). We suggest that there are three potential explanations for this absence of a negative footprint illusion with the apples task.

First, the illusion might be less pronounced for small physical items (e.g., fruit), in comparison to larger items (e.g., cars or buildings). Second, it might be sensitive to the relative quantity of eco-friendly "green" items that are added to conventional items. In this respect we note that Study 1 implemented tasks with 75 conventional houses plus 25 eco-friendly houses (i.e., the eco-friendly addition was 33% of the conventional item set) versus 10 conventional apples plus 5 eco-friendly apples (i.e., the eco-friendly addition was 50% of the item set). Third, the scale that was employed with the apples task, where ratings were possible in the range from 1 to 9, might have led to the absence of a negative footprint illusion because this scale was less sensitive than the one used in the context of the buildings task, which ranged from 1 to 100.

In Study 2, we sought to disentangle these competing explanations by implementing three key changes to the carbon footprint estimation tasks. First, we included an additional carbon footprint estimation with an item between the size of apples and buildings. In this task participants were asked to rate the carbon footprint of a fleet of conventional cars, followed by a rating for the same fleet of conventional cars to which eco-friendly cars had been added. Second, the carbon footprint estimation scale was made consistent across each of the three tasks to avoid any impact of scale sensitivity. Third, the ratio of conventional items to eco-friendly additions was standardized across each task, resulting in eco-friendly additions equal to 50% of the conventional items (e.g., 10 apples plus 5 eco-friendly "green" apples).

As in Study 1, we aimed to determine the extent to which susceptibility to the negative footprint illusion is associated with environment-specific dispositions (measured by the endorsement of compensatory green beliefs and the environmental concerns). However, in a deviation from Study 1, the response scale for the Compensatory Green Beliefs Questionnaire was extended from a 5-point to a 9-point Likert scale. The purpose of this was twofold: (i) to increase the sensitivity to the scale; and (ii) to maintain consistency with previous research (MacCutcheon et al., 2020), which revealed a significant relationship between endorsement of compensatory green beliefs and the negative footprint illusion.

The current study also sought once again to explore the relationship between people's general dispositions toward Type 2, reflective reasoning and the negative footprint illusion, but with some amendments to the scales and tasks. Study 1 employed the brief AOT-7 scale (Haran et al., 2013) as opposed to the full AOT-41 scale (e.g., Stanovich and West, 1997; Sá et al., 1999). The AOT-7 correlates relatively poorly with the AOT-41 (r = 0.66) in comparison to the recently developed AOT-17 scale (r = 0.89; Svedholm-Häkkinen and Lindeman, 2018). Furthermore, the AOT-7 does not permit the exploration of the latent factors underpinning the scale (dogmatism, fact resistance, liberalism and belief personification). Therefore, in Study 2 we employed the AOT-17, which retains an acceptable level of internal consistency (see Heijltjes et al., 2015), provides a stronger correlation with the original AOT-41, and permits an examination of the latent sub-factors.

In Study 2, we also employed a shortened version of the CRT, adopting only the three items that were added to the original version by Primi et al. (2016). We implemented this change as we were concerned about the familiarity that our research participants had with the original three item CRT, especially given suggestions that prior exposure to the CRT might result in higher scores (Haigh, 2016). Study 1 indicated that participants' familiarity of the original CRT items (i.e., whether they had previously encountered any of the items) was much greater (33%) than for the new items of the CRT-L (0.03%). That said, comparable mean percentage solution rates were observed between the original CRT (45%) and the CRT-L (50%), which aligns with Bialek and Pennycook's (2018) evidence that prior exposure to the CRT does not influence its predictive power. Nevertheless, to mitigate against any issues relating to prior exposure to the original CRT, we decided simply to employ the new three items from the CRT-L.

Study 1 failed to show any association between incorrect judgments in the conjunction fallacy tasks and individual differences in susceptibility to the negative footprint illusion and we therefore omitted these tasks from Study 2. However, we retained the measure of impulsivity (Barratt, 1959) to capture any potential impulsive personality traits that might play a role in incorrect responding on tasks that induce a negative footprint illusion. In this respect it is noteworthy that in Study 1 the impulsivity scale showed a highly significant positive correlation with the CRT-*Intuitive* measure as well as highly significant negative correlation with the CRT-*Reflective* measure and AOT score, suggesting overlapping variance with these measures that might manifest in individual differences in people's susceptibility to the negative footprint illusion in a higher-powered study.

Method

Participants

A power analysis was employed to determine an appropriate *a priori* sample size. This indicated a minimum sample size of 266 participants for a 0.4 Cohen's d with 90% power. The participants were 269 adults (93 male) with a mean age of 31 years (SD = 11 years). All participants were recruited via Prolific Academic and received the standard platform payment. The study received Ethical Clearance from the Ethics Board (PsySoc: 507) at the University of Central Lancashire, United Kingdom.

