

# The Psychology of Managerial Capital Allocation

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### *Doctor of Philosophy (Science)*

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## Statement of Originality

<sup>10</sup> This is to certify that to the best of my knowledge, the content of this thesis is  
<sup>11</sup> my own work. This thesis has not been submitted for any degree or other purposes.

<sup>12</sup> I certify that the intellectual content of this thesis is the product of my own  
<sup>13</sup> work and that all the assistance received in preparing this thesis and sources have  
<sup>14</sup> been acknowledged.

<sup>15</sup> Shir Dekel

לאמא ואבא

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Two years ago, my coding knowledge consisted of what I learnt at the *HTML Basics* class that I took when I was nine years old. Since then, I wrote the text of my thesis in Emacs and developed almost everything else as reproducible code. This would have not been possible without the support of the online R community on GitHub, Stack Exchange, and Twitter.

Chapters 4, 5, and 6 of this thesis were edited by Elite Editing, and editorial intervention was restricted to Standards D and E of the Australian Standards for Editing Practice.

Thank you all.

# Preface

## <sup>48</sup> Naviating the document on a computer

<sup>49</sup> There are links throughout the PDF document that facilitate navigation between cross-references. For most PDF viewers these links are identified by **dark blue** text. Otherwise, if you are using Adobe Acrobat you may instead have to <sup>50</sup> look for the cursor to turn into a hand pointer (). Clicking on these links will <sup>51</sup> take you to the relevant hypothesis (e.g., Hypothesis [2.1](#)), footnote,<sup>1</sup> citation (e.g., <sup>52</sup> Kahneman & Tversky, [1979](#)), figure (e.g., Figure [1.1](#)), table (e.g., Table [2.1](#)), or <sup>53</sup> section (e.g., Chapter [1](#)) that they reference.

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<sup>57</sup> The sections of the thesis can also be navigated using the linked page numbers <sup>58</sup> in the Contents section below. To display the Table of Contents as a sidebar in <sup>59</sup> Preview go to   and then click  at the top right of the <sup>60</sup> sidebar. Alternatively, you can use the key combination  +  +  (the option and command keys with the number 3). To display the Table of Contents as a <sup>61</sup> sidebar in Adobe Acrobat go to    .

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## <sup>69</sup> Appendices

<sup>70</sup> Many experiments were conducted throughout the development of this thesis. <sup>71</sup> Further, each experiment included multiple measures and analyses, and not all of <sup>72</sup> these were directly relevant for the thesis. Therefore, the main body of the text <sup>73</sup> contains the content that was deemed most important, while three appendices con- <sup>74</sup> tained the rest of the content. These appendices contain reports of supplementary <sup>75</sup> experiments, experimental materials, additional measures, data simulations, power <sup>76</sup> analyses, and extra explanatory material. The appendices are organised by the

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<sup>1</sup>Example of a footnote.

77 relevant empirical chapter: Appendix A for Chapter 2, Appendix B for Chapter 4,  
78 and Appendix C for Chapter 6.

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## 81 Reproducibility

82 The thesis used R (Version 4.0.2; R Core Team, 2020)<sup>2</sup> for the analyses and  
83 plotting of data, and for the generation of experimental materials. `rmarkdown`  
84 (Xie et al., 2018) was used with `bookdown` (Xie, 2016) to compile the document  
85 itself. `renv` (Ushey, 2021) was used to create reproducible environments and  
86 `targets` (Landau, 2021b) was used to create a reproducible pipeline. Typesetting  
87 was done with L<sup>A</sup>T<sub>E</sub>X, based on the `oxforddown` template (Lyngs, 2019). All  
88 the components required to reproduce this document can be found through this  
89 link: <https://tinyurl.com/shir-dekel>. This GitFront repository is simply a copy of  
90 a private GitHub repository, and can be navigated and cloned using a similar  
91 interface to GitHub.

92

93

## 94 References

95 A chapter-specific reference list is included at the end of Chapters 1, 2, 4, 6,  
96 and 7. The complete list of references is reported after the appendices.

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2Furthermore, the following R-packages were used `afex` (Version 0.28.1; Singmann et al., 2021), `aggregation1` (Version 1.0; Dekel, 2021a), `aggregation2` (Version 1.0; Dekel, 2021b), `aggregation3` (Version 1.0; Dekel, 2021c), `aggregation4` (Version 1.0; Dekel, 2021d), `alignment1` (Version 1.0; Dekel, 2021e), `alignment2` (Version 1.0; Dekel, 2021f), `alignment3` (Version 1.0; Dekel, 2021g), `alignment4` (Version 1.0; Dekel, 2021h), `alignment5` (Version 1.0; Dekel, 2021i), `alignment6` (Version 1.0; Dekel, 2021j), `alignment7` (Version 1.0; Dekel, 2021k), `alignment8` (Version 1.0; Dekel, 2021l), `anecdotes1` (Version 1.0; Dekel, 2021m), `anecdotes2` (Version 1.0; Dekel, 2021n), `broom` (Version 0.7.7.9000; Bolker & Robinson, 2020; D. Robinson et al., 2021), `broom.mixed` (Version 0.2.6; Bolker & Robinson, 2020), `conflicted` (Version 1.0.4; Wickham, 2019a), `cowplot` (Version 1.1.1; Wilke, 2020), `devtools` (Version 2.4.1; Wickham, Hester, et al., 2021), `dflow` (Version 0.0.0.9000; McBain, 2020), `dplyr` (Version 1.0.7.9000; Wickham, François, et al., 2021), `emmeans` (Version 1.5.4.9004; Lenth, 2021), `forcats` (Version 0.5.1; Wickham, 2021a), `ggbeeswarm` (Version 0.6.0; Clarke & Sherrill-Mix, 2017), `ggplot2` (Version 3.3.4; Wickham, 2016), `janitor` (Version 2.1.0; Firke, 2021), `knitr` (Version 1.33; Xie, 2015), `lme4` (Version 1.1.27; Bates et al., 2015), `magick` (Version 2.6.0; Ooms, 2021), `magrittr` (Version 2.0.1; Bache & Wickham, 2020), `Matrix` (Version 1.3.2; Bates & Maechler, 2021), `MOTE` (Version 1.0.2; Buchanan et al., 2019), `papaja` (Version 0.1.0.9997; Aust & Barth, 2020), `patchwork` (Version 1.1.1; Pedersen, 2020), `printy` (Version 0.0.0.9003; Mahr, 2021), `purrr` (Version 0.3.4; Henry & Wickham, 2020), `pwr` (Version 1.3.0; Champely, 2020b), `rlang` (Version 0.4.11.9000; Henry & Wickham, 2021), `rmdfiltr` (Version 0.1.3; Aust, 2020), `scales` (Version 1.1.1; Wickham & Seidel, 2020), `shiR` (Version 0.0.0.9000; Dekel, 2021o), `shirthesis` (Version 0.0.0.9000; Dekel, 2021p), `snakecase` (Version 0.11.0; Grosser, 2019), `stringr` (Version 1.4.0; Wickham, 2019b), `tarchetypes` (Version 0.2.0.9000; Landau, 2021a), `tibble` (Version 3.1.2; Müller & Wickham, 2021), `tidyr` (Version 1.1.3; Wickham, 2021b), `tinylabels` (Version 0.2.1; Barth, 2021), `usethis` (Version 2.0.1; Wickham & Bryan, 2021), and `yaml` (Version 2.2.1; Stephens et al., 2020).

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99 **Ethics**

100 The research in this thesis was approved by The University of Sydney Human  
101 Research Ethics Committee (HREC).

102 Project No.: 2019/056

103 Project Title: Business decision making

# Abstract

104

105 Capital allocation decisions are critical for large organisations. Management re-  
106 search mainly considers such decisions from an organisational perspective, largely  
107 overlooking potential psychological influences. Therefore, this thesis investigated  
108 cognitive processes that affect capital allocation decisions. Three studies exam-  
109 ined how participants integrated multiple kinds of cues when making their deci-  
110 sions. Each study presented participants with both statistical information and non-  
111 numerical semantic information. In each study, participants had the opportunity to  
112 leverage a statistical concept that arguably should be the sole basis of the decision.  
113 The first study showed participants sequential risky choices without intermittent  
114 feedback. Participants could have combined the risk across decisions to reduce the  
115 overall potential loss. However, they struggled to do this unless it was depicted  
116 visually. The second study asked participants to allocate a budget across a set  
117 of business projects. Participants could have used the variance associated with  
118 the provided forecast estimates to choose which metrics to use for the allocation.  
119 However, they only appropriately used this information when it was expressed  
120 verbally and did not when it was expressed numerically. In the third study,  
121 participants saw projects with conflicting statistical and anecdotal evidence. The  
122 anecdotes were either similar or dissimilar to the target project. Participants  
123 could have clarified the conflicting evidence by using provided information about  
124 the distribution from which the anecdote was sampled. However, they ignored  
125 this information. Despite this, participants' use of the anecdote depended on its  
126 similarity to the target project. These results show that people's capital allocation  
127 decisions are bounded by a limited understanding of certain statistical concepts,  
128 but that they are capable of more nuanced choice when properly scaffolded.

# Contents

129

130	<b>List of Figures</b>	xiv
131	<b>List of Tables</b>	xxii
132	<b>List of Abbreviations</b>	xxiii
133	<b>1 Introduction</b>	1
134	1.1 Capital Allocation in Hierarchical Organisations . . . . .	4
135	1.2 The Psychology of Capital Allocation . . . . .	8
136	1.2.1 Risky Choice . . . . .	11
137	1.2.2 Project Similarity . . . . .	15
138	1.2.3 Reasoning From Past Cases . . . . .	17
139	1.3 Chapter Overview . . . . .	19
140	References . . . . .	21
141	<b>2 Effect of Choice Bracketing on Risk Aggregation in Repeated-Play Gambles With no Feedback</b>	28
142	2.1 Introduction . . . . .	29
143	2.1.1 Multi-Play Gambles . . . . .	31
144	2.1.2 Repeated-Play Gambles . . . . .	32
145	2.1.3 Choice Bracketing . . . . .	35
146	2.1.4 Internal Capital Market Investment Context . . . . .	38
147	2.2 Experiment 1 . . . . .	39
148	2.2.1 Method . . . . .	41
149	2.2.2 Results . . . . .	44
150	2.2.3 Discussion . . . . .	46
151	2.3 Experiment 2 . . . . .	51
152	2.3.1 Method . . . . .	52
153	2.3.2 Results . . . . .	55
154	2.3.3 Discussion . . . . .	56

## *Contents*

156	2.4 General Discussion . . . . .	57
157	2.4.1 Theoretical Implications . . . . .	59
158	2.4.2 Practical Implications . . . . .	63
159	2.4.3 Future Research . . . . .	64
160	References . . . . .	65
161	<b>3 Joint Evaluation of Multiple Projects</b>	<b>70</b>
162	<b>4 Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation</b>	<b>74</b>
163	4.1 Introduction . . . . .	74
164	4.1.1 Experiment Summary . . . . .	77
165	4.2 Experiment 1 . . . . .	78
166	4.2.1 Method . . . . .	81
167	4.2.2 Results . . . . .	84
168	4.2.3 Discussion . . . . .	86
169	4.3 Experiment 2 . . . . .	87
170	4.3.1 Method . . . . .	88
171	4.3.2 Results . . . . .	89
172	4.3.3 Discussion . . . . .	92
173	4.4 Experiment 3 . . . . .	94
174	4.4.1 Method . . . . .	95
175	4.4.2 Results . . . . .	98
176	4.4.3 Discussion . . . . .	100
177	4.5 General Discussion . . . . .	102
178	References . . . . .	105
180	<b>5 Seeking Alignment in Past Cases</b>	<b>107</b>
181	<b>6 Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity</b>	<b>110</b>
182	6.1 Introduction . . . . .	111
183	6.1.1 Anecdotal Bias . . . . .	112
184	6.1.2 Anecdotal Bias in Business . . . . .	113
185	6.1.3 Effect of Similarity . . . . .	115
186	6.1.4 Experiment Summary . . . . .	116
187	6.2 Experiment 1 . . . . .	117

## *Contents*

189	6.2.1	Method . . . . .	119
190	6.2.2	Results . . . . .	125
191	6.2.3	Discussion . . . . .	129
192	6.3	Experiment 2 . . . . .	130
193	6.3.1	Method . . . . .	133
194	6.3.2	Results . . . . .	135
195	6.3.3	Discussion . . . . .	142
196	6.4	General Discussion . . . . .	143
197	6.4.1	Theoretical Implications . . . . .	145
198	6.4.2	Practical Implications . . . . .	147
199	References . . . . .		148
200	<b>7</b>	<b>Discussion</b>	<b>150</b>
201	7.1	Summary of Results . . . . .	151
202	7.2	Theoretical Implications . . . . .	154
203	7.2.1	Statistical Concepts . . . . .	155
204	7.2.2	Decision Aides . . . . .	157
205	7.2.3	How Bounded is Bounded Rationality? . . . . .	158
206	7.2.4	Expertise Effects . . . . .	158
207	7.3	Practical Implications . . . . .	159
208	7.4	Conclusion . . . . .	161
209	References . . . . .		163
210	<b>A</b>	<b>Chapter 2 Appendix</b>	<b>166</b>
211	A.1	Experiment 1 . . . . .	167
212	A.1.1	Method . . . . .	167
213	A.1.2	Results . . . . .	169
214	A.2	Experiment 2 . . . . .	170
215	A.2.1	Method . . . . .	170
216	A.2.2	Results . . . . .	171
217	A.3	Experiment 3 . . . . .	173
218	A.3.1	Method . . . . .	174
219	A.3.2	Results . . . . .	178
220	A.3.3	Discussion . . . . .	183
221	A.4	Experiment 4 . . . . .	185
222	A.4.1	Method . . . . .	185

*Contents*

223	A.4.2 Results . . . . .	188
224	A.4.3 Discussion . . . . .	193
225	<b>B Chapter 4 Appendix</b>	<b>195</b>
226	B.1 Experiment 1 . . . . .	196
227	B.1.1 Method . . . . .	197
228	B.1.2 Results . . . . .	198
229	B.1.3 Discussion . . . . .	204
230	B.2 Experiment 2 . . . . .	206
231	B.2.1 Method . . . . .	206
232	B.2.2 Results . . . . .	207
233	B.3 Experiment 3 . . . . .	220
234	B.3.1 Method . . . . .	220
235	B.3.2 Results . . . . .	223
236	B.4 Experiment 4 . . . . .	224
237	B.4.1 Method . . . . .	226
238	B.4.2 Results . . . . .	228
239	B.4.3 Discussion . . . . .	231
240	B.5 Experiment 5 . . . . .	231
241	B.5.1 Method . . . . .	232
242	B.5.2 Results . . . . .	232
243	B.5.3 Discussion . . . . .	234
244	B.6 Experiment 6 . . . . .	234
245	B.6.1 Method . . . . .	235
246	B.6.2 Results . . . . .	236
247	B.6.3 Discussion . . . . .	238
248	B.7 Experiment 7 . . . . .	238
249	B.7.1 Method . . . . .	238
250	B.7.2 Results . . . . .	239
251	B.7.3 Discussion . . . . .	240
252	B.8 Experiment 8 . . . . .	241
253	B.8.1 Method . . . . .	244
254	B.8.2 Results . . . . .	250
255	B.8.3 Discussion . . . . .	250

*Contents*

256	<b>C Chapter 6 Appendix</b>	<b>253</b>
257	C.1 Experiment 1 . . . . .	253
258	C.1.1 Method . . . . .	255
259	C.1.2 Results . . . . .	259
260	C.2 Experiment 2 . . . . .	262
261	C.2.1 Method . . . . .	266
262	C.2.2 Results . . . . .	269
263	<b>References</b>	<b>274</b>

# List of Figures

265	1.1	An example of the value function in prospect theory. . . . .	14
266	2.1	The outcome probability distribution of the Samuelson (1963) gamble (50% chance of gaining \$200 and a 50% of losing \$100) played 10 times. Green bars represent gains and red bars represent losses. .	33
267			
268	2.2	Example of a project choice display in Experiment 1. . . . .	43
269			
270	2.3	The display seen by those in the aware condition of Experiment 1. .	43
271			
272	2.4	Mean proportions of decisions to invest in each set of 10 projects, by awareness condition. Error bars represent 95% confidence intervals. Raw data are plotted in the background. . . . .	45
273			
274	2.5	Mean proportions of decisions to invest in each set of 10 projects, by presentation condition. Error bars represent 95% confidence intervals. Here, however, the intervals are so narrow that they are sometimes obscured by the mean indicators in the plot. Raw data are plotted in the background. . . . .	46
275			
276	2.6	Mean proportions of decisions to invest in each set of 10 projects, by similarity and presentation conditions. In mixed factorial designs, error bars cannot be used to make inferences by “eye” across all conditions. Therefore, error bars are not included. Raw data are plotted in the background. . . . .	47
277			
278	2.7	Proportion of project acceptance in the separate presentation condition, by trial and awareness conditions. LOESS method was used for smoothing over trials and the shading represents 95% confidence intervals. . . . .	48
279			
280	2.8	An example of a display seen by those in the separate distribution-present condition of Experiment 2. . . . .	54
281			
282	2.9	Mean proportion of project acceptance for the presentation, awareness, and distribution effects. The condition on the left of each effect is the reference condition (separate presentation, naive awareness, distribution absent). As such, it is identical for the three effects. Error bars represent 95% confidence intervals. Raw data are plotted in the background. . . . .	55
283			

## *List of Figures*

296	2.10 Mean project acceptance for separate presentation, distribution ab-	
297	sent condition, by awareness and trial. LOESS method was used	
298	for smoothing over trials and the shading represents 95% confidence	
299	intervals. . . . .	56
300	4.1 An example of a high alignment display in Experiment 1. . . . .	82
301	4.2 An example of a low alignment display in Experiment 1. . . . .	83
302	4.3 The allocation task. . . . .	84
303	4.4 Mean allocation across NPV, by project alignment and reliability	
304	level conditions. In mixed factorial designs, error bars cannot be	
305	used to make inferences by “eye” across all conditions. Therefore,	
306	error bars are not included. Raw data are plotted in the background.	
307	When interpreting this figure, consider the linear trends in NPV. . .	86
308	4.5 An example of a low alignment, low reliability display in Experiment 2. . . . .	90
309	4.6 An example of a high alignment, high reliability display in Experi-	
310	ment 2. . . . .	91
311	4.7 Mean allocation across NPV, by project alignment and reliability	
312	level conditions. Error bars represent 95% confidence intervals, cal-	
313	culated from the within-subjects standard errors using the method	
314	from Cousineau and O’Brien (2014). Raw data are plotted in the	
315	background. . . . .	92
316	4.8 An example of a low alignment, low verbal reliability display in	
317	Experiment 3. . . . .	97
318	4.9 An example of a high alignment, high numerical reliability display	
319	in Experiment 3. . . . .	98
320	4.10 Mean allocation across NPV, by alignment, reliability level, and	
321	reliability type conditions. Error bars represent 95% confidence	
322	intervals, calculated from the within-subjects standard errors using	
323	the method from Cousineau and O’Brien (2014). Raw data are	
324	plotted in the background. . . . .	101
325	6.1 Focal project display for the anecdote only condition in Experi-	
326	ment 1. Here, Project A was the target project and Project B was	
327	the comparison project. . . . .	121
328	6.2 Focal project display for the statistics only, anecdote & statistics,	
329	and anecdote & enhanced statistics conditions in Experiment 1.	
330	Here, Project A was the target project and Project B was the	
331	comparison project. . . . .	122

## *List of Figures*

332	6.3	Anecdote for participants in the high similarity condition in Experiment 1. . . . .	124
333			
334	6.4	Anecdote for participants in the low similarity condition in Experiment 1. . . . .	125
335			
336	6.5	Mean allocation to the target project for the statistics only condition and the two anecdote & statistics conditions. Error bars represent 95% confidence intervals. Raw data are plotted in the background. . . . .	126
337			
338	6.6	Mean allocation to the target project, by anecdote similarity and evidence type conditions (excluding the statistics only condition). Error bars represent 95% confidence intervals. Raw data are plotted in the background. . . . .	127
339			
340	6.7	Mean allocation to the target project, by specific relevance rating and similarity condition. LOESS method was used for smoothing over trials and the shading represents 95% confidence intervals. Raw data are plotted in the background. . . . .	128
341			
342	6.8	An example of the anecdote display in the negative valence, low similarity condition of Experiment 2. . . . .	135
343			
344	6.9	An example of the focal projects in the negative valence, low similarity condition of Experiment 2. Here, Project 1 was the target project and Project 2 was the comparison project. . . . .	136
345			
346	6.10	An example of an anecdote display in the positive valence, high similarity condition of Experiment 2. . . . .	137
347			
348	6.11	An example of the focal projects in the positive valence, high similarity condition of Experiment 2. Here, Project 2 was the target project and Project 1 was the comparison project. . . . .	138
349			
350	6.12	Mean allocation to the target project, by evidence type, similarity, and valence conditions. In mixed factorial designs, error bars cannot be used to make inferences by “eye” across all conditions. Therefore, error bars are not included. Raw data are plotted in the background. . . . .	139
351			
352	6.13	Mean allocation difference between the statistics only condition and the anecdote & statistics condition, by similarity and valence conditions. The horizontal dashed line shows the point in which the two allocations were equivalent. Values above this line show the higher allocation to the target project when participants were shown statistics only compared with when they were shown statistics with an anecdote. Error bars represent 95% confidence intervals, calculated from the within-subjects standard errors using the method from Cousineau and O’Brien (2014). Raw data are plotted in the background. . . . .	141
353			
354			
355			
356			
357			
358			
359			
360			
361			
362			
363			
364			
365			
366			
367			
368			
369			
370			

## *List of Figures*

371	6.14 Mean allocation to the target project, by specific relevance rating, similarity condition, and valence condition. LOESS method was used for smoothing over trials and the shading represents 95% con- fidence intervals. Raw data are plotted in the background. . . . .	142
375	A.1 Experiment 1 instructions. . . . .	167
376	A.2 The outcome distribution of the 10 gambles used in Experiment 1. .	168
377	A.3 Proportion of project acceptance by trial, similarity, awareness, and 378 presentation conditions. LOESS is used for smoothing over trials, and the shading represents 95% confidence intervals. . . . .	169
379		
380	A.4 Experiment 2 project number question. . . . .	171
381	A.5 Experiment 2 binary portfolio question. . . . .	172
382	A.6 Experiment 2 numerical portfolio question. . . . .	173
383	A.7 Number of projects participants reported seeing, by condition. . . .	174
384	A.8 Mean choice of investing in all 10 projects for the presentation, awareness, and distribution effects. Note, the condition on the left 386 of each effect is the reference condition (separate presentation, naive 387 awareness, distribution absent). As such, it is identical for the three 388 effects. . . . .	175
389		
390	A.9 Mean number of projects chosen in the follow-up for the presenta- 391 tion, awareness, and distribution effects. Note, the condition on the 392 left of each effect is the reference condition (separate presentation, naive awareness, distribution absent). As such, it is identical for the 393 three effects. . . . .	176
394		
395	A.10 Mean project acceptance for the 10 gambles. The format of the labels indicates: (gain probability, gain value; loss probability, loss 396 value). . . . .	177
397		
398	A.11 Mean project acceptance for the gambles' expected value, positive probability, and positive outcome. . . . .	177
399		
400	A.12 An example of a project display in Experiment 3. . . . .	178
401		
402	A.13 Experiment 3 project expectation question. . . . .	178
403		
404	A.14 Mean project acceptance for the similarity effect. . . . .	179
405		
406	A.15 Mean proportion of project acceptance for the similarity effect. . . .	180
403	A.16 Mean project acceptance by similarity and trial. . . . .	180
404		
405	A.17 Number of projects participants expected to see, by similarity. . . .	181
406		
403	A.18 Number of projects participants reported seeing, by similarity. . . .	182
404		
405	A.19 Mean choice of investing in all 10 projects for the similarity effect. .	182
406		

## *List of Figures*

407	A.20 Mean number of projects chosen in the follow-up for the similarity effect. . . . .	183
409	A.21 Mean project acceptance for the 10 gambles. The format of the labels indicate: (gain probability, gain value; loss probability, loss value). . . . .	184
412	A.22 Mean project acceptance for the gambles' expected value, positive probability, and positive outcome. . . . .	184
414	A.23 Instructions for those in the naive condition of Experiment 4. . . . .	186
415	A.24 Instructions for those in the aware condition of Experiment 4. . . . .	186
416	A.25 An example of a project display in Experiment 4. . . . .	187
417	A.26 Mean project acceptance for the awareness effect. . . . .	188
418	A.27 Mean proportion of project acceptance for the awareness effect. . . . .	189
419	A.28 Mean project acceptance by awareness and trial. . . . .	189
420	A.29 Number of projects participants expected to see, by awareness. . . . .	190
421	A.30 Number of projects participants reported seeing, by awareness. . . . .	191
422	A.31 Mean choice of investing in all 20 projects for the awareness effect. .	192
423	A.32 Mean number of projects chosen in the follow-up for the awareness effect. . . . .	192
425	A.33 Mean project acceptance for the 20 gambles. The format of the labels indicate: (gain probability, gain value; loss probability, loss value). . . . .	193
428	A.34 Mean project acceptance for the gambles' expected value, positive probability, and positive outcome. . . . .	194
430	B.1 Experiment 1 low reliability instructions. . . . .	198
431	B.2 Experiment 1 high reliability instructions. . . . .	199
432	B.3 The instructions for the no NPV condition in Experiment 1. . . . .	200
433	B.4 The forecasting task. . . . .	200
434	B.5 The ranking task. . . . .	201
435	B.6 The confidence task. . . . .	201
436	B.7 The justification task. . . . .	202
437	B.8 Mean ranking. . . . .	202
438	B.9 Mean confidence. . . . .	203
439	B.10 Mean forecasts. . . . .	205
440	B.11 Mean forecast SD. . . . .	205
441	B.12 Experiment 2 instructions. . . . .	207

## *List of Figures*

442	B.13 Experiment 2 NPV test. . . . .	208
443	B.14 Experiment 2 NPV test answers. . . . .	209
444	B.15 Experiment 2 NPV knowledge rating task. . . . .	209
445	B.16 Variance lecture slide 1. . . . .	209
446	B.17 Variance lecture slide 2. . . . .	210
447	B.18 Variance lecture slide 3. . . . .	210
448	B.19 Variance lecture slide 4. . . . .	211
449	B.20 Variance lecture slide 5. . . . .	211
450	B.21 Variance lecture slide 6. . . . .	212
451	B.22 Variance lecture slide 7. . . . .	212
452	B.23 Variance lecture slide 8. . . . .	213
453	B.24 Variance lecture slide 9. . . . .	213
454	B.25 Variance lecture slide 10. . . . .	214
455	B.26 Variance lecture slide 11. . . . .	214
456	B.27 Variance lecture slide 12. . . . .	215
457	B.28 Variance lecture slide 13. . . . .	215
458	B.29 Variance lecture slide 14. . . . .	216
459	B.30 Mean ranking. . . . .	217
460	B.31 Mean confidence. . . . .	217
461	B.32 Mean allocation by NPV, reliability level, alignment, and phase. . . . .	219
462	B.33 Mean NPV knowledge rating. . . . .	220
463	B.34 Experiment 3 predicted data. . . . .	221
464	B.35 Alignment Experiment 3 power curve. Labels indicate lowest sample size above 80% power. . . . .	222
465	B.36 Experiment 3 verbal reliability instructions. . . . .	224
466	B.37 Experiment 3 numerical reliability instructions. . . . .	225
467	B.38 An example of an interstitial display in Experiment 3. . . . .	225
468	B.39 Experiment 4 low reliability instructions. . . . .	227
469	B.40 Experiment 4 high reliability instructions. . . . .	228
470	B.41 Mean project allocation in Experiment 4. Error bars represent 95% confidence intervals based on the multivariate model. Note that this mixed factorial design does not allow for using confidence intervals to make inferences by “eye” across conditions. . . . .	229
471	B.42 Mean confidence. Error bars represent 95% confidence intervals based on the multivariate model. Note that this mixed factorial design does not allow for using confidence intervals to make inferences by “eye” across conditions. . . . .	230

## *List of Figures*

479	B.43 An example of the forecasting task in Experiment 5. . . . .	233
480	B.44 Mean forecasts. . . . .	234
481	B.45 Mean forecast SD. . . . .	235
482	B.46 Mean forecasts. . . . .	237
483	B.47 Mean forecast SD. . . . .	237
484	B.48 Instructions for the no hint condition. . . . .	240
485	B.49 Instructions for the salience only condition. . . . .	241
486	B.50 Instructions for the salience + hint condition. . . . .	242
487	B.51 The projects display. . . . .	243
488	B.52 Mean allocation. . . . .	243
489	B.53 Mean ranking. . . . .	244
490	B.54 Experiment 8 low alignment, verbal reliability display. Cropped for space (full display had eight projects). . . . .	246
491		
492	B.55 Experiment 8 low alignment, numerical reliability display. Cropped for space (full display had eight projects). . . . .	247
493		
494	B.56 Experiment 8 high alignment, verbal reliability display. Cropped for space (full display had eight projects). . . . .	248
495		
496	B.57 Experiment 8 high alignment, numerical reliability display. Cropped for space (full display had eight projects). . . . .	249
497		
498	B.58 Mean project allocation, for the low alignment condition. Error bars represent 95% confidence intervals. . . . .	251
499		
500	B.59 Mean project allocation, for the high alignment condition. Error bars represent 95% confidence intervals. . . . .	252
501		
502	C.1 Power curves for the similarity and anecdote effects. . . . .	256
503	C.2 Experiment 1 general instructions. The two boxes were split between two separate web-pages. . . . .	256
504		
505	C.3 Experiment 1 specific instructions for those in the anecdotes only condition. . . . .	257
506		
507	C.4 Experiment 1 specific instructions for those in the anecdote & statistics condition. . . . .	257
508		
509	C.5 Experiment 1 specific instructions for those in the anecdote & enhanced statistics condition. . . . .	258
510		
511	C.6 Experiment 1 specific instructions for those in the statistics only condition. . . . .	259
512		
513	C.7 Follow-up questions in Experiment 1. . . . .	260

## *List of Figures*

514	C.8 Mean similarity rating of Project A (the target project) to the 515 anecdote. Error bars represent 95% confidence intervals. . . . .	261
516	C.9 Mean rating of how relevant participants thought the anecdote was 517 to Project A (the target project). Error bars represent 95% confi- 518 dence intervals. . . . .	262
519	C.10 Mean rating of how relevant participants thought the anecdote was 520 to other oil projects. Error bars represent 95% confidence intervals. . . . .	263
521	C.11 Mean allocation to the target project by similarity rating. The 522 shading represents 95% confidence intervals. . . . .	264
523	C.12 Rating of how relevant participants considered the anecdote to the 524 target project, by similarity rating. The shading represents 95% 525 confidence intervals. . . . .	264
526	C.13 Anecdotes Experiment 2 predicted data for the negative valence 527 condition . . . . .	265
528	C.14 Anecdotes Experiment 2 predicted data for the positive valence 529 condition . . . . .	265
530	C.15 Anecdotes Experiment 2 power curve. Labels indicate lowest sample 531 size above 80% power. . . . .	267
532	C.16 General instructions for Experiment 2. . . . .	268
533	C.17 Experiment 2 specific instructions for those in the anecdotes only 534 condition. . . . .	269
535	C.18 Experiment 2 specific instructions for those in the combined condition.	270
536	C.19 Experiment 2 specific instructions for those in the statistics only 537 condition. . . . .	270
538	C.20 An example of one of the follow-up question displays in Experiment 2.	271
539	C.21 An example of an interstitial display in Experiment 2. . . . .	271
540	C.22 Mean similarity rating of Project A (the target project) to the 541 anecdote. Error bars represent 95% confidence intervals. . . . .	272

# List of Tables

542

543	2.1	Experiment 1 group allocation.	41
544	2.2	Experiment 2 group allocation.	53
545	4.1	Experiment 1 group allocation.	81
546	4.2	Experiment 3 group allocation.	95
547	6.1	Experiment 1 group allocation.	119
548	6.2	Experiment 2 group allocation.	133
549	A.1	Experiment 3 group allocation.	175
550	A.2	Logistic regression table of project acceptance by similarity and trial.	179
551	A.3	Experiment 4 group allocation.	186
552	A.4	Logistic regression table of project acceptance by awareness and trial.	190
553	B.1	Experiment 4 group allocation.	226
554	B.2	Experiment 5 group allocation.	232
555	B.3	Experiment 6 group allocation.	236
556	B.4	Experiment 7 group allocation.	239

## List of Abbreviations

- 558 **GE** . . . . . General Electric.  
559 **NPV** . . . . . Net present value.  
560 **EUT** . . . . . Expected utility theory.  
561 **MBA** . . . . . Master of Business Administration.  
562 **EV** . . . . . Expected value.  
563 **ANOVA** . . . . Analysis of variance.

*Executive: A man who can make quick decisions and  
is sometimes right.*

—Elbert Hubbard (1914, p. 52)

# 1

564

565

## Introduction

566

### Contents

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567

1.1 Capital Allocation in Hierarchical Organisations . . . . .	4
1.2 The Psychology of Capital Allocation . . . . .	8
1.2.1 Risky Choice . . . . .	11
1.2.2 Project Similarity . . . . .	15
1.2.3 Reasoning From Past Cases . . . . .	17
1.3 Chapter Overview . . . . .	19
References . . . . .	21

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578

577

578     Much of modern life depends on large organisations. General Electric (GE)  
579     makes the engines that power our aircrafts, Johnson & Johnson makes our shampoo,  
580     and Google allows us to search the internet. The areas of our lives that are less  
581     affected by such private firms depend on public sector organisations such as public  
582     hospitals, schools, and police. The justification for the existence of organisations  
583     of this size is that the particular combination of individual divisions, alongside a  
584     corporate management, will lead to better performance for each of the divisions  
585     than they would have been able to generate individually. In other words, the  
586     assumption is that such organisations create a synergy—the quality of the whole  
587     will be greater than the sum of its parts.

588     Multi-divisional organisations are typically organised in a hierarchical structure,

## *1. Introduction*

589 with a corporate management team and subsidiary divisions. Each division can be  
590 made up of several business units. For instance, some of GE's divisions include GE  
591 Aviation and GE Healthcare. Similarly, in the public sector, a hospital system may  
592 operate through multiple individual hospitals in different regions.

593 Such organisations therefore need to make capital allocation decisions. That is,  
594 given a limited amount of financial resources, how best to invest in the multiple  
595 divisions? Equally? Pick a winner? What metric should be used to compare across  
596 divisions? Capital allocation is a critical process to the operation and development  
597 of multi-divisional organisations.

598 The products and services that arise from organisations are necessarily a result  
599 of the work of many people. In GE, for instance, the factories that generate aircraft  
600 engines need to be staffed by production line workers, accountants are needed  
601 for bookkeeping, and software engineers are needed to design and maintain the  
602 production systems. Despite this, many important strategic decisions ultimately  
603 come from a very small number of people. The decisions that the CEO or other  
604 lower level executives make can have large consequences on the life of the company.

605 It is often assumed that a few people having a lot of decision-making power is  
606 for the best. Managers of large organisations often appear to be bold and effective  
607 decision-makers. It appears that their position of power and wealth was necessarily  
608 arrived at through high competence and rational decision-making, suggesting that  
609 the organisation is in good hands. However, there are three reasons why it may  
610 be concerning that much of an organisation's future—and by extension often many  
611 more components of the economy—depends on the decisions of a few individuals.  
612 First, the role of survivorship bias in obtaining the manager's role is unclear, be-  
613 cause the number of managers that used the same management strategy and failed  
614 is unknown. Second, decades of research has shown that people's decision-making  
615 is often fallible and that job experience does not always alleviate this fallibility.  
616 Third, managers of large organisations often face uncertain environments, which  
617 increases the likelihood of managers facing psychological biases.

## 1. Introduction

618 There are many examples of companies that suffered due to such biases. Over-  
619 confidence and confirmation bias likely played a part in Blockbuster's famous  
620 refusal of an offer to buy Netflix in 2000 (Meissner et al., 2015). Further, Roxburgh  
621 (2003) identified how Equitable Life Assurance Society unnecessarily anchored on  
622 previous interest rate performance and was unprepared when rates changed. In an  
623 example of the sunk-cost fallacy, the London Stock Exchange continued investing  
624 in an automated settlement system even when it no longer remained profitable.  
625 The Bank of England needed to step in and stop the project. Overconfidence in  
626 market entry is also a common issue, illustrated by EMI's introduction into the  
627 medical-diagnostics market with the CT scanner (Camerer & Lovallo, 1999; Horn  
628 et al., 2005). By underestimating the competition and overestimating their own  
629 capabilities they eventually incurred losses and exited the market.

630 One class of biases has not been well studied: capital allocation biases. While  
631 some previous work investigating these biases exists (e.g., Bardole et al., 2011),  
632 many questions still remain unanswered. This is a rather large hole in the literature  
633 because capital allocation decisions are at the centre of executive and lower level  
634 managers' roles. When making capital allocation decisions, there are elements of  
635 the decision-making environment that can be deceiving for managers. This thesis  
636 examines how the framing of a series of business projects affects people's decisions  
637 about those projects. Specifically, the same set of projects, presented in aggregate  
638 form, is much more likely to be accepted. Further, sometimes people are distracted  
639 by extraneous semantic information, such as the relative similarity of the options.

640 The results of the thesis show that although people in general make sensible  
641 decisions, they fail to use critical information to inform their decisions. Specifically,  
642 information about metric variance is ignored even when other metrics are available.  
643 Further, people seem to appropriately use statistical and anecdotal information  
644 based on relevance to the situation at hand, but ignore information about the sam-  
645 pling of the anecdote. Not appropriately using these kinds of statistical concepts  
646 has important financial consequences, discussed below.

## 1. Introduction

647 All the experiments in the thesis use laypeople, except for one experiment. How-  
648 ever, past work generally shows the same biases in managers and laypeople (with  
649 some showing more bias in managers, e.g., Haigh & List, 2005). Further, upcoming  
650 studies will directly test managers to determine any potential expertise effects.

651 Section 1.1 will explain how the capital allocation process functions in hier-  
652 archical organisations and why it is necessary to analyse such a process with  
653 a psychological approach. Section 1.2 reviews the literature on decision-making  
654 biases and how these may apply to capital allocation decisions. Section 1.3 will  
655 then summarise the rest of the thesis chapters.

### 656 1.1 Capital Allocation in Hierarchical Organisa- 657 tions

658 The purpose of a multi-divisional organisation is to generate more value than  
659 any of the individual divisions combined. The whole should be greater than the  
660 sum of its parts. Previous work suggests that this is achieved due to factors such  
661 as reduced transaction costs (Coase, 1937; Liebeskind, 2000; Teece, 1980, 1982;  
662 Williamson, 1981), shared resources (Barney, 1991; Wernerfelt, 1984), increased  
663 competitive advantage (Porter, 1980, 1985), increased monitoring (Gertner et al.,  
664 1994), and increased synergies (Barney, 1988). The underlying logic is the same:  
665 a multi-divisional organisation will be successful if it manages its divisions using  
666 processes and resources that are shared or, better yet, are complementary.

667 In order to successfully manage multiple units, large organisations developed  
668 a hierarchical structure. Bower (1970) identified three levels of the typical man-  
669 agement hierarchy: business, division, and corporate. These are equivalent to  
670 front-line (or bottom), middle, and top level managers (Noda & Bower, 1996).  
671 Early theorists suggested that the strategy for the organisation's growth is driven  
672 completely by the top managers; the rest of the organisation simply enacts their  
673 proposals. However, Mintzberg and Waters (1985) emphasised the role of an  
674 emergent strategy, in which lower level managers affect change in the organisation's

## 1. Introduction

675 strategy. Other work proposed and found evidence for an iterated process in which  
676 a strategic context may be set by top managers, but business projects advanced  
677 by lower level managers also contribute to driving the strategy of the organisation  
678 (Bower, 1970; Burgelman, 1983; Noda & Bower, 1996).

679 The way that capital is allocated in an organisation is very important to its  
680 growth and longevity. This process is a part of the broader process of resource  
681 allocation. A *resource* can refer to many types of assets that an organisation owns,  
682 both tangible and intangible, of which capital is only one (Wernerfelt, 1984). The  
683 capital allocation process itself is an important driver of the strategic outcomes  
684 of an organisation (Bower, 1970; Bower & Gilbert, 2005), and as a result, is an  
685 important influence on its financial performance (e.g., Arrfelt et al., 2015; Bardole  
686 et al., 2010). Sengul et al. (2019, p. 72) describe intra-firm capital allocation  
687 as “(i) a process of determination, comparison, and selection among multiple in-  
688 vestment alternatives, (ii) taking place across organizational levels of the firm,  
689 and (iii) influenced and constrained by the external context in which the firm is  
690 situated.” In capital allocation, business-level managers typically formulate project  
691 proposals, which their division managers then evaluate. The division managers then  
692 choose the projects to send for final approval with the corporate managers. The  
693 supply of available capital is also influenced by external sources such as investors,  
694 competitors, and customers. However, this thesis focuses on the comparison and  
695 selection processes that are relevant during business project evaluation.

696 Managers ultimately have only limited information about the projects that  
697 they evaluate. They typically have access to descriptive information about the  
698 investment and its known properties, but also are provided with financial metrics  
699 that estimate the returns on the investment. There are many such metrics; they  
700 usually attempt to encapsulate a trade-off between predicted future gains, present  
701 losses (in the form of the capital spent to pay for the investment), and opportunity  
702 costs. Examples include net present value (NPV), internal rate of return, return on  
703 investment, cost-benefit, and pay-back period. This thesis focuses on NPV, since it  
704 is one of the most frequently used metrics for project evaluation (Graham & Harvey,

## 1. Introduction

705 2001; Graham et al., 2015; Remer et al., 1993). NPV is the difference between  
706 the money that a project is forecasted to make and the initial investment in its  
707 development (accounting for the time value of money), as shown in Equation (1.1):

$$\text{NPV} = \sum_{t=0}^n \frac{R_t}{(1+i)^t}, \quad (1.1)$$

708 where  $t$  is the time of the cash flow,  $i$  is the discount rate,  $R_t$  is the net cash  
709 flow, and  $n$  is the total number of periods. NPV is a useful metric because simply  
710 knowing that it is positive suggests that the project that it describes should be  
711 profitable. Therefore, metrics such as these have a strong influence on the decision  
712 of the manager evaluating a project.

713 However, there are other influences on project evaluations other than the value  
714 of the financial metrics. For instance, politics within or outside the company  
715 can lead to situations in which a decision is based on social influence or even  
716 manipulation (Garbuio & Lovallo, 2017). Such influence is not necessarily negative;  
717 it may involve qualitative feedback from, for instance, a more senior manager  
718 (Thamhain, 2014). Research has also shown that the media can have a tangible  
719 influence on managerial decision-making (Bednar et al., 2013; B. Liu & McConnell,  
720 2013). Other sources of influence are the organisational structures and incentives  
721 that are in place both externally (Kokkinis, 2019) and internally to the organisation  
722 (Rajan et al., 2000; Ullrich & Tuttle, 2004). Such dynamics have also been the  
723 subject of economic modelling investigations (Cavagnac, 2005; Ortner et al., 2017;  
724 Reichelstein, 1997). Project proposals might also be affected by certain approval  
725 structures. For instance, managers might submit overly-optimistic project propos-  
726 als if they know that the corporate team only accepts projects with a certain  
727 minimum NPV forecast.

728 Another potential organisational influence on capital allocation is the extent  
729 of diversification present in an organisation. A diversified organisation is one that  
730 possesses different divisions that are unrelated in some way. Penrose (1959/2009,  
731 p. 96) defined it as such:

## 1. Introduction

732 a firm diversifies its productive activities whenever, without entirely  
733 abandoning its old lines of product, it embarks upon the production of  
734 new products, including intermediate products, which are sufficiently  
735 different from the other products it produces to imply some significant  
736 difference in the firm's production or distribution programmes.

737 Previous work found that organisations that are made up of more related  
738 divisions are more successful than those that are made up of unrelated divisions  
739 (Harrison et al., 1993; Rumelt, 1974; Shelton, 1988; Wernerfelt & Montgomery,  
740 1988). This is also true within business divisions (P. S. Davis et al., 1992). However,  
741 *more* diversified firms have also been shown to be associated with profitability  
742 (Grant & Jammie, 1988). This is usually explained by the ability for such  
743 firms to avoid risk associated with any one market. Some of the discrepancy in  
744 diversification findings has been explained to be due to the specific measures used  
745 (Lubatkin & Shrieves, 1986). It may also be because most studies used Standard  
746 Industrial Classification (SIC) codes to measure diversification (e.g., Rumelt, 1974),  
747 whereas others operationalised it using other approaches (e.g., resource-based;  
748 Harrison et al., 1993).

749 The advantage that related organisations have had has been explained through  
750 *synergies* (Barney, 1988). That is, an organisation with two divisions that can use  
751 their resources to better one another are better off together than separately. The  
752 1960s saw a general rise in mergers and acquisitions from executives seeking to  
753 diversify their organisations. However, doing so simply for the sake of increasing  
754 divisions, rather than an understanding of the possible synergies, leads to the  
755 organisation actually being worth less than the sum of its parts (known as a  
756 *diversification discount*; Lang & Stulz, 1994). In fact, many organisations that  
757 acquired other businesses to diversify subsequently end up divesting them (Porter,  
758 1987). For instance, in 2018 Australian conglomerate Wesfarmers demerged its  
759 Coles division, a successful retailer. Since then, the share price for both companies  
760 has risen by approximately 62% and 32%, respectively (Boyd, 2021).

## 1. Introduction

While much of the performance of an organisation depends on influences that are external to the individual managers (e.g., organisational, political), psychological factors are often also quite consequential. For instance, on the one hand, organisational factors such as relevant support teams and approval processes may influence capital allocation depending on the extent of an organisation's extent of diversification. On the other hand, psychological factors such as ability of managers to compare between business project proposals may also impact allocation differently depending on the organisation's diversification. It is likely to be more difficult for a manager to evaluate project proposals from two dissimilar divisions than it is to evaluate those from two similar divisions. The organisational influences discussed above often assume that the manager that is making the decisions acts rationally, as per traditional economic theory. However, surveys of executives show that CEOs and CFOs often rely on non-financial factors for capital allocation decisions (Graham et al., 2015). Executives in these surveys identified manager reputation and confidence as two of the most important factors for capital allocation decisions. Further, research in psychology has shown that cognitive biases can influence such capital allocation decisions. Section 1.2 discusses such biases and the relevant implications for the thesis.

## 1.2 The Psychology of Capital Allocation

Managers of large organisations are generally assumed to have a superior decision-making capability compared with non-managers. However, managerial decision-making involves many of the same processes that have been shown to be affected by psychological biases in the general population (Das & Teng, 1999; McCray et al., 2002; Schwenk, 1984). Further, an organisation's success ultimately depends on strategic decisions made by top level managers (Mazzolini, 1981). Therefore, despite early work attempting to analyse such decisions using a structured organisational analysis (e.g., Mintzberg et al., 1976), it is important to understand the potential influence of psychological biases on managerial decisions. Research in the field of behavioural strategy has started to do this (Powell et al., 2011).

## 1. Introduction

790 Psychological research has shown that people tend to make decisions that are in-  
791 consistent with neoclassical economic theory. For instance, expected utility theory  
792 (EUT; Friedman & Savage, 1948; von Neumann et al., 1944) assumed that people  
793 have complete information when making decisions. However, both laypeople and  
794 managers of organisations are limited in the amount of information that they have  
795 and their ability to use it (Cyert et al., 1956; Simon, 1955). Such inconsistencies  
796 with economic prescription are likely to have evolutionary origins, so are sure to  
797 be adaptive in certain environments (Bettis, 2017; Gigerenzer, 2008; Haselton  
798 et al., 2009). However, there are many situations in which such inconsistency  
799 with economic theory can have bad consequences.

800 Research has shown many ways in which the allocation of capital in an organisa-  
801 tion can be influenced by psychological biases. For instance, Benartzi and Thaler  
802 (2001) found that people tend to allocate their retirement fund equally between  
803 the available options, regardless of their composition. This *naive diversification*  
804 bias was also found in capital allocation for hierarchical firms (Bardolet et al.,  
805 2011). Managers allocated capital equally across the available divisions in the firm,  
806 regardless of performance. Analysis of real companies found that this behaviour  
807 is damaging to firm performance because it means that lower performing business  
808 units get subsidised by higher performing units, which are not operating at their  
809 full potential (Arrfelt et al., 2015; Bardolet et al., 2010). Subsequent studies  
810 found that business unit size also matters; capital allocation to both the smallest  
811 and largest units is disproportionate to their actual profitability levels (Bardolet  
812 et al., 2017). This was attributed to a combination of naive diversification and  
813 political power effects.

814 Relatedly, people tend to continue expending capital into investments that  
815 appear to be failing (Staw, 1981). This *escalating commitment* is another way  
816 that psychological biases can influence allocation in an organisation. This pattern  
817 of decision-making is likely a consequence of the sunk cost fallacy, in which people  
818 avoid “cutting their losses” even when they know that they cannot recuperate  
819 an investment (Parayre, 1995).

## 1. Introduction

Managers also do not always seem to seek profit maximisation. Shapira and Shaver (2014) offered managers and Master of Business Administration (MBA) students two investments from a hypothetical firm: one with the same expected returns as the average of the firm's current investments and one with lower returns than the firm's average returns. However, both investments were profitable, so to maximise firm profits both should be chosen. Instead, participants were more likely to only choose the first investment. It seems that the firm's average returns served as an anchor, so participants did not want to reduce the firm's average returns, regardless of profitability.

The way that information is presented can also influence allocations. For instance, Yates et al. (1978) showed that people's evaluations are sensitive to the level of detail in the information provided. They found that people devalued descriptions of university courses more when they had less detail. This may be relevant for managers evaluating project proposals. A proposal might appear more attractive simply due to the level of detail in it, even if the level of detail does not correspond with the quality of each proposal.

Further, people tend to be over-confident in their decisions and forecasts. This has been shown in laypeople (E. J. Langer, 1975; Mannes & Moore, 2013; Puri & Robinson, 2007; Soll & Klayman, 2004), as well as in IT professionals (McKenzie et al., 2008) and managers (Barone-Adesi et al., 2013; Kahneman & Lovallo, 1993; Lovallo & Kahneman, 2003). This is important for higher-level managers that evaluate project proposals because the metrics that rely on forecast estimates may be biased by the over-confidence of the lower-level manager that created the proposal. Further, the higher-level manager evaluating the proposal may in turn be over-confident about its prospects due to factors that are unrelated to the underlying value. Overconfidence is also seen when considering the success of projects in hindsight (Bukszar & Connolly, 1988; Christensen-Szalanski & Willham, 1991). This means that it is less likely that managers will be able to effectively learn from both past mistakes and successes due to the potentially erroneous belief that the outcome was anticipated.

## 1. Introduction

Managers often create sensitivity analyses, estimating the worst case, best case, and most likely scenario for a forecast. However, these are likely to be anchored on past experiences that further the manager's existing beliefs. In fact, prior research has shown that people are poor at constructing subjective probability distributions (e.g., Alpert & Raiffa, 1982; Schaefer & Borcherding, 1973; Tversky & Kahneman, 1974; von Holstein, 1971). Therefore, this suggests that even if the lower-level managers that construct project proposals calibrate their forecasts so that they are not over-confident, they are still likely to provide inaccurate estimates of their degree of confidence.

The above summarises many of the currently known psychological biases related to capital allocation. This thesis focuses on three essential processes within the capital allocation process: (a) risky choice, (b) the comparison between diversified businesses, and (c) the influence of prior experience. Each of these is prone to separate biases, that are also interrelated. The subsequent subsections review the literature for these processes.

### 1.2.1 Risky Choice

Neoclassical theories such as EUT suggest that when faced with multiple risky options people should choose the option with the highest expected value (EV), all else being equal. This means multiplying the value of each option by its probability and comparing the resulting values (first documented in Pascal, 1670/1999). For instance, imagine being presented with the following two choices:

- A) a gamble that involves a 50% chance gaining \$200 and a 50% chance of losing \$100; or
- B) gaining/losing nothing.

In option A, the EV is calculated as  $200 \cdot 0.5 - 100 \cdot 0.5 = 50$ . Since the EV for option A (50) is higher than the EV for option B (0), EUT would suggest that option A should be chosen.

## 1. Introduction

This basic principle was extended by Bernoulli (1738/1954), who suggested that a person's subjective value of money differs depending on their current wealth. This *diminishing marginal utility* suggests that the more money a person already has, the less value acquiring more money will have for him. For example, the experience of a rich man that finds \$10 on the street is very different to the experience of a homeless man that finds \$10 (Bradley, 2013). Even though \$10 was gained in both cases, \$10 has less value to a person that already has, for example, \$1,000, than for a person that initially only has \$10. This principle is usually modelled as an power function (with a fractional exponent).

Prospect theory (Kahneman & Tversky, 1979; Tversky & Kahneman, 1992) challenged EUT by suggesting that people's subjective value of money does not depend on their state of wealth—it depends on a change of wealth from a reference point. This is important because people's subjective value of money is different depending if they are gaining or losing money. Specifically, losses have a stronger psychological impact than equivalent gains. This disparity is one of the most settled and consistent findings in psychology and economics, having been well-replicated (e.g, Ruggeri et al., 2020). The fact that losses loom more than equivalent gains for the vast majority of people is referred to as *loss aversion* (Kahneman & Tversky, 1979). This finding was the primary reason that Daniel Kahneman won the Nobel Prize in Economics in 2002 (Kahneman, 2003). Loss aversion has been found with small amounts of money in experimental settings (Kahneman & Tversky, 1979; Tversky & Kahneman, 1992) and with millions of dollars in corporate settings (Koller et al., 2012; Swalm, 1966). The effect has been found in young children (Harbaugh et al., 2001), the numerous disparate cultures in which it has been tested (Weber & Hsee, 1998), and even in capuchin monkeys (Chen et al., 2006). Furthermore, a neural basis for loss aversion was identified (Tom et al., 2007). Therefore, loss aversion is clearly central to human cognition and behaviour.

The function that represents the value of a prospect describes both loss aversion and diminishing marginal utility, as shown in Equation (1.2):

## 1. Introduction

$$v(x) = \begin{cases} x^\alpha & \text{if } x \geq 0 \\ -\lambda(-x)^\beta & \text{if } x < 0, \end{cases} \quad (1.2)$$

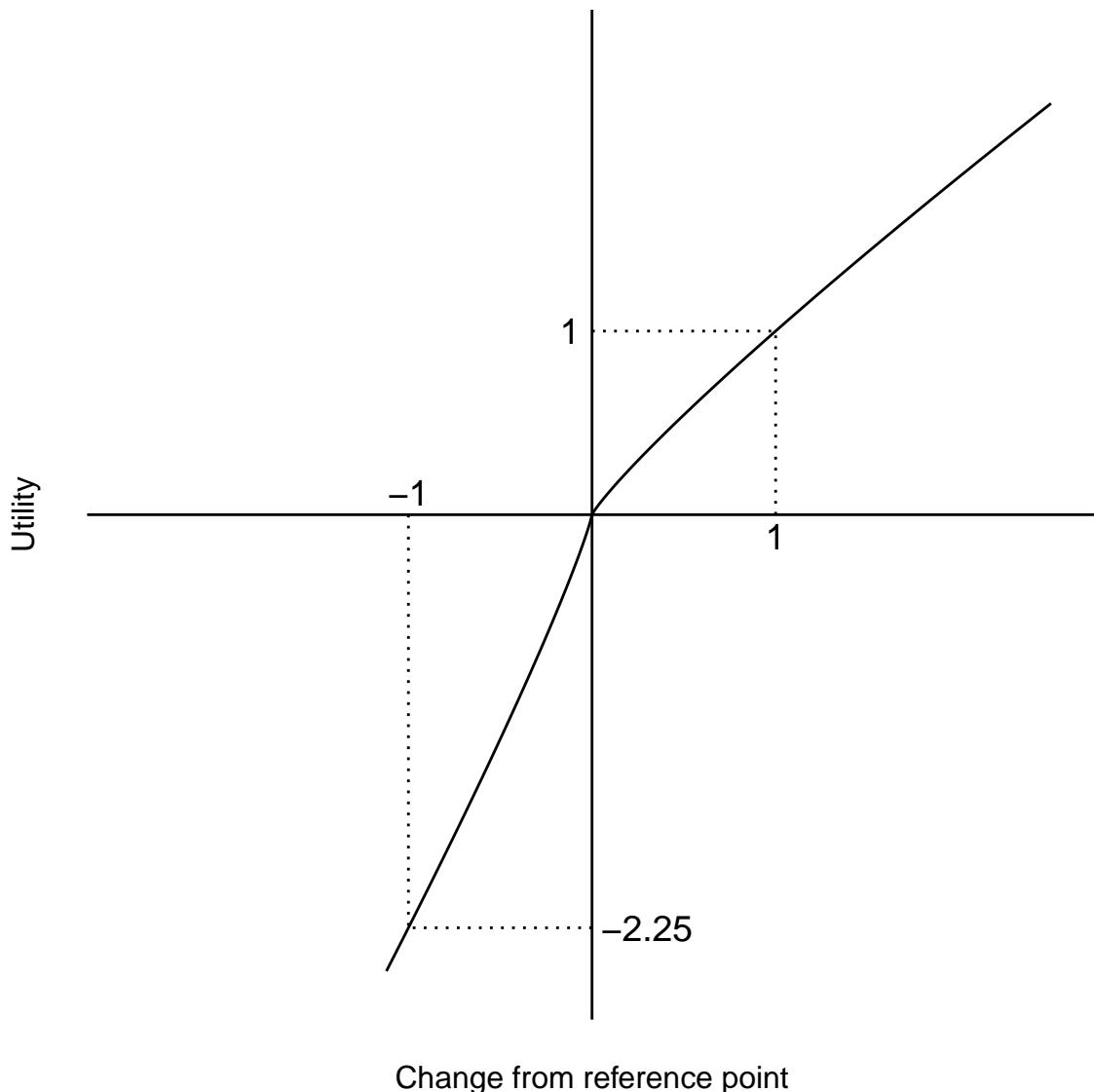
where  $x$  is the possible outcome,  $\lambda$  represents the loss aversion coefficient, and  $\alpha$  and  $\beta$  represent the diminishing marginal utility for gains and losses, respectively.

In other words, loss aversion means that losses have more impact than equivalent gains. In fact, the impact of loss aversion can be expressed even more precisely, as a measurement of the ratio of the slopes of the curve for gains and losses. This measure tells us the average amount that losses have more impact than equivalent gains. In a sequel to the original prospect theory paper, Tversky and Kahneman (1992) measured a median coefficient ( $\lambda$ ) of 2.25 of loss aversion. This means that people respond to losses 2.25 times more than equivalent gains. Similarly, this paper measured a median exponent (representing diminishing marginal utility,  $\alpha$  and  $\beta$ ) of 0.88 for both gains and losses. This means that people discount money the more of it they have by a rate of  $x^{0.88}$ .

Figure 1.1 shows loss aversion as the function being steeper in the domain of losses than the domain of gains. It shows diminishing marginal utility by the slight curve of the function. Equivalent changes in actual wealth from the references point (x-axis) have different impacts on the changes' subjective value (y-axis). An increase in wealth ( $x = 1$ ) brings about an equivalent increase of value ( $y = 1^{0.88} = 1$ ). However, a decrease in the same amount of wealth ( $x = -1$ ) brings about a decrease in value 2.25 times the value of the equivalent gain ( $y = -2.25 \cdot (-(-1))^{0.88} = -2.25$ ).

This research is relevant to capital allocation because the project proposals that managers evaluate invariably involve an element of risk. Therefore, managers are likely to be affected by similar effects on risk that have been shown in laypeople. However, hierarchical organisations offer an even more complex situation. Lovallo et al. (2020) found that the risk profiles of lower-level managers are lower than those of the top managers. They suggest that this may be due to lower-level managers' loss aversion to accepting projects that may jeopardise their job. However, the top

## 1. Introduction



**Figure 1.1:** An example of the value function in prospect theory.

managers recognise that a loss in one or more business units is likely to be offset by gains in other units. Such an inconsistency in risk profiles across the levels of an hierarchical organisation fails to take advantage of the benefits of risk aggregation, which has long been understood in external markets (Markowitz, 1952).

Lovallo et al. (2020) suggested that lower-level managers' failure to aggregate risk to the degree desired by top executives is costing companies approximately a third of the total EV of new project proposals. This is an example of a negative consequence associated with ignoring statistical concepts such as risk aggregation. It is thus critical to identify ways to support risk aggregation across organisational

## 1. Introduction

hierarchies. The psychological literature shows that people's risk aggregation is facilitated through various choice bracketing manipulations. However, there has been no work that investigated such situations without providing participants with feedback in between decisions; this critically limits the external validity of this work because in the real world, organisations evaluate several projects before seeing the outcomes of any one decision. The experiments presented in Chapter 2 investigate the effects of choice bracketing on risk aggregation without feedback.

### 1.2.2 Project Similarity

When evaluating project proposals, managers are likely to be influenced by the relative similarity of the available options to each other. The extent to which this may be true is important especially since the increase firm diversification. Organisations are not only varied by the number of divisions which they possess but also by the extent of diversification. This means that managers are likely to find themselves comparing across dissimilar types of projects.

As mentioned above, there are likely many organisational and financial reasons why the extent of diversification in an organisation would impact its performance. However, the impact of psychological factors has not been investigated. Specifically, project similarity, which is an organisational factor, is likely to affect the project comparison process, which is a psychological factor. This may then have downstream consequences on firm performance through, for instance, the kinds of financial metrics that are used and how they are evaluated. Having more similar projects to compare may mean more attributes on which to evaluate, whereas a dissimilar comparison may lead to a situation in which a manager has to rely on potentially unreliable metrics.

Structure-mapping theory (Gentner, 1983; Gentner & Markman, 1997) provides a model of comparison that psychologically distinguishes similar and dissimilar allocation tasks. This framework models comparison as a process of bringing conceptual structures into alignment which, when possible, puts shared dimensions into correspondence. Alignment both highlights when two conceptual structures

## 1. Introduction

share dimensions, but also highlights how the two structures differ along those shared dimensions, called *alignable differences*. For example, when comparing two oil discovery projects, all the relevant processes of planning an exploration and measuring the amount of hydrocarbons in a prospect might be identical, but the specific amount measured will be different. This is the alignable difference: a difference between the two projects that is constrained within the same conceptual structure. However, when comparing between an oil field and a refinery, there will be significantly more *non-alignable differences*, because the two domains do not share component dimensions. That is, many of the processes that exist in the exploration business unit have a significantly different dimensional structure to those in the refinery business unit, such that it will be difficult to find meaningful alignments. More non-alignable differences mean that there are less opportunities to make meaningful comparisons, and so would make predicting relative project success and ranking their priority more difficult. Chapter 4 experimentally examines business project comparisons and how project alignment affects capital allocation decisions.

When evaluating projects, managers make use of financial metrics, such as NPV. However, such metrics are reliant on forecast estimates of, for instance, future cash flows. Do managers take into account such inherent variance in their decisions? This is especially important to investigate given the above discussion. In cases of non-alignable comparison managers may rely on a potentially unreliable metric. On the other hand, in an alignable comparison, managers might have the option to base their decisions on the relative reliability of different metrics. It is important to remember that all such decisions are often very consequential for the manager. That is, the project could ultimately make the company money and lead to future opportunities for the manager, or potentially cause financial harm to the company (and subsequently lead to a job loss). This is another example of the way in which ignoring certain statistical concepts—here metric variance—can have negative consequences for an organisation.

## 1. Introduction

1000 Psychological research shows that laypeople are in general quite poor at using  
1001 numerical variance information (Batteux et al., 2020; Galesic & Garcia-Retamero,  
1002 2010; Konold et al., 1993; Vivalt & Coville, 2021). However, it is unclear to what ex-  
1003 tent managers would be sensitive to variance information in the metrics associated  
1004 with the projects that they evaluate. On the one hand, perhaps managers' financial  
1005 training will allow a consideration of such variance estimates, but this might not  
1006 manifest in a situation in which managers have already been shown to be prone  
1007 to biases. Chapter 4 investigates whether participants are as sensitive to verbally-  
1008 instructed reliability information as they are to numerical reliability information.

### 1009 1.2.3 Reasoning From Past Cases

1010 Managers often use past events to reason and make predictions about the future  
1011 (Einhorn & Hogarth, 1987). Such past events may be those that happened to the  
1012 individual manager, a case from the organisation's history, or from an external  
1013 source. This will especially be the case in a project evaluation scenario when  
1014 a given project is hard to compare with the other projects at hand. However,  
1015 managers evaluating project proposals may make inappropriate comparisons when  
1016 considering the target project to other cases. For instance, people tend to limit the  
1017 size of the comparison set to a small number. This is often only a handful of cases,  
1018 or even one. Doing this might mean only considering potentially irrelevant surface  
1019 similarity to the current situation and not aligning the underlying causal structure.  
1020 Further, this might mean not considering other similar projects.

1021 Tversky and Kahneman (1974) discussed a number of biases that may influence  
1022 such processes. The availability bias is seen when people mistake the ease of  
1023 retrieval of information for its frequency. Further, research on analogical retrieval  
1024 showed that people are more likely to retrieve surface similar cases than those  
1025 with a relational connection (Gentner et al., 1993). As such, managers are likely  
1026 to recall cases that may not be sufficiently relevant to their target situation and  
1027 be overly-confident about the frequency of such cases occurring. Such a focus  
1028 on a particular case might then also lead to an anchoring effect, wherein other

## *1. Introduction*

1029 decisions might be disproportionately seen as relevant. Tversky and Kahneman  
1030 (1974) also found that people are not sensitive to properties of sample size such as  
1031 the greater amount of non-representative outcomes in small samples. This means  
1032 that managers are even less likely to appreciate the importance of considering a  
1033 large sample of cases when drawing conclusions to a target problem. Tversky and  
1034 Kahneman (1974) also note an insensitivity to predictability, in which people do  
1035 not take into account the reliability of the information that they have to make a  
1036 prediction. This might mean that managers may struggle to ideally weigh evidence  
1037 of varying degrees of reliability.

1038 External sources that may be used to compare to a target situation include  
1039 business case studies. Considering such examples of prior business decisions or  
1040 events are the way that most MBA students learn about the business world. Pub-  
1041 lications such as Forbes or Harvard Business Review publicise various businesses'  
1042 successes and failures and so may create an allure to use such case studies in the  
1043 decision-making process. On the other hand, managers may have access to more  
1044 aggregated data about their industry from, for instance, consultancy companies.  
1045 How do managers use these various types of evidence in their decision-making?

1046 Research on this topic suggests that managers tend to prefer anecdotes over  
1047 statistics, unless aided (Wainberg, 2018). This is a concern because Gavetti et al.  
1048 (2005) suggests that managers often make use of case studies quite poorly. The anal-  
1049 ogy literature draws a distinction between surface similarity, in which a mapping is  
1050 made between easily identifiable but potentially functionally irrelevant attributes,  
1051 and relational similarity, in which the underlying mechanism is considered. Are  
1052 managers sensitive to the deeper causal mechanisms that underlie the anecdotes  
1053 they judge? Or are they simply influenced by surface similarity? Chapter 6  
1054 investigates the extent to which people use anecdotes or aggregated data based  
1055 on the relevance of the anecdote to the target project during capital allocation. It  
1056 also considers whether people are sensitive to information about the distribution  
1057 from which the anecdote was sampled. Ignoring this statistical concept can have

## 1. Introduction

1058 negative consequences for an organisation by potentially over- or under-estimating  
1059 the relevance of a past case and therefore making an ill-informed investment.

## 1060 1.3 Chapter Overview

1061 In sum, the potential consequences of a diversified hierarchical structure are  
1062 that business projects will be considered one at a time, and if they are considered  
1063 together, disparate project types will make comparisons hard. Considering projects  
1064 one by one might mean that risk is not aggregated across projects and therefore  
1065 value is lost. The difficulty to compare will lead to both potentially relying on  
1066 unreliable metrics, and relying on improper anecdotal evidence. The thesis is  
1067 that people often go half-way. They do not completely disregard the normative  
1068 strategy, but also struggle to use statistical concepts such as aggregation, vari-  
1069 ance, and sampling.

1070 The previous section identified three capital allocation processes that are cur-  
1071 rently under-studied and so are important to investigate further. First, the eval-  
1072 uation of individual project proposals may lead to managers only considering  
1073 such projects one at a time, despite the opportunity of aggregating a portfolio  
1074 of such projects. The choice bracketing literature suggests that there are ways  
1075 of facilitating such aggregation, but does not investigate this without providing  
1076 participants inter-trial feedback. Second, in situations in which managers compare  
1077 multiple projects, the structural alignment literature suggests that managers in  
1078 diversified firms will struggle to allocate capital, more than those in more integrated  
1079 firms. Further, these managers may not be sensitive to the variance inherent in  
1080 the financial metrics they rely on. Third, a difficulty to compare across existing  
1081 projects may instead mean a reliance on prior case studies from personal or external  
1082 experience. Research on anecdotal bias suggests that managers may rely more on  
1083 such case studies than on aggregated data, but it is unclear whether their decisions  
1084 will depend on anecdote relevance. Further, it is unclear if they will appropriately  
1085 use information about the anecdote's sample distribution.

## *1. Introduction*

1086       The rest of this thesis investigates the psychology of capital allocation decisions  
1087       in three chapters that describe empirical work, two theoretical chapters, and a  
1088       general discussion chapter. Chapter 2 describes two experiments that investigate  
1089       the effects of choice bracketing on risk aggregation without feedback. Chapter 3 is  
1090       a short theoretical chapter that discusses the difference between evaluating project  
1091       proposals with inherent budget estimates and the process of allocating an existing  
1092       budget top-down. Chapter 4 describes three experiments that investigate the effects  
1093       of alignment and reliability type—verbal or numerical—on allocations. Chapter 5  
1094       is another short theoretical chapter that discusses the trade-offs that people make  
1095       when using information to evaluate project proposal options. Chapter 6 describes  
1096       two experiments that investigate the effects of anecdote similarity on the anecdotal  
1097       bias. Finally, Chapter 7 discusses the theoretical and practical implications of the  
1098       empirical chapters and concludes the thesis.

## 1. Introduction

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*Cultivate the habit of surveying and testing a prospective action before undertaking it. Before you proceed, step back and look at the big picture, lest you act rashly on raw impulse.*

—Epictetus (ca. 125/1995)

# 2

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## Effect of Choice Bracketing on Risk Aggregation in Repeated-Play Gambles With no Feedback

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### Contents

1402

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1404

2.1	Introduction	29
2.1.1	Multi-Play Gambles	31
2.1.2	Repeated-Play Gambles	32
2.1.3	Choice Bracketing	35
2.1.4	Internal Capital Market Investment Context	38
2.2	Experiment 1	39
2.2.1	Method	41
2.2.2	Results	44
2.2.3	Discussion	46
2.3	Experiment 2	51
2.3.1	Method	52
2.3.2	Results	55
2.3.3	Discussion	56
2.4	General Discussion	57
2.4.1	Theoretical Implications	59
2.4.2	Practical Implications	63
2.4.3	Future Research	64
	References	65

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## 2. Effect of Choice Bracketing on Risk Aggregation

### 2.1 Introduction

1426 Investors know not to put all their eggs in one basket. Ever since work on  
1427 modern portfolio theory (Markowitz, 1952), it has been clear that combining the  
1428 risk of a set of individual investments reduces the overall risk of the portfolio of  
1429 investments. But what about situations in which it is not clear that a set of  
1430 investments fit together as a portfolio? Personal decisions such as buying a car  
1431 or moving cities are typically evaluated independently, as are business decisions  
1432 such as a farm investing in new cropping technology or a multi-business firm  
1433 building a mine.

1434 While these decisions are separated in time, they are often not so far apart that  
1435 it is easy to learn from past outcomes (and sometimes the outcomes themselves  
1436 are unclear). This is because the outcomes of large investments are often delayed.  
1437 Therefore, the decision-maker cannot always use the knowledge of the returns of one  
1438 investment when evaluating a subsequent investment. Any results that a farmer  
1439 may identify from using a new technology will only become apparent after many  
1440 seasons of use. Similarly, it will take many years for a multi-business firm to  
1441 begin to estimate whether the output of a mine resulted in the expected return  
1442 on investment. These are the decisions that this chapter investigates: sequences  
1443 of large risky choices without immediate outcomes.

1444 Risk aggregation is the combination of probability or variance information (or  
1445 both) associated with certain outcomes for the purpose of understanding that infor-  
1446 mation more comprehensively (Bjørnson & Aven, 2019). However, the psychological  
1447 literature suggests that this process may be difficult for people to use. Work on  
1448 prospect theory (Kahneman & Tversky, 1979) suggests that people's evaluation  
1449 of gambles does not conform to expected utility theory and is prone to framing  
1450 effects. Specifically, people typically evaluate gambles one by one (Kahneman &  
1451 Lovallo, 1993; Rabin & Weizsäcker, 2009; Tversky & Kahneman, 1981). Therefore,  
1452 it is unlikely that people will be able to aggregate risk when they do not perceive a  
1453 series of investments as a portfolio. So, what would encourage people to aggregate

## 2. Effect of Choice Bracketing on Risk Aggregation

1454 risk? The literature on *choice bracketing* (Read et al., 1999) shows that grouping  
1455 a set of individual gambles together facilitates risk aggregation. Therefore, the  
1456 current work provides two primary contributions. First, this work is the first  
1457 to investigate the effect of choice bracketing on risk aggregation in independent  
1458 gambles evaluated without immediate returns. Second, this work introduces novel  
1459 choice bracketing manipulations.

1460 The earlier work on risk aggregation essentially did the aggregating work for the  
1461 participants. For example, experimenters provided participants with an outcome  
1462 probability distribution, usually with an explicit indication to group the choices  
1463 together, such as by asking for a single decision to be made on a set of identical  
1464 gambles. Other work addressed the more realistic situation of a set of independent  
1465 gambles. However, most of this work provided participants with the outcomes  
1466 of their choices before the subsequent choice. In these paradigms participants  
1467 experienced individual outcomes from the eventual outcome distribution of the  
1468 gambles, meaning that aggregation was confounded with learning.

1469 As mentioned above, in real life there is usually a significant delay between  
1470 the choice a person or firm makes and the outcome of that choice, and there  
1471 are likely to be several interim choices in the meantime. This is especially true  
1472 for business executives, who would typically have to wait months or years before  
1473 beginning to understand the consequences of their decision, and even then the  
1474 outcome may be unclear. However, previous work did not investigate the effect  
1475 of choice bracketing on risky choice without feedback. This is surprising, since  
1476 choice bracketing is exactly the kind of process that should promote aggregation  
1477 in these more realistic decisions. Therefore, this chapter investigated new ways  
1478 of encouraging participants to bracket their risky choices, but with a paradigm  
1479 that involves a series of independent choices without feedback. In this way, the  
1480 paradigm is more isometric with real-life risky choice.

## 2. Effect of Choice Bracketing on Risk Aggregation

### 2.1.1 Multi-Play Gambles

Despite the difficulties of risk aggregation, people seem to aggregate “naively” when considering multiple gambles. Samuelson (1963) told of a colleague who rejected a gamble that involved a 50% chance of gaining \$200 and a 50% of losing \$100, despite the gamble’s positive EV. That is,  $200 \cdot 0.5 - 100 \cdot 0.5 = 50$ . Rejection of a positive EV gamble out of fear of the possible loss is classic loss aversion. However, the same colleague said he would accept 100 plays of the same gamble. Samuelson argued that this choice is irrational.<sup>1</sup> Intuitively, it is clear that over the course of 100 gambles, the positive EV wins out, and a net loss of money is extremely unlikely. Samuelson’s colleague was more risk averse when making a single decision about one gamble (a *single-play* gamble), than when making a single decision about multiple (in this case 100) identical gambles (a *multi-play* gamble).<sup>2</sup>

Wedell and Bockenholt (1994) replicated the Samuelson (1963) anecdote experimentally with a gamble involving a potential gain of \$100 and a potential loss of \$50. Participants accepted the multi-play gamble of 100 plays more than the single-play gamble. This effect has since been replicated with different outcomes and probabilities, both with hypothetical and real money. Some participants often require fewer than 10 plays of a previously rejected gamble in order to accept it (DeKay & Kim, 2005; Keren, 1991; Montgomery & Adelbratt, 1982; Redelmeier & Tversky, 1992). Other similar studies found a multi-play effect that was in the predicted direction but not significant (Barron & Erev, 2003; Benartzi & Thaler, 1999; Klos et al., 2005; T. Langer & Weber, 2001). Further, the effect is not seen when participants do not perceive gamble outcomes as fungible (DeKay, 2011; DeKay et al., 2006; DeKay & Kim, 2005) or when choice is continuous rather than discrete (Bristow, 2011).

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<sup>1</sup>Other work suggests that it is consistent with expected utility theory, once certain assumptions are added (e.g., Aloysius, 2007; Ross, 1999). However, a normative discussion is out of the scope of the present work.

<sup>2</sup>This chapter uses the terminology for gamble types used in Bristow (2011), and Camilleri and Newell (2013).

## 2. Effect of Choice Bracketing on Risk Aggregation

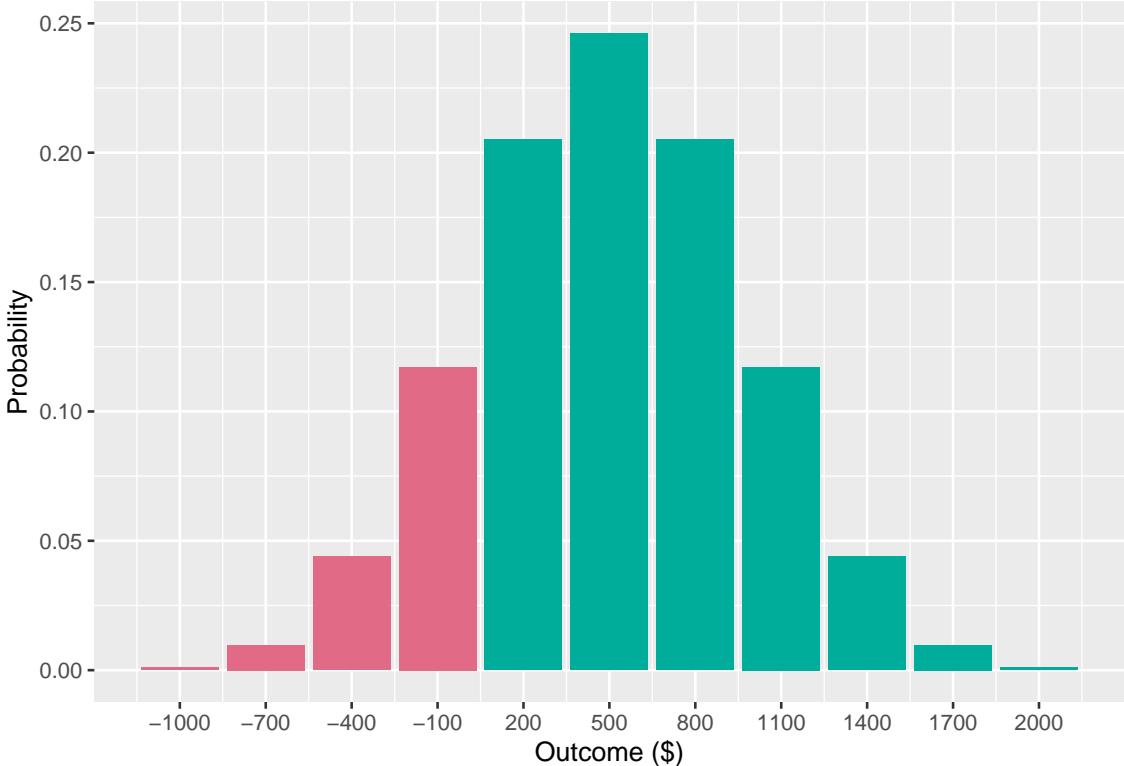
1506        However, multi-play effects are likely robust, since there is also evidence that  
1507        such gambles reduce a variety of cognitive biases. These include common-ratio  
1508        effects (DeKay et al., 2006; Keren, 1991; Keren & Wagenaar, 1987), preference  
1509        reversals (Wedell & Böckenholt, 1990), ambiguity aversion (H.-H. Liu & Colman,  
1510        2009), and the illusion of control (Koehler et al., 1994). Participants are also more  
1511        likely to use explicitly provided EVs in multi-play gambles (Li, 2003), show eye  
1512        movements more congruent with an EV model than single-play gambles (Su et al.,  
1513        2013), and judge multi-play gambles as riskier (Joag et al., 1990).

1514        People prefer multi-play gambles that are displayed with an aggregated outcome  
1515        distribution of those gambles than those without (Benartzi & Thaler, 1999; Coombs  
1516        & Bowen, 1971; DeKay & Kim, 2005; Keren, 1991; Klos, 2013; T. Langer & Weber,  
1517        2001; Redelmeier & Tversky, 1992; Venkatraman et al., 2006; Webb & Shu, 2017).  
1518        This is because these distributions present the probabilities of all the different  
1519        possible outcomes, so very clearly show the rarity of a loss. Note that this does not  
1520        seem to hold when returns are calculated as percentages, rather than fixed dollar  
1521        amounts (Stutzer, 2013); and when participants do not perceive gamble outcomes  
1522        as fungible (DeKay & Kim, 2005). However, when this effect is demonstrated, the  
1523        multi-play gamble is usually set up such that its (binomial) outcome distribution  
1524        shows a relatively low chance of losing any money and a very low chance of losing  
1525        a lot of money. For instance, Figure 2.1 shows the outcome distribution of the  
1526        Samuelson (1963) gamble played 10 times. Outcome distributions of this sort do  
1527        the aggregating work for the participants, making the attractiveness of the multi-  
1528        play gamble clearer. This work suggests that participants can comprehend and  
1529        respond to aggregated risk, but that they struggle to compute the aggregation  
1530        without external help.

### 1531        2.1.2 Repeated-Play Gambles

1532        Decisions in real life are usually sequential and rarely identical as in the multi-  
1533        play paradigm (cf. Barron & Erev, 2003). That is, people tend to be confronted  
1534        with individual choices whose outcomes and outcome probabilities are different

## 2. Effect of Choice Bracketing on Risk Aggregation



**Figure 2.1:** The outcome probability distribution of the Samuelson (1963) gamble (50% chance of gaining \$200 and a 50% of losing \$100) played 10 times. Green bars represent gains and red bars represent losses.

from one choice to another and these choices occur at different points in time. In a business setting this can be seen in decisions about whether to invest in new projects; proposals and opportunities differ widely and occur at different times. Managers are never simply asked: “here are 10 identical investments to consider; do you want all or none of them?”