Design

A 3 × 2 within-participants design was employed. The factors were item type (buildings vs. cars vs. apples) and carbon footprint estimation task (conventional vs. conventional plus eco-friendly "green" addition). The dependent variables were the carbon footprint rating for each task and the outcomes of each of the individual difference measures (environmental concerns, endorsement of compensatory green beliefs, actively open-minded thinking, impulsiveness, CRT performance), which are described in more detail below. As in Study 1, additional measures were also taken for self-reported solution strategies for the carbon footprint estimation tasks and subjective confidence in solutions.

Materials

Carbon Footprint Estimation Tasks

Three carbon footprint estimation tasks were constructed, in which participants were asked to rate the carbon footprint of apples, cars and buildings. Participants were given contextual information relating to both the conventional items, and the conventional + "green" eco-friendly items. Participants rated the carbon footprint of a basket of 10 apples, and subsequently the carbon footprint of a basket of 10 apples + 5 "green" eco-friendly apples (15 apples in total). They rated the carbon footprint of a fleet of 30 petrol cars, and subsequently the carbon footprint of a fleet of 30 petrol cars + 15 "green" eco-friendly cars (45 cars in total). Finally, they rated the carbon footprint of a community of 50 conventional houses, and a community of 50 conventional houses + 25 "green" eco-friendly houses (75 houses in total).

For each of the three tasks, participants made their conventional and conventional plus "green" eco-friendly addition carbon footprint estimation ratings on a sliding scale from 1 (1ow carbon footprint) to 100 (high carbon footprint). The score attributed to the scale position was not visible to participants when making their estimation. This was to minimize participants' attempts to make carbon footprint estimations relative to a previous task scenario. The sliding scale was set to the middle (this translated to a score of 50). In contrast to Study 1, participants were provided with an anchor point for each task, relative to each carbon producing item. For example, in the context of the apples task participants were provided with the following: "*As a reference point for your estimations, consider that a basket of 10 mixed fruit would score in the middle of the scale.*" Further details of each negative footprint illusion task can be found in the **Supplementary Material**. Following each carbon footprint estimation, participants rated their confidence in their given response (on a scale from 1 = "not very confident" to 9 = "very confident') and indicated their solution strategy (on a scale from 1 = "intuition" to 9 = "analysis"). Please see the **Supplementary Material** for detailed carbon footprint estimation task descriptions and graphics.

Environmental and General Dispositional (Individual Differences) Measures

These measures remained largely consistent with Study 1, with the following notable changes. First, in the compensatory green beliefs task, participants rated each statement on a 9-point scale (1 = "strongly disagree" to 9 = "strongly agree"), rather than on a 5-point scale, as in Study 1. Second, the conjunction fallacy tasks were omitted. Third, the AOT-7 was replaced with the AOT-17 (Svedholm-Häkkinen and Lindeman, 2018). Finally, we retained just the three less familiar items from the 6-item CRT-L (Primi et al., 2016).

Procedure

Participants read an information sheet and completed a check box consent form. They completed one of the three carbon footprint estimation tasks, provided a confidence and solution strategy rating, before completing the individual differences questionnaires in a fixed order: (i) Biospheric, Altruistic and Egoistic Environmental Concerns; (ii) AOT-17; (iii) Barratt Impulsiveness Scale; and (iv) Compensatory Green Beliefs Questionnaire. Following the presentation of the four scales, participants completed a further carbon footprint estimation task, followed by the three CRT questions, presented in a random order. Participants then completed the final carbon footprint estimation task. Carbon footprint estimation task presentation order was fully counterbalanced across participants. Participants were debriefed at the end of the study.

Results

Rating the Carbon Footprint for the Apples, Cars and Buildings Item Types

Analyses were conducted on the raw carbon footprint estimation scores (1 indicating a low carbon footprint to 100 indicating a high carbon footprint). A 3 (item type: apples vs. cars vs. buildings) $\times$ 2 (rating: conventional vs. conventional plus eco-friendly "green") repeated-measures ANOVA was conducted on the carbon footprint ratings to explore the presence of a negative footprint illusion across the three different carbon producing item types. A Mauchley's Test of Sphericity indicated that the assumption of sphericity had been violated for both item type ($\chi^2 = 48.94$, $p < 0.001$) and the rating by item type interaction ($\chi^2 = 33.15$, $p < 0.001$) and a Greenhouse–Geisser correction was applied.

There was a significant main effect of item type, $F(1.71,459.10) = 539.92$, $p < 0.001$, $\eta^2_p = 0.67$. The carbon footprint ratings were significantly higher for the cars item type ($M = 73.84$, $SE = 0.91$) in comparison to the buildings item type ($M = 69.28$, $SE = 0.89$, $p < 0.001$) or the apples item type ($M = 36.66$, $SE = 1.03$, $p < 0.001$). The carbon footprint ratings for the buildings item type were significantly higher than for the apples item type ($p < 0.001$). Incidentally, participants estimated cars to have the highest carbon footprint, followed by buildings, and then apples. The ratings for the conventional items only ($M = 63.51$, $SE = 0.62$) were significantly higher than for the conventional plus eco-friendly "green" items ($M = 56.34$, $SE = 0.81$); $F(1,268) = 97.02$, $p < 0.001$, $\eta^2_p = 0.27$. A significant reduction in rating scores with the addition of eco-friendly "green" items, indicates the presence of a general negative footprint illusion.

There was a significant interaction between item type and rating, $F(1.79,479.95) = 50.26$, $p < 0.001$, $\eta^2_p = 0.16$. Pairwise comparisons with a Bonferroni adjustment applied for multiple comparisons were conducted to reveal the source of the interaction. A negative footprint illusion was identified for the buildings item type, with the conventional plus eco-friendly "green" ratings ($M = 64.06$, $SE = 1.13$) resulting in significantly lower carbon footprint estimations than for the conventional ratings only ($M = 74.49$, $SE = 0.89$), $p < 0.001$. This effect was also found with the cars item type, again with conventional plus eco-friendly "green" ratings ($M = 68.50$, $SE = 1.15$) resulting in significantly lower carbon footprint estimations than for the conventional ratings only ($M = 79.17$, $SE = 0.97$), $p < 0.001$. However, for the apples item type, the conventional ratings ($M = 36.86$, $SE = 1.12$) did not differ significantly from the conventional plus eco-friendly "green" ratings ($M = 36.46$, $SE = 1.12$), $p = 0.65$, therefore suggesting the presence of a "zero footprint illusion" for this task (see Holmgren et al., 2021).

Ratings of Solution Confidence for the Apples, Cars and Buildings Item Types

A further 3 (item type: apples vs. cars vs. buildings) $\times$ 2 (rating: conventional vs. conventional plus eco-friendly "green") repeated-measures ANOVA was conducted on the raw confidence rating scores. A Mauchley's Test of Sphericity indicated that the assumption of sphericity had been violated for both item type ($\chi^2 = 17.38$, $p < 0.001$) and the rating by item type interaction ($\chi^2 = 7.18$, $p = 0.028$). A Greenhouse–Geisser correction was applied to adjust for the violation of sphericity. A main effect of item type was identified, $F(1.88,504.23) = 27.79$, $p < 0.001$, $\eta^2_p = 0.09$. Participants indicated significantly higher confidence when rating the carbon footprint of cars ($M = 5.71$, $SE = 0.09$) in comparison to buildings ($M = 5.40$, $SE = 0.10$) and apples ($M = 5.00$, $SE = 0.11$), both $ps < 0.001$. Participants were also significantly more confident when rating the carbon footprint of buildings in comparison to apples ($p < 0.001$). Furthermore, a main effect of rating, $F(1,268) = 11.39$, $p < 0.001$, $\eta^2_p = 0.04$, indicated that confidence was greatest when making a carbon footprint rating of conventional items ($M = 5.46$, $SE = 0.09$) in comparison to conventional plus "green" eco-friendly items ($M = 5.28$, $SE = 0.09$).

There was a significant interaction between item type and rating, $F(1.95,522.14) = 5.99$, $p = 0.003$, $\eta^2_p = 0.02$. Pairwise comparisons with a Bonferroni adjustment explored the source of this interaction. For the buildings task, there was no significant difference in confidence ratings for the conventional rating ($M = 5.44$, $SE = 0.11$) and the conventional plus eco-friendly rating ($M = 5.36$, $SE = 0.10$), $p = 0.30$. The same pattern was found for the apples task; there was no significant difference in confidence ratings for the conventional rating (M = 5.03, SE = 0.12) and the conventional plus eco-friendly rating (M = 4.98, SE = 0.11), p = 0.60. However, for the cars task, participants were significantly more confident in their conventional rating (M = 5.91, SE = 0.10) in comparison to their conventional plus eco-friendly rating (M = 5.51, SE = 0.10), $p < 0.001$.

Ratings of Solution Strategy for the Apples, Cars and Buildings Item Types

A final 3 (item type: apples vs. cars vs. buildings) × 2 (rating: conventional vs. conventional plus eco-friendly "green") repeated-measures ANOVA was conducted on the raw solution strategy rating scores. A Greenhouse–Geisser correction was applied to the rating by item type interaction effect (χ^2 = 17.24, $p < 0.001$), given a Mauchley's Test of Sphericity revealed the assumption of sphericity had been violated. For the solution strategy ratings, there was a significant main effect of item type, $F(2{,}536)$ = 12.03, $p < 0.001$, η_p^2 = 0.04, with no significant difference in solution strategy between the buildings item type (M = 5.02, SE = 0.11) and cars item type (M = 5.03, SE = 0.12), p = 1.00. Solution strategy responses were significantly more intuitive for the apples item type (M = 4.56, SE = 0.11) in comparison to the buildings and cars item types (both ps < 0.001). There was a significant main effect of rating, $F(1{,}268)$ = 4.00, p = 0.047, η_p^2 = 0.02. Conventional ratings were significantly more intuitive (M = 4.80, SE = 0.10) than conventional plus eco-friendly ratings (M = 4.91, SE = 0.10). For the solution strategy scores, there was no significant interaction between item type and rating, $F(1.82{,}504.46)$ = 1.01, p = 0.33, η_p^2 = 0.00.