In *repeated-play* (rather than multi-play) gamble paradigms, participants make decisions about a series of individual gambles. Research using this paradigm found that people are less risk averse both when outcomes for a series of gambles are evaluated less frequently and the subsequent decisions are made less frequently (Bellemare et al., 2005; Beshears et al., 2016; Gneezy & Potters, 1997; Thaler et al., 1997). People are also less risk averse (for positive EV gambles) when they receive feedback after each decision or are able to sample from the distribution of possible outcomes before making a choice (Barron & Erev, 2003; Camilleri & Newell, 2011, 2013; Hertwig et al., 2004; Jessup et al., 2008; Ludvig & Spetch,

## 2. Effect of Choice Bracketing on Risk Aggregation

1549 2011; Wulff et al., 2018). Other work found that loss aversion is mitigated when  
1550 people are explicitly instructed to consider the options as a part of a portfolio  
1551 (Sokol-Hessner et al., 2012; Sokol-Hessner et al., 2009).

1552 These studies are closer to real-life decisions than the multi-play gamble paradigm  
1553 because they involve a set of separate gamble decisions rather than a single decision  
1554 about a set of gambles. However, for the most part, the experiments used in  
1555 the repeated-play gamble literature use various forms of feedback throughout the  
1556 course of the experiment. That is, participants are shown the outcomes of their  
1557 gambles before they make more decisions. This paradigm is known as *experience-*  
1558 *based choice*. In *description-based choice*, on the other hand, the gamble is simply  
1559 presented to the participant without any feedback, as in the multi-play gambles  
1560 above. In real life, people rarely see the immediate outcomes of their risky choices,  
1561 and even less so in business settings, where any return on investment often takes  
1562 years to manifest.

1563 Only a limited number of studies have used a repeated-play paradigm without  
1564 feedback. For instance, Jessup et al. (2008) and Hertwig et al. (2004) investigated  
1565 the effects of feedback in repeated-play gambles on the weighting of small probabili-  
1566 ties, and had a no-feedback control condition. Other work similarly used individual  
1567 description-based gambles presented sequentially (e.g., Ert & Erev, 2013; Joag  
1568 et al., 1990). However, these studies did not attempt to facilitate participants' risk  
1569 aggregation. Haisley et al. (2008) provided limited evidence for facilitating risk  
1570 aggregation. They gave participants the opportunity to buy five (negative EV)  
1571 lottery tickets, and either presented them one at a time, or together. Participants  
1572 bought fewer tickets, when they considered them jointly, thereby maximising EV.  
1573 However, the experimenters did not specify the outcomes and probabilities of each  
1574 gamble, meaning that it is unclear if participants understood the independent  
1575 lotteries as identical or non-identical. This reduces the external validity of the  
1576 study, as most independent risky choice involves non-identical outcomes and prob-  
1577 abilities. In sum, these studies were not designed to research how to facilitate  
1578 risk aggregation and reduce loss aversion. The experiments in this chapter are

## 2. Effect of Choice Bracketing on Risk Aggregation

novel because their goal is to facilitate risk aggregation without the experimental artefact of immediate feedback.

### 2.1.3 Choice Bracketing

Research in psychology and economics has identified ways of facilitating risk aggregation by encouraging people to group their choices. Specifically, people aggregate more when they consider the consequences of their choices together (broad bracketing) than when they consider them individually (narrow bracketing; Read et al., 1999). In multi-play gambles (especially when displayed with an outcome distribution), choices are inherently bracketed broadly because a single choice is made about multiple gambles. Similarly, studies that used repeated-play gambles facilitated risk-tolerance through what can in hindsight be considered broad bracketing. For instance, when Thaler et al. (1997) presented gamble outcomes less frequently, they allowed participants to consider longer time increments with a single evaluation.

Both the original Samuelson (1963) anecdote and its subsequent replications show that people do have an intuition for aggregation even without the risk being calculated exactly for them. This chapter tests whether that same intuition can be elicited and applied across sets of unique bets. What are the minimal conditions required to encourage aggregation? The multi-play gamble work suggests that participants can engage in a more intuitive form of aggregation when provided with the right contextual cues. Investigating the effects of more subtle cues will help shed light on the cognitive processes underlying choice bracketing. Of course, the effects of more subtle cues would not eliminate the utility of explicit financial education, but they will help the design of decision-making contexts to best align with such instruction.

One way of potentially facilitating risk aggregation is to highlight to participants the number of total options that are available to them. Sokol-Hessner et al. (2009) and Sokol-Hessner et al. (2012) reduced risk aversion using lengthy instructions that encouraged participants to “think like a trader.” This meant considering all the

## 2. Effect of Choice Bracketing on Risk Aggregation

1608 repeated-play gambles as a portfolio, as opposed to considering them individually.  
1609 However, this was quite a strong manipulation that is perhaps unrealistic in real  
1610 world. A more subtle cue could involve simply making participants aware that  
1611 they are going to be making a series of choices. If people possess an intuitive  
1612 understanding of aggregation, as suggested above, then this kind of contextual cue  
1613 will also facilitate aggregation.

1614 In addition to simply informing participants that they will make a series of  
1615 choices, making the choices more readily comparable may facilitate broad brack-  
1616 eting, and thus risk aggregation. Consider the inverse situation wherein a lack  
1617 of comparability between choices may prevent broad bracketing, such as when  
1618 an executive for a multi-business firm makes decisions across multiple distinct  
1619 industries. Of course, the similarity of decision contexts does not change the  
1620 maths of risk aggregation, but may well affect whether people do aggregate risk  
1621 across decisions. DeKay and Kim (2005) found that multi-play effects are not seen  
1622 when choices are not considered fungible. For instance, participants aggregated  
1623 across dollar amounts, but not across patients in a medical decision. Therefore,  
1624 people may behave similarly when considering a set of dissimilar choices if they  
1625 do not consider them fungible.

1626 There is further suggestive evidence that the similarity of a set of choices to  
1627 one another will affect choice bracketing. Choices whose differences are easy to  
1628 compare (alignable differences) are weighted heavier than those that are difficult  
1629 to compare (Markman & Loewenstein, 2010; Markman & Medin, 1995). Increased  
1630 similarity across a set of choices may both highlight the ability for those choices  
1631 to be bracketed, and further facilitate risk aggregation through the comparable  
1632 attributes. However, it is possible that increased similarity will facilitate risk  
1633 aggregation even without a tangible benefit to the underlying calculations. That  
1634 is, it is possible that simply manipulating the similarity of financially-irrelevant  
1635 semantics of a choice set will make people less risk averse. If so, then this will be by  
1636 virtue of an implicit risk aggregation in which the mere awareness of the possibility  
1637 of a grouping of choices reduces risk aversion. It is important to investigate the

## 2. Effect of Choice Bracketing on Risk Aggregation

1638 effect of similarity especially because in managerial settings, executives in multi-  
1639 business firms will often have to make comparisons across industries that are hard  
1640 to compare. For instance, GE currently develops both analytic software products  
1641 and jet engines for the military. They had been even more diversified previously,  
1642 at one stage simultaneously developing home appliances and owning the NBC  
1643 television network.

1644 In addition to the similarity between choices, how choices are presented may af-  
1645 fect how easily they are compared, and thus whether or not the multiple subsequent  
1646 effects listed above would come to fruition. As mentioned above, Haisley et al.  
1647 (2008) found a higher degree of EV maximisation when gambles were presented  
1648 jointly, rather than separately. Similarly, Hsee et al. (1999) found that people's  
1649 choices were affected by whether they viewed the attributes of the choices separately  
1650 or jointly. Their *evaluability hypothesis* suggests that attributes that are difficult to  
1651 evaluate will have a greater impact on joint presentation than separate presentation.  
1652 Joint presentation is a form of broad bracketing because it forces a participant to  
1653 view of all the components of a decision together. Participants may therefore  
1654 be more likely to consider aggregating the risk involved in a set of choices when  
1655 all those choices are in view. Joint presentation potentially reduces the working  
1656 memory load otherwise needed to maintain that set of choices. Therefore, it is  
1657 quite possible that a combination of highly similar choices, presented jointly will  
1658 lead to the highest likelihood of broad bracketing, and thus risk aggregation.

1659 Moher and Koehler (2010) replicated Gneezy and Potters (1997), but separately  
1660 manipulated the number of gambles seen per trial and feedback frequency. They  
1661 found that participants were less risk averse when viewing a set of three gambles  
1662 per trial, than when viewing only one. However, they only found this effect with  
1663 a set of identical outcomes. When outcomes were non-identical, there was no  
1664 effect of presentation. However, participants were always presented with gamble  
1665 outcomes for each trial, so it is unclear to what extent this influenced participants'  
1666 ability to bracket broadly. In fact, when seeing gambles separately, participants  
1667 were less risk averse when receiving feedback for each trial, compared to every

## *2. Effect of Choice Bracketing on Risk Aggregation*

1668 three trials. Testing a presentation manipulation without the confound of feedback  
1669 will help to clarify this effect.

### 1670 2.1.4 Internal Capital Market Investment Context

1671 Executives of large, successful firms are often viewed as fearless risk-takers who  
1672 take on risky projects to generate innovation and growth. However, the available ev-  
1673 idence suggests that executives do not view themselves that way (March & Shapira,  
1674 1987; Swalm, 1966). Executives typically evaluate multiple investments over time.  
1675 Risk aggregation is sensible when investments are only partially correlated (i.e.,  
1676 the success of one does not influence the success of another). It is sensible to  
1677 take on a set of risky investments with positive EV, where each investment has  
1678 some chance of loss, because those that succeed will make up for those that failed.  
1679 These benefits are well-known in stock market investment settings, thanks to Nobel  
1680 laureate Harry Markowitz's work on modern portfolio theory (1952).

1681 However, it is unclear whether the general public and even business managers  
1682 use this concept, due to the extent of risk aversion in both those populations (e.g.,  
1683 March & Shapira, 1987; Tversky & Kahneman, 1992). In fact, executives treat  
1684 risk like the rest of us; they view investments one at a time, are risk averse in the  
1685 domain of gains, and are risk seeking in the domain of losses (Lovallo et al., 2020;  
1686 MacCrimmon et al., 1986; Swalm, 1966). However, it is understandable why risk  
1687 aggregation is foreign to most people; outside of an investment portfolio selection  
1688 situation, it is unlikely for people to spontaneously group a selection of individual  
1689 risky choices. Usually in life, people encounter risky choices sequentially, and so  
1690 the risk of each individual choice is more salient than the aggregated risk of an  
1691 arbitrary combination of choices.

1692 Lovallo et al. (2020) showed that executives treat investments within their own  
1693 company in isolation. In multi-business firms, the managers of each business unit  
1694 often make the investment decisions about individual projects. Therefore, they  
1695 often do not consider the scope of their decisions in the context of the entire  
1696 company. For instance, Nobel laureate Richard Thaler offered 25 division managers

## 2. Effect of Choice Bracketing on Risk Aggregation

1697 working for the same firm a hypothetical investment that involves a 50% chance  
1698 of gaining \$2 million for the company and a 50% chance of losing \$1 million  
1699 (1999). Only three managers said they would accept the investment. However,  
1700 the CEO indicated that he would have clearly preferred managers to accept all  
1701 the investments. To each middle-manager, the choice represents a risk of loss for  
1702 their division and potentially their job, whereas for the CEO the entire portfolio  
1703 of choices represents a worthwhile risk.

1704 This chapter investigates risky choice in the context of business project in-  
1705 vestment internal to a company because this is a real-world context where choice  
1706 bracketing is important and currently under-appreciated (Lovallo et al., 2020).  
1707 The participants in these experiments were taken from a population that does  
1708 not have extensive managerial experience. However, in such a population a lack  
1709 of risk aggregation is most likely more common, and the variables used here are  
1710 readily applicable to the financial decisions that laypeople make. For instance,  
1711 one of the real-world applications of the choice bracketing literature has been to  
1712 use outcome distributions and increased time horizons to encourage investment in  
1713 high risk, but high EV, retirement funds (e.g., Benartzi & Thaler, 1999). Oth-  
1714 erwise, people typically prefer low risk, low EV, funds. Further, using laypeople  
1715 eliminates potential differences in prior experience with the management-based  
1716 decision-context. Upcoming research will focus on managers with context-specific  
1717 experience to investigate the effects of that experience.

## 1718 **2.2 Experiment 1**

1719 Experiment 1 investigated the effect of three choice bracketing manipulations  
1720 on risky choice in hypothetical capital allocation scenarios. Previous research had  
1721 low ecological validity because of the use of multi-play paradigms or feedback.  
1722 In this experiment, the risky choice task was a description-based repeated-play  
1723 paradigm. This means that participants had to make a choice about whether  
1724 to accept a number of different hypothetical investments, but were not provided

## *2. Effect of Choice Bracketing on Risk Aggregation*

1725 with the outcome of their choices after each decision. The variables of interest  
1726 were the similarity of the choices, whether the choices were presented together or  
1727 separately, and whether participants were aware of the number of choices that  
1728 they would be making.

1729 The values and probabilities of the gambles were set up such that each individual  
1730 gamble, as well as the aggregation of all the gambles, would be attractive to a  
1731 rational agent interested in maximising EV. As such, the key dependent measure  
1732 was the proportion of risky choices participants accepted.

1733 Previous research suggests that people will be willing to make more risky choices  
1734 when explicitly told to bracket their choices (Sokol-Hessner et al., 2012; Sokol-  
1735 Hessner et al., 2009). Therefore, Experiment 1 tested the following hypothesis:

1736 **Hypothesis 2.1—awareness main effect.** Participants that know how many  
1737 projects to expect will make more risky choices than participants that are unaware.

1738 Further, previous work suggests that joint presentation is a form of broad brack-  
1739 eting (e.g., Hsee et al., 1999; Moher & Koehler, 2010). Therefore, Experiment 1  
1740 tested the following hypothesis:

1741 **Hypothesis 2.2—presentation main effect.** Participants will make more risky  
1742 choices when seeing projects jointly than when seeing them separately.

1743 Similarity of options has also been shown to affect the way people bracket  
1744 their choices (e.g., DeKay & Kim, 2005). Therefore, Experiment 1 tested the  
1745 following hypothesis:

1746 **Hypothesis 2.3—similarity main effect.** Participants that see projects from  
1747 the same industry will make more risky choices than participants that see projects  
1748 from different industries.

## 2. Effect of Choice Bracketing on Risk Aggregation

**Table 2.1:** Experiment 1 group allocation.

Similarity	Awareness	N
High	Aware	53
High	Naive	53
Low	Aware	47
Low	Naive	45
Total		198

### 2.2.1 Method

#### 2.2.1.1 Participants

One hundred and ninety-eight participants (82 female) were recruited from the online recruitment platform Prolific. Participants were compensated at a rate of £5 an hour (Prolific is based in the UK). The average age was 32.52 years ( $SD = 11.42$ ,  $min. = 18$ ,  $max. = 69$ ). Participants reported an average of 7.01 years ( $SD = 9.1$ ,  $min. = 0$ ,  $max. = 42$ ) working in a business setting, and an average of 1.7 years ( $SD = 2.85$ ,  $min. = 0$ ,  $max. = 20$ ) of business education. The mean completion time of the task was 12.04 min ( $SD = 11.29$ ,  $min. = 3.1$ ,  $max. = 112.4$ ). Table 2.1 shows the allocation of participants to the different conditions.

#### 2.2.1.2 Materials

**2.2.1.2.1 Instructions** Participants were told to imagine that they are executives in a large company and that they will need to decide about investing in a number of hypothetical business projects. The appendix shows these instructions in Figure A.1.

**2.2.1.2.2 Risky Investment Task** Participants saw 10 short descriptions of business projects, and were asked whether they would invest in that project or not. Each description included the name of the hypothetical business, the amount they forecast the project to cost, the amount the project is forecast to make, and probabilities for these forecasts. The project values were selected so that the

## 2. Effect of Choice Bracketing on Risk Aggregation

1769 projects appeared attractive when aggregated, and unattractive when segregated  
1770 (see T. Langer & Weber, 2001). These values were different for each project, but  
1771 followed a set of constraints for each project's EV and the probability of any loss  
1772 given the outcome distribution of all 10 projects ( $P(\text{loss}_{\text{aggregated}})$ ). Further, there  
1773 was a constraint on the gambles' loss aversion coefficient ( $\lambda$ ), which is a measure  
1774 of people's sensitivity to losses compared to gains. The constraints were:

- 1775 1.  $\text{EV} > 0$ ;  
1776 2.  $\lambda < 2.25$ ; and  
1777 3.  $P(\text{loss}_{\text{aggregated}}) < 0.1$ .

1778 As such, each project cannot be considered to be a loss in terms of expected  
1779 value, but also would not be an easy choice for investment, because of the low  $\lambda$   
1780 (made to be lower than the median loss aversion coefficient calculated in Tversky &  
1781 Kahneman, 1992). Further, since people are especially sensitive to loss probabilities  
1782 (Kahneman & Tversky, 1979; Zeisberger, 2020), an arbitrarily low  $P(\text{loss}_{\text{aggregated}})$   
1783 was chosen to make investment in the complete set of projects seem attractive. The  
1784 actual probability of a loss given the outcome distribution used in the experiment  
1785 was 0.09. This was calculated by summing all probabilities in the Poisson binomial  
1786 distribution whose outcomes were less than zero. For comparison,  $P(\text{loss}_{\text{aggregated}})$   
1787 = 0.17 for 10 plays of the Samuelson (1963) gamble. The highest probability of  
1788 a loss for any single gamble ( $P(\text{loss}_{\text{single}})$ ) was 0.80. Figure 2.2 shows an example  
1789 of a description of a project in this task.

1790 In the high similarity condition, these project descriptions were all about one  
1791 type of project (in this case an oil well project) and were all from the same business.  
1792 In the low similarity condition, each project was from a different industry. In the  
1793 joint presentation condition, the 10 projects were all displayed on the one webpage,  
1794 whereas in the separate presentation condition each was displayed on a different  
1795 webpage. Participants in the aware condition saw the display shown in Figure 2.3  
1796 before their separate presentation display. Those in the naive condition simply

## 2. Effect of Choice Bracketing on Risk Aggregation

Refinera is a business in your company that proposes to construct an oil well project, which they forecast will cost \$40 million. If the project succeeds, forecasts show the company would make \$240 million. Research suggests that there is a 20% chance of the project succeeding. Therefore, **there is 20% chance of gaining \$200 million and a 80% chance of losing \$40 million on the investment.**

Would you invest in the project?

\*

Yes  No

**Continue**

**Figure 2.2:** Example of a project choice display in Experiment 1.

You will now see 10 projects. Decide whether you would like to invest in each one.

**Continue**

**Figure 2.3:** The display seen by those in the aware condition of Experiment 1.

1797 proceeded without this message. The financial and probability values were identical  
1798 regardless of condition, and the order of each set of 10 projects was randomised.

1799 Although the project descriptions were succinct, and the decisions in the task  
1800 were made quickly, they reflect real decisions in businesses in critical ways. Compa-  
1801 nies that consider their forecast estimates probabilistically (i.e., do not simply use  
1802 the most likely estimate as the only estimate) do frame their options as likelihoods  
1803 of certain monetary outcomes.

1804 **2.2.1.2.3 Outcome Distribution Decision** Participants were asked if they  
1805 would invest in the last 10 projects they saw and were provided with a graph of  
1806 the outcome probability distribution of the 10 projects. Figure A.2 shows this  
1807 graph. A coding error was discovered after collecting data. This was an error  
1808 in the generation of gambles, which meant that the outcome distribution decision  
1809 data could not be used. Therefore, the effect of outcome distribution will not be  
1810 discussed until Experiment 2, which fixed this issue. Appendix A.1.2.2 presents an

## 2. Effect of Choice Bracketing on Risk Aggregation

1811 analysis of these data, and describes the coding error and its implications.

1812 **2.2.1.2.4 Follow-up Gambles** Participants were shown four further sets of  
1813 gambles (11 total) that checked participant attention and replicated the gambles  
1814 from Samuelson (1963) and Redelmeier and Tversky (1992). See Appendix A.1.1.1.3  
1815 for details.

1816 **2.2.1.3 Procedure**

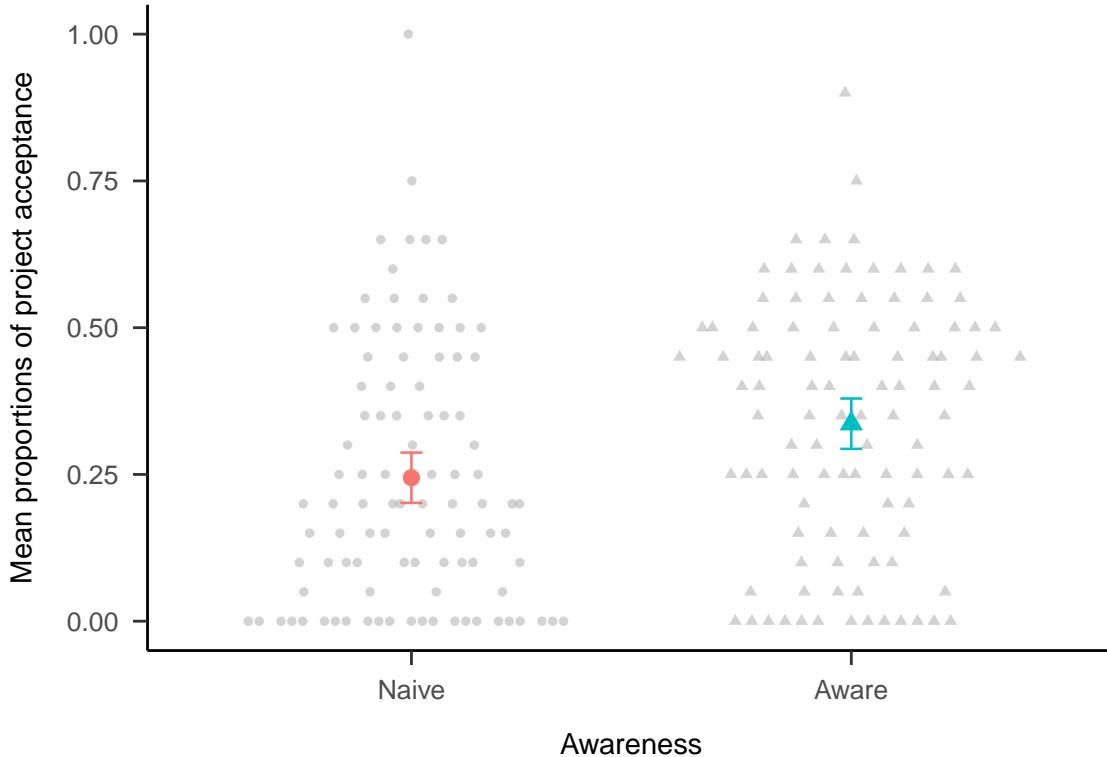
1817 Participants read the instructions and completed the risky investment task, first  
1818 in the separate presentation condition, and then in the joint condition. They then  
1819 made the outcome distribution decision and responded to the 11 follow-up gambles.

1820 **2.2.2 Results**

1821 **2.2.2.1 Project Choice**

1822 A three-way analysis of variance (ANOVA) was conducted to investigate the  
1823 effects of similarity, awareness, and presentation on the proportion of participants'  
1824 decision to invest in the 10 projects. As seen in Figure 2.4, participants invested  
1825 more when they were told that there will be 10 projects, compared with when they  
1826 were not told this,  $F(1, 194) = 9.52, p = .002, \hat{\eta}_p^2 = .047$ . As seen in Figure 2.5,  
1827 participants invested more when viewing the projects jointly, compared with when  
1828 they viewed them separately,  $F(1, 194) = 28.14, p < .001, \hat{\eta}_p^2 = .127$ . Although  
1829 there was no main effect of similarity,  $F(1, 194) = 1.63, p = .204, \hat{\eta}_p^2 = .008$ ,  
1830 the interaction between similarity and presentation was significant,  $F(1, 194) =$   
1831  $4.31, p = .039, \hat{\eta}_p^2 = .022$  (see Figure 2.6). Specifically, the presentation effect  
1832 was stronger in the high similarity condition,  $\Delta M = 0.07$ , 95% CI [0.04, 0.09],  
1833  $t(194) = 5.29, p < .001$ , than in the low similarity condition,  $\Delta M = 0.03$ , 95% CI  
1834 [0.00, 0.05],  $t(194) = 2.06, p = .041$ . These findings suggest that it is possible to  
1835 facilitate risk aggregation with subtle choice bracketing manipulations.

## 2. Effect of Choice Bracketing on Risk Aggregation

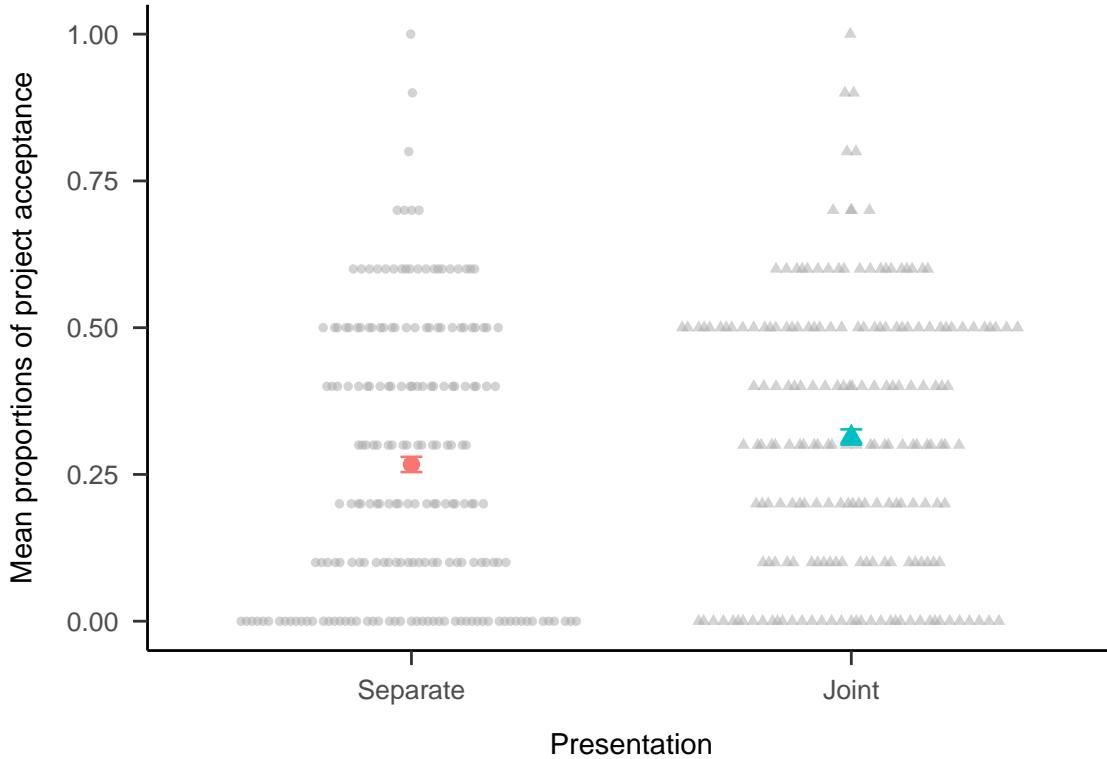


**Figure 2.4:** Mean proportions of decisions to invest in each set of 10 projects, by awareness condition. Error bars represent 95% confidence intervals. Raw data are plotted in the background.

### 1836 2.2.2.2 Trial-by-Trial Analysis

1837 Exploratory analyses were conducted into the possible effects of the manip-  
 1838 ulations on a trial-by trial basis. Figure A.3 shows the data for all conditions.  
 1839 However, the key findings are in the separate presentation. As Figure 2.7 shows,  
 1840 in the separate condition people are more likely to accept projects over the 10  
 1841 trials, but this interacts with awareness,  $b = 0.04$ , 95% CI [0.01, 0.08],  $z = 2.32$ ,  
 1842  $p = .021$ . Specifically, the relationship between choice and trial is stronger in the  
 1843 aware condition,  $b = 0.11$ , 95% CI [0.06, 0.16],  $z = 4.54$ ,  $p < .001$ , than in the  
 1844 naive condition,  $b = 0.03$ , 95% CI [-0.03, 0.08],  $z = 1.01$ ,  $p = .311$ . It seems that  
 1845 participants that were told the total number of projects became less risk averse as  
 1846 the experiment proceeded, regardless of the gamble values.

## 2. Effect of Choice Bracketing on Risk Aggregation



**Figure 2.5:** Mean proportions of decisions to invest in each set of 10 projects, by presentation condition. Error bars represent 95% confidence intervals. Here, however, the intervals are so narrow that they are sometimes obscured by the mean indicators in the plot. Raw data are plotted in the background.

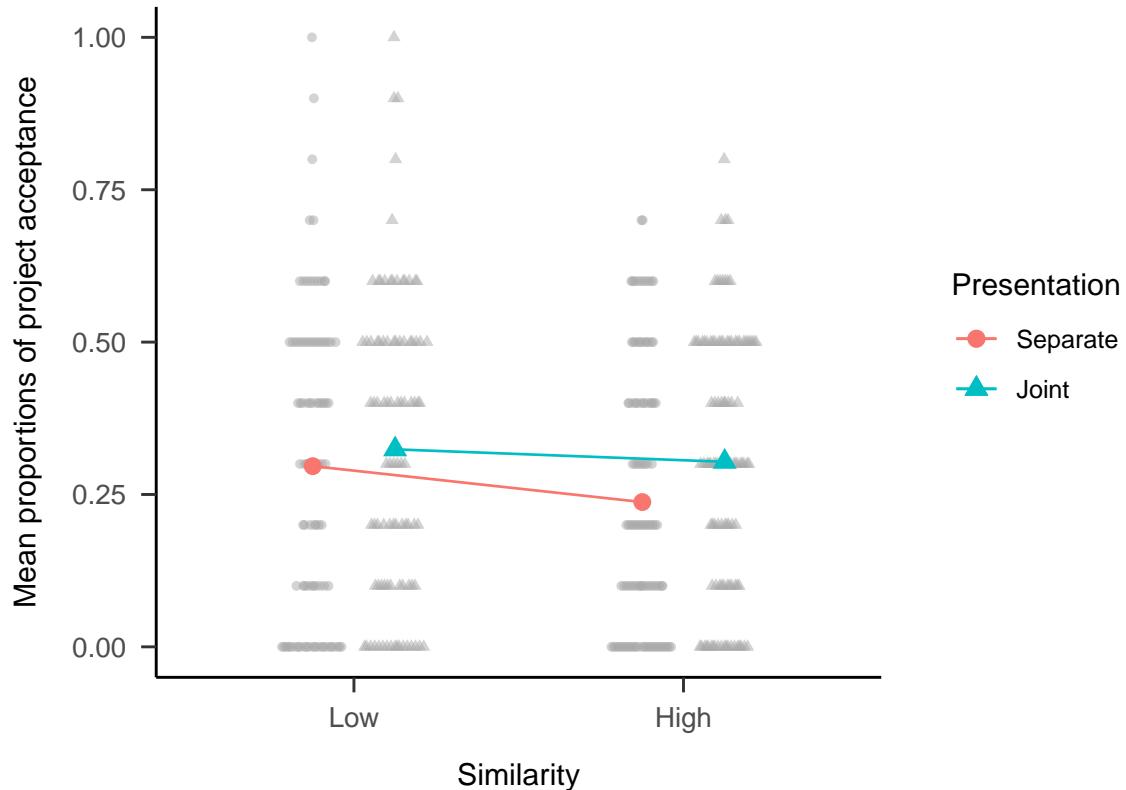
### 1847 2.2.3 Discussion

1848 Experiment 1 found evidence for most of the hypotheses. Specifically, people  
1849 made more risky choices when considering those choices jointly on the same page,  
1850 compared to on separate pages; and when they knew how many choices were in  
1851 the set. Further, the results showed an interaction between project similarity  
1852 and presentation. Exploratory analyses showed that participants' risk aversion  
1853 decreased as they proceeded through the trials, but only when participants were  
1854 aware of the number of projects.

#### 1855 2.2.3.1 Presentation Effect

1856 The presentation effect may be a result of one of two mechanisms. A math-  
1857 ematical aggregation explanation would mean that participants are combining  
1858 the gambles into a mental representation of the probability distribution and then

## 2. Effect of Choice Bracketing on Risk Aggregation



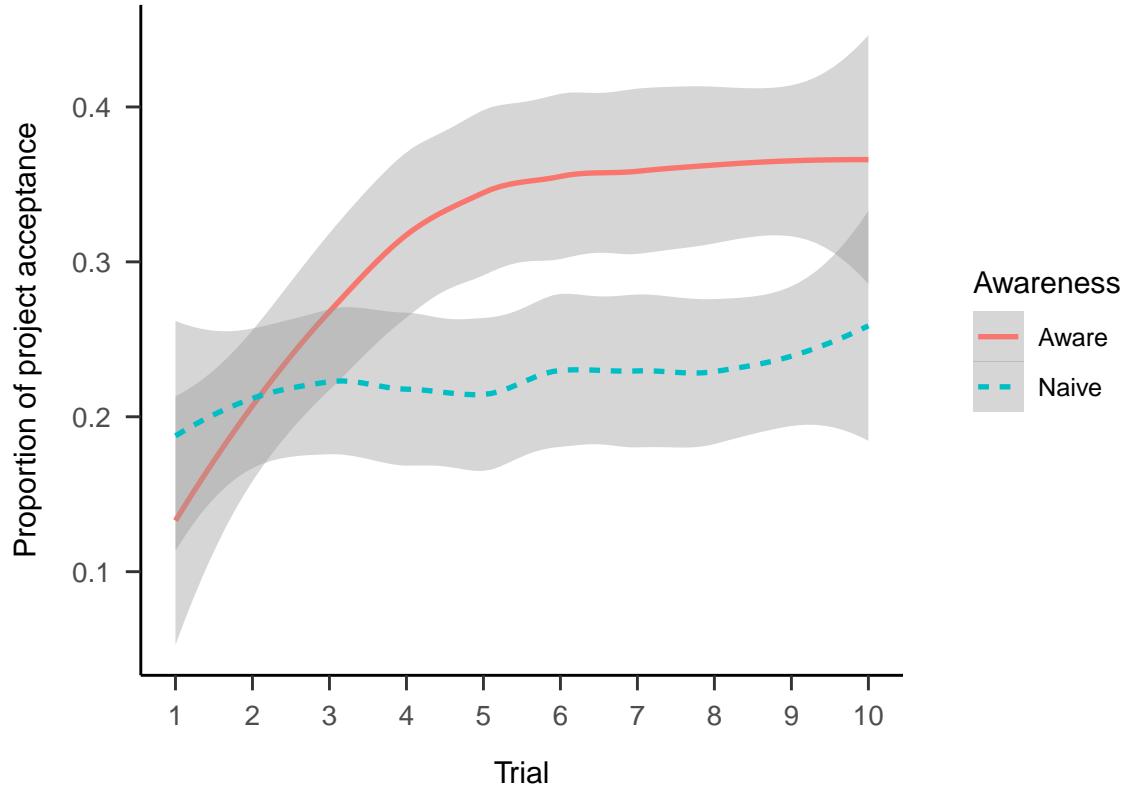
**Figure 2.6:** Mean proportions of decisions to invest in each set of 10 projects, by similarity and presentation conditions. In mixed factorial designs, error bars cannot be used to make inferences by “eye” across all conditions. Therefore, error bars are not included. Raw data are plotted in the background.

1859 deciding based on the attractiveness of that distribution. A joint presentation of  
 1860 choices would facilitate this combination. On the other hand, people may also be  
 1861 using a sort of naive aggregation process when they are encouraged to group their  
 1862 choices together. A naive aggregation explanation would suggest that participants  
 1863 in the joint condition are simply more likely to realise that a few big wins could  
 1864 offset a few losses. Participants could have been encouraged by the joint display  
 1865 to consider the set of projects together. This could then lead to the conclusion  
 1866 that investing in a higher number of gambles might mean that the gains of some  
 1867 projects will pay off the losses of the other projects.

### 1868 2.2.3.2 Awareness Effect

1869 Experiment 1 found that participants that viewed the projects separately were  
 1870 more likely to invest in the projects as the trials went on, regardless of the actual

## 2. Effect of Choice Bracketing on Risk Aggregation



**Figure 2.7:** Proportion of project acceptance in the separate presentation condition, by trial and awareness conditions. LOESS method was used for smoothing over trials and the shading represents 95% confidence intervals.

1871 gambles. Having an awareness of the total number of projects in the set could  
 1872 increase the likelihood that participants would naively aggregate. Specifically,  
 1873 knowing the number of total projects might increase the salience of the idea that the  
 1874 gains of some projects will offset the losses of others, because it reinforces a focus  
 1875 on the entire set. Another possibility is that participants had a certain aspiration  
 1876 level (Lopes, 1996) that they were attempting to reach. This might mean that  
 1877 they invested more as the task proceeded after realising that the gambles were not  
 1878 becoming significantly more favourable. Barron and Erev (2003, p. 219) specifically  
 1879 did not tell participants about the number of gambles they would experience to  
 1880 “avoid an ‘end of task’ effect (e.g., a change in risk attitude).” Barron and Erev  
 1881 (2003) provided participants with feedback, but this should not be necessary for  
 1882 an aspiration level explanation since participants only need to be aware of the  
 1883 potential for certain gains.

## 2. Effect of Choice Bracketing on Risk Aggregation

This result may also be due to a Gambler's fallacy effect or the law of small numbers. This effect is characterised by people's expectation of a pattern to follow the underlying distribution of the function that generates each component. For instance, someone observing the results of a coin flip that look like HTTHHTTT might anticipate that the likelihood of "heads" is higher than that of "tails," despite the actual likelihood being 50% for either. This effect occurs in sequential decision-making, so may be relevant for the repeated-play decisions in Experiment 1. Barron and Leider (2010) found that the gambler's fallacy (in a roulette prediction task) emerges when information about past outcomes was displayed sequentially, but not when it is displayed all at once. Haisley et al. (2008) found evidence for the gambler's fallacy with a repeated-play gamble paradigm. As such, it is possible that an effect such as the Gambler's fallacy can explain the effect of the awareness manipulation. That is, participants may have thought that after a few gambles that they considered risky, the last ones were more likely to materialise. Further, this would be more likely to occur for those that knew the total number of projects, because they knew when the sequence was approaching its end.

### 2.2.3.3 Similarity Effect

Experiment 1 did not find a main effect of similarity in the individual choice data as predicted in Hypothesis 2.3. Instead, choice similarity interacted with the presentation condition. This interaction is harder to explain since it was not hypothesised. In fact, the results seem to suggest the opposite to what was originally expected. Initially, it was predicted that people would be less risk averse in the high similarity condition, due to the better ability to consider the isolated projects as a grouped set. Similarity was thought to act as a broad bracket, and therefore increase aggregation. That is, it was expected that seeing a set of similar projects would help participants aggregate risk when seeing them separately, more than when projects are dissimilar. Instead, project acceptance was actually numerically higher in the low than in the high similarity condition ( $\Delta M = -0.06$ ,

## 2. Effect of Choice Bracketing on Risk Aggregation

1912 95% CI [-0.12, 0.00],  $t(228.14) = -1.83, p = .068$  when projects were presented  
1913 separately, averaging over awareness conditions.

1914 There was no significant difference between similarity conditions regardless  
1915 of presentation condition. However, allocations were significantly higher in the  
1916 joint presentation condition than in the separate condition for both high and low  
1917 similarity. The interaction seems to have been found due to the larger difference in  
1918 the high similarity condition. Perhaps the ability to aggregate risk when projects  
1919 are presented together is more made more salient when projects are similar.

1920 Specifically, the interaction seems to be driven by the separate high similarity  
1921 condition being lower, rather than by the joint high similarity being higher, as  
1922 would have been expected. As such, participants could have been engaged in a  
1923 naive *diversification*, rather than a naive aggregation. In “true” diversification,  
1924 people would choose a set of projects that are partially (and ideally negatively)  
1925 correlated, as per Markowitz (1952). However, in reality people that intend to  
1926 diversify only seem diversify naively, meaning that they neglect co-variation when  
1927 diversifying (e.g., Hedesstrom et al., 2006). Instead, they only seem to be looking  
1928 for variety, rather than diversification in the strict sense. This *diversification bias*  
1929 is also seen in product choices (Read & Loewenstein, 1995).

1930 In Experiment 1, participants may have considered the high similarity condition  
1931 as a sign that the set of projects may not be sufficiently “diversified.” However, this  
1932 explanation would also predict the joint presentation condition to be lower in the  
1933 high similarity condition. So, perhaps when in the separate condition, participants  
1934 were constantly thinking that they might be getting a different project in the next  
1935 display, so rejected more projects because of the lack of diversification, but not  
1936 realising that they would not be getting any other type of project. Those in the  
1937 joint presentation, on the other hand, were able to see all ten projects, so already  
1938 knew that there were no other projects in the set, and so were less likely to reject  
1939 projects on the basis of the hope for different projects in the future.

## 2. Effect of Choice Bracketing on Risk Aggregation

### 2.2.3.4 Limitations

This experiment had two major limitations. First, proper counterbalancing was not used in the high alignment project domain, nor in the order of the within-subjects manipulation of presentation. As such, it is unclear what role these elements played in the results, especially in the presentation condition, in which participants always saw the separate condition first. Second, as mentioned above in Section 2.2.1.2.3, there was a mistake in the generation of the gamble values that meant that the individual gambles did not correspond with the distribution that participants saw. Both of these limitations were addressed in Experiment 2.

## 2.3 Experiment 2

Experiment 2 investigated the effect of presentation, awareness, and distribution on project choice. For the distribution manipulation, half of the sample saw an outcome probability distribution as in the previous literature (e.g., Redelmeier & Tversky, 1992; Webb & Shu, 2017) to determine their risk aversion when the gambles are explicitly aggregated. In contrast to most of the repeated-play choice literature, each choice was presented without subsequent feedback. Further, in contrast to Experiment 1, the distribution was displayed alongside each gamble, as opposed to only at the very end. This is an important manipulation because finding out whether it is effective will (a) add to the understanding of the conditions necessary for mathematical aggregation (beyond a mere intuitive sense of aggregation), and (b) suggest new ways to encourage aggregation in real-world applications.