Individual Differences Measures

Consistent with Study 1, a change score was computed for the apples, cars and buildings tasks. For each of the change scores, and the individual differences variables, scores were checked for skew and kurtosis, and all were deemed to be within an acceptable range. The relationship (Pearson correlation coefficients) between the change scores for the apples, cars and buildings tasks and individual difference measures (environmental concerns, compensatory green beliefs, impulsivity, CRT-*Reflective* and CRT-*Intuitive*) are shown in **Table 2**. The relationship between the change scores and the AOT-17 total score and sub-scales (dogmatism, fact resistance, liberalism and belief personification) and impulsivity, are shown in **Table 3**.

Table 2 reveals that environmental concerns, compensatory green beliefs and impulsivity are not associated with the change scores for the apples, cars or buildings tasks. **Table 3** reveals that the overall AOT-17 score is significantly positively correlated with the change scores for the buildings task (r = 0.143, p = 0.019, with 2% of the variance in the change score being accounted for), and the cars task (r = 0.226, $p < 0.001$, with 5.1% of the variance in the change score accounted for), but not for the apples task (r = 0.099, p = 0.106), for which no negative footprint illusion was found. Thus, the disposition to engage in Type 2 actively open-minded thinking is associated with a significant reduction in susceptibility to the negative footprint illusion in the context of both the buildings and cars item types. **Table 3** also indicates that the change score for the buildings task was significantly related to the fact resistance sub-scale of the AOT-17 (r = 0.200, $p < 0.001$), whilst the change scores for the cars tasks was significantly related to both the fact resistance (r = 0.201, $p < 0.001$) and dogmatism sub-scales (r = 0.205, $p < 0.001$). Some protection from susceptibility to the negative footprint illusion therefore seems to be afforded by the ability to engage in actively open-minded thinking, particularly in terms of fact resistance, and potentially with respect to dogmatic thinking.

It is also important to note that borderline significant correlations were obtained between the ability to engage in reflective thought (as measured by the CRT-*Reflective* score) and the buildings task change score (r = 0.111, p = 0.053) and the cars task change score (r = 0.104, p = 0.088). Thus, there is some evidence to indicate that the ability to engage in Type 2 reflective thinking (indexed by the CRT) might offer protection from susceptibility to the negative footprint illusion.

TABLE 2 | Correlation matrix showing the relationship (Pearson correlation coefficients) between individual differences measures and the carbon footprint change scores for the apples, cars and buildings item types.

Variables	1.	2.	3.	4.	5.	6.	7.	8.	9.
1. Change score for apples item type									
2. Change score for cars item type	0.135*								
3. Change score for buildings item type	0.113	0.591**							
4. Environmental concerns	0.053	−0.071	−0.031						
5. Compensatory green beliefs	0.086	−0.097	−0.011	−0.210**					
6. Impulsivity	−0.087	−0.036	−0.094	0.003	0.111				
7. Attentional impulsivity	0.055	0.034	−0.010	0.112	0.036	0.595**			
8. Motor impulsivity	−0.126*	−0.059	−0.114	0.047	0.105	0.823**	0.261**		
9. Non-planning impulsivity	−0.082	−0.034	−0.069	−0.106	0.099	0.852	0.359**	0.512**	

N = 269 for each cell.
**Correlation is significant at the p < 0.05.*
***correlation is significant at the p < 0.001.*

TABLE 3 | Correlation matrix showing the relationship (Pearson correlation coefficients) between the actively open-minded thinking scale and sub-scales (dogmatism, fact resistance, liberalism and belief personification), impulsivity and the carbon footprint change scores for the apples, cars and buildings item types.

Variables	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
1. Change score for apples item type											
2. Change score for cars item type	0.135*										
3. Change score for buildings item type	0.113	0.591**									
4. Actively open-minded thinking (AOT)	0.099	0.226**	0.143*								
5. AOT dogmatism	0.081	0.205**	0.082	0.875**							
6. AOT fact resistance	0.106	0.201**	0.200**	0.816**	0.613**						
7. AOT liberalism	−0.071	0.035	0.012	0.407**	0.253**	0.268**					
8. AOT belief personification	0.083	0.097	0.043	0.448**	0.218**	0.097	−0.062				
9. CRT-*Intuitive*	0.074	−0.037	−0.053	−0.061	−0.116	−0.059	0.067	0.032			
10. CRT-*Reflective*	−0.074	0.104	0.118	0.212**	0.228**	0.160**	−0.005	0.099	−0.723*		
11. Impulsivity	−0.087	−0.036	−0.094	−0.077	−0.033	−0.062	0.049	−0.137*	0.115	−0.101	