In past work, participants were shown ordinary binomial distributions, since multi-play gambles are identical. However, there has not been an investigation of *non-identical* gamble distributions in this context. Doing this requires using a *Poisson* binomial distribution, which allows for multiple trials with different probabilities.

Further, Experiment 2 addressed potential order effects in Experiment 1 by manipulating all the main variables between-subjects. Manipulating presentation

## 2. Effect of Choice Bracketing on Risk Aggregation

1968 between-subjects, removes the potentially confounding factor of reduced risk aver-  
1969 sion over time.

1970 Experiment 2 again tested Hypotheses 2.1, and 2.2, from Experiment 1. Further,  
1971 following the finding in Experiment 1 that participants in the aware condition  
1972 seemed to become more risk-taking as the experiment progressed, Experiment 2  
1973 tested the following hypothesis:

1974 **Hypothesis 2.4—interaction of trial number and awareness.** Participants  
1975 will make more risky choices as the trials progress, but only when they are aware  
1976 of the total number of projects in the set.

1977 Further, multi-play gambles with outcome distributions have been shown to  
1978 reduce risk aversion compared to multi-play gambles without distributions (e.g.,  
1979 Redelmeier & Tversky, 1992; Webb & Shu, 2017). Therefore, Experiment 2 tested  
1980 the following hypothesis:

1981 **Hypothesis 2.5—distribution effect.** Participants will make more risky choices  
1982 when presented with an aggregated outcome distribution than when making the  
1983 same decisions individually.

### 1984 2.3.1 Method

#### 1985 2.3.1.1 Participants

1986 One hundred and sixty-four participants (51 female) were recruited from the  
1987 online recruitment platform Prolific. Participants were compensated at a rate of £5  
1988 an hour (Prolific is based in the UK). The average age was 26.39 years ( $SD = 8.63$ ,  
1989  $min. = 18$ ,  $max. = 72$ ). Participants reported an average of 2.55 years ( $SD = 5.34$ ,  
1990  $min. = 0$ ,  $max. = 43$ ) working in a business setting, and an average of 1.67 years  
1991 ( $SD = 2.94$ ,  $min. = 0$ ,  $max. = 20$ ) of business education. The mean completion  
1992 time of the task was 6.53 min ( $SD = 5.15$ ,  $min. = 1.18$ ,  $max. = 39.93$ ). Table 2.2  
1993 shows the allocation of participants to the different conditions. Appendix A.2.1.1.1  
1994 describes the power analysis conducted to arrive at this sample size.

## 2. Effect of Choice Bracketing on Risk Aggregation

**Table 2.2:** Experiment 2 group allocation.

Awareness	Distribution	Presentation	N
Aware	Absent	Separate	40
Naive	Absent	Joint	41
Naive	Absent	Separate	41
Naive	Present	Separate	42
Total			164

### 1995 2.3.1.2 Materials

1996 **2.3.1.2.1 Instructions** Participants were shown the same instructions as in  
1997 Experiment 1 (see Section 2.2.1.2.1).

1998 **2.3.1.2.2 Risky Investment Task** Participants saw a similar display to the  
1999 one in Experiment 1 (see Section 2.2.1.2.2), but with new gamble values, in order  
2000 to fix the mistake in the Experiment 1 gamble value calculation (detailed in the  
2001 appendix Section A.1.2.2).

2002 The presentation and awareness manipulations were as in Experiment 1. How-  
2003 ever, in the distribution-present condition participants saw the outcome probability  
2004 distribution of all the projects alongside the description, rather than after all the  
2005 projects were seen (see Figure 2.8).

2006 **2.3.1.2.3 Follow-up** Participants were asked how many projects they thought  
2007 they saw, whether they were willing to accept all or none of the projects, and  
2008 how many they would be willing to accept if they had to choose a number. Ap-  
2009 pendix A.2.1.2.1 shows these questions.

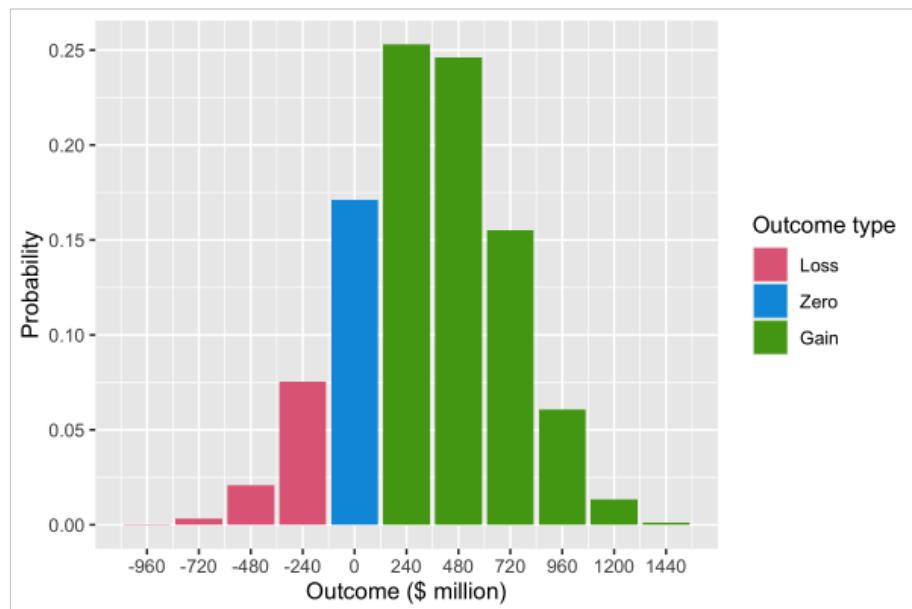
### 2010 2.3.1.3 Procedure

2011 Participants read the instructions and completed the risky investment task in  
2012 their respective conditions. After seeing the individual projects, participants were  
2013 then asked the three follow-up questions.

## 2. Effect of Choice Bracketing on Risk Aggregation

Below is the probability distribution of final outcomes if all projects were chosen.

The numbers on the x-axis (labelled 'Outcome') represent the final amounts of money possible if you chose to invest in all the projects. The numbers on the y-axis (labelled 'Probability') represent the likelihoods of each of the possible outcomes. Negative final outcomes (losses) are shown in red, positive final outcomes (gains) are shown in green, and a final outcome of zero (no loss or gain) is shown in blue.



Indicate below whether you would invest in the following:

Refinera is a business in your company that proposes to construct an oil well project, which they forecast will cost \$40 million. If the project succeeds, forecasts show the company would make \$240 million. Research suggests that there is a 30% chance of the project succeeding. Therefore, **there is 30% chance of gaining \$200 million and a 70% chance of losing \$40 million on the investment.\***

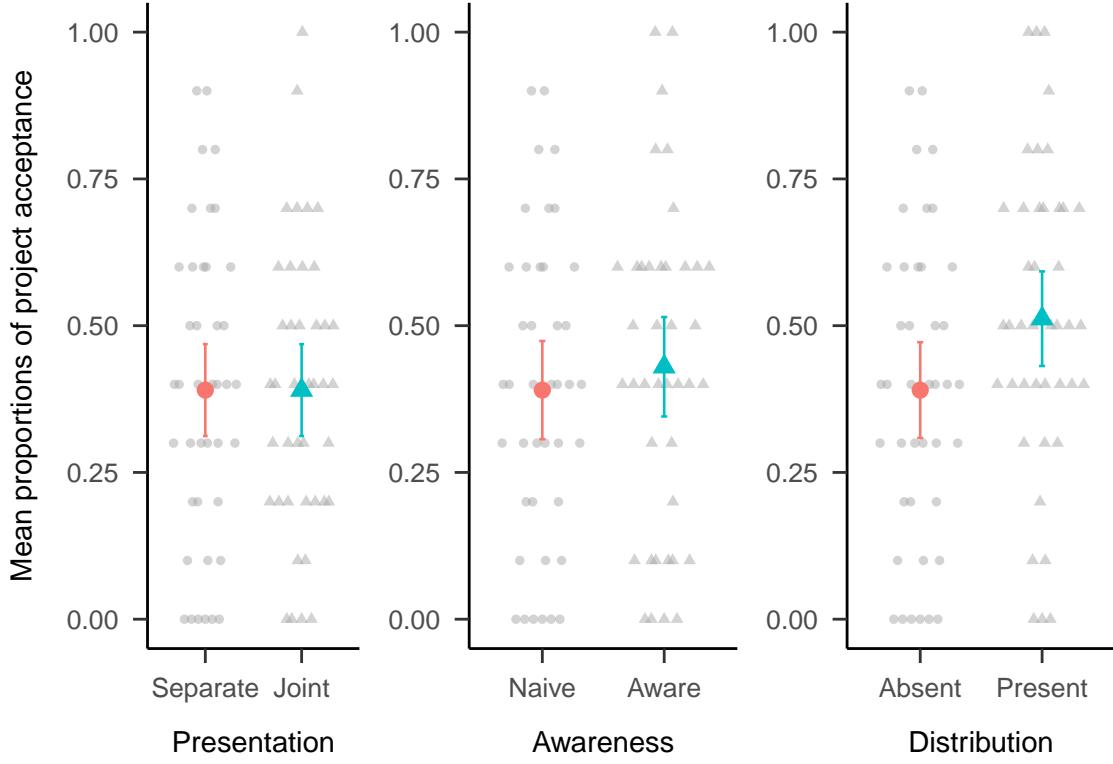
Yes

No

[Continue](#)

**Figure 2.8:** An example of a display seen by those in the separate distribution-present condition of Experiment 2.

## 2. Effect of Choice Bracketing on Risk Aggregation



**Figure 2.9:** Mean proportion of project acceptance for the presentation, awareness, and distribution effects. The condition on the left of each effect is the reference condition (separate presentation, naive awareness, distribution absent). As such, it is identical for the three effects. Error bars represent 95% confidence intervals. Raw data are plotted in the background.

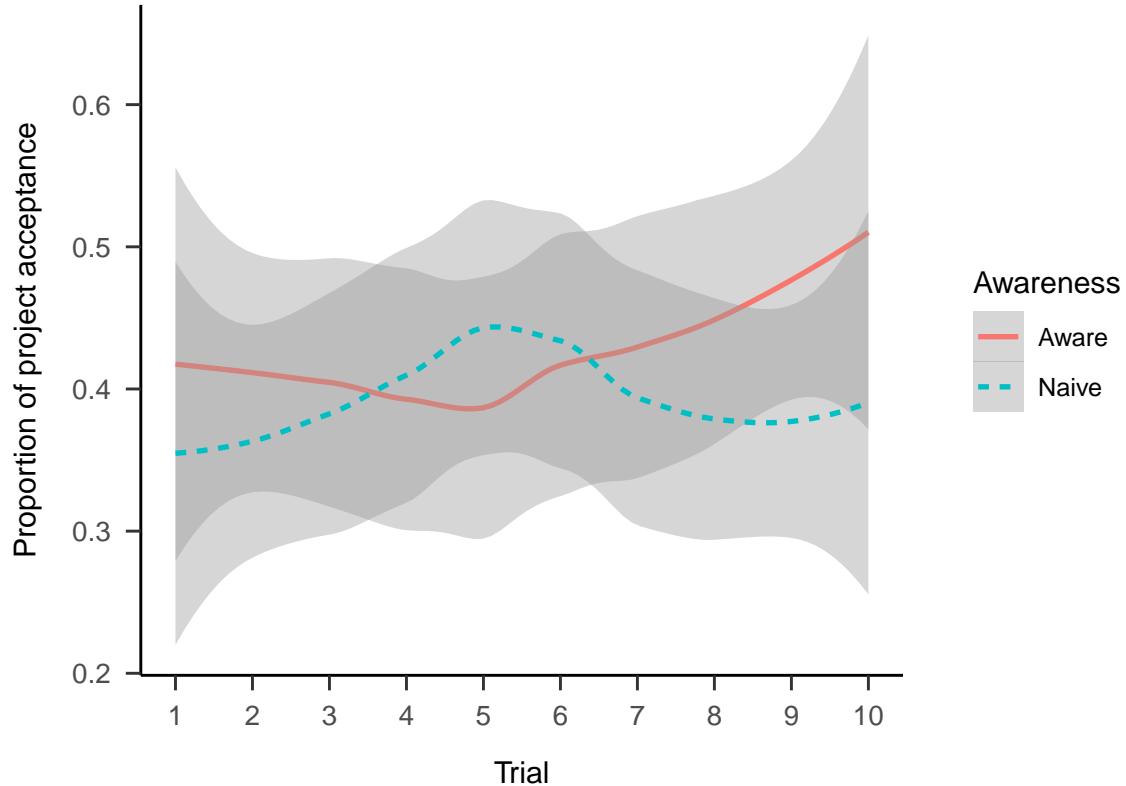
### 2014 2.3.2 Results

#### 2015 2.3.2.1 Project Investment

2016 The project investment data were analysed as proportions of choice per participant, as in Experiment 1. Each experimental condition was compared to the same control condition (separate presentation, naive awareness, and distribution absent).  
 2017 Figure 2.9 shows these data. The difference between presentation conditions was  
 2018 not significant,  $F(1, 80) = 0.00, p > .999, \hat{\eta}_p^2 = .000$ . Similarly, the difference  
 2019 between awareness conditions was not significant,  $F(1, 79) = 0.44, p = .508,$   
 2020  $\hat{\eta}_p^2 = .006$ . However, those that saw a distribution chose to invest significantly  
 2021 more (51.19%) than those that did not see a distribution (39.02%),  $F(1, 81) = 4.46,$   
 2022  $p = .038, \hat{\eta}_p^2 = .052$ .

2023 Further, as Figure 2.10 shows, it doesn't seem as if the previous awareness

## 2. Effect of Choice Bracketing on Risk Aggregation



**Figure 2.10:** Mean project acceptance for separate presentation, distribution absent condition, by awareness and trial. LOESS method was used for smoothing over trials and the shading represents 95% confidence intervals.

2026 by trial effect was replicated.

### 2027 2.3.2.2 Follow-up

2028 The portfolio choice data from both the number and binary questions were  
2029 congruent with the above, finding that those in the distribution condition were  
2030 more likely to invest (see Appendix A.2.2).

### 2031 2.3.3 Discussion

2032 Experiment 2 found support for Hypothesis 2.5. Seeing an outcome distribution  
2033 of a business project portfolio had a strong effect on participants' decision-making.  
2034 Participants indicated that they would invest in more projects and were more likely  
2035 to indicate that they would invest in the entire portfolio. However, the awareness  
2036 and presentation effects found in Experiment 1 (see Section 2.2.2) did not replicate.

## 2. Effect of Choice Bracketing on Risk Aggregation

2037 These findings provide evidence for choice bracketing. That is, people do seem  
2038 to be primarily considering gambles one at a time. Further, these findings suggest  
2039 that that the main bottleneck for appropriately aggregating a set of gambles  
2040 is a computational one. That is, people simply cannot mentally combine the  
2041 outcomes and probabilities in a way that sufficiently approximates the outcome  
2042 distribution display.

2043 The lack of replication of the awareness and presentation effects provides evi-  
2044 dence against a naive aggregation account of the distribution effect. Specifically  
2045 this suggests that the distribution effect is a result of a lack of ability to mathemat-  
2046 ically combine risk, rather than naive aggregation. If some of the bottleneck was  
2047 attributable to a lack of realisation that the individual gambles could be grouped  
2048 together, then the effects from Experiment 1 should have replicated. Instead it  
2049 seems that even when people have an opportunity to consider an entire set of  
2050 risky choices together (and consider that the gains may outweigh the losses), they  
2051 do not do this.

2052 In Experiment 2, all the gambles came from the same domain. This was done  
2053 to attempt to replicate the relevant effects from Experiment 1. However, there  
2054 could have been something about that particular domain that led to the lack of  
2055 replication. A follow-up experiment addressed this issue by presenting participants  
2056 with 20 gambles from 10 different industries and still did not replicate the awareness  
2057 effect (see Appendix A.4).

## 2058 2.4 General Discussion

2059 When making one decision about a series of risky choices, it is clear that people  
2060 have an intuitive sense of the advantages of risk aggregation (e.g., Samuelson, 1963).  
2061 However, because risky choices are typically made one at a time in the real world,  
2062 this chapter aimed to identify whether (and how) this intuition could be leveraged  
2063 in this more realistic scenario. Overall, there was little evidence that subtle cues  
2064 could tap into this intuitive advantage of risk aggregation, and clear visualisations

## 2. Effect of Choice Bracketing on Risk Aggregation

of outcome distributions were needed to assist people's risk aggregation. This suggests that the act of deciding can create a strong cognitive barrier to treating a series of decisions as if they were one. However, as elaborated below, the success of the outcome distribution for overcoming this cognitive barrier in the current paradigm is a novel and important finding.

This chapter found that some choice bracketing facilitated risk aggregation in description-based repeated-play gambles. This paradigm has never been a target of research. Early work on risk aggregation involved multi-play gambles, which treated gambles as simultaneous and identical. However, most risky choice outside the lab involves considering multiple choices independently, as in repeated-play paradigms. Most repeated-play paradigms have involved providing participants with feedback, or allowing them to sample from outcome distributions. Large real-life investments are different, as their outcomes are not eventuated immediately (and do not allow for distribution sampling). The limited prior work using description-based repeated-play gambles did not consider the effect of choice bracketing on risk aggregation. As such, the paradigm used in this chapter allowed for the investigation of choice bracketing in a way that is more isomorphic with real-life prescriptions.

Experiment 1 found evidence for the effects of similarity, presentation, and awareness of the number of projects. Experiment 2 found evidence for the effect of an outcome distribution but did not replicate the presentation and awareness effects. Subsequent follow up experiments (reported in Appendices A.3 and A.4) again tested the similarity and awareness effects. These experiments found evidence for naive diversification (an advantage for low similarity) when considering all projects once and did not replicate the trial-by-trial interaction from Experiment 1.

Therefore, in addition to the novelty of the paradigm itself, this chapter found that choice bracketing facilitates risk aggregation, if aided by the aggregated distribution. As per Hypothesis 2.5, Experiment 2 found that showing a distribution of outcome probabilities without inter-trial feedback reduced risk aversion. Further, there was mixed evidence for Hypothesis 2.3, such that people were less risk averse

## 2. Effect of Choice Bracketing on Risk Aggregation

when the set of projects they saw were dissimilar, but only when offered them as a portfolio (see Appendix A.3). There was only minimal evidence for Hypotheses 2.1 and 2.2, suggesting that viewing projects together and an awareness of the number of projects are not sufficient to encourage aggregation. Altogether, it seems that subtle contextual cues are often not sufficient to encourage risk aggregation and that people need risk to be aggregated for them explicitly in order to understand the benefits of aggregation.

### 2.4.1 Theoretical Implications

The finding that participants are less risk averse when provided with an aggregated outcome distribution is congruent with previous work (e.g., Redelmeier & Tversky, 1992). However, when distributions have been previously used, gambles were identical—as in multi-play paradigms—and used immediate feedback for repeated-play paradigms (e.g., Benartzi & Thaler, 1999). As mentioned previously, both these paradigms have limited ecological validity because usually people are faced with non-identical sequential choices and do not receive immediate feedback. This work is the first to provide evidence for this aggregation effect with non-identical gambles without feedback.

The other choice bracketing findings that showed little success with aiding aggregation are less congruent with previous research. Sokol-Hessner et al. (2009) and Sokol-Hessner et al. (2012) found that encouraging participants to make decisions akin to a professional investor increased the amount of risky choices they made. The results showed that a subtler manipulation—whether or not participants were aware of the number of choices to be made—is not sufficient to encourage aggregation. Hsee et al. (1999) found that useful, but hard-to-interpret, attributes were used more when the options were presented jointly, rather than separately. In the case of these experiments, the “hard to interpret” element of the decision set was the risk of the projects. Contrary to Hsee et al. (1999), it seems that risk was not always accounted for more when projects were presented jointly, rather than separately. More study is needed to understand whether the effects that were

## 2. Effect of Choice Bracketing on Risk Aggregation

seen in Experiment 1 but not replicated in the subsequent experiments are due to statistical chance or unexplored elements of the experiment.

Research on the effect of option similarity on choice (e.g., Markman & Medin, 1995) suggests that alignable differences are more important than non-alignable differences. Further, the effects of multi-play gambles and outcome distributions on risk aggregation are only seen when participants perceive the options as fungible (e.g., DeKay & Kim, 2005). As such, it was predicted that a set of investments that involve the same type of investment would be seen as more similar, and therefore be considered as fungible. Hypothesis 2.3 predicted that this would facilitate a broad bracketing, and therefore more risk aggregation.

Instead, the results showed that choice similarity did not affect individual project allocations. However, when participants were given an all-or-nothing choice for the entire set of projects, those that viewed dissimilar projects were more likely to take the entire set projects than those that viewed similar projects. This is different from the initial hypothesis, however, it may still suggest an effect of choice bracketing. That is, this effect was only found when participants were asked about the entire portfolio of projects, rather than when they had a chance to make a choice about each project. The way that the question was framed may have acted to broadly bracket the choices by forcing the choice.

A diversified portfolio is one whose investments are uncorrelated or negatively correlated. According to portfolio theory (Markowitz, 1952), a diversified portfolio is preferred to one that is not diversified, because it reduces the probability of a loss. When some investments have losses, others will have gains—the root of “don’t keep all your eggs in one basket.” Typically, questions of gamble aggregation assume that each gamble is independent. That is, the gambles are uncorrelated. As such, aggregation of a portfolio already assumes that the portfolio is somewhat diversified (or at least that the gambles aren’t perfectly correlated).

In the case of the similarity effect, the choice bracketing did not seem to encourage aggregation, but instead appears to have encouraged a naive diversification

## 2. Effect of Choice Bracketing on Risk Aggregation

2153 (Hedesstrom et al., 2006; Read & Loewenstein, 1995). It could not have been ac-  
2154 tual diversification, because the projects did not contain correlational information.  
2155 Rather, participants could have been more eager to accept the project portfolio  
2156 due to the higher variability between projects (due to the similarity manipulation).

2157 This finding suggests that there may be trade-off between aggregation and  
2158 diversification. The literature shows that people prefer multi-play gambles to  
2159 single-play gambles. However, participants in this chapter were more likely to  
2160 aggregate diverse repeated-play gambles to similar repeated-play gambles when  
2161 these were bracketed broadly. Therefore, people are likely to still need choice  
2162 bracketing. That is, diverse repeated-play gambles that are not bracketed are  
2163 simply individual single-play gambles.

2164 One way to test this explanation is by using identical gambles. This chapter  
2165 used unique gambles to increase ecological validity. However, the above explana-  
2166 tion would predict that participants prefer non-identical repeated-play gambles to  
2167 identical repeated-play gambles when these are bracketed. However, when these  
2168 gambles are not presented as a portfolio, it is likely that the identical gambles  
2169 would be preferred overall because the non-identical gambles would be represented  
2170 as individual single-play gambles.

2171 It is also possible that similarity effects were not seen because the sequence  
2172 of gambles itself led to naive aggregation for all conditions. One way that this  
2173 could be tested is by interweaving other tasks in-between the gambles to break  
2174 them up. Then similarity may play a role by allowing bracketing across otherwise  
2175 distinct gambles. Multiple sets of gambles can be interwoven with similarity  
2176 alone creating the potential sets. The prediction is that without similarity the  
2177 gambles would not be aggregated.

### 2178 2.4.1.1 How Does Choice Bracketing Facilitate Aggregation?

2179 Much of the literature (e.g., Benartzi & Thaler, 1999) is not clear about why  
2180 choice bracketing occurs. Some explain the effect of bracketing on aggregation

## 2. Effect of Choice Bracketing on Risk Aggregation

2181 using risk aversion (e.g., Read et al., 1999), while others refer to the increased  
2182 weighting of potential losses (Webb & Shu, 2017).

2183 Decision-from-experience *sampling* studies explain the underweighting of rare  
2184 events (as opposed to the overweighting that occurs with decisions-from-description)  
2185 by sampling bias and recency effects (e.g., Hertwig et al., 2004; Wulff et al., 2018).  
2186 That is, they explain that people are less risk averse for positive EV gambles  
2187 because when they sample from the distribution they only sample a small amount  
2188 (usually approximately 20 times) so they do not experience rare events very often.  
2189 Also, the latter half of the sequence of sampling is significantly more predictive  
2190 than the former (recency effect). Some decision-from-experience *feedback* studies  
2191 explain this effect by “choice inertia” (Camilleri & Newell, 2011). That is, “the  
2192 tendency to repeat the last choice, irrespective of the obtained outcome” (p. 383).  
2193 However, there is not much more elaboration beyond this. Repeated-play gambles  
2194 show more underweighting than multi-play gambles. This is said to be due to a  
2195 “reliance on a very small set of samples” (Camilleri & Newell, 2013, p. 64). However,  
2196 this explanation does not account for repeated-play effects independently.

2197 The experiments in this chapter shed some light about the mechanisms behind  
2198 why choice bracketing may affect risk aggregation in repeated-play gambles without  
2199 feedback. Two explanations were proposed: participants may realise that some  
2200 gains will offset the losses, or they may need explicit aggregation. Not finding  
2201 evidence for the subtle choice bracketing manipulations suggests that people do  
2202 not intuitively consider that the gains of their choices may offset the potential  
2203 losses. Perhaps the possibility of recouped losses would become more salient when  
2204 other participants are explicitly told of this possibility, as in Sokol-Hessner et al.  
2205 (2009). Their explicit instruction manipulation is introduced above as appearing  
2206 unrealistically strong, but the results of this chapter suggest that people do need  
2207 very explicit scaffolding in order to use risk aggregation.

## 2. Effect of Choice Bracketing on Risk Aggregation

### 2208 2.4.2 Practical Implications

2209 This research implies some prescriptions for capital allocation decision-making.  
2210 For instance, even if managers implement processes that encourage a joint evalua-  
2211 tion of projects, this may be insufficient to encourage aggregation. Projects need  
2212 to very explicitly be considered as individual components in a portfolio in order  
2213 to facilitate better risk aggregation. Some companies are already implementing  
2214 processes that make this more explicit (Lovallo et al., 2020). This is especially  
2215 important for those that would still have to evaluate projects separately. Further,  
2216 this work shows the importance of being explicit about the forecasted probabilities  
2217 of project success. Doing this is necessary for the aggregation process. Even more  
2218 ideal would be to forecast project success using an entire probability distribution for  
2219 the different possible outcomes. However, research shows that people struggle to  
2220 construct such distributions (e.g., Alpert & Raiffa, 1982; Schaefer & Borcherding,  
2221 1973; Tversky & Kahneman, 1974; von Holstein, 1971) and Chapter 4 shows that  
2222 people struggle to use such variance information when making allocation decisions.  
2223 Regardless, the benefits of risk aggregation can be used even if forecast information  
2224 is limited (e.g., only a point estimate and a probability) and only one project is  
2225 being considered. Specifically, a proposed project can be seen in a larger context  
2226 by aggregating it with projects from the immediate past.

2227 Interestingly, participants were less risk averse about a portfolio of projects  
2228 when industries differed, compared to when they were all from the same industry.  
2229 Simply manipulating the similarity of financially-irrelevant semantics of a set of  
2230 choices affected participants' risk aversion. This has implications for managerial  
2231 settings. Executives in multi-business firms often have to make capital allocation  
2232 decisions that involve comparing dissimilar projects. How can an oil well explo-  
2233 ration project be appropriately compared to an oil refinery? Or to a microchip  
2234 project? Chapter 4 suggests that evaluating dissimilar business projects is more  
2235 difficult to comparing similar projects. The current work suggests that managers  
2236 may actually be *less* likely to realise the benefits of aggregation when they are in a

## *2. Effect of Choice Bracketing on Risk Aggregation*

2237 less diversified company. As such, managers should complement an understanding  
2238 of aggregation with that of diversification. This might help to avoid being biased  
2239 by a lack of variety of projects despite a potentially high level of diversification.

### 2240 **2.4.3 Future Research**

2241 The main novelty of the experiments in this chapter comes from increasing  
2242 ecological validity of risky choice problems by removing inter-trial feedback. Fu-  
2243 ture work should test even more realistic scenarios. Such studies should involve  
2244 managers, ideally in multi-business firms. Investigating whether the choice brack-  
2245 eting findings from these experiments replicates in a sample of managers will  
2246 help to determine whether these results could be applied to real-world managerial  
2247 decision-making. This is especially important since Haigh and List (2005) found  
2248 that professional traders show more myopic loss aversion than students. Further,  
2249 the similarity, awareness, and presentation manipulations should be tested with  
2250 managers since it is possible that they have a greater sense of naive aggregation  
2251 and are therefore more likely to be more amenable to such manipulations. The  
2252 addition of extra payment for better performance on the task might also assist in  
2253 making the task more isomorphic with real-world managerial decisions. Further,  
2254 in the present experiments, participants viewed the projects all in the space of one  
2255 session. However, this is not completely isomorphic to real life, where managers  
2256 make many other decisions that are unrelated to the large risky investments at their  
2257 companies. Future research should test participants over a longer period of time  
2258 (as in Beshears et al., 2016) in order to see whether the effects of the manipulations  
2259 replicate in a more realistic environment.

## 2. Effect of Choice Bracketing on Risk Aggregation

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# 3

2460

2461

## Joint Evaluation of Multiple Projects

2462 Chapter 2 found that people struggle to aggregate risk even when provided  
2463 with choice bracketing cues that could have built on an intuitive sense of how  
2464 aggregation reduces risk. The finding that people are more likely to accept many  
2465 gambles at once (e.g., Samuelson, 1963; Wedell & Bockenholt, 1994), even without  
2466 any aids to calculate risk, suggests that people can gain an intuition for the benefits  
2467 of aggregation. Yet, in the current work, people instead considered projects one  
2468 at a time and only leveraged the benefits of aggregation when given an explicit  
2469 visualisation of what it entails.

2470 This shows that it is important to change organisational policy to encourage  
2471 considering business projects jointly. Doing this means that the risk can be concur-  
2472 rently aggregated. In real-life capital allocation scenarios, when managers evaluate  
2473 projects sequentially, an aggregated distribution can also be presented using any  
2474 number of projects that were considered in the recent past. This means that a strat-  
2475 egy of project risk aggregation can be implemented at any stage in an organisation's  
2476 lifespan. Relatively new ventures can implement these recommendations by waiting  
2477 until a certain number of project proposals have been accrued before aggregating.

2478 Considering projects jointly is also useful for accountability purposes. The usual  
2479 incentive structure in organisations that judges each project outcome independently

### *3. Joint Evaluation of Multiple Projects*

2480 is likely to punish risk-taking due to its potential negative consequences and not  
2481 due to the information that was available at the time of evaluation. Framing  
2482 a set of projects as a portfolio means that any subsequent success or failure of  
2483 one project can be traced back to the entire batch, and the performance of the  
2484 whole portfolio can be evaluated.

2485 Business projects might not always be either accepted or rejected, as in Chap-  
2486 ter 2. Instead, top-level managers might ask for project proposals from lower-level  
2487 managers, and then allocate funds from the available budget. An organisation  
2488 might also have a initial “culling” phase, and a subsequent ranking phase. When  
2489 initially considering a set of projects, some might be rejected according to certain  
2490 rules. For instance, an NPV might not meet a certain minimum cut-off. The  
2491 remaining projects in the set can then be ranked in order of priority and receive  
2492 an allocation of capital from the budget.

2493 A few potential problems arise at the point that projects are considered jointly  
2494 for ranking and allocation. For instance, it might not be easy to compare between  
2495 the projects in the set. As discussed in Chapter 1, diversification of business units  
2496 has become very popular in large organisations. Therefore, most hierarchical  
2497 organisations are likely to face difficult comparisons when deciding on how to  
2498 rank and allocate capital to projects that originated in different divisions. A non-  
2499 hierarchical organisation that develops one type of product may be able to simply  
2500 compare across any number of intrinsic project attributes, whereas a diversified  
2501 organisation is likely to have to rely on more abstract financial metrics, such as NPV.  
2502 Such metrics are “abstract” because they can be applied in almost any domain.

2503 For instance, when comparing across two oil well projects, there can be both  
2504 attributes intrinsic to the project, such as the amount of hydrocarbons that are ex-  
2505 tracted per hour, and also the more abstract financial metrics. There is a potential  
2506 interaction between the ease that managers have to compare across the projects and  
2507 the kinds of measures that are used to make the comparison. Two similar projects,  
2508 such as two oil wells can be evaluated using litres of hydrocarbons extracted per  
2509 hour, whereas an oil refinery cannot. In the case that two dissimilar projects are

### *3. Joint Evaluation of Multiple Projects*

2510 compared, managers can use financial metrics to compare across domains. This  
2511 can lead to comparable accuracy as long as the abstract metrics are as reliable  
2512 as the intrinsic project features.

2513 A concern that arises out of a reliance on such metrics is that underlying  
2514 variance is not taken into account. Forecast estimates such as NPV rely on many  
2515 assumptions and contain much inherent uncertainty, so managers that use them  
2516 should be cautious about over-relying on them. Chapter 4 tests people's sensitivity  
2517 to forecast estimate variance information. That is, will people use NPV more  
2518 when the variance information suggests that it is a reliable measure, than when  
2519 the information suggests that it is unreliable?

2520 Chapter 2 manipulated project presentation and found no significant difference  
2521 between when projects were considered jointly or separately. This was explained  
2522 by the bounds on people's ability to intuitively aggregate. However, it was unclear  
2523 what components of the projects people focused on both because they were not  
2524 explicitly manipulated and because the task involved a binary choice (accept or  
2525 reject). A relative allocation measure for multiple projects with systematically  
2526 varied attributes would allow to determine the influence of those different attributes.  
2527 Therefore, Chapter 4 considers the situation in which people are already presented  
2528 with choices together and asked to evaluate the projects by allocating a hypo-  
2529 thetical budget.

2530 Further, Chapter 4 identifies the factors that affect people's decisions indepen-  
2531 dently from the potential risk of losing hypothetical money, which is a large reason  
2532 for the effects in the previous chapter. Risk aversion is accounted for by making  
2533 it clear that no losses are possible. This is achieved by using only positive NPVs,  
2534 which implies that the project is not forecasted to lose money.

2535 Chapter 4 also manipulates how easy the project attributes are to compare.  
2536 This helps identify the ways that decision-making in a diversified organisation may  
2537 be different to that of a more integrated organisation. Chapter 2 manipulated  
2538 similarity by either showing a set of projects from the same industry or a set  
2539 from different industries. This was meant to simulate an integrated and diversified

### *3. Joint Evaluation of Multiple Projects*

2540 firm, respectively. This manipulation was not as strong because there were no  
2541 project attributes that could be aligned or not. That is, there was nothing actually  
2542 non-alignable. This may explain the equivocal similarity effect. In Chapter 4,  
2543 alignability is more fully manipulated by having project attributes be critical to  
2544 the evaluation. These project features are shown explicitly so that the difficulty  
2545 of the comparison is more obvious.

*It is not possible to compare apples and oranges. But it is possible to compare apples and oranges in terms of some specific attribute—to say that apples deliver twice as many calories per dollar or that oranges deliver twice as many vitamin C units per dollar.*

—C. L. Robinson (1944, p. 13)

# 4

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## Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

2549

### Contents

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2551

4.1	Introduction	74
4.1.1	Experiment Summary	77
4.2	Experiment 1	78
4.2.1	Method	81
4.2.2	Results	84
4.2.3	Discussion	86
4.3	Experiment 2	87
4.3.1	Method	88
4.3.2	Results	89
4.3.3	Discussion	92
4.4	Experiment 3	94
4.4.1	Method	95
4.4.2	Results	98
4.4.3	Discussion	100
4.5	General Discussion	102
	References	105

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### 4.1 Introduction

2570

One of the most important tasks faced by executives is the allocation of capital within their companies. This requires the ranking of projects by importance

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

and predicted success, and allocating the limited capital accordingly (not unlike a scientific funding agency). Ranking of projects necessitates comparing them across a number of dimensions. For example, the executive of an oil company may have received multiple oil exploration proposals. Determining what makes one oil exploration project better than another is relatively simple. However, consider a different scenario in which the executive must allocate capital between an oil exploration project and an oil refinery project. The dimensions of oil refinery projects that distinguish superior from inferior projects may be totally different from those of oil exploration projects. Consider a funding agency having to decide between two cognitive scientists or between a cognitive scientist and a physicist in awarding a fellowship. What makes a physics proposal better for the field of physics than a cognitive science proposal for cognitive science?

Structure-mapping theory (Gentner, 1983; Gentner & Markman, 1997) provides a model of comparison that psychologically distinguishes these two kinds of allocation tasks. This framework models comparison as a process of mapping and alignment of the shared dimensions of two conceptual structures. This mapping process reveals the shared dimensions of the two structures as well as the differences in those shared dimensions (known as *alignable differences*). For example, when comparing two oil exploration projects, the process for measuring the quantity of hydrocarbons in a prospective oil field may be identical, but the specific quantities measured will differ. This is known as an alignable difference; that is, the difference constrained within the same dimension. However, when comparing an oil field and a refinery, there will be a significantly higher number of *non-alignable differences*, because the two domains do not share component dimensions. That is, the dimensional structure of processes in the exploration project will be substantially different from that of processes in the refinery project, making it difficult to find meaningful alignments. With a higher number of non-alignable differences, there are fewer opportunities to make meaningful comparisons, leading to greater difficulty in predicting project success and ranking projects. This chapter experimentally examined project comparisons and how such comparisons may affect

#### *4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation*

2603 capital allocation decisions. The working hypothesis is that projects that have a  
2604 higher number of alignable differences will lead to more precise and informed project  
2605 predictions and rankings compared with projects with non-alignable differences.

2606 However, what happens when a task demands that two domains be aligned  
2607 but they are too disparate to align? Experimentally, this territory is somewhat  
2608 uncharted. It is expected that, when required, people will grasp at any piece of  
2609 information available and attempt to abstract and infer that which is reasonable to  
2610 ease the alignment. This occurs frequently in business settings. Because corporate  
2611 enterprises continue to embrace diversification strategies in their investments, they  
2612 must constantly make capital allocation decisions involving highly disparate do-  
2613 mains. To overcome these difficult comparisons, executives rely on various financial  
2614 measures that, in theory, may be applied to any project or business proposal. These  
2615 financial measures work well to ease the burden of difficult comparisons because  
2616 they ignore the complexities of individual projects and focus solely on financial  
2617 information such as total cost and projected profits. Therefore, projects that  
2618 are difficult to compare may be evaluated more easily by comparing individual  
2619 numerical measures.

2620 The most common financial measure that is used by executives in order to  
2621 value business project proposals is NPV (Graham & Harvey, 2001; Graham et al.,  
2622 2015; Remer et al., 1993). NPV is the difference between the forecasted revenue of a  
2623 project and the initial investment in its development (accounting for the time value  
2624 of money), as shown in Equation (1.1). NPV is commonly used in decisions about  
2625 capital allocation and investment. The basic rule is that if a project has a positive  
2626 NPV, it is financially viable, and if it has a negative NPV, it is not. However,  
2627 the use of NPV has been criticised, by both academics and practitioners (Fox,  
2628 2008; Willigers et al., 2017). The main criticism is that there can be underlying  
2629 sources of variance in NPV that are not reflected in the final measure, which is  
2630 expressed as a single numerical value. For instance, NPV is dependent on the  
2631 projected cash inflows for each year of the project. However, financial forecasting  
2632 is frequently inaccurate and prone to optimism bias (Lovallo & Kahneman, 2003;

#### *4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation*

2633 Puri & Robinson, 2007). Therefore, there is bound to be variation in the reliability  
2634 of NPV measures as a function of the forecasting error in the cash flow calculations.  
2635 Project duration and the discount rate are further sources of variance that may be  
2636 hidden by the single numerical value of NPV.

2637 The secondary goal of this research is to investigate the extent to which people  
2638 are sensitive to variance information (from financial forecasting) when making  
2639 capital allocation decisions. This consideration is especially important in the capital  
2640 allocation situations illustrated above, when executives need to compare projects  
2641 with disparate domains and must, therefore, rely on NPV. This matters because  
2642 the NPV of different domains may have different underlying forecasting error,  
2643 potentially compromising the utility of using NPV as the basis of comparison. Do  
2644 executives sufficiently account for the inherent sources of variance in the measure on  
2645 which they rely so heavily? Research shows that people are effective at extracting  
2646 variance information when exposed to numerical sequences (Rosenbaum et al.,  
2647 2020). However, they struggle to use variance information when it is represented  
2648 numerically (Batteux et al., 2020; Galesic & Garcia-Retamero, 2010; Konold et al.,  
2649 1993; Vivaldi & Coville, 2021).

##### **2650 4.1.1 Experiment Summary**

2651 Experiment 1 investigated the effect of project alignment on the decision-making  
2652 of naive participants asked to allocate capital to a set of fictional projects. Naive  
2653 participants were assumed to have no requisite knowledge about NPV reliability;  
2654 thus, NPV reliability level was manipulated by directly telling participants whether  
2655 or not the given NPV was reliable. For this experiment, it was predicted that when  
2656 projects are alignable, participants who are told NPV is reliable would use it in their  
2657 decision-making, while participants who are told that NPV was unreliable would  
2658 not use it in their decision-making. However, when projects are not alignable, it  
2659 was predicted that participants would use NPV, regardless of the stated NPV  
2660 reliability level.

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

2661 Experiment 2 investigated the decision-making of management students in a  
2662 similar situation to Experiment 1. The main difference was that instead of telling  
2663 participants whether or not the NPV was reliable, the level of *numerical* NPV—  
2664 reliability—that is, the width of the numerical range around the average NPV—  
2665 was manipulated. Similar to Experiment 1, it was predicted that participants  
2666 would rely more on NPV in non-alignable projects than in alignable projects.  
2667 However, it was predicted that numerical reliability level would have no effect  
2668 because there is little evidence that people are sensitive to variance information  
2669 when it is shown numerically.

2670 Experiment 3 also tested the effects of project alignment and reliability level in  
2671 a non-business population but manipulated both verbal and numerical reliability  
2672 to enable a direct comparison. The term *reliability level* is used to describe the  
2673 manipulation of whether NPV was expressed as a reliable measure or not, while  
2674 *reliability type* is used to describe the manipulation of whether reliability was ex-  
2675 pressed verbally or numerically. Experiment 3 predicted a reliability level effect for  
2676 the verbal reliability condition but not the numerical reliability condition. Further,  
2677 this experiment used project descriptions with clearer profitability indicators and  
2678 added a larger selection of business industries.

## 2679 4.2 Experiment 1

2680 Experiment 1 investigated the effects of project alignment and explicit NPV  
2681 reliability information on capital allocation decisions. The structural alignment  
2682 literature suggests that people place more weight on alignable differences than  
2683 they do on non-alignable differences. It was expected that participants would rely  
2684 more on NPV than on other product attributes in their decision-making because  
2685 NPV may be applied to every product. However, this effect should vary with  
2686 participants' perceived NPV reliability level. That is, if other project dimensions  
2687 are alignable, the use of NPV may depend on its reliability. However, it was  
2688 predicted that in projects with low alignment, there will be a greater reliance on

#### *4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation*

2689 NPV as the sole alignable difference, regardless of its stated reliability. These  
2690 effects were measured by considering the linear relationship between NPV and the  
2691 money allocated to each project. Critically, the NPV and intrinsic features of  
2692 each project shown to participants were inversely related. Therefore, a positive  
2693 NPV trend will indicate a heavier reliance on NPV, whereas a negative trend will  
2694 indicate a heavier reliance on the intrinsic project features. First, Experiment 1  
2695 tested the following omnibus hypothesis:

2696 **Hypothesis 4.1—overall effect.** The alignment  $\times$  reliability level  $\times$  NPV inter-  
2697 action is significant.

2698 Initially, specific effects were tested by excluding the no NPV condition (in  
2699 which participants were not given NPV information). Given the difficulty of  
2700 comparing dissimilar projects, participants were expected to rely more heavily  
2701 on NPV when project attributes are not alignable compared with when they are  
2702 alignable. Therefore, Experiment 1 tested the following hypothesis:

2703 **Hypothesis 4.2—alignment effect.** The linear NPV trend will be stronger for  
2704 projects with low alignment than for projects with high alignment.

2705 Participants' budget allocations were expected to depend on the provided NPV  
2706 reliability information. However, this is more likely when there are multiple aligned  
2707 metrics from which to choose compared with when NPV only is alignable. There-  
2708 fore, Experiment 1 tested the following hypothesis:

2709 **Hypothesis 4.3—the NPV reliability level effect depends on alignment.**  
2710 The NPV  $\times$  reliability level interaction will be stronger in the high alignment than  
2711 in low alignment.

2712 Specifically, when projects are similar, it is expected that participants will rely  
2713 more on NPV if they are told that NPV is reliable (leading to a positive NPV trend)  
2714 but more on the intrinsic features of projects if they are told that NPV is unreliable  
2715 (leading to a negative NPV trend). However, when projects are dissimilar, it is

#### *4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation*

2716 expected that participants will rely solely on NPV, regardless of what they are  
2717 told about its reliability. Therefore, Experiment 1 tested the following hypotheses:

2718 **Hypothesis 4.4—NPV reliability level in high alignment.** When projects  
2719 have high alignment, the NPV trend will be stronger when NPV reliability is high  
2720 compared with when NPV reliability is low.

2721 **Hypothesis 4.5—NPV reliability level in low alignment.** When projects  
2722 have low alignment, the NPV trend will not differ significantly between the two  
2723 reliability level conditions.

2724 A no NPV condition was used to gain a better understanding of participants'  
2725 baseline response to materials when they had no information about NPV. The  
2726 extent of participants' reliance on NPV was determined by comparing this no  
2727 NPV condition to the conditions in which NPV was present. When projects are  
2728 similar, this condition was expected to be equivalent to the low NPV reliability  
2729 condition because in this condition participants should disregard NPV. When  
2730 projects are dissimilar, this condition was expected to show the average participant  
2731 value judgements of the project descriptions, because they only had the intrinsic  
2732 project features for their evaluations. This was expected to result in a flat NPV  
2733 trend. Therefore, Experiment 1 tested the following hypotheses:

2734 **Hypothesis 4.6—effect of NPV information for projects with high align-  
2735 ment.** For projects with high alignment, the positive NPV trend will be stronger  
2736 for projects with high NPV reliability compared with projects with no NPV infor-  
2737 mation.

2738 **Hypothesis 4.7—effect of NPV information for projects with low align-  
2739 ment.** For projects with low alignment, the positive NPV trend will be stronger  
2740 for projects with both low and high NPV reliability compared with projects with  
2741 no NPV information.

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

**Table 4.1:** Experiment 1 group allocation.

Project alignment	Reliability level of net present value (NPV)	N
High	High	26
High	Low	17
High	No NPV	17
Low	High	21
Low	Low	16
Low	No NPV	21
Total		118

#### 4.2.1 Method

##### 4.2.1.1 Participants

One hundred and eighteen participants (55 female) were recruited from the online recruitment platform Prolific. Participants were compensated at a rate of £5 an hour (Prolific is based in the UK). The average age was 29.42 years ( $SD = 9.25$ ,  $min. = 18$ ,  $max. = 73$ ). Table 4.1 shows the allocation of participants to the different conditions. NPV was varied within subjects.

##### 4.2.1.2 Materials

**4.2.1.2.1 Instructions** Participants, who did not necessarily have business experience, were first shown an instructions page with information about the task and NPV. These instructions also informed participants about whether NPV as a financial measure was reliable or unreliable for the specific project. Participants in the low NPV reliability level conditions were told that NPV was an unreliable metric, while those in the high NPV reliability level conditions were told that NPV was a reliable metric. Instructions provided to participants in the no NPV condition did not include an explanation of NPV or its reliability. Critically, participants were asked to invest in products with a high objective value (because a higher-quality product is not always better in the consumer goods market). Given that participants may not use this instruction when directly viewing the projects, Experiment 3 used projects whose attributes inherently expressed their quality. Appendix B.1.1.1 shows the instructions used in Experiment 1.

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

	Project 1	Project 2	Project 3	Project 4	Project 5
Product	Laptop	Laptop	Laptop	Laptop	Laptop
RAM (GB)	4	8	32	2	16
Hard drive (GB)	500	750	2000	250	1000
Resolution (px)	900	1080	1440	768	1200
Processor (Ghz)	2.4	3.2	3.8	1.6	3.6
NPV (\$)	663	495	70	887	252

**Figure 4.1:** An example of a high alignment display in Experiment 1.

2763    **4.2.1.2.2 Project Display** Participants were provided with a set of fictional  
 2764 business projects to which they were asked to allocate capital. Alignment manipula-  
 2765 tion was reinforced through visual presentation. Projects with high alignment were  
 2766 displayed in a table listing their various attributes (see Figure 4.1). In this group,  
 2767 each project involved the same product type with consistent concrete attributes.  
 2768 The table format was more appropriate for the high alignment condition because all  
 2769 dimensions were shared. In contrast, projects with low alignment were presented as  
 2770 paragraphs describing their relevant attributes (see Figure 4.2). In this group, each  
 2771 project was a different product with concrete attributes specific to that product. In  
 2772 both alignment conditions, each project description included an NPV. Critically,  
 2773 the values of the concrete attributes were always in conflict with the NPV. For  
 2774 instance, Project 4 always had the lowest value for each concrete attribute but  
 2775 always had the highest NPV. This meant that participants' allocations could be  
 2776 used as a proxy for their degree of dependence on NPV.

2777    Presentation style was potentially a confounding factor. This was addressed in  
 2778 Experiment 3 by using the table format for both alignment conditions.

2779    **4.2.1.2.3 Allocation** Participants completed a capital allocation task (see Fig-  
 2780 ure 4.3) adapted from Bardolet et al. (2011) in which they were asked to allocate

#### *4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation*

##### PROJECT 1

This project is about developing a new shampoo. It will have a 400 mL capacity, and the patented Dandruff Reduction Factor was 17 at testing. The fragrance was optimally effective for 3 metres, and the Safety Authority gave it a 81% safety rating. The NPV is estimated to be \$685.

##### PROJECT 2

This project is about developing a new laundry machine. The machine will have a 12-star energy rating and an 8L capacity. The maximum speed rate is 900 rpm and it will have six different cycle programs. The NPV is estimated to be \$500.

##### PROJECT 3

This project is about developing a new mountain bike. It will have a tensile strength of 910 megapascals, and a suspension for travel of 200mm. It will have a 12-speed cassette and is guaranteed for at least three tours. The NPV is estimated to be \$81.

##### PROJECT 4

This project is about developing a new laptop computer. It will have 2GB of RAM and a hard drive with 250GB capacity. The resolution will be 768px, and the processor speed will be 1.6 Ghz. The NPV is estimated to be \$894.

##### PROJECT 5

This project is about developing a new backpack. It will have eight separate compartments for different types of storage, and a total capacity of 30L. The company will offer a four-year warranty, and the material is an 800-denier nylon fabric. The NPV is estimated to be \$251.

**Figure 4.2:** An example of a low alignment display in Experiment 1.

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

How will you allocate your budget across these five projects (percentages must sum to 100%)?

	0	10	20	30	40	50	60	70	80	90	100
PROJECT 1	<input type="text"/>										0
PROJECT 2	<input type="text"/>										0
PROJECT 3	<input type="text"/>										0
PROJECT 4	<input type="text"/>										0
PROJECT 5	<input type="text"/>										0
Total:											0

**Figure 4.3:** The allocation task.

2781 a hypothetical yearly budget across the given five projects.

2782 **4.2.1.2.4 Additional Measures** Other measures apart from allocation were  
2783 included. The stimuli for and analyses of these measures are reported in Ap-  
2784 pendices B.1.1.1 and B.1.2, respectively. Specifically, participants were asked to  
2785 forecast the future returns of the projects (see Figure B.4), rank the projects (see  
2786 Figure B.5), indicate their confidence in their decisions (see Figure B.6), and justify  
2787 their decisions (see Figure B.7).

2788 **4.2.1.3 Procedure**

2789 After reading the relevant instruction page, participants allocated to the low  
2790 alignment conditions completed the forecasting task directly beneath each project  
2791 display. For the high alignment conditions, this was done directly beneath all  
2792 projects. Participants were then asked to rank the projects and subsequently answer  
2793 the allocation, confidence, and justification questions.

2794 **4.2.2 Results**

2795 A mixed factorial ANOVA was conducted to investigate the effects of project  
2796 alignment and NPV reliability level on participants' budget allocations. As shown

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

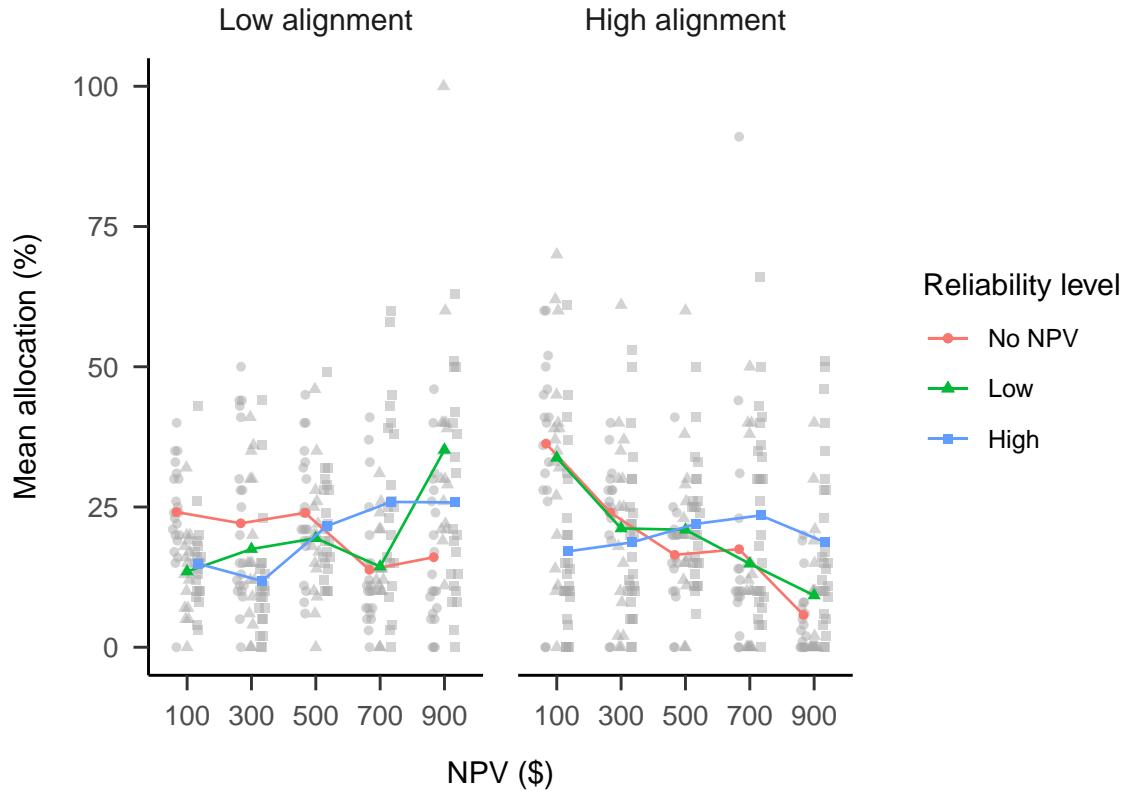
in Figure 4.4, the alignment  $\times$  NPV reliability level  $\times$  NPV interaction was significant,  $F(6.57, 367.76) = 2.18, p = .039, \eta_p^2 = .038$ . The analyses excluding the no NPV condition showed the expected results. The NPV trend averaged across both reliability level conditions was stronger for the low alignment conditions than for the high alignment conditions,  $M = 61.70, 95\% \text{ CI } [33.02, 90.37], t(76) = 4.29, p < .001$ . This shows that people relied more on NPV when projects were dissimilar than when they were similar.

Further, the NPV  $\times$  NPV reliability level interaction was stronger in the high alignment conditions than in the low alignment conditions,  $M = 67.81, 95\% \text{ CI } [10.47, 125.16], t(76) = 2.36, p = .021$ . Specifically, in the high alignment conditions, the NPV trend was stronger in the high NPV reliability condition than in the low NPV reliability condition,  $M = -63.47, 95\% \text{ CI } [-100.00, -26.94], t(112) = -3.44, p = .001$ . In the low alignment conditions, there was no significant difference between the two reliability conditions,  $M = 4.35, 95\% \text{ CI } [-34.52, 43.21], t(112) = 0.22, p = .825$ . This shows that participants only used the NPV reliability information in their allocation decisions when projects were similar, not when they were dissimilar.

The comparison with the no NPV condition revealed the expected pattern. For the high alignment group, the linear NPV trend was significantly weaker in the no NPV condition than in the high NPV reliability condition,  $M = 75.70, 95\% \text{ CI } [39.17, 112.24], t(112) = 4.11, p < .001$ , but not the low NPV reliability condition,  $M = 12.24, 95\% \text{ CI } [-27.94, 52.41], t(112) = 0.60, p = .547$ . However, in the low alignment group, the linear NPV trend was significantly weaker for the no NPV condition compared with both the low NPV reliability condition,  $M = 64.63, 95\% \text{ CI } [25.76, 103.50], t(112) = 3.29, p = .001$ , and the high NPV reliability condition,  $M = 60.29, 95\% \text{ CI } [24.14, 96.43], t(112) = 3.30, p = .001$ .

The mean ranking, confidence, and forecast data were all largely congruent with the allocation findings (see Appendix B.1.2). The results also show that the forecasts of those in the low alignment condition had higher standard deviations

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation



**Figure 4.4:** Mean allocation across NPV, by project alignment and reliability level conditions. In mixed factorial designs, error bars cannot be used to make inferences by “eye” across all conditions. Therefore, error bars are not included. Raw data are plotted in the background. When interpreting this figure, consider the linear trends in NPV.

than those in the high alignment condition (see Appendix B.1.2.4). However, this was not replicated in subsequent experiments (see Appendices B.5.2.2 and B.6.2.2).

### 4.2.3 Discussion

Experiment 1 found evidence for the effect of project alignment on laypeople’s decision-making in capital allocation scenarios. Specifically, when projects were comparable, participants used NPV when they were told that it was reliable, but did not when they were told that it was unreliable. However, they used NPV regardless of its reliability when it was the only shared dimension across products.

Experiment 1 manipulated *verbal* NPV reliability. That is, participants were explicitly told whether NPV was considered to be a reliable metric or not. However, in the real-world the reliability of a metric is more commonly expressed in numerical form, such as a range around an estimate. Experiment 2 attempted to replicate the

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

alignment effects, while manipulating the *numerical* NPV reliability associated with each project, rather than the verbal reliability as used in Experiment 1. Further, people with sufficient experience with financial theory and analysis may be able to successfully draw inferences from such information. Therefore, Experiment 2 used a sample of students enrolled in a Master of Management degree, instead of the laypeople used in Experiment 1.

### 4.3 Experiment 2

Experiment 2 investigated the effects of project alignment and numerically-expressed NPV reliability information on capital allocation decisions. In Experiment 1, the information about NPV reliability level was communicated explicitly (e.g., “NPV is unreliable”). However, in Experiment 2, only the actual NPV information itself was communicated without the conclusion about its reliability. Specifically, participants were given a range of predicted values (akin to a confidence interval). Therefore, while Experiment 1 manipulated *verbal* NPV reliability, Experiment 2 manipulated *numerical* NPV reliability. Further, Experiment 2 included participants with more business experience. This experiment tested whether the previous findings of an alignment effect would be replicated using participants with more business experience. The experiment also tested whether this population is sensitive to variance in forecasts.

Hypothesis 4.2 was again tested to investigate the alignment effect in the new sample. However, the other hypotheses tested in Experiment 1 were not retested because Experiment 2 manipulated numerical reliability and did not include a no NPV condition. Research has shown that people are poor at reasoning with numerical variance information (Batteux et al., 2020; Galesic & Garcia-Retamero, 2010; Konold et al., 1993; Vivaldi & Coville, 2021). Therefore, Experiment 2 tested the following hypothesis:

**Hypothesis 4.8—no effect of numerical reliability.** The NPV  $\times$  reliability level interaction is not significant in either alignment condition.

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

2866 Experiment 2 also investigated the ability to quickly change participants' understanding, if they did not initially use numerical NPV reliability in their allocations.  
2867 Therefore, participants were presented with a short lecture on the importance of  
2868 paying attention to variance in financial decision-making. However, this lecture was  
2869 not sufficient to inform participants' use of numerical reliability (see Appendix B.2).  
2870 Further, Experiment 2 investigated whether participants would be over-confident  
2871 in their understanding of NPV (as in Long et al., 2018). These results are also  
2872 reported in Appendix B.2 because they are not highly relevant to this chapter.  
2873

##### 2874 4.3.1 Method

###### 2875 4.3.1.1 Participants

2876 Fifty-four participants (28 female) were recruited from a Master of Management  
2877 degree at an Australian university. Age information was not recorded. Both the  
2878 reliability level (low and high) and project alignment (low and high) conditions  
2879 were presented to subjects, and the order of presentation was counterbalanced.

###### 2880 4.3.1.2 Materials

2881 4.3.1.2.1 Instructions Participants were shown similar instructions to those  
2882 used in Experiment 1 (see Section 4.2.1.2.1). However, they were given more NPV  
2883 information (including discount rate and initial investment). Appendix B.2.1.1.1  
2884 shows the full instructions.

2885 4.3.1.2.2 NPV Test Participants were asked to complete a short, simple test  
2886 to check their understanding of NPV (see Appendix B.2.1.1.2).

2887 4.3.1.2.3 Project Display As shown in Figures 4.5 and 4.6, projects were  
2888 displayed as they were in Experiment 1. However, a second set of projects with  
2889 different product types and descriptions was added to enable within-subjects manip-  
2890 ulation. Along with the single numerical NPV, participants were provided with the  
2891 forecasted cash flow ranges used to calculate the NPV. In the low NPV reliability  
2892 condition, ranges were  $\pm 85\%$  around the mean (e.g., \$150–\$1,850 if forecast mean

#### *4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation*

2893 was \$1,000); while in the high NPV reliability condition, ranges were  $\pm 5\%$  around  
2894 the mean (e.g., \$950–\$1,050 if the forecast mean was \$1,000). A wide range  
2895 indicated that the measure had low reliability, while a narrow range indicated  
2896 that the measure had high reliability. Participants were told to treat each set  
2897 of projects independently.

2898 **4.3.1.2.4 NPV Knowledge Ratings** Participants were asked to rate their  
2899 confidence in knowledge of NPV at multiple points in the experiment. Appendix B.2.1.1.3  
2900 shows an example of this display.

2901 **4.3.1.2.5 Variance Lecture** Participants were given a short lecture on the im-  
2902 portance of paying attention to variance information in an attempt to increase their  
2903 use of numerical reliability information in their allocations (see Appendix B.2.1.1.4  
2904 for more details and the lecture slides).

2905 **4.3.1.3 Procedure**

2906 Participants were provided with the instructions and an explanation of NPV  
2907 before completing a simple test to demonstrate their understanding of NPV. They  
2908 then completed four counterbalanced capital allocation trials (one for each condi-  
2909 tion combination) before viewing a brief presentation on the importance of paying  
2910 attention to variance in financial decision-making. Participants then repeated two  
2911 of the trials that they had completed earlier. They were shown the allocation values  
2912 they had provided earlier and were given the opportunity to change them. Par-  
2913 ticipants rated their knowledge of NPV before and after completing the NPV test  
2914 and then rated it again after completing the four project displays. They were then  
2915 asked to rate their knowledge of NPV before and after the variance presentation.

2916 **4.3.2 Results**

2917 A within-subjects factorial ANOVA was conducted to investigate the effects of  
2918 NPV, project alignment, and numerical NPV reliability on participants' project

#### *4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation*

##### PROJECT 1

This project is about developing a new pair of wireless earphones. They will have a frequency response of 16-40,000Hz and a sensitivity of 90 db/mW. The battery life is 8 hours and the pair will isolate noise up to 35 dB. The range of the cash inflow for the first year is \$861-\$10,619. The NPV is \$227.27.

##### PROJECT 2

This project is about developing a new wrist watch. It will be water resistant up to 50m and will have one extra timing feature. The hardness of the glass face is rated 4 on the Moh scale and the strap is 10% leather. The range of the cash inflow for the first year is \$966-\$11,914. The NPV is \$881.82.

##### PROJECT 3

This project is about developing a new treadmill. It will 12 training programs for different interests and abilities and 10 speed levels. It will also have two small compartments for storage and three adjustable inclination levels. The range of the cash inflow for the first year is \$832.50-\$10,267.50. The NPV is \$81.82.

##### PROJECT 4

This project is about developing a new couch. It will have a guarantee for 10 years and a lightfastness level of 5. The cover's ability to resist abrasion has been tested to handle 50,000 cycles and it has a softness rating of 70%. The range of the cash inflow for the first year is \$906-\$11,174. The NPV is \$490.91.

##### PROJECT 5

This project is about developing a new bottle of perfume. It will have a 100 mL capacity, and the scent will be able to last for 12 hours. The fragrance concentration will be 30%, and there will be two layers of notes to the scent. The range of the cash inflow for the first year is \$925.50-\$11,414.50. The NPV is \$654.55.

**Figure 4.5:** An example of a low alignment, low reliability display in Experiment 2.

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

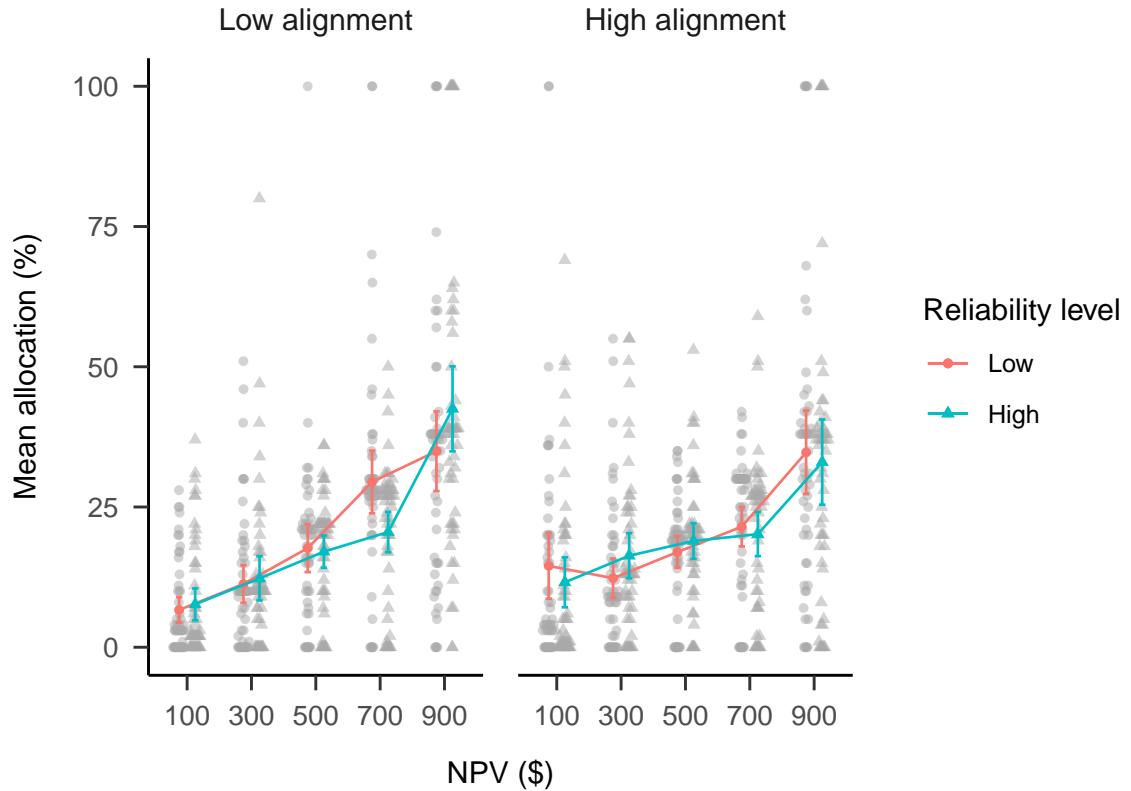
	Project 1	Project 2	Project 3	Project 4	Project 5
Product	Laptop	Laptop	Laptop	Laptop	Laptop
RAM (GB)	4	8	32	2	16
Hard drive (GB)	500	750	2000	250	1000
Resolution (px)	900	1080	1440	768	1200
Processor (Ghz)	2.4	3.2	3.8	1.6	3.6
Cash inflow range for Year 1 (\$)	\$5,890-\$6,510	\$5,738-\$6,342	\$5,244-\$5,796	\$6,137-\$6,783	\$5,538.50-\$6,121.50
NPV (\$)	\$636.36	\$490.91	\$18.18	\$872.73	\$300.00

**Figure 4.6:** An example of a high alignment, high reliability display in Experiment 2.

allocations (see Figure 4.7). The alignment  $\times$  NPV reliability level  $\times$  NPV interaction was significant,  $F(2.81, 148.75) = 3.95$ ,  $p = .011$ ,  $\hat{\eta}_p^2 = .069$ . However, this appeared to be driven by the difference between alignment conditions in the interaction between the quadratic NPV trend and NPV reliability level,  $\Delta M = -42.28$ , 95% CI  $[-76.96, -7.59]$ ,  $t(53) = -3.14$ ,  $p = .011$ , even after applying a Šidák correction. The same interaction but using a the linear NPV trend was not significant,  $\Delta M = -6.13$ , 95% CI  $[-31.50, 19.25]$ ,  $t(53) = -0.62$ ,  $p = .954$ . Further, the linear NPV trend did not differ between the reliability level conditions in either the low alignment condition,  $\Delta M = -3.19$ , 95% CI  $[-18.77, 12.40]$ ,  $t(53) = -0.41$ ,  $p = .683$  or the high alignment condition,  $\Delta M = 2.94$ , 95% CI  $[-12.63, 18.52]$ ,  $t(53) = 0.38$ ,  $p = .706$ . However, averaged across reliability level, the linear NPV trend was stronger in the low alignment condition than in the high alignment condition,  $\Delta M = 28.19$ , 95% CI  $[5.57, 50.81]$ ,  $t(53) = 2.50$ ,  $p = .016$ . This suggests that participants relied more on NPV when projects were dissimilar compared with when they were similar.

The ranking data were congruent with these results, while the confidence data

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation



**Figure 4.7:** Mean allocation across NPV, by project alignment and reliability level conditions. Error bars represent 95% confidence intervals, calculated from the within-subjects standard errors using the method from Cousineau and O'Brien (2014). Raw data are plotted in the background.

were less so. Further, the findings on over-confidence from Long et al. (2018, Study 1) were not replicated with NPV knowledge, and the variance lecture did not facilitate participants' use of numerical reliability information. These analyses are reported in Appendix B.2.2.

#### 4.3.3 Discussion

Based on participants with real-world business experience, Experiment 2 replicated the alignment effect found in Experiment 1. That is, participants relied more on NPV when faced with a set of dissimilar projects than when faced with similar projects, supporting Hypothesis 4.2. Experiment 2 also found evidence for Hypothesis 4.8, with no significant differences between the numerical reliability conditions. While Experiment 2 did not replicate the interaction found in Experiment 1, it should be emphasised that these are two different effects. In

#### *4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation*

2947 Experiment 1, participants were explicitly told whether the NPV measure was  
2948 reliable, while in Experiment 2, they were provided with variance information  
2949 that merely implied NPV reliability. Thus, the results of Experiment 2 show  
2950 that business students were affected by the comparability of projects but not by  
2951 numerical NPV reliability information. Specifically, participants appeared to focus  
2952 only on the NPV itself for a specific project, not on the underlying noisiness  
2953 of the measure.

2954 The participants in Experiment 2 seemed to rely on NPV more than those in  
2955 Experiment 1. This was seen by the steeper linear trends in Experiment 2. This  
2956 discrepancy may be due to the difference in domain experience and exposure to  
2957 financial metrics in formal study. However, the extra explanation and testing of  
2958 NPV for the management students may have also increased its salience. In sum,  
2959 the Experiment 2 sample showed clearer trends of NPV reliance, but importantly  
2960 was still affected by similarity even when it was manipulated within-subjects.

2961 Experiment 1 tested NPV reliability expressed verbally, while Experiment 2  
2962 tested NPV reliability expressed numerically. However, the difference in findings  
2963 was confounded by the different populations that were sampled. Further, in both  
2964 experiments, the business projects consisted of a limited number of domains. It is  
2965 unclear to what extent these specific domains influenced the results. These projects  
2966 were centred around consumer products, which were chosen to be more easily  
2967 accessible to participants without business experience. However, the individual  
2968 features of a project do not necessarily indicate its profitability. For instance, a  
2969 laptop with a low storage capacity may be more profitable than one with a high  
2970 storage capacity because of consumer goods markets. Experiment 3 addressed  
2971 these limitations.

2972 Another limitation of Experiments 1 and 2 was the potential confounding effect  
2973 of presentation style. The two alignment conditions differed in the number of  
2974 alignable differences, but also in the way that the information was presented. The  
2975 information in the low alignment condition was presented as paragraphs, while  
2976 the information in the high alignment condition was presented as a table. While

#### *4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation*

2977 it is likely that these data types would be presented in this way in the business  
2978 setting, it is important to rule out that this difference did not unnecessarily increase  
2979 task difficulty. Therefore, Experiment 3 attempted to replicate this effect while  
2980 controlling for presentation style

## **2981 4.4 Experiment 3**

2982 Experiment 3 investigated the effects of project alignment, NPV, NPV reliabil-  
2983 ity type and NPV reliability level on participants' budget allocations. Experiment 1  
2984 manipulated NPV reliability level using verbal prompts. That is, participants were  
2985 explicitly told whether or not NPV was reliable for a certain project. Experiment 2  
2986 investigated whether people were able to extract the same reliability information  
2987 using numerical prompts. That is, participants were provided with NPVs with  
2988 either wide or narrow ranges, indicating either low or high reliability, respectively.  
2989 Moreover, given that laypeople were sampled for Experiment 1, and Master of  
2990 Management students were sampled for Experiment 2, it was not possible to  
2991 compare the two reliability types (verbal and numerical) without ruling out the  
2992 potential confounding effect of population type. Thus, similar to Experiments 1  
2993 and 2, Experiment 3 manipulated project alignment, NPV and NPV reliability  
2994 level but also added reliability type. Further, presentation style was a possible  
2995 confounding factor in the previous experiments. That is, projects in the high  
2996 alignment condition were always displayed in a table, while projects in the low  
2997 alignment condition were always displayed as paragraphs. This possible confounder  
2998 was excluded in Experiment 3 by using the same presentation style for both align-  
2999 ment conditions.

3000 In Experiment 3, the expected results for the verbal reliability condition repli-  
3001 cated those of Experiment 1. The numerical reliability condition may replicate the  
3002 findings of Experiment 2. However, a pilot experiment (detailed in Appendix B.8)  
3003 found no significant differences between numerical reliability conditions. Appendix B.3  
3004 shows a simulation of the hypothesised effects, with the numerical reliability effects

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

**Table 4.2:** Experiment 3 group allocation.

Project alignment	Reliability type	N
High	Explicit	112
High	Implicit	112
Low	Explicit	112
Low	Implicit	112
Total		448

3005 based on the findings of the pilot experiment. Therefore, Experiment 3 retested  
3006 Hypotheses 4.1, 4.2, 4.3, 4.4, and 4.5 for the verbal reliability condition, but was  
3007 agnostic between whether the numerical reliability condition will look more like the  
3008 pattern found in the pilot experiment or the pattern found in Experiment 2.

##### 3009 **4.4.1 Method**

###### 3010 **4.4.1.1 Participants**

3011 Four hundred and forty-eight participants (176 female) were recruited from  
3012 the online recruitment platform Prolific. Participants were compensated at a rate  
3013 of £5 an hour (Prolific is based in the UK). The average age was 41.65 years  
3014 ( $SD = 10.3$ ,  $min. = 29$ ,  $max. = 78$ ). Participants reported an average of 6.94  
3015 years ( $SD = 8.23$ ,  $min. = 0$ ,  $max. = 43$ ) working in a business setting, and an  
3016 average of 3.73 years ( $SD = 6.27$ ,  $min. = 0$ ,  $max. = 45$ ) of business education.  
3017 The mean completion time of the task was 11.35 min ( $SD = 10.79$ ,  $min. = 1.92$ ,  
3018  $max. = 183.7$ ). Table 4.2 shows the allocation of participants to the different  
3019 conditions. The two reliability level conditions (low and high) were presented  
3020 within subjects and the order of their presentation was randomised. Similar to  
3021 the previous experiments, NPV varied within subjects. Therefore, each participant  
3022 saw two separate project displays. Appendix B.3.1.1.1 describes the power analysis  
3023 conducted to arrive at the sample size.

#### *4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation*

##### **3024 4.4.1.2 Materials**

**3025 4.4.1.2.1 Instructions** Participants were given instructions similar to those in  
3026 the previous experiments, with an added explanation about the NPV reliability  
3027 information for each reliability type (see Appendix B.3.1.2.1). Further, they com-  
3028 pleted a test of basic NPV understanding. Further, they completed a test on basic  
3029 NPV understanding, which also functioned as an attention check.

**3030 4.4.1.2.2 Project Display** The project displays were similar to those used in  
3031 the previous experiments. However, participants were given the same presentation  
3032 style for both alignment conditions. Each display had a table describing the  
3033 projects in the set, including ranking and allocation inputs. Project details were  
3034 presented as bullet points within the relevant cells of the table. Figure 4.8 shows  
3035 an example of a low alignment, low verbal reliability display; and Figure 4.9 shows  
3036 an example of a high alignment, high numerical reliability display.

**3037** Three elements were counterbalanced: (a) the association between reliability  
3038 level and project set (two variations), (b) the association between business name  
3039 and NPV (five latin square variations), and (c) project variation (five variations  
3040 per alignment condition). When counterbalancing for the high alignment group,  
3041 projects varied by project type (e.g., whether the five projects all described oil  
3042 wells or microchips). When counterbalancing for the low alignment group, projects  
3043 varied by their intrinsic features (e.g., whether the oil well project in the set  
3044 indicated a probability of finding oil of 96% or 90%). Table column order and  
3045 project display order were both randomised.

**3046 4.4.1.2.3 Interstitial Page** Prior to each project being displayed, participants  
3047 were shown an interstitial page, which was used to (a) introduce the next display,  
3048 and (b) check the participant's attention (given that no input was required, partici-  
3049 pants could easily skip the page without reading the text). See Appendix B.3.1.2.2  
3050 for an example.

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

Carefully read through the project descriptions below and then do the following: 1. Rank the projects between 1 (highest) and 5 (lowest) in order of investment priority in the relevant "Project Ranking" row input; and 2. Allocate each project a percentage (a number between 0 and 100) of the total budget in the relevant "Project Allocation" row input.

Relevant information	Project 1	Project 2	Project 3	Project 4	Project 5
<b>Project ranking</b>	Ranking: <input type="text"/>	Ranking: <input type="text"/>	Ranking: <input type="text"/>	Ranking: <input type="text"/>	Ranking: <input type="text"/>
<b>Project allocation (%)</b>	Allocation: <input type="text"/>	Allocation: <input type="text"/>	Allocation: <input type="text"/>	Allocation: <input type="text"/>	Allocation: <input type="text"/>
<b>Business name</b>	Pressbloom	Cweb	Pharmacore	Erectic	Railmont
<b>Project type</b>	<u>national newspaper</u>	<u>software</u>	<u>pharmaceutical</u>	<u>high-rise construction</u>	<u>railway</u>
<b>Predicted project features</b>	<ul style="list-style-type: none"> <li>Newspapers printed: 50,000 a day</li> <li>Number of weekly advertisers: 80</li> <li>Ink that is not discarded due to impurities: 5,000L a day</li> </ul>	<ul style="list-style-type: none"> <li>Code written: 1,000 lines a day</li> <li>Security rating: 60%</li> <li>Number of potential customers in first year: 3 million</li> </ul>	<ul style="list-style-type: none"> <li>Pills pressed: 300,000 an hour</li> <li>Shelf life: 20 months</li> <li>Probability of symptom reduction after a week: 90%</li> </ul>	<ul style="list-style-type: none"> <li>High-rises built: 8 a year</li> <li>Probability that the builders complete construction within a month of the due date: 70%</li> <li>Number of tenant expressions of interest: 100</li> </ul>	<ul style="list-style-type: none"> <li>Railway lines built: 5 a decade</li> <li>Number of seats filled by paying customers at peak hour: 2,000</li> <li>Time before the train carriages will need to be serviced: 12 years</li> </ul>
<b>NPV (\$)</b>	501 million. (In this industry, NPV is an <b>unreliable</b> predictor of a project's profits.)	611 million. (In this industry, NPV is an <b>unreliable</b> predictor of a project's profits.)	722 million. (In this industry, NPV is an <b>unreliable</b> predictor of a project's profits.)	806 million. (In this industry, NPV is an <b>unreliable</b> predictor of a project's profits.)	416 million. (In this industry, NPV is an <b>unreliable</b> predictor of a project's profits.)

[Continue](#)

**Figure 4.8:** An example of a low alignment, low verbal reliability display in Experiment 3.

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

Carefully read through the project descriptions below and then do the following: 1. Rank the projects between 1 (highest) and 5 (lowest) in order of investment priority in the relevant "Project Ranking" row input; and 2. Allocate each project a percentage (a number between 0 and 100) of the total budget in the relevant "Project Allocation" row input.

Relevant information	Project 1	Project 2	Project 3	Project 4	Project 5
<b>Project ranking</b>	Ranking: <input type="text"/>	Ranking: <input type="text"/>	Ranking: <input type="text"/>	Ranking: <input type="text"/>	Ranking: <input type="text"/>
<b>Project allocation (%)</b>	Allocation: <input type="text"/>	Allocation: <input type="text"/>	Allocation: <input type="text"/>	Allocation: <input type="text"/>	Allocation: <input type="text"/>
<b>Business name</b>	Liquid Pipeline	Enfuel	Petroyield	Refinera	Oilpier
<b>Project type</b>	<u>oil well</u>	<u>oil well</u>	<u>oil well</u>	<u>oil well</u>	<u>oil well</u>
<b>Predicted project features</b>	<ul style="list-style-type: none"> <li>• Oil extracted: 3,400L an hour</li> <li>• Time the machinery lasts before requiring maintenance: 11 years</li> <li>• Probability of finding oil: 96%</li> </ul>	<ul style="list-style-type: none"> <li>• Oil extracted: 2,000L an hour</li> <li>• Time the machinery lasts before requiring maintenance: 7 years</li> <li>• Probability of finding oil: 90%</li> </ul>	<ul style="list-style-type: none"> <li>• Oil extracted: 3,870L an hour</li> <li>• Time the machinery lasts before requiring maintenance: 13 years</li> <li>• Probability of finding oil: 99%</li> </ul>	<ul style="list-style-type: none"> <li>• Oil extracted: 2,470L an hour</li> <li>• Time the machinery lasts before requiring maintenance: 8 years</li> <li>• Probability of finding oil: 92%</li> </ul>	<ul style="list-style-type: none"> <li>• Oil extracted: 2,940L an hour</li> <li>• Time the machinery lasts before requiring maintenance: 10 years</li> <li>• Probability of finding oil: 94%</li> </ul>
<b>NPV (\$)</b>	494-546 million. (Midpoint: 520.)	792-876 million. (Midpoint: 834.)	409-453 million. (Midpoint: 431.)	697-771 million. (Midpoint: 734.)	598-662 million. (Midpoint: 630.)