N = 269 for each cell.
**Correlation is significant at the $p < 0.05$.*
***Correlation is significant at the $p < 0.001$.*

Discussion

The findings of this study confirmed the presence of a significant negative footprint illusion for participants rating the carbon footprint of larger carbon producing objects in the form of cars (cf. Holmgren et al., 2021) and buildings (cf. Holmgren et al., 2018a), when: (i) the carbon footprint estimation was measured on a consistent scale across these carbon producing item types; (ii) a relative anchor point was provided for each conventional (initial) carbon footprint estimate; and (iii) the ratio of conventional versus conventional plus eco-friendly items was kept constant across item types. A reduction of approximately 10% in participants' ratings of the combined carbon footprint of conventional plus eco-friendly ("green") buildings or cars was observed over the carbon footprint of a set of conventional buildings or cars on their own.

In terms of the apples task, we observed a zero footprint illusion, whereby participants did not appear to attribute any significant increase *or* decrease in their carbon footprint estimation. The occurrence of a zero footprint illusion has previously been seen in the literature (Holmgren et al., 2021) and can be viewed as also reflecting non-normative performance, although the mechanism that underpins the manifestation of this effect remains unclear. The absence of a negative footprint illusion with the apples task is contrary to previous research that has demonstrated the illusion with other items of food (e.g., Gorissen and Weijters, 2016). We speculate about potential reasons for the absence of the illusion with the apples task in the "General Discussion" section.

The confidence ratings for the carbon footprint estimations indicate a degree of metacognitive awareness in responding, as was also evident in Study 1. For the cars task, participants were significantly more confident in their conventional only carbon footprint estimations, in comparison to their conventional plus eco-friendly estimations. The solution strategy ratings where a negative footprint illusion was observed (i.e., for the cars and buildings tasks), were significantly more analytic in nature, than for the apples task, where more intuitive responses were noted. Furthermore, the conventional ratings were deemed by participants to be significantly more intuitive than the conventional plus eco-friendly ratings. We note, however, that the pattern of findings relating to confidence and strategy judgments is different to that seen in Study 1, which suggests that these measures may be highly context-sensitive for reasons that we consider further in the "General Discussion" section.

In terms of the role of environment-specific reasoning dispositions and their relation to individual variation in susceptibility to the negative footprint illusion, the findings from this study remain consistent with those from Study 1. Study 2 did not demonstrate any significant relationship with either environmental concerns (Schultz, 2001), or endorsement of compensatory green beliefs (Kaklamanou et al., 2015). The role of environment-specific reasoning has been disputed in the literature on the negative footprint illusion, with only one study to date demonstrating a relationship between endorsement of compensatory green beliefs and the number of trees required to offset the carbon footprint of a community of buildings (MacCutcheon et al., 2020), with this relationship yet to be replicated.

Although the findings from the present study did not indicate any role for environment-specific reasoning dispositions in the manifestation of the negative footprint illusion, our findings instead provide evidence for a limited role of *general* dispositions to engage in Type 2 reflective reasoning. These dispositions were measured in this study in terms of actively open-minded thinking, as indexed by the AOT-17, which showed a strong correlation with the avoidance of the negative footprint illusion on the buildings and cars tasks. These Type 2 reflective reasoning dispositions were also indexed by CRT-*Reflective* scores, which showed a weak but nevertheless non-significant correlation with avoidance of the negative footprint illusion – again for the buildings and cars tasks. Measures of Type 1 intuitive responding in terms of impulsivity (Barratt, 1959) or as indexed by the CRT-*Intuitive* scores showed no reliable associations with the emergence of a negative footprint illusion on any of the tasks.

We note that the use of the 17-item AOT scale (Svedholm-Häkkinen and Lindeman, 2018) in the present study supported the findings revealed in Study 1 with the shorter 7-item AOT scale (Haran et al., 2013). The propensity to engage in actively open-minded thinking was associated with a reduced susceptibility to the negative footprint illusion when measured on the longer and more reliable 17-item scale. It remains debatable as to whether actively open-minded thinking is a unitary phenomenon, and Svedholm-Häkkinen and Lindeman (2018) have argued for the presence of four distinct yet intercorrelated sub-factors. An exploration of these sub-factors revealed a key role of reduced fact-resistance and reduced dogmatic thinking in underpinning the significant relationship between reduced susceptibility to the negative footprint illusion and actively open-minded thinking. In addition, we note the significant correlation identified between the engagement in reflective thought (CRT-*Reflective*), and the AOT-17 ($r = 0.212, p < 0.001$), which provides a further demonstration of the overlapping relationship between these measures, supporting the view that actively open-minded thinking aligns closely with the disposition to engage in Type 2 reflective thinking (e.g., Stanovich, 2009).