[Continue](#)

**Figure 4.9:** An example of a high alignment, high numerical reliability display in Experiment 3.

#### 4.4.2 Results

3051 A mixed factorial ANOVA was conducted to investigate the effects of NPV,  
 3052 project alignment, NPV reliability level, and NPV reliability type on participants'  
 3053 project allocations (see Figure 4.10 for the main results and Appendix B.3.2.1  
 3054 for the remainder of the hypothesised allocation effects). The four-way interac-  
 3055 tion (alignment  $\times$  reliability level  $\times$  NPV  $\times$  reliability type) was not significant,  
 3056  $F(3.20, 1, 420.19) = 0.71, p = .555, \hat{\eta}_p^2 = .002$ . Regardless, the primary hypotheses  
 3057 were supported.

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

##### 3059 4.4.2.1 Verbal Reliability

3060 The three-way interaction (alignment  $\times$  reliability level  $\times$  NPV amount) in the  
3061 verbal reliability condition was not significant,  $\Delta M = 13.42$ , 95% CI [−1.27, 28.11],  
3062  $t(444) = 1.80$ ,  $p = .073$ . This is because NPV reliability level interacted with NPV  
3063 in both alignment conditions. This is a different pattern from Experiment 1 where  
3064 there was no effect of NPV reliability level in the low alignment condition. In  
3065 the high alignment condition, the interaction between the linear NPV trend and  
3066 NPV reliability level was significant,  $\Delta M = -36.63$ , 95% CI [-47.02, -26.25],  
3067  $t(444) = -6.93$ ,  $p < .001$ . Specifically, the trend was stronger for the high reliability  
3068 condition,  $\Delta M = 27.26$ , 95% CI [17.69, 36.83],  $t(444) = 5.60$ ,  $p < .001$ , compared  
3069 with the low reliability condition,  $\Delta M = -9.38$ , 95% CI [-18.86, 0.11],  $t(444) =$   
3070  $-1.94$ ,  $p = .053$ . This shows that, similar to Experiment 1, participants' allocations  
3071 depended on verbally expressed NPV reliability. In low alignment, there was also an  
3072 interaction between the linear NPV trend and NPV reliability level,  $\Delta M = -23.21$ ,  
3073 95% CI [-33.60, -12.83],  $t(444) = -4.39$ ,  $p < .001$ . This suggests that allocations  
3074 also depended on verbal reliability in the low alignment condition.

3075 However, another aspect of the data suggests a greater use of NPV in the low  
3076 alignment condition. The linear NPV trend was stronger in the low alignment  
3077 condition than in the high alignment condition when averaged over reliability level,  
3078  $\Delta M = 28.97$ , 95% CI [17.68, 40.26],  $t(444) = 5.04$ ,  $p < .001$ . This suggests that  
3079 when NPV reliability was expressed verbally, similar to Experiment 1, participants  
3080 relied more on NPV when projects were dissimilar than when they were similar.

3081 Overall, participants used NPV less when it was described as less reliable in  
3082 both high and low alignment conditions, and further, used NPV more when projects  
3083 were less alignable regardless of how reliable NPV was described to be.

##### 3084 4.4.2.2 Numerical Reliability

3085 The numerical reliability data were analysed differently to the verbal reliability  
3086 data because the effects of interest here were the alignment and reliability level

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

3087 effects. The linear NPV trend was stronger in the low alignment condition, av-  
3088 eraged over reliability level (with Bonferroni adjustment),  $\Delta M = 15.19$ , 95% CI  
3089 [0.78, 29.60],  $t(444) = 2.64$ ,  $p = .034$ . This pattern was the same as that found for  
3090 the verbal reliability condition above and in Experiment 2. Further, the linear NPV  
3091 trend was not significantly different between the reliability level conditions for both  
3092 the low alignment condition,  $\Delta M = 1.64$ , 95% CI [-11.61, 14.90],  $t(444) = 0.31$ ,  
3093  $p > .999$ , and high alignment condition,  $\Delta M = -1.21$ , 95% CI [-14.46, 12.05],  
3094  $t(444) = -0.23$ ,  $p > .999$ . This indicates that participants did not use numerical  
3095 NPV reliability to inform their allocations.

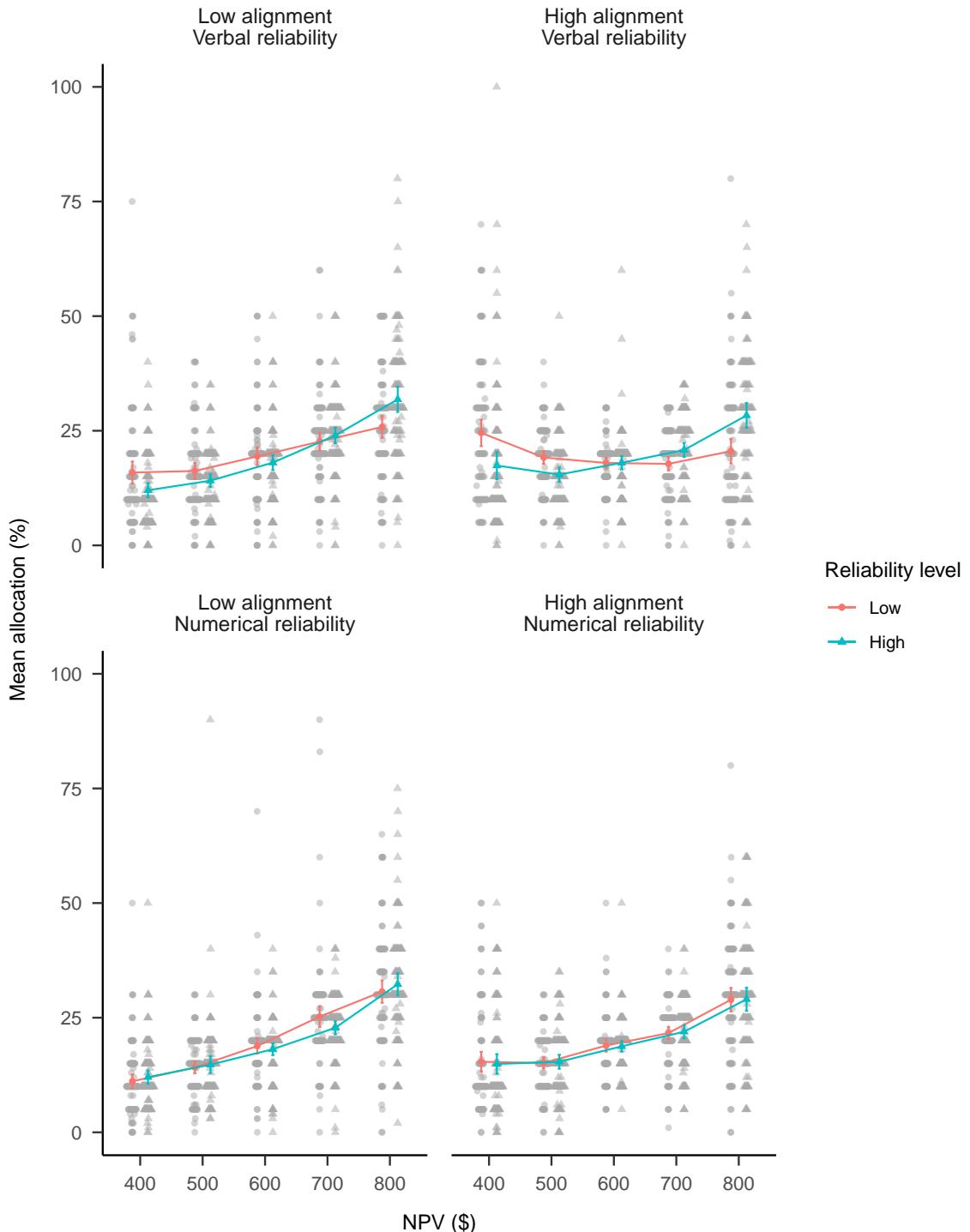
3096 Similar to the verbal reliability condition, the use of NPV was stronger in the low  
3097 alignment condition than it was in the high alignment condition. However, unlike  
3098 the verbal reliability condition, allocations did not depend on numerical reliability  
3099 in either the low or the high alignment condition. In the verbal reliability condition,  
3100 allocations depended on NPV reliability level in both alignment conditions.

#### 3101 4.4.3 Discussion

3102 Hypotheses 4.1, 4.2, 4.3, and 4.4 were supported in the verbal reliability condi-  
3103 tion. This shows that, while overall participants preferred to use NPV as a proxy  
3104 for project quality in their allocations, they still used verbal reliability information.  
3105 Specifically, when projects were similar, participants used NPV when they were  
3106 told that it was reliable, and used alternative metrics when told that it was not  
3107 reliable. However, in Experiment 3, no support was found for Hypothesis 4.5. It  
3108 was expected that participants in the low alignment condition would use NPV  
3109 regardless of the reliability level conditions, as in Experiment 1. Rather, they used  
3110 NPV less when told that it was unreliable. However, they primarily used NPV  
3111 overall, as shown by the positive NPV trend in both reliability level conditions.

3112 Further, Experiment 3 replicated the finding of Experiment 2 for the numerical  
3113 reliability condition. Specifically, participants relied more on NPV when projects  
3114 were dissimilar but, critically, did not use numerical range information to influence  
3115 their allocations. A pilot study (documented in Appendix B.8) replicated the

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation



**Figure 4.10:** Mean allocation across NPV, by alignment, reliability level, and reliability type conditions. Error bars represent 95% confidence intervals, calculated from the within-subjects standard errors using the method from Cousineau and O'Brien (2014). Raw data are plotted in the background.

#### *4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation*

3116 results of Experiment 1 in the verbal reliability condition, but did not replicate the  
3117 results of Experiment 2 in the numerical reliability condition. That is, when faced  
3118 with numerical ranges as the NPV reliability information, participants did not even  
3119 use the midpoint in their decisions. The results of Experiment 3 suggest that the  
3120 finding in the pilot experiment may have been spurious or due to an unexplored  
3121 component of the experimental design, but this can only be determined with  
3122 future research.

## **3123 4.5 General Discussion**

3124 Across three experiments there were two main findings: (a) NPV is used more  
3125 when options are difficult to compare in the low alignment conditions; and (b)  
3126 people do not consider numerical variance information, despite this being important  
3127 to the reliability of the NPV forecasts. This pattern with numerical reliability  
3128 information contrasted with the frequent use of verbal indicators of reliability  
3129 level. This numerical variance neglect is surprising, since other work showed  
3130 that people can readily extract variance information when experiencing numerical  
3131 sequences (Rosenbaum et al., 2020). Both the verbal and numerical effects were  
3132 consistent for both naive and experienced participants, indicating their persistence.  
3133 People make use of metrics with alignable differences when required to compare  
3134 disparate options. However, they do not always use alternative metrics, even when  
3135 they are available.

3136 Experiment 1 found that participants did not use NPV in their allocation  
3137 decisions when they were told that it was unreliable but did use it when told it  
3138 was reliable. Experiment 2 found that participants with some business experience  
3139 relied more on NPV for capital allocation when the rest of the information was  
3140 non-alignable compared with when it was alignable. However, they did not take  
3141 into account numerical reliability information when making these decisions. Experi-  
3142 ment 3 found further evidence of these effects within one experimental design.

#### *4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation*

3143 Alignable differences have been shown to be important into decision-making in  
3144 many settings (Markman & Loewenstein, 2010; Markman & Medin, 1995). The  
3145 experiments presented in this chapter are novel in terms of the effects of project  
3146 alignment on capital allocation. Further, these experiments considered the extent  
3147 to which the reliability of an alignable measure (NPV) affects the way in which  
3148 it is used. This depended on the availability of other alignable differences in the  
3149 set of choices. If other alignable differences were available, then participants were  
3150 willing to reduce their use of a reportedly unreliable alignable measure (or use it  
3151 when told that it was reliable). However, when no other alignable differences were  
3152 available, then the alignable, albeit unreliable, measure was more likely to be used.  
3153 This was found in both Experiments 1 and 3, as well as in a pilot study to a lesser  
3154 extent (reported in Appendix B.4).

3155 Financial measures such as NPV are useful because of their alignability. That  
3156 is, they may serve as an alignable difference, regardless of the inherent similarities  
3157 between a set of projects. Psychologically, these measures are useful because they  
3158 allow for relevant inferences (Lassaline, 1996) and because they offer an abstraction  
3159 of concrete details (Doumas & Hummel, 2013). However, the structural alignment  
3160 account does not directly speak to real-world implications when there is a need  
3161 for non-alignable comparisons. NPV is a type of abstraction that facilitates the  
3162 comparison of different aspects of a company. For instance, the use of NPV may  
3163 facilitate the comparison of an oil field project with a refinery project. However,  
3164 this increased alignment could actually hide important information because it does  
3165 not consider the finer complexities inherent in each business unit. The forecasts  
3166 used to calculate NPV for each business unit are based on different indicators,  
3167 and there are likely to be differences between each unit's estimates. Thus, one  
3168 can imagine a continuum of comparisons in which the usefulness of comparison  
3169 increases with the level of alignability but depends on the level of abstraction that  
3170 is required to achieve the alignment.

3171 The finding that participants, even those with some business experience, did  
3172 not sufficiently consider variance information is surprising but understandable. It is

#### *4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation*

surprising because financial decision-making largely depends on the consideration of different sources of variance (e.g., risk, volatility, and uncertainty). At the same time, it is understandable because research from psychology and statistics education shows that statistics students and people in general have a poor ability to draw statistical inferences (e.g., Galesic & Garcia-Retamero, 2010; Konold et al., 1993). Future research should investigate the conditions under which individuals' sensitivity to variance information may be facilitated. For instance, it is unclear whether it is merely salience that is lacking, meaning that visual aids could be useful, or whether a further explicit explanation of statistical inference is necessary. The findings of a pilot experiment suggest that participants struggle to use numerical NPV reliability information, even when given explicit instructions (see Appendix B.7).

A possible limitation of these experiments is the use of NPV as the only financial metric. In the business world, there are many metrics that serve similar functions and are used as tools to deal with non-alignable options. Therefore, future research should attempt to replicate the current findings using different financial measures.

Future research should also investigate the boundary conditions of the reliability type effect. That is, people appear to respond to explicit reliability information but not to variance information that only implies reliability. Future research should attempt to identify the minimal variance information that participants need to understand the relevant implications for reliability. Participants may simply not notice the variance information or assume that it is irrelevant. For instance, future research could test participants in a condition in which the variance information is more salient.

#### 4. Project Similarity Bias and Variance Neglect in Forecast Metric Evaluation

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## Seeking Alignment in Past Cases

3258 Chapter 4 found that people do not sufficiently perceive the importance of  
3259 numerical variance information in capital allocation. This is important when  
3260 business projects are dissimilar because people may fail to pay attention to the  
3261 differing variance underlying NPV across different domains. However, there are also  
3262 implications for high alignment scenarios. When projects are alignable, managers  
3263 are likely to be able to use abstract metrics as well as intrinsic project features.  
3264 Managers may use a metric such as NPV, the variance of which may suggest a lack  
3265 of reliability, despite being able to use intrinsic project features. Therefore, they  
3266 may miss the opportunity to use different and potentially more reliable measures.

3267 Therefore, the evaluation of a non-alignable set of projects has many poten-  
3268 tial pitfalls. This situation is likely to occur in most hierarchical organisations,  
3269 especially those that are highly diversified. As discussed in Chapter 3, a solution  
3270 for managers who fail to aggregate the risk of multiple projects may be for them  
3271 to concurrently evaluate projects as a portfolio. However, the solution to the  
3272 evaluation of dissimilar projects in diversified organisations is likely to involve  
3273 significantly more difficult structural changes in the organisation. For instance,  
3274 this may mean divesting certain divisions of the organisation, as GE has done in  
3275 the last few years (Scott, 2018).

## 5. Seeking Alignment in Past Cases

3276 Other solutions are also possible. For instance, organisations may develop a  
3277 more normative use of metrics and take into account underlying uncertainties.  
3278 However, this change may require substantially more statistical reasoning abilities  
3279 than should be expected of managers without better decision-making guidelines.  
3280 Another solution for managers is to seek evidence from similar projects from outside  
3281 of the organisation. This may be useful because a diversified organisation may not  
3282 have enough points of reference for a project proposal. It would also mean that  
3283 substantial organisational restructuring such as divestment or training managers  
3284 in statistical reasoning would not be required.

3285 Evidence from similar projects may come in the form of an individual case study  
3286 from another organisation or a research report that describes a statistical result.  
3287 Case studies are especially important in managerial decision-making because they  
3288 are used extensively in business school teaching materials. Therefore, managers are  
3289 likely to seek case studies that may be used to inform their decisions. However, do  
3290 they believe that a single case study is more useful than statistical data? The litera-  
3291 ture on anecdotal bias suggests that they might. Chapter 6 considers the influence  
3292 of anecdotes on project allocation when they conflict with statistical evidence.

3293 Previous work shows that people often do not give evidence appropriate weight-  
3294 ing in their decisions (Griffin & Tversky, 1992). Statistical and anecdotal evidence  
3295 often conflict because statistical estimates commonly refer to the mean value of  
3296 a distribution, while individual cases may be sampled from either tail of the  
3297 distribution. This comparison may produce conflicting information, especially if  
3298 the distribution is skewed; therefore, it is important to appropriately weigh their  
3299 influence when making a decision. In the same way that intrinsic project features  
3300 conflicted with the abstract financial metrics in Chapter 4, anecdotal evidence  
3301 conflicts with financial metrics of the target project in Chapter 6.

3302 Chapter 6 considers how people deal with such conflicting information. That is,  
3303 do they focus on one metric or use a trade-off? In the previous chapter, participants  
3304 did not appear to predominantly use any one specific cue. The fact that those  
3305 in the low alignment condition relied more on NPV compared with those in the

## *5. Seeking Alignment in Past Cases*

3306 high alignment condition means that the latter were still referring to intrinsic  
3307 project features to some extent. Specifically, the influence of different measures  
3308 may have been integrated in a type of trade-off. However, there was no clear way  
3309 of determining this because the allocation measure was aggregated in the analysis.  
3310 In Chapter 6, however, conditions are set up so that it is possible to determine  
3311 whether participants were using anecdotes exclusively, partially, or not at all.

*We like stories, we like to summarize, and we like to simplify*

—Nassim Nicholas Taleb (2007, p. 63)

# 6

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## Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

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### Contents

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<b>6.1 Introduction</b>	111
6.1.1 Anecdotal Bias	112
6.1.2 Anecdotal Bias in Business	113
6.1.3 Effect of Similarity	115
6.1.4 Experiment Summary	116
<b>6.2 Experiment 1</b>	117
6.2.1 Method	119
6.2.2 Results	125
6.2.3 Discussion	129
<b>6.3 Experiment 2</b>	130
6.3.1 Method	133
6.3.2 Results	135
6.3.3 Discussion	142
<b>6.4 General Discussion</b>	143
6.4.1 Theoretical Implications	145
6.4.2 Practical Implications	147
<b>References</b>	148

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## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

### 6.1 Introduction

A good story is often more persuasive than data. While usually harmless in daily settings, poor judgement arising from a bias towards anecdotal evidence can lead to large-scale negative consequences. Perhaps the most prominent example of such an error in judgement is the belief that a vaccine causes a certain disorder based on isolated stories, despite contradictory scientific evidence. An analogous error exists in settings such as managerial decision-making. In business, managers use analogies, known as *case studies*, as a part of their strategic decision-making. Case studies are examples of previous situations considered similar by the decision-maker and are used to draw inferences about a target problem. Case studies are known as *anecdotes* when comparing them with aggregated data.

Many businesses use case studies to inform their decisions but often struggle to use them successfully (Gavetti & Rivkin, 2005). This may be attributable to the prominence of companies that are either highly successful or highly unsuccessful. That is, people are often uninterested in average outcomes but are captivated by both positive and negative extreme outcomes. The increased salience of an anecdote may increase its influence over that of useful statistical data. Further, increased anecdotal salience may also shift attention away from structural similarities in favour of more surface similarities. Both of these issues may explain the unsuccessful use of case studies.

The first consideration when using a case study is its merit relative to available aggregated statistical data. That is, if the case study is a single data point in a set of other relevant cases, then using the statistical properties of the larger sample is more inferentially informative than using a single case from within the sample (unlike perhaps when the single case is somehow the most relevant example from the sample). Despite the utility and availability of large sample data, research has shown that people often prefer anecdotal evidence over statistical data (Freling et al., 2020; Jaramillo et al., 2019; Reinard, 1988; Shen et al., 2015).

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

3365        However, if this larger sample is not available (or is ignored), then the second  
3366        consideration when using a case study is the extent of its similarity to the tar-  
3367        get problem. Research on the psychology of similarity judgements distinguishes  
3368        between surface and relational similarity (Gentner, 1983). The consensus of this  
3369        research is that the more conceptual structures that two cases share, the more useful  
3370        they are in decision-making (Lassaline, 1996; Markman & Medin, 1995). Therefore,  
3371        case studies that are similar to a target problem on a merely surface level are less  
3372        useful than those that are related through a shared conceptual structure.

3373        Previous research has considered the role of similarity and analogical reasoning  
3374        in business-related decision-making (e.g., Gavetti et al., 2005). Others have inves-  
3375        tigated the influence of anecdotes in capital allocation decisions and the impact  
3376        of anecdote similarity on their persuasiveness (summarised below). However, it  
3377        is unclear to what extent an anecdote's similarity to the target problem will af-  
3378        fect its influence on capital allocation decisions. Further, it is unclear whether  
3379        people will be sensitive to information about the distribution from which the  
3380        anecdote was sampled.

### 3381        6.1.1 Anecdotal Bias

3382        Anecdotal bias refers to the influence of anecdotal evidence over statistical  
3383        evidence on people's beliefs. Journalists, for instance, are well aware of the power of  
3384        anecdotes. An analysis of approximately 29,000 New York Times editorials showed  
3385        a reliance on anecdotes to drive arguments (Al Khatib et al., 2017). While some  
3386        studies have concluded that statistics are more persuasive than anecdotes (e.g.,  
3387        Allen & Preiss, 1997; Hoeken, 2001; Hornikx, 2005) and others provided more  
3388        cautious conclusions (Winterbottom et al., 2008), a number have found evidence  
3389        for anecdotal bias (e.g., Jaramillo et al., 2019; Ratcliff & Sun, 2020; Reinard, 1988;  
3390        Reinhart, 2006; Shen et al., 2015). Zebregs et al. (2015) suggest that this disparity  
3391        in findings might be attributable to statistics affecting beliefs and attitudes, and  
3392        anecdotes affecting intention. A more recent meta-analysis of 61 studies found that,  
3393        overall, statistical evidence is more persuasive than anecdotal evidence (Freling et

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

3394 al., 2020). However, even if statistical evidence is more persuasive overall, anecdotes  
3395 that add no additional information to co-presented statistics may still influence  
3396 people's judgement (Jaramillo et al., 2019). Further, the meta-analysis found that  
3397 people tend to prefer anecdotal evidence over statistical data when the stakes are  
3398 more emotional, medical, or relevant to the decision-maker. In business, decisions  
3399 are clearly relevant to the decision-maker.

### 3400 6.1.2 Anecdotal Bias in Business

3401 It is important to investigate anecdotal bias in business because of its impli-  
3402 cations for managers' use of case studies. There are many cases of managers  
3403 successfully using analogies from anecdotal cases but also of failures to analyse  
3404 correctly (Gavetti et al., 2005; Gavetti & Rivkin, 2005). There is very little research  
3405 on anecdotal bias in business, but the existing work finds clear evidence of the  
3406 effect. In fact, the recent meta-analysis by Freling et al. (2020) included the work  
3407 of Wainberg et al. (2013) as one such paper.

3408 Wainberg et al. (2013) gave a sample of managers and other professionals a  
3409 choice between two audit firms, which varied in terms of their audit deficiencies  
3410 for various clients. The experiment was designed in such a way that the statistical  
3411 evidence favoured one firm, while the anecdotal evidence favoured the other firm.  
3412 Participants were allocated to one of five conditions. Participants in the *anecdotes*  
3413 *only* condition were given anecdotal examples of firm deficiencies, while those in  
3414 the *anecdotes & statistics* condition were given the same anecdotal examples as  
3415 well as the number of clients and deficiencies found. However, participants were  
3416 not explicitly provided with the proportions of these values. Participants in the  
3417 *statistics only* condition were given this proportions information as well as the  
3418 number of clients without deficiencies but no detailed examples of deficiencies.  
3419 The *anecdotes & enhanced statistics* condition included both anecdotes and the  
3420 information in the statistics only condition. The terminology here is confusing  
3421 because nothing about the way the statistics are presented to the participants is  
3422 "enhanced" beyond how they are presented in the statistics only condition. However,

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

3423 the *anecdotes & enhanced statistics—judgment orientation* condition emphasised  
3424 the importance of proportions and keeping absolute numbers in context.

3425 Wainberg et al. (2013) measured the percentage of participants who chose  
3426 firms favoured by the statistical data, finding evidence of anecdotal bias. Par-  
3427 ticipants in the anecdotes only and anecdotes & statistics conditions equally chose  
3428 the firm favoured by statistical data. However, participants in the anecdotes &  
3429 enhanced statistics condition were less likely to choose this firm compared with  
3430 those in the statistics only condition, even when the underlying proportions were  
3431 made explicit. This shows evidence of anecdotal bias because participants ignored  
3432 contradictory statistical data. The lack of difference between the anecdotes &  
3433 statistics condition and the anecdotes only condition implies that the anecdotal  
3434 bias effect was “complete.” That is, the presented statistics did not play a role  
3435 in influencing participants’ choice of firm. A “partial” effect would have occurred  
3436 if more participants in the anecdotes & statistics condition had chosen the firm  
3437 compared with participants in the anecdotes only condition. This would have  
3438 meant that statistics played at least some role in influencing choice.

3439 The other important finding in this work is that anecdotal bias was reduced by  
3440 highlighting relevant statistical features and providing an explanation of statistical  
3441 inference. This is important because it suggests that potential psychological biases  
3442 can be reduced with a reframing of provided information and an explanation of  
3443 relevant statistical concepts.

3444 Wainberg (2018) conducted a similar study to that of Wainberg et al. (2013) but  
3445 with a capital budgeting task as opposed to a binary choice. Participants had to  
3446 choose between three production line machines for a mid-sized company that prints  
3447 circuit boards. The statistical data suggested that Machine A was better than  
3448 Machine B, and Machine B was better than Machine C. Participants were given  
3449 only statistical information or statistical information along with an anecdote. The  
3450 anecdote was in the form of an email from a colleague who recommended against  
3451 Machine A (the best option). Similar to Wainberg et al. (2013), participants were  
3452 assigned to *anecdote & statistics* and *statistics only* conditions. In the *judgement*

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

3453 *orientation I* and *judgement orientation II* conditions, participants were told to  
3454 “think like a scientist” and received either a short or a long explanation, respectively,  
3455 of the importance of statistical inference.

3456 Wainberg (2018) found evidence for anecdotal bias. Including a contradictory  
3457 anecdote alongside statistical evidence (the anecdote & statistics condition) re-  
3458 duced the proportion of participants who chose Machine A. The study also found  
3459 that the addition of instructions that emphasised scientific thinking reduced this  
3460 bias. Unlike Wainberg et al. (2013), Wainberg (2018) could not determine whether  
3461 the anecdotal bias was a complete or partial because there was no anecdote only  
3462 condition. Further, neither work considered the effect of the anecdote’s similarity  
3463 to the target problem.

### 3464 6.1.3 Effect of Similarity

3465 Arguably, the extent of one’s reliance on an anecdote should depend on its  
3466 similarity to the target problem. Previous work has examined the importance  
3467 of weighting previous cases according to their similarity to the present situation  
3468 (Gilboa & Schmeidler, 1995; Lovallo et al., 2012). For instance, consider a medical  
3469 treatment with contradictory statistical and anecdotal evidence; that is, a large-  
3470 scale aggregated study has found that the treatment has 99% efficacy, while some-  
3471 one reports on social media that they became sick as a side-effect of the treatment.  
3472 While the decision to use the treatment should be informed more by the aggregated  
3473 data than by the anecdotal data, an individual may have reason to be concerned  
3474 if the person who became sick was their identical twin. Therefore, the inference  
3475 that the individual may also need to be cautious about the treatment arises from  
3476 a specific causal model based on the shared genetics of the two cases.

3477 There have been mixed results regarding the effect of anecdote similarity on  
3478 the extent of anecdotal bias. Hoeken and Hustinx (2009, Study 3) found evidence  
3479 for the effect of similarity on anecdotal bias for a variety of claims. As well as  
3480 manipulating whether participants received a claim supported by anecdotal or  
3481 statistical evidence, they manipulated whether the anecdotal evidence was similar

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

3482 or dissimilar to the claim that it was supporting. They found that similar anecdotes  
3483 were more persuasive than dissimilar anecdotes. Using a student sample, Hoeken  
3484 (2001) did not find evidence for the effect of similarity to a local government  
3485 proposal. Similarly, Hornikx (2018) considered the effect of similarity on anecdotal  
3486 bias in local government policy decision-making. The researchers did not find an  
3487 effect for similarity or for anecdotes. However, they measured persuasiveness, and  
3488 it may be that requiring participants to make more concrete decisions will create  
3489 a more realistic scenario.

3490 Apart from the need to determine the effect of similarity on the anecdotal  
3491 bias effect, it is important to clarify how such an effect might work. Research on  
3492 analogical reasoning has distinguished between simple surface similarity and deeper  
3493 relational similarity (Gentner, 1983). As mentioned above, one's use of an anecdote  
3494 should depend on the extent to which it is associated by an underlying causal  
3495 mechanism or mere surface similarity. Imagine a manager of a multi-divisional  
3496 company deciding on the allocation of capital between an oil well project and a  
3497 technology project. Would hearing of a recent failed oil well project at another  
3498 company influence the manager's allocation decision? If so, would it influence  
3499 the manager's decision because the anecdote has similarities to the target oil well  
3500 project (surface similarity)? Or would the manager seek out the underlying reason  
3501 for the failure of the other company's oil well project to identify whether it is  
3502 relevant to the target oil project (relational similarity)? The experiments presented  
3503 in this chapter investigated whether the anecdotal bias effect arose from causal  
3504 inductive reasoning or merely the surface similarity with the target project.

### 3505 6.1.4 Experiment Summary

3506 Experiment 1 investigated whether anecdotal bias in a capital allocation paradigm  
3507 depends on anecdote similarity. Further, it tested whether providing additional  
3508 statistical information encourages participants to consider the statistics over the  
3509 anecdote. Experiment 1 used a negative anecdote, which is an example of an  
3510 unsuccessful case. This kind of anecdote has been shown to produce anecdotal bias

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

in both medical (Jaramillo et al., 2019) and business (Wainberg, 2018) decision-making. However, Jaramillo et al. (2019) found less bias in positive anecdotes, which are examples of successful cases, and Wainberg (2018) did not consider these at all. Therefore, Experiment 2 attempted to replicate the effect of similarity on anecdotal bias using a positive anecdote. Further, Experiment 2 provided participants with information about the sample distribution of the anecdote, whereas Experiment 1 did not. This allowed for an informal test of whether people are sensitive to such information.

## 6.2 Experiment 1

Experiment 1 investigated the effects of anecdote similarity and bias on capital allocation. Participants were assigned to the same conditions as those in Wainberg (2018) except that an anecdote only condition was included and the judgement orientation I condition was excluded. They were then asked to allocate a hypothetical budget between two business projects. Participants were also presented with a case study that was either similar or dissimilar to the target project (but still from the same industry). Further, for the conditions in which statistical evidence was provided, participants were presented with aggregated information about the success of similar projects in the form of NPV as well as a reliability measure. One project was clearly better than the other in terms of the statistical data, but the anecdotal evidence suggested the opposite.

Previous research has found that people are persuaded more by negative anecdotes than by positive statistical data in capital allocation scenarios (Wainberg, 2018). While studies have shown that similar anecdotes are more persuasive than dissimilar anecdotes (Hoeken & Hustinx, 2009, Study 3), it is unclear how the anecdotal bias effect may depend on anecdote similarity. Thus, the main question is whether anecdotal bias will be greater when the anecdote is similar to the target project compared with when it is dissimilar. The target project was supported by the statistics but was inconsistent with the anecdotes. Further, Experiment 1

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

3539 only used negative anecdotes. Therefore, the experiment would show evidence of  
3540 anecdotal bias if participants assigned to the statistics only condition allocated  
3541 more money to the target project compared with those in the anecdote & statistics  
3542 condition. Therefore, Experiment 1 tested the following hypothesis:

3543 **Hypothesis 6.1—anecdotal bias depends on the similarity of negative  
3544 anecdotes.** Budget allocations to the target project will be higher when statistics  
3545 only are presented compared with when statistics are accompanied by an anecdote  
3546 with high similarity to the target project. In addition, budget allocations will not  
3547 be affected by anecdotes with low similarity. That is, the statistics only condition  
3548 will not differ from the low-similarity anecdote & statistics condition.

3549 Experiment 1 predicted that that the anecdotal bias effect will be complete,  
3550 as in Wainberg et al. (2013). Specifically, the participants presented with the  
3551 high-similarity anecdote along with the statistics would not use any statistical  
3552 information. Testing the high similarity condition will provide an equivalent test  
3553 to that of Wainberg et al. (2013). Therefore, Experiment 1 tested the following:

3554 **Hypothesis 6.2—effect of statistics for negative anecdotes.** Participants in  
3555 the high-similarity anecdote & statistics condition (without the enhanced statistics  
3556 explanation) and those in the high-similarity anecdote only condition will allocate  
3557 capital equally to the target project.

3558 Participants with additional information on the importance of scientific thinking  
3559 and statistical data may be less affected by anecdotes. Wainberg (2018) termed this  
3560 the *judgement orientation II* condition, while in this experiment it is termed the  
3561 *anecdote & enhanced statistics condition*. Unlike the anecdote & enhanced statistics  
3562 condition in Wainberg et al. (2013), the statistical information here is actually  
3563 “enhanced” because of the accompanying text. Experiment 1 tested whether the  
3564 effect of additional information on anecdotal bias would be replicated in a capital  
3565 allocation scenario. Therefore, Experiment 1 tested the following hypothesis:

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

**Table 6.1:** Experiment 1 group allocation.

Evidence type	Project alignment	N
Anecdote & enhanced statistics	High	41
Anecdote & enhanced statistics	Low	41
Anecdote & statistics	High	41
Anecdote & statistics	Low	40
Anecdote only	High	41
Anecdote only	Low	40
Statistics only	NA	40
Total		284

3566 **Hypothesis 6.3—effect of enhanced statistics for negative anecdotes.** Par-  
3567 ticipants in the high-similarity anecdote & enhanced statistics condition will allo-  
3568 cate more capital to the target project than those in the high-similarity anecdote  
3569 & statistics condition.

### 3570 6.2.1 Method

#### 3571 6.2.1.1 Participants

3572 Two hundred and eighty-four participants (197 female) were recruited from a  
3573 cohort of psychology undergraduates at The University of Sydney. Participants  
3574 were compensated with course credit. The average age was 20.84 years ( $SD =$   
3575 4.93,  $min. = 18$ ,  $max. = 58$ ). Participants reported an average of 1.68 years  
3576 ( $SD = 3.63$ ,  $min. = 0$ ,  $max. = 32$ ) working in a business setting, and an average  
3577 of 0.81 years ( $SD = 1.57$ ,  $min. = 0$ ,  $max. = 12$ ) of business education. The  
3578 mean completion time of the task was 22.24 min ( $SD = 97.45$ ,  $min. = 1.67$ ,  
3579  $max. = 1,101.48$ ). Table 6.1 shows the allocation of participants to the different  
3580 conditions. Appendix C.1.1.1 describes the power analysis conducted to arrive  
3581 at this sample size.

#### 3582 6.2.1.2 Materials

3583 **6.2.1.2.1 Instructions** All participants were first shown general instructions  
3584 explaining the task. Subsequent instructions shown to participants depended on

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

their experimental condition. Those in the anecdote only condition were told that they would be shown a case study of a failed project and an analysis of why it failed. Those in the statistics only condition were told that they would be shown NPV and reliability information for two focal projects. They were told that these values were sourced from a study with a large sample. Those in the anecdote & statistics condition were shown both of these instructions and were also told that the information in the anecdote had been included in the aggregated study data. Those in the anecdote & enhanced statistics condition were shown the same instructions as those in the anecdote & statistics condition, but were also provided with the explanation of scientific thinking used by Wainberg (2018). Appendix C.1.1.2.1 shows the instructions used in Experiment 1.

**6.2.1.2.2 Allocation Task** In the allocation task, participants were asked to allocate a hypothetical budget to one of two projects from two different businesses. In this chapter, these projects are referred to as the *focal* projects, with one being the *target* project and the other the *comparison* project. The target project was used as the reference for the similarity manipulation. That is, the anecdote was either high or low in similarity to the target project. Further, the data analyses presented in Section 6.2.2 used allocations to the target project as the dependent variable. The comparison project was simply the other focal project to which participants were allocating. It was always a different type of project to both the target and anecdote projects.

Participants were presented with information about the name, location, integration (vertical or horizontal), and organisational structure (centralised or decentralised) of each business (see Appendix C.1.1.2.2 for an explanation of these terms). Further, they were presented with information about the features of each project that are typically available to managers prior to investment. Participants in the anecdote only condition were shown only this information (see Figure 6.1). Those in the anecdote & statistics, anecdote & enhanced statistics, and statistics only conditions were shown this information along with measures of NPV and

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

### Target projects

Allocate your budget between the following two projects using percentage values (the two values should sum to 100):

Relevant information	Project A	Project B
Business name	Enfuel	Microxy
Investment	oil well	microchip
Location	Texas, USA	Manchester, UK
Integration	vertical	horizontal
Structure	centralised	decentralised
Predicted project features	<ul style="list-style-type: none"> <li>• Oil extracted: 2200L an hour</li> <li>• Time the machinery lasts before requiring maintenance: 8 years</li> <li>• Probability of finding oil: 88%</li> <li>• Type of well: onshore</li> </ul>	<ul style="list-style-type: none"> <li>• Microchips produced: 4000 an hour</li> <li>• Usable semiconductor yield after testing: 60%</li> <li>• Compatible PCs in the market: 75%</li> <li>• Type of integrated circuit: digital</li> </ul>

Project A allocation:  %

Project B allocation:  %

**Figure 6.1:** Focal project display for the anecdote only condition in Experiment 1. Here, Project A was the target project and Project B was the comparison project.

<sup>3614</sup> overall reliability rating (see Figure 6.2). Participants entered their allocation data

<sup>3615</sup> beneath this table in two text boxes labelled *Project A allocation* and *Project B*

<sup>3616</sup> *allocation*, respectively.

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

### Target projects

Allocate your budget between the following two projects using percentage values (the two values should sum to 100):

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Predicted project features	<ul style="list-style-type: none"> <li>• Oil extracted: 2200L an hour</li> <li>• Time the machinery lasts before requiring maintenance: 8 years</li> <li>• Probability of finding oil: 88%</li> <li>• Type of well: onshore</li> </ul>	<ul style="list-style-type: none"> <li>• Microchips produced: 4000 an hour</li> <li>• Usable semiconductor yield after testing: 60%</li> <li>• Compatible PCs in the market: 75%</li> <li>• Type of integrated circuit: digital</li> </ul>
Overall reliability rating (%)	95	87
NPV (\$)	900	100

Project A allocation:  %

Project B allocation:  %

**Figure 6.2:** Focal project display for the statistics only, anecdote & statistics, and anecdote & enhanced statistics conditions in Experiment 1. Here, Project A was the target project and Project B was the comparison project.

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

3617     **6.2.1.2.3 Anecdote** Participants who were presented with an anecdote (those  
3618     in either the anecdote only, anecdote & statistics, or anecdote & enhanced statistics  
3619     conditions) were shown a description of another business project and an accompa-  
3620     nying analysis. Figures 6.3 and 6.4 show the anecdotes for those in the high and  
3621     low similarity conditions, respectively. The project description had a similar layout  
3622     to that of the focal projects. That is, it contained information about the business  
3623     name, location, integration, and organisational structure of the business. It also  
3624     detailed several predicted features of the project. Beneath this description was a  
3625     paragraph presenting an analysis of why the project had failed. This paragraph  
3626     referenced each of the features in the description to justify the failure of the project.

3627       Participants in the high similarity condition were shown a description of a  
3628     project from a business with the same type of investment as the target project  
3629     (Project A). All categorical attributes were identical to those in Project A, but  
3630     all numerical attributes were lower. The analysis explained that the numerical  
3631     attributes had failed because they had not reached certain cut-offs. Critically,  
3632     these cut-offs were all higher than the matching values in Project A. This was done  
3633     to ensure that the numerical attributes in the anecdote appeared more relevant  
3634     than those in Project A. For instance, in Project A, oil extraction was set at  
3635     2,200 L/hr, and in the anecdote it was 2,000 L/hr, while the cut-off was set at  
3636     3,000 L/hr. Thus, the failure of the anecdotal project arising from insufficient  
3637     oil extraction would appear more relevant because the oil extraction in both the  
3638     anecdotal project and Project A was lower than the cut-off value. Note, however,  
3639     that there was uncertainty about the generalisability of these cut-off values because  
3640     the participants did not receive an explicit indication of whether these values were  
3641     meant to generalise to other cases.

3642     **6.2.1.2.4 Follow-up Questions** Participants who were shown the anecdote  
3643     were subsequently presented with follow-up questions. They were asked about how  
3644     similar they believed the anecdote was to the target project, how relevant it was

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

### Case study

- Business details:
  - Business name: Refinera
  - Location: New Mexico, USA
  - Integration: vertical
- Investment: oil well
- Predicted project features:
  - Oil extracted: 2000L an hour
  - Time the machinery lasts before requiring maintenance: 7 years
  - Probability of finding oil: 80%
  - Type of well: onshore

Refinera struggled to establish itself in the regional market because of what scientists now know is a hydrocarbon shortage in the New Mexico area. A centralised organisational structure meant that key operational decisions were delayed with what needed to be a timely process. Being vertically integrated meant that these delays caused losses at the retail sites due to miscalculations of petrol supply. To make up for this, a post hoc analysis concluded that oil was needed to be extracted at a rate of 3000L an hour and sites have at least a 96% probability of finding oil before management approved the project. Further, machinery needed to have thought to last at least 10 years before requiring maintenance, because maintenance costs further offset the initial investment after the 7 years of development. Further, the well was quite susceptible to crude oil price changes due to it being an onshore well, and so added additional financial setbacks over the course of the project.

**Figure 6.3:** Anecdote for participants in the high similarity condition in Experiment 1.

3645 to their allocations and how relevant it would be for their judgements about other  
3646 projects of that type (see Appendix C.1.1.2.3).

#### 3647 6.2.1.3 Procedure

3648 Participants were introduced to the study through the general instructions  
3649 followed by the specific instructions for their condition. Participants were then  
3650 presented with the allocation task and a description of the focal projects. All  
3651 participants except those in the statistics only condition were also presented with  
3652 the anecdote description and analysis, and the follow-up questions.

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

### Case study

- Business details:
  - Business name: Refinera
  - Location: Zhuhai, China
  - Integration: horizontal
- Investment: oil well
- Predicted project features:
  - Oil extracted: 1400L an hour
  - Time the machinery lasts before requiring maintenance: 5 years
  - Probability of finding oil: 56%
  - Type of well: offshore

Refinera struggled to establish itself in the regional market because of what scientists now know is a hydrocarbon shortage in the Zhuhai area. A decentralised organisational structure meant that communication across relevant business units was delayed with what needed to be a timely process. Being horizontally integrated meant that these delays caused losses at the other well sites due to a drain on the collective resources. To make up for this, a post hoc analysis concluded that oil was needed to be extracted at a rate of 2100L an hour and sites have at least a 67% probability of finding oil before management approved the project. Further, machinery needed to have thought to last at least 8 years before requiring maintenance, because maintenance costs further offset the initial investment after the 5 years of development. Further, the well was quite difficult to construct due to it being an offshore well, and so added additional financial setbacks over the course of the project.

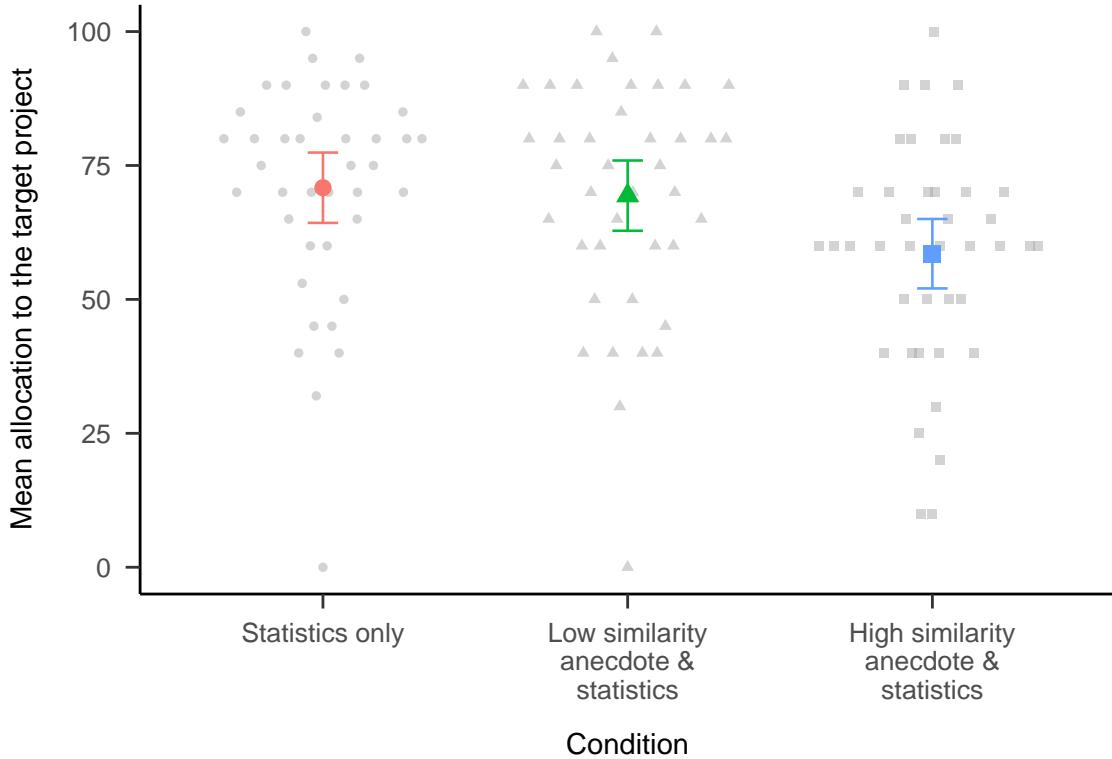
**Figure 6.4:** Anecdote for participants in the low similarity condition in Experiment 1.

### 6.2.2 Results

#### 6.2.2.1 The Effect of Similarity on Anecdotal Bias

Anecdotal bias was tested by comparing the statistics only condition with both the high- and low-similarity anecdote and statistics conditions (see Figure 6.5). The omnibus one-way ANOVA of these three conditions was significant,  $F(2, 118) = 4.19$ ,  $p = .018$ ,  $\hat{\eta}_p^2 = .066$ . Planned comparisons show that participants in the statistics only condition allocated a higher percentage of their budget to the target project compared with participants in the high-similarity anecdote with statistics condition,  $\Delta M = -12.31$ , 95% CI  $[-21.53, -3.09]$ ,  $t(118) = -2.64$ ,  $p = .009$ ; but not the low-similarity anecdote with statistics condition,  $\Delta M = -1.48$ , 95%

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity



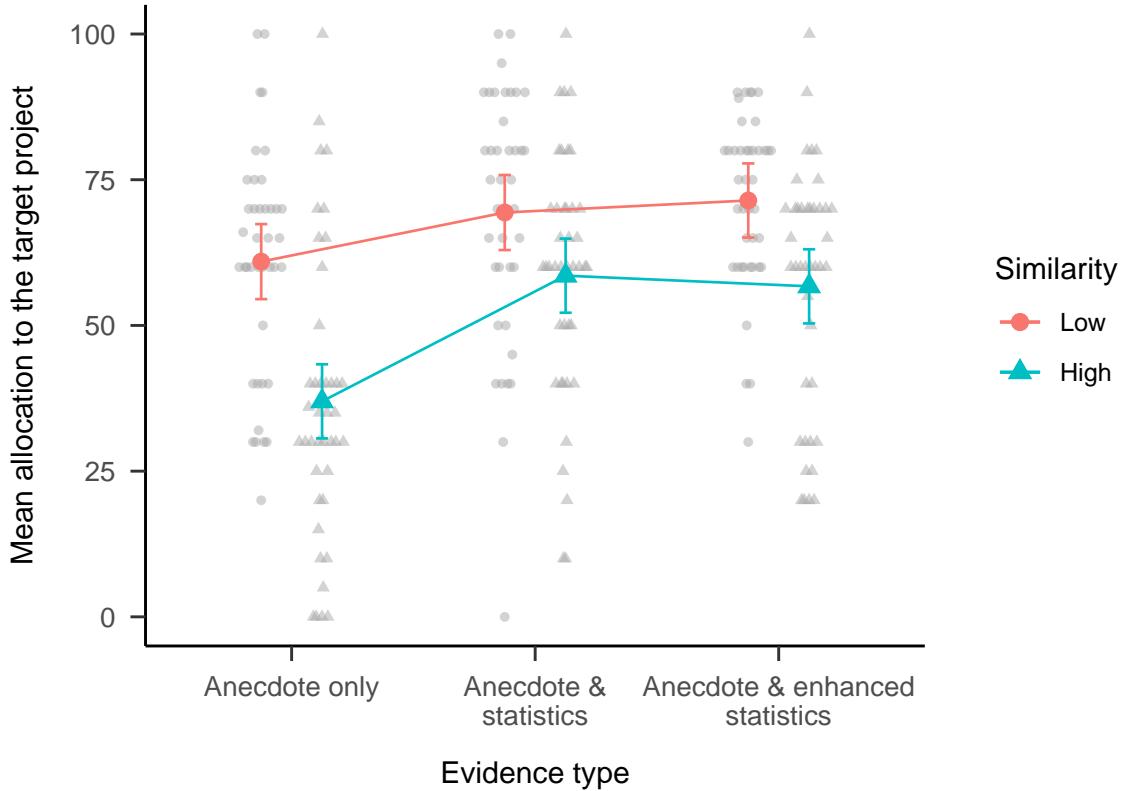
**Figure 6.5:** Mean allocation to the target project for the statistics only condition and the two anecdote & statistics conditions. Error bars represent 95% confidence intervals. Raw data are plotted in the background.

3663 CI [-10.75, 7.80],  $t(118) = -0.31$ ,  $p = .753$ . These findings provide evidence of  
 3664 anecdotal bias in the high similarity condition only.

### 3665 6.2.2.2 The Effect of Enhanced Statistics

3666 The effect of enhanced statistics was investigated by testing the interaction  
 3667 of anecdote similarity and evidence type (anecdote & statistics and anecdote &  
 3668 enhanced statistics conditions, excluding the anecdote only and statistics only  
 3669 conditions). As shown in Figure 6.6, the two-way interaction was not significant,  
 3670  $M = 3.89$ , 95% CI [-8.86, 16.65],  $t(238) = 0.60$ ,  $p = .548$ . Further, the differ-  
 3671 ence between the anecdote & statistics condition and the anecdote & enhanced  
 3672 statistics condition (averaged over similarity conditions) was also not significant,  
 3673  $\Delta M = -0.12$ , 95% CI [-6.50, 6.26],  $t(238) = -0.04$ ,  $p = .971$ . This suggests  
 3674 that providing participants with information about how to think statistically is  
 3675 not sufficient to facilitate a focus on statistics.

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

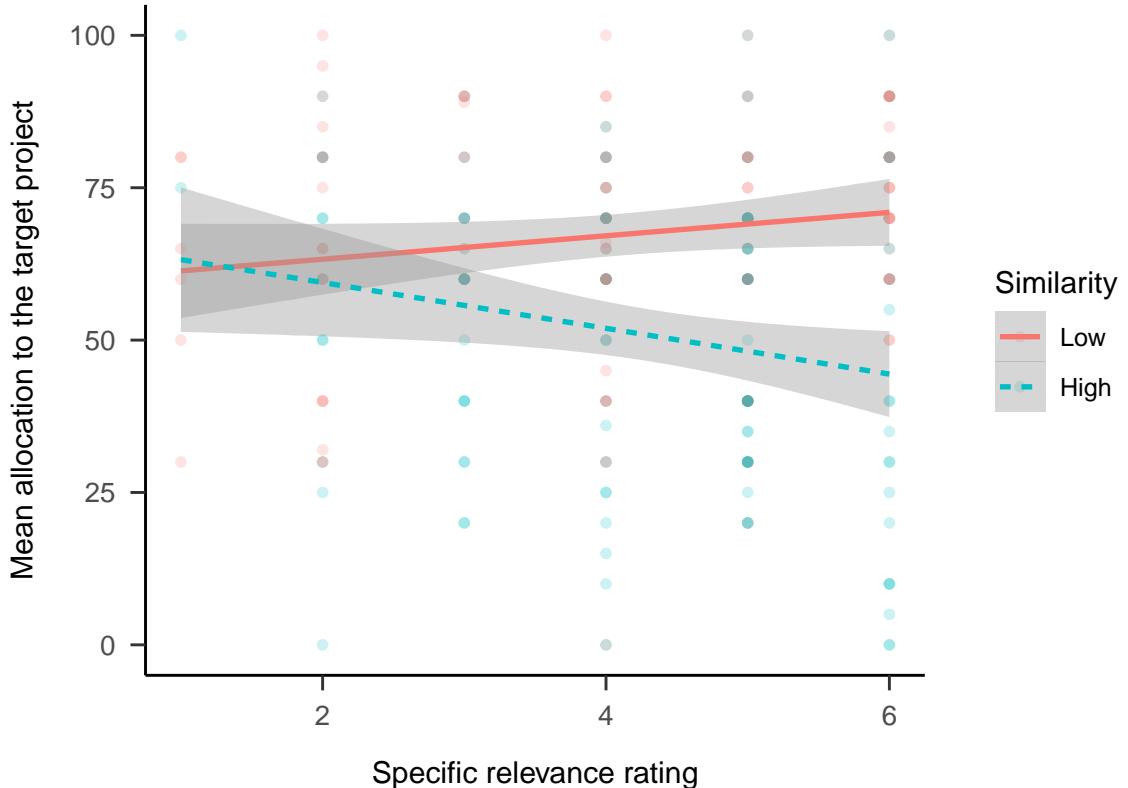


**Figure 6.6:** Mean allocation to the target project, by anecdote similarity and evidence type conditions (excluding the statistics only condition). Error bars represent 95% confidence intervals. Raw data are plotted in the background.

### 3676 6.2.2.3 The Effect of Statistics

3677 To identify the influence of statistics on participants' allocations, a two-way  
 3678 ANOVA of the interactions between anecdote similarity (low and high) and evi-  
 3679 dence type (anecdote only and anecdote & statistics conditions, excluding the anec-  
 3680 dote & enhanced statistics and statistics only conditions) was conducted (see Fig-  
 3681 ure 6.6). The interaction between anecdote condition and similarity (excluding the  
 3682 enhanced statistics condition) was significant,  $M = -13.14$ , 95% CI  $[-25.93, -0.34]$ ,  
 3683  $t(238) = -2.02$ ,  $p = .044$ . Specifically, the difference in allocations between the  
 3684 anecdote only condition and the anecdote & statistics condition was greater when  
 3685 the anecdote was similar,  $\Delta M = -21.56$ , 95% CI  $[-32.33, -10.80]$ ,  $t(238) =$   
 3686  $-4.72$ ,  $p < .001$ ; compared with when it was dissimilar,  $\Delta M = -8.43$ , 95% CI  
 3687  $[-19.32, 2.47]$ ,  $t(238) = -1.82$ ,  $p = .164$ . These findings provide evidence of  
 3688 partial anecdotal bias in the high similarity condition because the anecdote &

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity



**Figure 6.7:** Mean allocation to the target project, by specific relevance rating and similarity condition. LOESS method was used for smoothing over trials and the shading represents 95% confidence intervals. Raw data are plotted in the background.

3689 statistics condition was lower than the statistics only condition (shown above) but  
 3690 higher than the anecdote only condition.

### 3691 6.2.2.4 Relevance Ratings

3692 Regression analyses were conducted to determine the relationship between al-  
 3693 locations and the follow-up relevance ratings. As shown in Figure 6.7, the spe-  
 3694 cific relevance ratings interacted with similarity condition,  $b = -2.84$ , 95% CI  
 3695  $[-4.80, -0.87]$ ,  $t(240) = -2.85$ ,  $p = .005$ . It appears that specific relevance ratings  
 3696 were related to allocations, but only in the high similarity condition. Further, there  
 3697 were no significant associations with the general relevance ratings. This suggests  
 3698 that participants applied reasoning to the connection between the anecdote and the  
 3699 target project as opposed to simply reacting to the failed project and associating  
 3700 that with that project's industry.

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

### 3701 6.2.3 Discussion

3702 Hypothesis 6.1 was supported. Participants in the anecdote & statistics condi-  
3703 tion allocated less capital to the target project compared with those in the statistics  
3704 only condition. However, this effect depended on anecdote similarity because this  
3705 only occurred in the high similarity condition, not in the low similarity condition.  
3706 Thus, while anecdotal bias was evident when the anecdote was similar to the target  
3707 project, participants were not influenced when the causal mechanisms did not align.  
3708 Contrary to Hypothesis 6.2, despite being influenced by the anecdote, participants  
3709 still made some use of the statistics. This is different from the findings of Wainberg  
3710 et al. (2013), who found no difference between the anecdote only and anecdote and  
3711 statistics conditions, indicating a complete anecdotal bias effect. Hypothesis 6.3  
3712 was also not supported because the added enhanced statistical instructions used to  
3713 encourage participants to use the statistical information did not reduce participants'  
3714 reliance on anecdotes.

3715 Experiment 1 was limited because it only considered a *negative* anecdote; that  
3716 is, a failed project. In real life, however, case studies often have a *positive* valence;  
3717 that is, the story of a successful company. In fact, in business, it is possible that the  
3718 anecdotes used are more likely to be positive because of survivorship bias. Jaramillo  
3719 et al. (2019) found an anecdotal bias effect for negative but not positive anecdotes.  
3720 This may be because the stimuli consisted of medical decisions and, in this domain,  
3721 the loss of health may be more strongly noted than an equivalent gain in health. In  
3722 Experiment 2 (discussed in the subsequent section) a positive anecdote was added  
3723 to investigate whether anecdote valence would affect anecdotal bias.

3724 It is unclear whether the effects found in Experiment 1 were related to par-  
3725 ticipants' perceptions of the type of sampling used to select the anecdotes. The  
3726 instructions in Experiment 1 did not explain how the anecdote displayed to par-  
3727 ticipants was chosen. Whether sampling is believed to be intentional or random  
3728 has been shown to affect people's decision-making (e.g., Hayes et al., 2019). In the  
3729 present experiments, participants' sampling assumptions may have changed the

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

extent to which they used the anecdote in their decisions. For example, it may be rational to choose the anecdote over the aggregated data if (a) the anecdote was not sampled randomly from a pool of anecdotes, and (b) the anecdote had a greater similarity to the target project compared with other anecdotes in the pool in relevant ways. That is, if the anecdote were chosen because of its high relevance to the target project, it would be irrational to ignore it. In Experiment 1, it was unclear whether participants may have held these beliefs. To control for these assumptions, in Experiment 2, the instructions further clarified that the anecdote (a) was sampled randomly from a pool of anecdotes, and (b) was not significantly more similar to the target project than any of the other anecdotes in the pool.

### 6.3 Experiment 2

Experiment 1 replicated the anecdotal bias effect found in the literature. That is, participants allocated less capital to a project when presented with an anecdote and conflicting statistics compared with when they were presented with the statistics only. However, this effect depended on anecdote similarity, such that anecdotal bias was stronger when the anecdote was similar to the current task compared with when it was dissimilar. A negative anecdote only was used Experiment 1 because previous research has found anecdotal bias for negative but not for positive anecdotes (Jaramillo et al., 2019). However, Jaramillo et al. (2019) investigated medical decision-making, and the effect of anecdote valence may be different in a business context. In the study by Jaramillo et al. (2019), the positive anecdote involved a treatment that led to a reduction in symptoms, while the negative anecdote involved symptoms persisting. This framing may have led participants to perceive the positive anecdote as a return to a reference point and the negative anecdote as a continuation of a reduction in wellbeing relative to the reference point. In business, however, both successful and failed business projects represent a deviation from a reference point. To test this difference further, manipulation of anecdote valence was added to Experiment 2.

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

3758 To increase the experiment's power, anecdote valence and anecdote similarity  
3759 were manipulated within subjects. Further, Experiment 2 did not include the  
3760 anecdote & enhanced statistics condition because Experiment 1 found no evidence  
3761 for its effect. All participants saw the statistics only condition, which did not  
3762 contain an anecdote; therefore, this did not need to be manipulated between  
3763 subjects. Therefore, each participant was shown five displays: one for the statistics  
3764 only condition, and four for either the anecdote only condition or the anecdote &  
3765 statistics condition. These four anecdote displays consisted of the similarity (low  
3766 and high)  $\times$  valence (negative and positive) conditions.

3767 In Experiment 1, assumptions about the pool from which the anecdote was  
3768 sampled were not clarified. In Experiment 2, participants were told that the  
3769 anecdote was sampled randomly and that it was not uniquely similar to the target  
3770 project. This was expected to lead to a reliance on statistical evidence, regardless  
3771 of the anecdote's similarity. However, people often struggle to use statistical  
3772 concepts presented descriptively, as seen in the enhanced statistics condition in  
3773 Experiment 1, the neglect of variance shown in Chapter 4, and the lack of risk  
3774 aggregation in descriptive risky decisions shown in Chapter 2. Therefore, it was  
3775 expected that the results of Experiment 1 would be replicated for the negative va-  
3776 lence condition. Further, it was expected that there would be a reverse effect in the  
3777 positive valence condition. Appendix C.2 shows a simulation of the hypothesised  
3778 effects. Therefore, Experiment 2 tested the following hypothesis:

3779 **Hypothesis 6.4—overall effect.** The three-way similarity  $\times$  valence  $\times$  anecdote  
3780 (excluding statistics only) interaction is significant

3781 The main effect of interest was the effect of anecdote similarity on anecdotal bias.  
3782 However, because in Experiment 2 all participants were presented with the statistics  
3783 only condition, a difference score was calculated to simplify the analyses. Specif-  
3784 ically, this was the difference between the allocation in the anecdote & statistics  
3785 conditions and the relevant allocation in the statistics only condition. A score that  
3786 is different from zero indicates deviation from the allocation when only statistics

## *6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity*

3787 were shown. For positive valence, a stronger influence of anecdote is indicated  
3788 by a lower difference score; whereas for negative valence, a stronger influence of  
3789 anecdote is indicated by a higher difference score. Therefore, Experiment 2 tested  
3790 the following hypotheses:

3791 **Hypothesis 6.5—anecdotal bias difference score for negative anecdotes.**

3792 For negative anecdotes, the difference between budget allocations to the target  
3793 project in the statistics only condition and the anecdote & statistics condition will  
3794 be higher when the anecdote is similar to the target project compared with when  
3795 it is dissimilar.

3796 **Hypothesis 6.6—anecdotal bias difference score for positive anecdotes.**

3797 For positive anecdotes, the difference between budget allocations to the target  
3798 project in the statistics only condition and the anecdote & statistics condition will  
3799 be lower when the anecdote is similar to the target project compared with when it  
3800 is dissimilar.

3801 Contrary to both Wainberg et al. (2013) and Hypothesis 6.2, Experiment 1  
3802 found that participants do integrate statistics in their decisions to some extent.  
3803 This effect was expected to be replicated in Experiment 2. Therefore, Experiment 2  
3804 tested the following hypotheses:

3805 **Hypothesis 6.7—effect of statistics for negative anecdotes.** For negative  
3806 anecdotes, budget allocations to the target project will be higher for the high-  
3807 similarity anecdote & statistics condition than for the high-similarity anecdote  
3808 only condition.

3809 **Hypothesis 6.8—effect of statistics for positive anecdotes.** For positive  
3810 anecdotes, budget allocations to the target project will be higher for the high-  
3811 similarity anecdote only condition than for the high-similarity statistics & anecdote  
3812 condition.

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

**Table 6.2:** Experiment 2 group allocation.

Evidence type	N
Anecdote & statistics	48
Anecdote only	48
Total	96

### 3813 6.3.1 Method

#### 3814 6.3.1.1 Participants

3815 Ninety-six participants (50 female) were recruited from the online recruitment  
3816 platform Prolific. Participants were compensated at a rate of £5 an hour (Prolific  
3817 is based in the UK). The average age was 41.69 years ( $SD = 11.29$ ,  $min. = 27$ ,  
3818  $max. = 74$ ). Participants reported an average of 7.19 years ( $SD = 8.34$ ,  $min. =$   
3819 0,  $max. = 43$ ) working in a business setting, and an average of 3.91 years ( $SD$   
3820 = 7.67,  $min. = 0$ ,  $max. = 50$ ) of business education. The mean completion  
3821 time of the task was 14.98 min ( $SD = 8.84$ ,  $min. = 2.57$ ,  $max. = 58.71$ ). Table 6.2  
3822 shows the allocation of participants to the different conditions. Anecdote similarity  
3823 and valence were manipulated within subjects. Therefore, each participant was  
3824 assigned to one of two between-subjects evidence type conditions (anecdote only  
3825 and anecdote & statistics) and saw five displays (statistics only, and one of each  
3826 of the four similarity and valence conditions). Appendix C.2.1.1.1 describes the  
3827 power analysis conducted to arrive at this sample size.

#### 3828 6.3.1.2 Materials

3829 **6.3.1.2.1 Instructions** Participants were shown similar instructions to those in  
3830 Experiment 1 (see Section 6.2.1.2.1). The general instructions page included a test  
3831 of the basic information expressed in the instructions. This test also functioned as  
3832 an attention check. As in Experiment 1, participants were also shown instructions  
3833 that were specific to their condition. These were shown on the same page as the rest  
3834 of the project display, above the case study and focal projects. The instructions

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

clarified that the anecdote had been randomly sampled and that all anecdotes in the pool were equally similar to the target project. Appendix C.2.1.2.1 shows the instructions used in Experiment 2.

**6.3.1.2.2 Allocation Task** As in Experiment 1, the allocation task included a table describing the two focal projects and (apart from the statistics only condition) a description and analysis of an anecdote. Figures 6.8 and 6.9 show the anecdote and focal projects, respectively, for the negative valence, low similarity condition. Figures 6.10 and 6.11 show the anecdote and focal projects, respectively, for the positive valence, high similarity conditions. In the statistics only condition, participants were only shown the focal projects display. Appendix C.2.1.2.2 details the counterbalancing and randomisation used in the experiment.

**6.3.1.2.3 Interstitial Page** Prior to the display, participants were shown an interstitial page, which was used to (a) introduce the display and (b) check the participant's attention (given that no input was required, participants could easily skip the page without reading the text). See Appendix C.2.1.2.4.

**6.3.1.2.4 Follow-up Questions** Participants were shown similar follow-up questions as in Experiment 1, except that in Experiment 2, rating scales were 1–7 instead of 1–6. See Appendix C.2.1.2.3 for a sample display of the follow-up questions.

### 6.3.1.3 Procedure

Participants were introduced to the study via the general instructions page. They were then shown five sets (presented in a random order) containing two pages each: a page showing the allocation task and a page with follow-up questions (except for the anecdotes only condition, in which participants were not shown the follow-up questions page). Each allocation task page contained specific instructions relevant to the condition followed by the anecdote analysis and description, and the description of the two focal projects. The only exception was the statistics only display, for which there was no anecdote description or analysis.

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

### Case study

Cweb struggled to establish itself in the regional market because of changes in privacy laws (that reduced consumer confidence in the business' apps) in the Mumbai area. A centralised organisational structure meant that poor performers took longer to be replaced, so some tasks needed considerable revision. Being vertically integrated meant that the project was reliant on in-house manufacturing and so was slow to adopt the newest technologies used by competitors. A post hoc analysis concluded that, to make up for these issues, the developers needed to write at least 800 lines a day and the application needed to be certified with a security rating of at least 68%. Further, the number of potential first-year customers needed to be at least 2 million. Further, the problems in the application were slow to solve because of the lack of large-scale quantitative data due to it being for enterprise, and so added additional financial setbacks over the course of the project.

- Business details:
  - Business name: Cweb
  - Location: Mumbai, India
  - Integration: vertical
  - Structure: centralised
- Investment: software
- Predicted project features:
  - Code written: 600 lines a day
  - Security rating: 51%
  - Number of potential customers in first year: 2 million
  - Target users: enterprise

**Figure 6.8:** An example of the anecdote display in the negative valence, low similarity condition of Experiment 2.

### 3862 6.3.2 Results

3863 This section reports only the data relevant to the Experiment 2 hypotheses.

3864 See Appendix C.2.2 for manipulation check analyses and analyses of the follow-  
3865 up rating data.

#### 3866 6.3.2.1 Overall Effect of Manipulations

3867 As shown in Figure 6.12, the similarity  $\times$  valence  $\times$  evidence type interaction  
3868 (excluding the statistics only condition) was not significant,  $F(1, 94) = 3.42, p =$   
3869  $.067, \hat{\eta}_p^2 = .035$ . However, the similarity  $\times$  valence interaction was significant,

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

Target projects

Allocate your budget between the following two projects using percentage values  
(the two values should sum to 100):

Relevant information	Project 1	Project 2
<b>Business name</b>	Codeck	Enfuel
<b>Project type</b>	software	oil well
<b>Location</b>	Austin, USA	Houston, USA
<b>Integration</b>	horizontal	vertical
<b>Structure</b>	decentralised	centralised
<b>Predicted project features</b>	<ul style="list-style-type: none"> <li>• Code written: 1000 lines a day</li> <li>• Security rating: 85%</li> <li>• Number of potential customers in first year: 3 million</li> <li>• Target users: ordinary consumers</li> </ul>	<ul style="list-style-type: none"> <li>• Oil extracted: 2000L an hour</li> <li>• Time the machinery lasts before requiring maintenance: 7 years</li> <li>• Probability of finding oil: 80%</li> <li>• Type of well: onshore</li> </ul>
<b>Project allocation (%)</b>	Allocation: <input type="text"/>	Allocation: <input type="text"/>
<b>Overall reliability rating (%)</b>	91	90
<b>NPV (\$)</b>	901	100

**Figure 6.9:** An example of the focal projects in the negative valence, low similarity condition of Experiment 2. Here, Project 1 was the target project and Project 2 was the comparison project.

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

### Case study

Microxy performed really well in the regional market because of decreased silicon taxes in the Montreal area. A decentralised organisational structure meant that the individual teams had greater autonomy to complete their tasks, increasing the efficiency of important project stages. Being horizontally integrated meant that the project can be easily marketed to the customer base of the other business units in the company. A post hoc analysis concluded that, to take advantage of these benefits, the microchips needed to be produced at a rate of at least 3200 an hour and the semiconductor yield needed to be at least 57%. Further, the percent of compatible devices needed to be at least 71%. Further, the chip has a relatively low power consumption due to it operating Reduced Instruction Set Computing, and so added additional financial resilience over the course of the project.

- Business details:
  - Business name: Microxy
  - Location: Montreal, Canada
  - Integration: horizontal
  - Structure: decentralised
- Investment: microchip
- Predicted project features:
  - Microchips produced: 4800 an hour
  - Usable semiconductor yield after testing: 63%
  - Compatible devices in the market: 79%
  - Type of chip architecture: Reduced Instruction Set Computing

**Figure 6.10:** An example of an anecdote display in the positive valence, high similarity condition of Experiment 2.

3870  $F(1, 94) = 76.41, p < .001, \hat{\eta}_p^2 = .448$ , as was the evidence type  $\times$  valence  
3871 interaction,  $F(1, 94) = 10.11, p = .002, \hat{\eta}_p^2 = .097$ . The analyses below elaborate  
3872 on the specific hypothesised effects.

#### 3873 6.3.2.2 Anecdotal Bias Depends on Anecdote Similarity

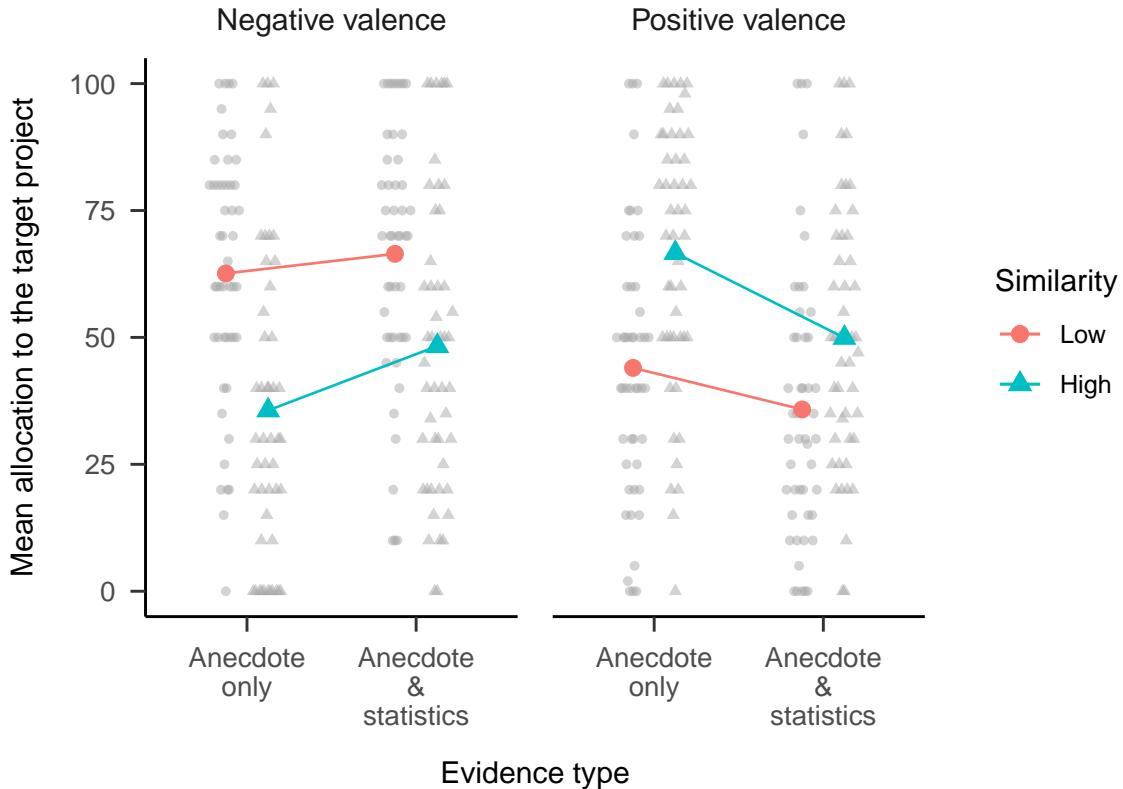
3874 To investigate whether anecdotal bias depended on anecdote similarity, the  
3875 differences in budget allocations between the statistics only condition and the two  
3876 anecdote & statistics conditions (high and low similarity) were calculated. The  
3877 values of the statistics only condition were different for each valence condition to  
3878 create equivalent comparisons. For the negative valence condition, participants

6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

Target projects		
Allocate your budget between the following two projects using percentage values (the two values should sum to 100):		
Relevant information	Project 1	Project 2
<b>Business name</b>	Solgistics	Altchip
<b>Project type</b>	shipping logistics	microchip
<b>Location</b>	Kuala Lumpur, Malaysia	Toronto, Canada
<b>Integration</b>	vertical	horizontal
<b>Structure</b>	centralised	decentralised
<b>Predicted project features</b>	<ul style="list-style-type: none"> <li>• Packages shipped: 800 a week</li> <li>• Number of orders that do not spend time in a bottleneck: 400 a day</li> <li>• Average accuracy of shipments: 90%</li> <li>• Shipping type: parcel</li> </ul>	<ul style="list-style-type: none"> <li>• Microchips produced: 4000 an hour</li> <li>• Usable semiconductor yield after testing: 60%</li> <li>• Compatible devices in the market: 75%</li> <li>• Type of chip architecture: Reduced Instruction Set Computing</li> </ul>
<b>Project allocation (%)</b>	Allocation: <input type="text"/>	Allocation: <input type="text"/>
<b>Overall reliability rating (%)</b>	93	90
<b>NPV (\$)</b>	905	105

**Figure 6.11:** An example of the focal projects in the positive valence, high similarity condition of Experiment 2. Here, Project 2 was the target project and Project 1 was the comparison project.

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity



**Figure 6.12:** Mean allocation to the target project, by evidence type, similarity, and valence conditions. In mixed factorial designs, error bars cannot be used to make inferences by “eye” across all conditions. Therefore, error bars are not included. Raw data are plotted in the background.

3879 allocated more money the high-NPV project; while for the positive valence con-  
 3880 dition, participants allocated more money to the low-NPV project. As shown in  
 3881 Figure 6.13, the similarity  $\times$  valence interaction was significant,  $F(1, 47) = 30.66$ ,  
 3882  $p < .001$ ,  $\hat{\eta}_p^2 = .395$ , as was the main effect of valence,  $F(1, 47) = 9.85$ ,  $p = .003$ ,  
 3883  $\hat{\eta}_p^2 = .173$ . The main effect of similarity was not significant,  $F(1, 47) = 0.53$ ,  
 3884  $p = .469$ ,  $\hat{\eta}_p^2 = .011$ .

3885 The effect of the anecdote is represented differently for each valence condition.  
 3886 As such, the interaction was further analysed by comparing the two similarity  
 3887 conditions for each valence condition. For negative anecdotes, the statistical values  
 3888 (e.g., NPV) associated with the target project were higher than those for the com-  
 3889 parison project. If participants were influenced by the negative anecdote they would  
 3890 therefore allocate less to the target. For negative anecdotes, a lower allocation to  
 3891 the target project is represented in Figure 6.13 as a positive value—the difference

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

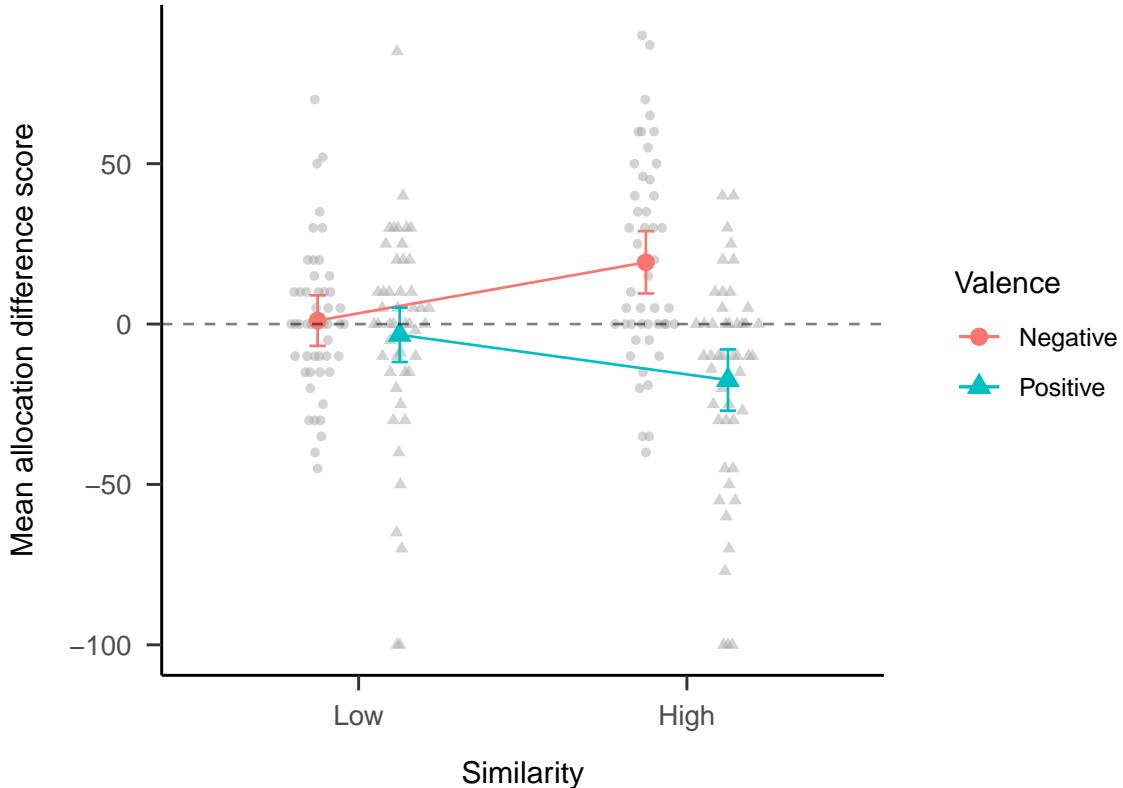
in allocation from when the participant did not see an anecdote. For positive anecdotes, the statistics were lower for the target project, so an influence of the anecdote is seen as a negative value in Figure 6.13. The hypothesised effect of negative anecdote similarity on anecdotal bias would suggest a higher difference score in high similarity than in low similarity. That is, more influence of the anecdote when it is similar than when it is dissimilar. For positive anecdotes a the hypothesised effect would suggest the reverse: a higher difference score in low similarity than in high similarity.

For negative anecdotes, the allocation difference was greater when the anecdote was similar to the target project than when it was dissimilar,  $\Delta M = -18.17$ , 95% CI  $[-26.17, -10.17]$ ,  $t(93.80) = -4.51$ ,  $p < .001$ . For positive anecdotes, the allocation difference was greater when the anecdote was dissimilar to the target project than when it was similar,  $\Delta M = 14.10$ , 95% CI  $[6.10, 22.11]$ ,  $t(93.80) = 3.50$ ,  $p = .001$ . This provides evidence that anecdotal bias depends on anecdote similarity for both negative and positive anecdotes. Participants appeared to be sensitive to the relevance of the anecdote to the target problem.

### 6.3.2.3 Effect of Statistics

As in Experiment 1, Experiment 2 investigated the extent to which statistical information influenced participants' allocations. As shown in Figure 6.12, for negative anecdotes, participants in the high-similarity anecdote & statistics condition allocated more to the target project than those in the high-similarity anecdote only condition,  $\Delta M = -12.67$ , 95% CI  $[-23.53, -1.81]$ ,  $t(336.36) = -2.29$ ,  $p = .022$ . For positive anecdotes, participants in the high-similarity anecdote only condition allocated more to the target project than those in the high-similarity anecdote & statistics condition,  $\Delta M = 16.71$ , 95% CI  $[5.85, 27.57]$ ,  $t(336.36) = 3.03$ ,  $p = .003$ . This provides evidence for the influence of statistics on participants' allocations for both negative and positive anecdotes.