GENERAL DISCUSSION

We have presented two empirical studies in which we demonstrated a negative footprint illusion in environmental decision making. In Study 1, the illusion was observed when participants were required to estimate the number of trees that would need to be planted to offset the carbon footprint of a community of conventional and eco-friendly "green" buildings. In Study 2, we replicated this finding when participants were asked to rate the carbon footprint of conventional and eco-friendly "green" buildings. Furthermore, we revealed in Study 2 that the carbon footprint illusion generalizes to rating the carbon footprint of conventional (petrol) cars and eco-friendly (electric) cars (cf. Holmgren et al., 2018a). These studies provide clear-cut evidence for a negative footprint illusion. However, it is also apparent that under some circumstances, participants can, in fact, engage in normative responding. More specifically, participants did not fall foul of the negative footprint illusion in the context of determining the carbon footprint of conventional and eco-friendly apples in Study 1. In Study 2, participants provided similar ratings for the conventional apples versus the conventional plus eco-friendly apples, thus demonstrating the presence of a zero footprint illusion whereby neither a significant increase *nor* decrease in carbon footprint ratings was demonstrated. Our findings therefore provide support for the existence of a negative footprint illusion in the context of rating the carbon footprint of buildings and cars, but not in the context of rating the carbon footprint of apples.

The normative responding in the context of the apples task in Study 1 and the presence of a zero footprint illusion with the apples task in Study 2, is at odds with previous research that has demonstrated the extension of the illusion to food items in the form of burgers (Kusch and Fiebelkorn, 2019), although not to fruit specifically. Our findings therefore indicate a potential role for the *volume* of CO_2 output in driving the emergence of a negative footprint illusion. We also note that although we did not explicitly require participants to make carbon footprint ratings of objects relative to each other, it is possible that participants displayed some sensitivity to object size as a consequence of our implementation of a counterbalanced repeated-measures design whereby *repeated* carbon footprint estimations were made. In Study 2, we observed significantly higher carbon footprint ratings for the cars and buildings tasks, in comparison to the apples task, despite retaining a consistent scale and a relative anchor point for each task.

We note, however, that not only did the apples task in Study 2 contain the smallest CO_2 producing items in the form of small fruit in comparison to either cars or buildings, but it also contained the lowest total number of objects across the three item types (i.e., 75 houses vs. 45 cars vs. 15 apples). This gives rise to the possibility that the limited "numerosity" of conventional items and eco-friendly additions might play a role in the absence of a negative footprint illusion with the apples task. In other words, whilst we standardized the *relative* number of conventional and eco-friendly items across tasks, the *absolute* number of items in the apples task was much lower than in the other two tasks. This numerosity issue might reduce the salience of the eco-friendly additions and diminish the averaging bias that is assumed to underpin task performance. This speculative hypothesis clearly requires further systematic investigation in future studies.

We additionally explored participants' relative confidence in responding as well as the self-reported solution strategies that they attributed to each carbon footprint estimation across each of the tasks. Interestingly, in Study 1, participants' confidence ratings and strategy ratings in relation to their carbon footprint estimates indicated the presence of a metacognitive component to these estimates, with confidence ratings and analytic reasoning ratings being significantly higher in the task involving apples, where there was no susceptibility to the negative footprint illusion, than in the task involving buildings, where susceptibility to the illusion was present. These metacognitive findings may provide some converging evidence in support of the negative footprint illusion being underpinned by Type 1 reasoning, as would arise from the operation of an intuitively applied averaging bias.

However, we note a word of caution in the interpretation of these latter metacognitive findings. The instructional context surrounding the buildings task in Study 1 was more complex than that surrounding the apples task. It might therefore be that participants' metacognitive judgments were sensitized to the perceived differential complexity of the buildings and apples item types, rather than to differences in the underpinning reasoning processes used to generate carbon footprint estimates. Indeed, this explanation is somewhat supported by the findings of Study 2, in which we simplified and streamlined the contextual information for each of the three tasks. It was then noted in Study 2, that participants' confidence in responding was highest in the tasks where they were susceptible to the illusion (the buildings and cars tasks) in comparison to the apples task, where a zero footprint illusion was identified. However, the confidence ratings were significantly higher for the conventional only ratings, than for the conventional plus eco-friendly ratings, perhaps suggesting some degree of awareness of the more cognitively challenging nature of the second conventional plus eco-friendly rating.

In terms of solution strategy, participants in Study 2 rated their responses as significantly more intuitive in the apples task in comparison to the buildings and cars tasks, whilst conventional ratings were significantly more intuitive than conventional plus eco-friendly ratings. These findings do not provide support for the concept of metacognitive awareness indicating a role of Type 1 intuitive reasoning in susceptibility to the negative footprint illusion. However, it is important to note that in Study 2, most of the solution strategy ratings fall around the middle of the 9-point response scales, with relatively low standard error values. Whilst participants were required to move the slider to prevent mid-scale responding, the findings do indicate a tendency for responding around the middle of the intuition versus analysis continuum scale, implying that participants were not making a commitment to responding in terms of either an intuitive or an analytic solution strategy.

The key focus of the present research was on the nature of individual differences in susceptibility to the negative footprint illusion. With respect to environment-specific thinking dispositions, our findings failed to reveal any effect of environmental concern (Schultz, 2001) or of the endorsement of compensatory green beliefs (Kaklamanou et al., 2015) in explaining susceptibility to the negative footprint illusion. This lack of evidence is at odds with previous findings reported by MacCutcheon et al. (2020), which demonstrated a link between compensatory green beliefs and susceptibility to the negative footprint illusion. Whilst we note methodological differences in the negative footprint illusion tasks employed here, thus preventing an exact replication, we failed to identify any such relationship when using both a 5-point scale (Study 1), and a 9-point scale (Study 2), with the latter being consistent with that employed by MacCutcheon et al. (2020).

The present research also explored a variety of measures that are indicative of a disposition to engage in Type 2 reflective thinking. These measures included actively open-minded thinking and performance on the CRT. We also investigated measures that are arguably indicative of a more intuitive, Type 1 thinking style, that is, impulsivity and susceptibility to the conjunction fallacy. In Study 1, nearly all these questionnaires and tasks failed to predict susceptibility to the negative footprint illusion, with the single exception being that of the actively open-minded thinking scale (AOT-7) when predicting susceptibility to the negative footprint illusion with the buildings item type, but not with the apples item type. Study 2 further supported this latter finding, with evidence that actively open-minded thinking was again associated with reduced susceptibility to the negative footprint illusion in the context of buildings and cars, but similarly to Study 1, not apples. In Study 1, the AOT-7 scale (Haran et al., 2013) predicted reduced susceptibility to the illusion on the buildings item type. Study 2 employed the longer AOT-17 scale (Svedholm-Häkkinen and Lindeman, 2018). The AOT-17 significantly predicted reduced susceptibility to the illusion in the context of both the buildings item type and the cars item type. These findings are the first that we are aware of to suggest that an increase in the disposition to engage in Type 2 actively open-minded thinking is associated with a limited degree of reduced vulnerability to the negative footprint illusion in the context of buildings and cars item types.

Actively open-minded thinking has previously been found to be positively associated with the belief that human activity is responsible for global warming (e.g., Stenhouse et al., 2018) and climate change (e.g., Kahan and Corbin, 2016). Such findings raise the possibility that individual beliefs regarding the anthropogenic causes of climate change might themselves be potentially important in predicting people's reduced susceptibility to the negative footprint illusion. Furthermore, actively open-minded thinking is associated with the acceptance of counterintuitive ideas (Sinatra et al., 2003) and the ability to evaluate arguments objectively (Stanovich and West, 1997). It is also associated with decreased vulnerability to cognitive biases such as belief bias (West et al., 2008) and confirmation bias (Baron, 1993). These prior findings align with the present observations that actively open-minded thinking reduces susceptibility to the negative footprint illusion, presumably because participants who score highly on the AOT-7 and AOT-17 scales are able to avoid succumbing to the averaging bias that has been claimed to form the underpinning basis of the illusion (e.g., Holmgren et al., 2018a).

The AOT-17 scale in Study 2 additionally permitted an exploration of the role of latent sub-factors (Svedholm-Häkkinen and Lindeman, 2018). This exploration revealed that the sub-factors associated with the carbon footprint estimation change scores in the cars task were dogmatism and fact resistance, whilst the only sub-factor associated with the carbon footprint estimation change scores in the buildings task was fact resistance. Dogmatism refers to the belief that there is a single, correct philosophy or way of doing things that people should follow. Fact resistance refers to one's tendency never to abandon beliefs, regardless of the available evidence. It has been suggested that this latter dimension relates to "flexible thinking," which is viewed by Baron (2019) as being central to the concept of actively open-minded thinking. In other words, those individuals who are most able to change their beliefs, values and opinions flexibly on the basis of new evidence exemplify what it means to have an actively open-minded thinking style. Indeed, as noted by Pennycook et al. (2020), the actively open-minded thinking scale was originally created to assess, in part, people's belief that it is good to seek evidence that may conflict with their intuitions.

In the context of tasks that induce a negative footprint illusion, it can readily be seen how useful it would be to have a thinking style whereby an intuitive "averaging" response to the carbon footprint estimation request is reflected upon more fully such that evidence is sought to determine whether a solution based on an averaging process is correct. It would be useful for future studies of the negative footprint illusion to test more directly the idea that those individuals who score highly on measures of actively open-minded thinking do indeed overturn initial intuitive responses in favor of correct responses after a period of reflective thinking. One research technique that can be deployed to explore this issue is the "two-response paradigm," which has been established by Thompson et al. (2011) to disentangle people's Type 1 intuitive reasoning responses from their Type 2 reflective responses (see also Thompson et al., 2013). In this paradigm, participants must first give a very fast, intuitive response and are subsequently allowed time to deliberate so that they can generate a revised response, if they wish to. In this way the consequential impact of Type 2 processing can be observed when initial, incorrect intuitive answers are replaced with correct answers after a period of reflective processing. It would, therefore, be predicted that those high in reflective reasoning dispositions, as indexed by scores on the AOT scale, would show a pattern of reasoning in which they switch from incorrect to correct responding under two-response conditions.

Studies 1 and 2 also provided evidence for a significant positive relationship between actively open-minded thinking and the CRT-*Reflective* score. Previous research has used various versions of the CRT to explore people's propensity to overcome intuitive thinking and to engage in Type 2 reflective thinking. Such concepts are relevant to the investigation of the averaging bias that may underpin the negative footprint illusion given that this bias might derive from the application of more rapid, automatic and intuitive reasoning (i.e., Type 1 processing; Evans and Stanovich, 2013a,b; see also De Neys, 2006). In contrast, the possibility of overcoming the bias might require slower, analytic and reflective reasoning (i.e., Type 2 processing; Evans and Stanovich, 2013a,b), which can enable the reasoner to derive an appreciation of the need to produce a summative response to the task rather than an averaging response. Study 1 did not demonstrate a role for the CRT in predicting reduced susceptibility to the negative footprint illusion. However, Study 2 revealed some limited evidence for the possible association between CRT-*Reflective* scores and avoidance of the negative footprint illusion in terms of a borderline significant relationship in the context of the buildings task (p = 0.053) and a marginal relationship in the context of the cars task (p = 0.088).

The lack of a clear-cut association between performance on the CRT and reduced susceptibility to the negative footprint illusion may at first sight seem curious considering the reliable association observed between actively open-minded thinking and reduced susceptibility to the illusion. Both the CRT and the AOT scales are presumably tapping a disposition to engage in Type 2 reflective reasoning, so why is one measure less predictive than the other when it comes to assessing avoidance of the negative footprint illusion? The resolution to this issue may relate to the proposal that the actively open-minded thinking measure is a *purer* index of Type 2 thinking dispositions than CRT performance (Newton et al., 2021). This is because people can perform successfully on the CRT for reasons other than through the engagement of Type 2 reflective thinking. Indeed, there is recent evidence suggesting that some people can "intuit" the correct answers to the CRT using rapid Type 1 thinking (Raoelison et al., 2020). As such, the CRT may well be a less reliable measure of the disposition toward reflective, analytic thinking than a measure based on the use of an actively open-minded thinking scale.

In sum, our research provides evidence in support of general dispositions to engage in reflective thinking (as captured by a measure of actively open-minded thinking) in offering only limited protection against susceptibility to the negative footprint illusion. Indeed, we note caution in potentially encountering Type 1 errors when conducting multiple repeated correlations such as those presented in both Studies 1 and 2. With respect to environment-specific thinking dispositions, we found no evidence for such dispositions being associated with susceptibility to, or avoidance of, the negative footprint illusion. Indeed, the lack of evidence regarding the role of environment-specific thinking dispositions lends support to the idea that the negative footprint illusion is a general cognitive bias, potentially underpinned by the application of an "averaging" process (Holmgren et al., 2018a). We speculate that whilst avoidance of the negative footprint illusion does appear to have a relationship to dispositions to engage in reflective thinking, the amount of variance explained by measures of actively open-minded thinking remains quite low, and limited to only actively open-minded thinking. Furthermore, we also note the absence of any significant relationship between CRT-*Intuitive* and CRT-*Reflective* scores and susceptibility to the illusion in Study 1, and only borderline significant correlations between CRT-*Reflective* and susceptibility to the negative footprint illusion in the cars and buildings item types in Study 2. This indeed indicates only a limited role in terms of general dispositions to engage in reflective thinking (as captured by a measure of actively open-minded thinking) in predicting susceptibility to the negative footprint illusion. Thus, avoidance of the illusion might have a stronger association with more fundamental aspects of cognitive processing that underpin successful thinking and reasoning. To date, however, we are not aware of any studies that have explored the association between individual differences in core aspects of cognition and the negative footprint illusion, with such investigations representing a fertile avenue for further, systematic research.

Finally, we note that it remains important to determine further individual differences beyond actively open-minded thinking that might underpin susceptibility to the negative footprint illusion in a bid to determine the ways in which psychological barriers, such as cognitive bias, might be alleviated in relation to people's environmentally oriented reasoning and decision making (Gifford, 2011). This, in turn, will hopefully shed light on practical interventions that might be implemented in real-world contexts, which will ultimately help individuals to make environmentally friendly choices (e.g., relating to the purchase of a new vehicle).

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by University of Gävle, Gävle, Sweden and University of Central Lancashire, Preston, United Kingdom. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

ET, JM, MH, HA, and LB contributed to the conception and design of the studies. ET, JM, and MN conducted the data coding and analysis. ET wrote the first draft of the manuscript with advice from JM. LB and JM worked on the second draft. LB wrote sections of the manuscript. All authors contributed to the article and approved the submitted version.

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SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2021.648328/full#supplementary-material

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