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

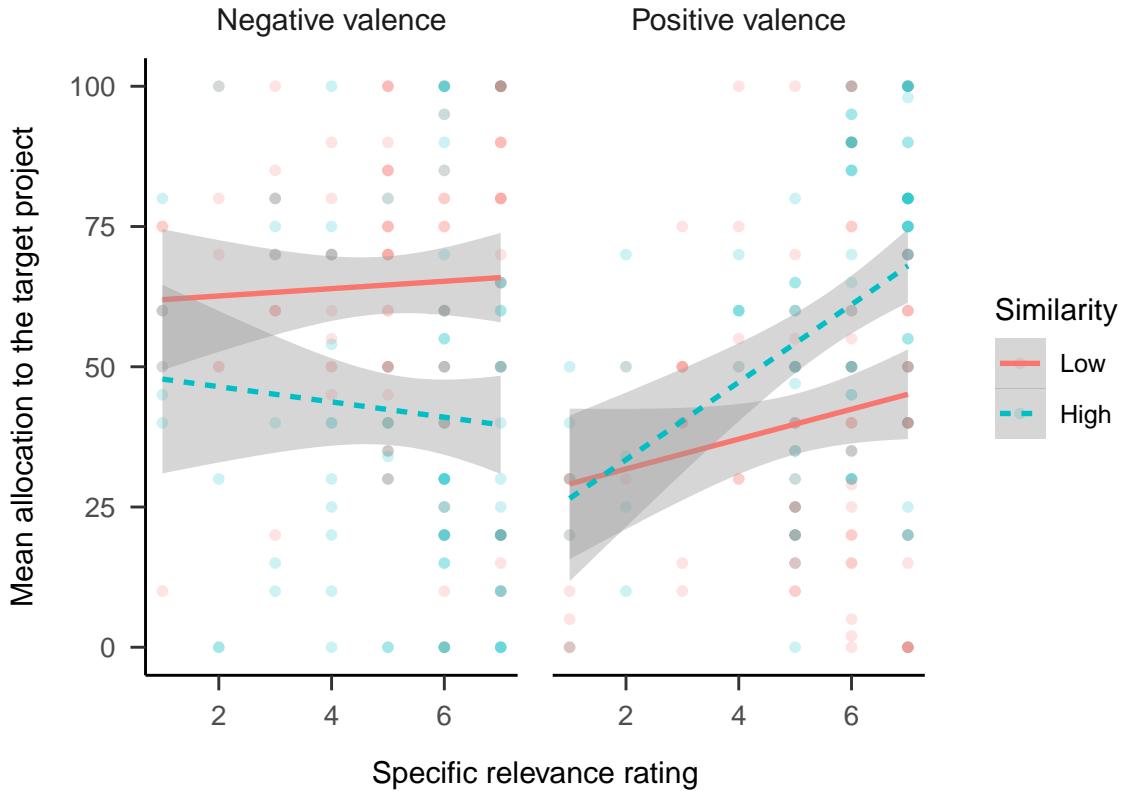


**Figure 6.13:** Mean allocation difference between the statistics only condition and the anecdote & statistics condition, by similarity and valence conditions. The horizontal dashed line shows the point in which the two allocations were equivalent. Values above this line show the higher allocation to the target project when participants were shown statistics only compared with when they were shown statistics with an anecdote. Error bars represent 95% confidence intervals, calculated from the within-subjects standard errors using the method from Cousineau and O'Brien (2014). Raw data are plotted in the background.

### 3919 6.3.2.4 Relevance Ratings

3920 Regression analyses were conducted to determine the relationship between allo-  
3921 cations and the follow-up relevance ratings. Figure 6.14 shows these data. While  
3922 the specific relevance ratings for negative anecdotes showed the same trends as in  
3923 Experiment 1, the interaction was not significant. Similarly, the ratings trends for  
3924 positive anecdotes were as hypothesised, but their interaction not significant. It  
3925 appears that specific relevance ratings were related to allocations, but only in the  
3926 high similarity condition. Further, there were no significant associations with the  
3927 general relevance ratings. This provides limited evidence that people were explicitly  
3928 reasoning about the connection between the anecdote and target.

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity



**Figure 6.14:** Mean allocation to the target project, by specific relevance rating, similarity condition, and valence condition. LOESS method was used for smoothing over trials and the shading represents 95% confidence intervals. Raw data are plotted in the background.

### 6.3.3 Discussion

Hypotheses 6.5 and 6.6 were supported because participants showed a stronger anecdotal bias effect when both positive and negative anecdotes had greater similarity to the target project. Further, as per Hypotheses 6.7 and 6.8, participants incorporated statistical information in their judgements, for both negative and positive anecdotes. Unlike in Experiment 1, the relevance rating data did not provide as clear indication that participants were using only the specific project information rather than merely its industry.

Therefore, Experiment 2 found that, unlike in the medical domain, the effect of anecdotes in financial decision-making does not depend on anecdote valence. Further, similar to the findings of Experiment 1, and unlike those of Wainberg et al. (2013), the anecdotal bias effect does not appear to be complete, with statistics still

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

3941 playing some role in participants' decisions, despite the effect of the anecdote.

## 3942 6.4 General Discussion

3943 Most of the hypotheses were supported. This chapter found that, in the  
3944 capital allocation context, people's decisions are influenced by anecdotes, even  
3945 when aggregated data are available. There were three novel findings: (a) the  
3946 anecdotal bias effect was only seen when participants considered the anecdote  
3947 sufficiently relevant to the target project, (b) participants integrated statistics  
3948 into their decisions, and (c) these effects were found in both negative and positive  
3949 anecdotes. Further, people did not consider verbal sample distribution information,  
3950 which could have helped to inform their decisions. This is surprising since other  
3951 work showed that generalisations are sensitive to sampling (Carvalho et al., 2021).

3952 The first novel finding from these experiments is that participants' use of  
3953 anecdotal evidence depended on the anecdote's similarity. Specifically, if the  
3954 anecdote appeared relevant, participants used it in their decisions. However, when  
3955 it appeared irrelevant, participants almost entirely relied on statistics. The findings  
3956 for high anecdote similarity are largely congruent with findings from other work  
3957 investigating anecdotal bias in business decision-making. As in Wainberg et al.  
3958 (2013) and Wainberg (2018), this chapter found that people allocated less capital  
3959 to a project when presented with statistical evidence and a similar but contradictory  
3960 anecdote than when they were presented with statistics alone.

3961 It appears that participants distinguished between the low- and high-similarity  
3962 anecdotes based on the structure of the anecdote. The low similarity condition  
3963 always included the same project type as the high similarity condition for all  
3964 domains. For instance, in one variation, both the high- and low-similarity anecdotes  
3965 involved oil well projects. However, the high-similarity anecdotes also matched the  
3966 target project in a number of specific features. This means that participants were  
3967 sensitive to the specific information in the anecdote description and analysis and  
3968 did not simply use the project type for their inferences. Further, participants'

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

answers to the follow-up questions indicated that they did not consider that the anecdote was necessarily relevant to other projects from the same industry. In other words, participants did not appear to carelessly use anecdotal evidence in their decisions; rather, they carefully considered the anecdote according to its particular causal structure.

The second novel finding from these experiments is that participants who were shown the anecdote with statistics did not completely disregard the statistical measures. Wainberg et al. (2013) found a complete anecdotal bias effect because results for the anecdote only and anecdote & statistics conditions were equivalent, meaning that the presented statistics had a negligible effect on participants' decisions. In contrast, the experiments discussed in this chapter showed a partial anecdotal bias effect, seen as a difference in allocations between the anecdote only and anecdote & statistics conditions. It appears that participants integrated both anecdotal and statistical information. This suggests that people's evaluation of evidence may be more sensitive than previously thought.

The discrepancy between these results and those in Wainberg et al. (2013) could be a result of the sampled population. Since Freling et al. (2020) found that anecdotes had a stronger effect when decisions were more personally relevant; thus, the managers recruited for the Wainberg et al. (2013) study may have simply been more personally invested in the task compared with the laypeople recruited for the experiments presented in this chapter. Similarly, Yang et al. (2015) found that anxiety increases anecdotal bias when making risky decisions. However, the discrepancy may also be attributable to the difference in the anecdote & statistics condition between the Wainberg et al. (2013) study and the present work. Specifically, the statistics presented in the anecdote & statistics condition in Wainberg et al. (2013) were not the same as those shown in the same study's statistics only condition, unlike in both the present experiments and Wainberg (2018). Instead, it was the anecdote & enhanced statistics condition that contained the same statistics as in the statistics only condition. This suggests that people only integrate statistics when they are sufficiently clear and no further interpretation is required.

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

3999        The third novel finding from these experiments is that anecdotal bias was found  
4000 for both negative and positive anecdotes. Most previous studies have included  
4001 negative anecdotes (i.e. those with negative consequences) such as a medication that  
4002 fails to reduce symptoms. However, there is little work in the literature involving  
4003 positive anecdotes (those with positive consequences). Jaramillo et al. (2019) found  
4004 an asymmetry in the anecdote effect—the effect of the anecdote was stronger when  
4005 the medication failed to improve symptoms (negative anecdote) compared with  
4006 when it did improve symptoms (positive anecdote). The present experiments found  
4007 a more symmetrical effect—the effects of both anecdotal bias and statistics were  
4008 found for both negative and positive anecdotes.

4009        The difference between the findings of this chapter and those of Jaramillo  
4010 et al. (2019) may be attributable to the latter's negative anecdote representing  
4011 a persistence in a negative shift from the status quo (i.e. good health). In the  
4012 business domain, both positive and negative anecdotes represent shifts from the  
4013 status quo (a company's financial position). Nevertheless, it was surprising to find  
4014 no asymmetry given the predictions of prospect theory. Loss aversion suggests  
4015 that participants will avoid projects that are similar to negative anecdotes more  
4016 than they will choose those similar to positive anecdotes. However, each choice  
4017 was associated with conflicting statistical information, so this may have cancelled  
4018 out the change from the reference point. Future research should use more realistic  
4019 incentives to investigate this effect further. Doing so will also increase the ecological  
4020 validity of the findings.

### 4021 6.4.1 Theoretical Implications

4022        The findings presented in this chapter add to the current understanding of  
4023 the way in which people use different types of evidence in their decision-making.  
4024 Previous research mostly investigated the relative influence of statistics and anec-  
4025 dotes by comparing anecdotal with statistical conditions. The current work shows  
4026 that comparing a joint anecdote & statistics condition with both an anecdote only  
4027 and statistics only condition enables a more specific investigation of participants'

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

4028 anecdotal bias. The influence of anecdotes can be seen in the comparison of  
4029 the statistics only and the anecdote & statistics conditions, while the effect of  
4030 statistics can be seen in the comparison of the anecdote & statistics condition and  
4031 the anecdote only condition. These two effects enable the determination of the  
4032 independent influences of anecdote & statistics. Use of such a design in future  
4033 research may help to further the understanding of conditions under which these  
4034 types of evidence are used.

4035 Some of the anecdotal bias literature is based on the assumption that using  
4036 anecdotal evidence over statistical evidence is necessarily irrational. This is likely  
4037 to have arisen from examples in the medical domain in which such decisions are  
4038 indeed irrational (e.g., believing that vaccines cause certain disorders, despite the  
4039 available evidence). In such cases, people over-rely on anecdotes and should be  
4040 relying more on aggregated data. However, a case could be made for the rational  
4041 use of an anecdote based on its similarity to the target problem. For instance,  
4042 there are times when an anecdote is so similar to the target situation (e.g., the  
4043 identical twin example discussed in Section 6.1.3) that it would be unwise not to  
4044 consider it. That is, the use of anecdote should depend on both (a) the extent of  
4045 underlying structural similarity to the target problem and (b) the distribution of  
4046 this similarity across the pool from which the anecdote was sampled. People should  
4047 use anecdotes if their causal structures are significantly more relevant compared  
4048 with other cases in the available data.

4049 However, similarity can also be misleading. For instance, if a case appears highly  
4050 similar but differs in terms of a key hidden dimension that is the real causal mech-  
4051 anism, then using the anecdote may be the wrong thing to do. What appears to  
4052 be important is being sensitive to relational rather than surface similarity. Future  
4053 research should investigate how varying participants' assumptions about sampling  
4054 from a data set of anecdotes influences their anecdotal bias. Such assumptions  
4055 can include the size of the sample, the shape of the distribution, and where in the  
4056 distribution the anecdote came from. Prior work found that people are sensitive  
4057 to distributional properties when generalizing (Carvalho et al., 2021), but it is

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

4058 not clear if this will replicate with descriptive cues such as in the experiments  
4059 in this chapter.

### 4060 6.4.2 Practical Implications

4061 The current work contributes to managerial decision-making by providing in-  
4062 sights into how managers make better decisions when using case studies and statis-  
4063 tical information. Managers of large companies are often in a difficult position; they  
4064 have incomplete information and are in an uncertain environment. Despite this,  
4065 different biases and responses to those biases may be anticipated for different levels  
4066 of uncertainty. For instance, a manager may be presented with both a convincing  
4067 case study that suggests a certain course of action as well as aggregated data. The  
4068 manager needs to be able to weigh the evidence accordingly.

4069 The work in this chapter suggests that there are three elements to consider: (a)  
4070 the quality of aggregated data (determined by factors such as sample size), (b) the  
4071 relative similarity of the cases in the data pool to the target situation, and (c) the  
4072 similarity of the anecdote to the target problem. For instance, an anecdote that is  
4073 similar to the target situation in terms of relevance and is significantly more similar  
4074 than other cases in the data set should carry more weight than an anecdote that  
4075 comes from a pool of cases that are all equally similar to the target problem. Lovallo  
4076 et al. (2012) found that similarity judgements increase prediction accuracy beyond  
4077 a simple regression model. Taking into account a project's relative similarity to  
4078 other cases is likely to further increase predictive validity.

4079 When aggregated data are not available, however, managers should rely more  
4080 on anecdotes that have greater similarities in terms of causal structure. That is,  
4081 they should be wary of merely using surface similarities to make inferences and  
4082 instead consider the underlying relational structures. The present data suggest  
4083 that laypeople can do this to some extent, with participants not being completely  
4084 swayed by the mere similarity of type of business project. However, future research  
4085 should investigate this further to better understand the boundaries of people's  
4086 analogical reasoning in capital allocation decisions.

## 6. Anecdotal Bias in Capital Allocation Depends on Anecdote Similarity

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*Work ... primarily concerned with the psychological processes that govern judgment and inference ... portrayed people as fallible, not irrational.*

—Amos Tversky

# 7

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## Discussion

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### Contents

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4180

4181	<b>7.1 Summary of Results</b> . . . . .	<b>151</b>
4182	<b>7.2 Theoretical Implications</b> . . . . .	<b>154</b>
4183	7.2.1 Statistical Concepts . . . . .	155
4184	7.2.2 Decision Aides . . . . .	157
4185	7.2.3 How Bounded is Bounded Rationality? . . . . .	158
4186	7.2.4 Expertise Effects . . . . .	158
4187	<b>7.3 Practical Implications</b> . . . . .	<b>159</b>
4188	<b>7.4 Conclusion</b> . . . . .	<b>161</b>
4189	<b>References</b> . . . . .	<b>163</b>

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4193     This thesis investigated the psychology of capital allocation decisions. The influence  
4194     of psychological factors on such decisions has not been sufficiently considered  
4195     in the literature despite their importance to the performance of hierarchical organisations.  
4196     This discrepancy is likely due to a greater focus of the role of organisational influences  
4197     on firm performance in the management literature. The thesis did not  
4198     investigate expertise effects, but instead focused largely on participants without  
4199     management experience. This allowed a study of the specific cognitive processes  
4200     without the potential confound of experience. Though, it is also worth noting that,  
4201     in the one case where the work examined people with management experience, the  
4202     pattern of results was largely the same as with naive participants. Each of the

## 7. Discussion

4203 empirical chapters investigated distinct but related processes that are relevant to  
4204 the capital allocation process. These chapters investigated whether people were  
4205 able to account for the benefits of aggregation when considering multiple projects  
4206 (Chapter 2), the influence of project feature alignability and metric variance when  
4207 comparing projects directly (Chapter 4), and the influence of project anecdote  
4208 similarity when the anecdote conflicts with statistical evidence (Chapter 6). Sec-  
4209 tion 7.1 will first summarise the results of the empirical chapters, and Sections 7.2  
4210 and 7.3 will then discuss their theoretical and practical implications, respectively.  
4211 Section 7.4 will conclude the thesis.

### 4212 7.1 Summary of Results

4213 Chapter 2 investigated participants' choice of risky business projects, when  
4214 these are displayed sequentially and without feedback in between decisions. This  
4215 design modelled the real-life situation that managers face in hierarchical organi-  
4216 sations: an evaluation of a set of separate business project proposals over time  
4217 with no immediate indication of the performance of those projects. Aggregating a  
4218 portfolio of such projects is likely to show a lower chance of potential loss overall  
4219 than might be originally assumed. The results from this chapter showed that  
4220 people not only did not do this spontaneously, but also were not facilitated by  
4221 manipulations that encouraged grouping choices together as a portfolio. People  
4222 only seemed to recognise the benefits of aggregation when they were presented with  
4223 an outcome probability distribution of the aggregated set of projects. There was no  
4224 strong evidence that more subtle manipulations aimed at encouraging aggregation  
4225 worked. Specifically, presenting projects together, specifying the total number of  
4226 projects, and presenting projects that were all from the same industry did not  
4227 reliably encourage aggregation.

4228 Chapter 4 investigated capital allocation when projects were evaluated jointly  
4229 and capital was allocated as a proportion of the budget, rather than a binary choice.  
4230 The main manipulation was whether all the project attributes were alignable, or

## *7. Discussion*

only the abstract financial metric (NPV) was alignable. NPV was also manipulated to be considered as either a reliable metric or not. This information was expressed either as explicit verbal instruction or as numerical ranges. The results showed that when reliability information was presented verbally, participants used NPV appropriately when all project attributes were completely alignable. That is, they used it when it was reliable and used the intrinsic project features when it was unreliable. When only NPV was alignable, participants relied on it almost exclusively. However, when reliability information was presented numerically, participants' allocation did not depend on the ranges—participants used NPV even when they had an opportunity to use the intrinsic features of the project. Overall, however, participants relied on NPV more when projects were low in alignment than when they were high in alignment.

Chapter 6 investigated the effect of anecdote similarity on allocations when the anecdote conflicted with the statistical data. Participants were asked to allocate a hypothetical budget between two projects. One of the projects (the target project) was clearly superior in terms of the provided statistical measures, but some of the participants also saw a description of a project with a conflicting outcome (the anecdotal project). This anecdotal project was always in the same industry as the target project. The anecdote description, however, either contained substantive connections to the target or not. Further, the anecdote conflicted with the statistical measures because it was either successful (positive anecdote) or unsuccessful (negative anecdote). The results showed that participants' decisions were influenced by anecdotes only when they believed that they were actually relevant to the target project. Further, they still incorporated the statistical measures into their decision. This was found for both positive and negative anecdotes. Further, participants were given information about the way that the anecdotes were sampled that suggested that the statistical information should have been used in all cases. Participants did not use this information in their decisions and still showed an anecdotal bias effect. Therefore, people seem to appropriately use anecdotes based on their relevance, but do not understand the implications of certain statistical concepts.

## 7. Discussion

Together, these results show the bounds of people's decision-making capability in capital allocation. The participants in these experiments in general behaved rationally but struggled to incorporate certain statistical concepts into their decisions. Further, when confronted with multi-attribute choice, participants tended to allocate capital using a trade-off strategy. This was seen in the conflict between intrinsic project attributes and NPV in Chapter 4 and the conflict between the anecdotal and statistical evidence in Chapter 6. Participants' allocations were informed by relevant factors when these were sufficiently clear (as in the verbal reliability condition in Chapter 4). However, participants struggled to do this when the factor involved using a relatively basic statistical concept. Each empirical chapter included such a concept: risk aggregation in Chapter 2, metric variance in Chapter 4, and sample distribution in Chapter 6. The aggregated distribution in Chapter 2 and the verbal reliability manipulation in Chapter 4 showed that a formal understanding of such concepts is not always necessary if they are expressed explicitly.

The statistical concepts used in these studies are all likely accessible for people without much formal mathematical knowledge. A basic concept of risk aggregation is clearly available to laypeople as seen in the responses to multi-play gambles (e.g., one vs. 100 gambles). Further, people certainly have a basic understanding of numerical ranges and that a wider range means more spread. Despite likely having this understanding, participants in the above experiments were unable to use it in the decisions. Similarly, other work has shown that generalisations are sensitive to sampling (Carvalho et al., 2021). Therefore, it is unlikely that the people in the thesis experiments simply lacked any understanding of these statistical concepts or (at least sensitivity to this kind of information). Instead there appear to be important contextual factors that critically support or prevent people from showing their intuitive understanding. Unfortunately, the methods used in this thesis more closely resemble real decisions managers make than the prior research that showed people do reason with these kinds of statistical concepts. Further, it is not clear that these effects will simply disappear with just more maths knowledge and business

## 7. Discussion

4291 experience. Previous work showed that expertise does not always remove biases  
4292 and in some cases it seems to augment such effects (e.g., Haigh & List, 2005).

### 4293 7.2 Theoretical Implications

4294 The main theoretical contribution of this thesis is the addition of evidence  
4295 that further specifies the conditions under which people make rational decisions  
4296 in capital allocation scenarios. People made good decisions most of the time,  
4297 but sometimes do not use relevant information in their decisions. Amos Tversky  
4298 explained in his response to Cohen (1981, p. 355) that the work on heuristics  
4299 and biases “portrayed people as fallible, not irrational.” That is, people are not  
4300 constantly making mistakes, but often behave rationally, largely due to adaptive  
4301 heuristics. However, sometimes shortcuts that are usually helpful can fail. Studying  
4302 such biases is similar to the way that optical illusions help understand the visual  
4303 system. In both cases, these are systems that most of the time function properly,  
4304 but sometimes reveal deficits.

4305 Similarly, Simon (1955) identified human rationality as *bounded*, meaning that  
4306 people’s cognitive processes are limited. The main aim of the thesis was to con-  
4307 tribute evidence for the ways that capital allocation decisions are bounded. To  
4308 this end, in each experiment, participants were given capital allocation scenarios  
4309 alongside both cues that describe their options and cues that frame the options in  
4310 different ways. Identifying which cues were used by participants in their decisions,  
4311 which cues were ignored, and which cues were integrated allowed to specify the  
4312 bounds of people’s cognitive capacity in these decisions. The experiments showed  
4313 that people struggle to use certain statistical concepts in their decisions, but that  
4314 they are also capable of making nuanced trade-offs and can be assisted by decision  
4315 aides. Understanding how decision-making in capital allocation is constrained and  
4316 biased is important in order to improve decision-making. Even if decisions are  
4317 largely consistent with normative concepts, falling prey to the biases identified in  
4318 this thesis can have severe consequences for organisations.

## 7. Discussion

### 4319 7.2.1 Statistical Concepts

4320 Chapter 2 presented participants with a capital allocation situation in which an  
4321 understanding of risk aggregation would have led to beneficial outcomes. Investing  
4322 in all the hypothetical projects would have led to a much higher chance of gaining  
4323 money than losing any. Each choice bracketing manipulation provided a hint of  
4324 the possibility of combining the choices in this way. However, participants did not  
4325 need to compute the aggregated value of the prospects themselves. An intuitive  
4326 understanding of aggregation involved considering that some of the gambles will  
4327 pay-off and make up for those that lost. However, this was not seen, with only weak  
4328 evidence that people were influenced by the more subtle choice bracketing manip-  
4329 ulations. Instead, people only seemed to respond to the concept of aggregation  
4330 when it was explicitly showcased. Showing people a distribution of the outcome  
4331 probabilities explicitly visualised the extent to which an aggregation of the risks  
4332 can lead to an incredibly low chance of loss.

4333 In Chapter 4, the NPVs that participants saw were critical to the allocation  
4334 task. In the low alignment condition, NPV was the only alignable attribute in the  
4335 comparison. In the high alignment condition, however, NPV was in competition  
4336 with the intrinsic project feature values. An understanding of how to use numerical  
4337 variance would have allowed participants to allocate capital according to the implied  
4338 reliability of the comparison metric. In the low alignment condition, NPV was the  
4339 only easy way to compare across projects, so it was a more useful cue than the  
4340 rest of the non-alignable values. However, in the high alignment condition, the  
4341 extent of numerical variance associated with each NPV could have been used to  
4342 determine NPV reliability. There were two ways to do this: (a) noticing that in the  
4343 low numerical reliability condition the ranges were all overlapping, and (b) noticing  
4344 the difference in the width of the ranges between the two within-subjects reliability  
4345 level conditions. By doing this, participants would have then been able to know  
4346 to (in the high alignment condition) use NPV when ranges were narrow and use  
4347 the intrinsic values more or exclusively when ranges were wider and overlapping.

## 7. Discussion

4348 Participants were able to do this sort of conditional allocation when reliability was  
4349 expressed explicitly as words, but not when it was expressed numerically.

4350 In Chapter 6, participants did not make use of descriptive information about  
4351 the anecdote sample distribution. As in Chapter 4, participants were confronted  
4352 with a conflict of cues: statistical information vs. a potentially relevant anecdote.  
4353 Regardless of the similarity manipulation, a consideration of the sample from  
4354 which the anecdote was sampled should have informed how the anecdote was used.  
4355 Imagine a distribution that represents the similarity of all the individual projects in  
4356 the sample. That is, the x-axis represents the similarity to the target project and  
4357 the y-axis is the frequency of projects that represent each level of similarity. Even if  
4358 the sampled anecdote appears very relevant to the target project, if the underlying  
4359 distribution of the sample is highly negatively skewed, such that most projects in  
4360 the sample are equivalently similar to the target, then the sampled anecdote is  
4361 not necessarily more informative than the aggregated measure. On the other hand,  
4362 if the underlying distribution is positively skewed, normally distributed, or even  
4363 uniform, then the fact that the sampled anecdote appears highly relevant to the  
4364 target project may actually mean that it is more informative than the aggregated  
4365 measure. Prior work shows that people can reason about distributions effectively  
4366 when experiencing the sampling directly (e.g., Carvalho et al., 2021; Hertwig et al.,  
4367 2004). Chapter 6 shows that people struggle to use this information when it is  
4368 described verbally.

4369 While people struggled to understand and use certain statistical concepts they  
4370 still seemed to be able to integrate multiple cues and create trade-offs. As discussed  
4371 in Chapter 5, both Chapters 4 and 6 provided participants with more than one cue  
4372 to use for project evaluation. In Chapter 4, people seemed to strike a trade-off  
4373 between NPV and the intrinsic project features as opposed to choosing one or  
4374 the other with a consistent strategy. In Chapter 6, the anecdotal and statistical  
4375 evidence provided conflicting cues for each target project. However, participants  
4376 allocated as if both the anecdotes and statistics had some relevance. Similar  
4377 to the above, participants appeared to integrate the influence of these two cues,

## 7. Discussion

as opposed to picking a consistent evidence reliance strategy for their allocation decisions. These findings might be explained through satisficing (Simon, 1955) or a constraint satisfaction model (e.g., Glöckner et al., 2014). Future research can test these explanations, as well as further clarify to what extent constructs such as need for cognition or mathematical skill may explain individual differences.

### 7.2.2 Decision Aides

While trade-offs allow people to integrate multiple cues, decision aides allow people to use statistical concepts for more nuanced decision-making. Chapter 2 found that people's understanding of risk aggregation was facilitated when the mathematical work was done for them and the aggregated values were displayed visually as a distribution. However, a follow-up experiment to Chapter 4 (detailed in Appendix B.7) found that even explicit instructions sometimes do not work. That is, even explaining the way that ranges can be used as reliability information and telling participants how to implement this in the capital allocation task did not facilitate proper use of ranges.

Future work should investigate the impact of visualisation on people's use of variance information in these situations. Much work has investigated visualising uncertainty information (Bostrom et al., 2008; Brodlie et al., 2012; T. J. Davis & Keller, 1997; Johnson & Sanderson, 2003; Kinkeldey et al., 2017; Kox, 2018; Lapinski, 2009; Lipkus & Hollands, 1999; Lipkus, 2007; MacEachren, 1992; Padilla et al., 2018; Pang et al., 1997; Potter et al., 2012; Ristovski et al., 2014; Spiegelhalter et al., 2011; Torsney-Weir et al., 2015). For instance, a Hypothetical Outcome Plot (Hullman et al., 2015; Kale et al., 2019) expresses variance information as dynamic plots and is one method that is likely to be beneficial to people's understanding of ranges as used in this thesis. Visualisation could also apply to the work in Chapter 6. Using a visual array as in Jaramillo et al. (2019) is likely to facilitate people's understanding of the importance of statistical evidence over anecdotes. However, an additional visualisation of the distribution of the underlying similarity to the target may also be necessary to facilitate understanding of the relevance of the

## 7. Discussion

4407 sample distribution. Ultimately, visualisations of the effects of certain statistical  
4408 concepts may be necessary for people to use them appropriately.

### 4409 7.2.3 How Bounded is Bounded Rationality?

4410 The boundary between the cues that participants were able to use and the  
4411 statistical concepts that they did not use is unclear. That is, the cues that they  
4412 were able to use were not trivial, and the concepts that they were not able to use are  
4413 relatively basic. For instance, the finding in Chapter 6 that people were able to use  
4414 relevance information to guide their allocations shows an ability to quite specific  
4415 information to inform choice. On the other hand, the statistical concepts that  
4416 participants ignored in each empirical chapter are all relatively intuitive. While  
4417 concepts of aggregation, variance, and sample distribution are typically studied at  
4418 a tertiary level, they can be understood when acted out or experienced.

4419 Clark and Karmilff-Smith (1993) proposed a distinction between two levels of  
4420 representing knowledge. At the *implicit* level an individual can only make use of a  
4421 certain system of knowledge, while it is only at the *explicit* level that they develop  
4422 insight into that system. For instance, young children can use closed class words  
4423 such as “the” or “to,” but only identify them as words later in development. Further,  
4424 children’s play often implicitly contains many mathematical concepts, despite the  
4425 children’s struggle to explicitly reason with the exact same concepts in more formal  
4426 problem-solving (Sarama & Clements, 2009). Adults may have a similar distinction  
4427 in knowledge representation. Concepts that can be used when experienced directly,  
4428 such as in risky choice from experience, are not represented in a way that they can  
4429 be used when presented descriptively, such as in risky choice from description. This  
4430 kind of distinction may explain why participants in the thesis experiments failed  
4431 to use concepts that have been shown to be accessible to laypeople.

### 4432 7.2.4 Expertise Effects

4433 Future research should investigate the potential expertise effects that may  
4434 influence the findings of the thesis. This is important because of the potential

## 7. Discussion

downstream effects of biased managerial decision-making. For instance, it is unclear to what extent psychological factors such as the ones discussed in this thesis may account for the finding that undiversified firms often perform better than diversified firms. On the one hand, business professionals tend to work with numbers, so the effects found in this thesis may be less pronounced for them. For instance, Smith and Kida (1991) reviewed the heuristics and biases literature and concluded that certain cognitive biases are not as strong for accounting professionals as they are for naive participants.

On the other hand, these effects may actually be stronger in managers. For instance, Haigh and List (2005) found that professional traders show more myopic loss aversion than students. Chapter 2 showed that people tend to consider risky choices one at a time and therefore tend to be more risk averse to a set of projects than they would be if the risks were aggregated. Managers might be even more risk averse in these situations because of the increased stakes for their jobs. Lovallo et al. (2020) discussed the ways in which managers tend to have a blind spot for such project evaluations: they aggregate their personal stock market portfolio, but not their intra-firm project portfolio.

Chapter 4 found evidence of variance neglect for both laypeople and Master of Management students. Further, in the case of the work in Chapter 6, it is possible that business managers prefer anecdotal cases to inform their decisions because of their higher salience, compared to statistical data. Managers are also more likely to feel as if the situation is relevant to them, which according to Freling et al. (2020) would predict more anecdotal bias.

### 7.3 Practical Implications

The findings of this thesis have a number of potential implications for managerial decision-making. Despite the uncertainty about potential expertise effects, this section assumes that the findings of the thesis generalise to experienced managers, if not in degree, at least qualitatively. Management researchers have suggested ways

## 7. Discussion

of overcoming psychological biases in managerial decision-making ever since such biases were identified. Many practitioner-oriented papers have used the findings of the judgement and uncertainty literature both to explain managerial decision-making processes and to suggest ways of reducing bias (Courtney et al., 1997; Courtney et al., 2013; Hall et al., 2012; Koller et al., 2012; Lovallo & Sibony, 2014; Sibony et al., 2017), with only some specifically focused on capital allocation decisions (Birshan et al., 2013). This section will review some of the implications the findings of this thesis may have on both organisational policies and manager decision-making.

The findings of Chapter 2 show that the framing of business project proposals is important for the way that people perceive their risk. Specifically, in order to better account for the risks of business projects it is important to (a) make it easier for managers to group projects together, and (b) aggregate a portfolio of projects for them. This suggests implementing organisational changes that will facilitate the capital allocation process. For instance, Lovallo et al. (2020) suggested that companies change the frequency that they evaluate projects to better allow for an aggregation of the projects. Doing this will enable an explicit computation of the aggregated values and therefore a visualisation of the outcome probability distribution. Such a process could facilitate aggregation without a need to rely on managers' intuition during sequential project evaluation decisions.

One implication of Chapter 4 is that it is important to expose the variance that underlies abstract financial measures. Further, translating such numerical variance estimates into clear verbal information would help facilitate managers' understanding and implementation of such estimates. Organisational changes could include reducing diversification so that there is less reliance on abstract metrics. This would allow for more of a comparison between alignable project attributes, potentially reducing forecast error. Koller et al. (2017) found that companies with more similar business units report faster growth and greater profitability than competitors, compared with companies with dissimilar business units. Further,

## 7. Discussion

4492 companies can also work to develop better metrics and establish norms about how  
4493 much to discount a metric given its underlying variance.

4494 The main implication of Chapter 6 is that managers should pay attention to  
4495 the way that they compare target projects to other cases. It is important to collect  
4496 prior cases that are relevant, and to have as many such cases as possible. Ideally,  
4497 each such prior case should be weighed by similarity (Lovallo et al., 2012). If this  
4498 is done, the prior distribution of the similarity of the sample would be taken into  
4499 account when computing subsequent aggregation. When identifying such similarity  
4500 ratings, it is important to focus on relevant underlying structure. This would  
4501 reduce any erroneous connections to cases that only have a mere surface similarity.  
4502 This distinction is also relevant in a situation in which only one prior case can be  
4503 found. Research on analogy shows that analogical comparison helps expose the  
4504 underlying relational structure between objects (e.g., Kurtz et al., 2013; Markman  
4505 & Gentner, 1993). Therefore, managers should take care to first identify such  
4506 relational structures first before making subsequent inferences.

4507 Addressing these psychological effects will help eliminate some of the biases  
4508 in the capital allocation process, but will not address other related biases. For  
4509 instance, the above effects all involve decisions that require an evaluation of fi-  
4510 nancial forecast estimates such as future cash flows and the related uncertainty.  
4511 Therefore, a further source of error could arise from the initial estimation of these  
4512 probability and cash flow values. For instance, such estimates could be influenced  
4513 by optimism or confidence biases. These biases, however, can in turn also be  
4514 addressed (Flyvbjerg et al., 2018).

## 4515 7.4 Conclusion

4516 Capital allocation decisions are consequential for large organisations. This the-  
4517 sis tested the conditions under which people behave rationally or are fallible when  
4518 allocating capital. The experiments found that participants struggle to incorporate  
4519 concepts such as risk aggregation, estimate variance, and sample distribution into

## *7. Discussion*

4520 their decisions. Participants only seemed to be able to do this when the concept  
4521 was expressed visually very explicitly. However, when there were multiple cues  
4522 for choice evaluation, the results also showed that participants were capable of  
4523 integrating conflicting information in their decisions. Identifying such cognitive  
4524 bounds helps to better understand how people evaluate multiple choices and helps  
4525 future research develop methods to facilitate better decisions.

## 7. Discussion

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# A

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## Chapter 2 Appendix

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### Contents

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4655

4656	<b>A.1 Experiment 1</b>	167
4657	A.1.1 Method	167
4658	A.1.2 Results	169
4659	<b>A.2 Experiment 2</b>	170
4660	A.2.1 Method	170
4661	A.2.2 Results	171
4662	<b>A.3 Experiment 3</b>	173
4663	A.3.1 Method	174
4664	A.3.2 Results	178
4665	A.3.3 Discussion	183
4666	<b>A.4 Experiment 4</b>	185
4667	A.4.1 Method	185
4668	A.4.2 Results	188
4669	A.4.3 Discussion	193

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4672

4673     This appendix contains supplementary materials and analyses for the two ex-  
4674     periments reported in Chapter 2. In addition, it also report two experiments that  
4675     were conducted to follow-up the findings in Experiments 1 and 2. Both follow-up  
4676     experiments tested project choice as in the first two experiments, but Experiment 3  
4677     further investigated the effect of similarity, and Experiment 4 further investigated  
4678     the effect of awareness.

4679     All four experiments featured probability outcome distributions. These were

## A. Chapter 2 Appendix

Imagine that you are an executive in a large company composed of many individual businesses.

You will see various projects from these businesses and have to decide whether you would like to invest in them.

Imagine that making good investment decisions will result in you receiving a generous bonus and a potential promotion, and that doing poorly will result in you receiving a large pay cut and a potential demotion.

We want to know what choices you would actually make in these scenarios.

[Continue](#)

**Figure A.1:** Experiment 1 instructions.

4680 Poisson binomial distributions that were calculated using the R package `poibin`,  
4681 which uses calculations described in Hong (2013).

## A.1 Experiment 1

### A.1.1 Method

#### A.1.1.1 Materials

4685 **A.1.1.1.1 Instructions** Participants were shown the instructions in Figure A.1.

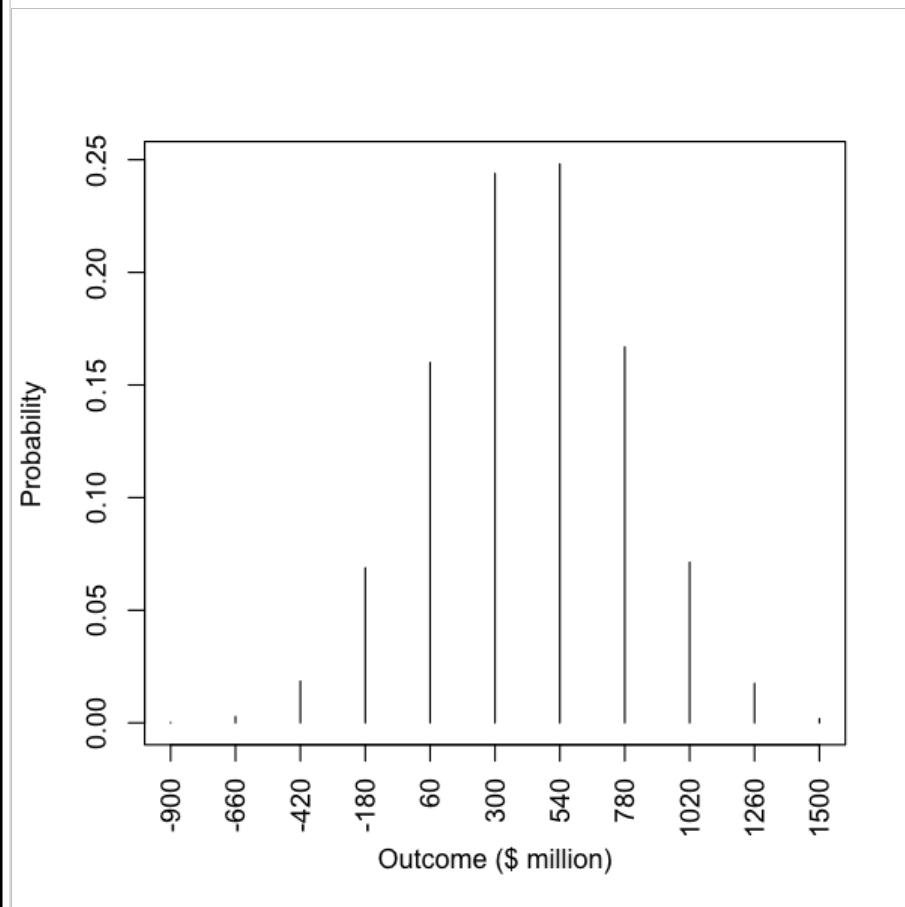
4686 **A.1.1.1.2 Outcome Distribution Decision** Figure A.2 shows the outcome  
4687 distribution display that participants saw in Experiment 1.

#### A.1.1.1.3 Follow-up Gambles

4689 **Negative EV Gambles** It was important to make sure that participants  
4690 were generally making decisions that were in line with EV theory and that the  
4691 sample was not abnormally risk tolerant. As such, participants saw two project  
4692 decisions that had a negative EV. Out of the 396 negative EV gambles included  
4693 (two per participant), all but four were rejected.

A. Chapter 2 Appendix

Below is the distribution of final outcomes after the last 10 investments you just saw. That is, each possible outcome is shown on the x-axis, and the probability of each outcome is shown on the y-axis. Regardless of what you decided previously, would you invest in all 10 of those investments, given the below distribution?



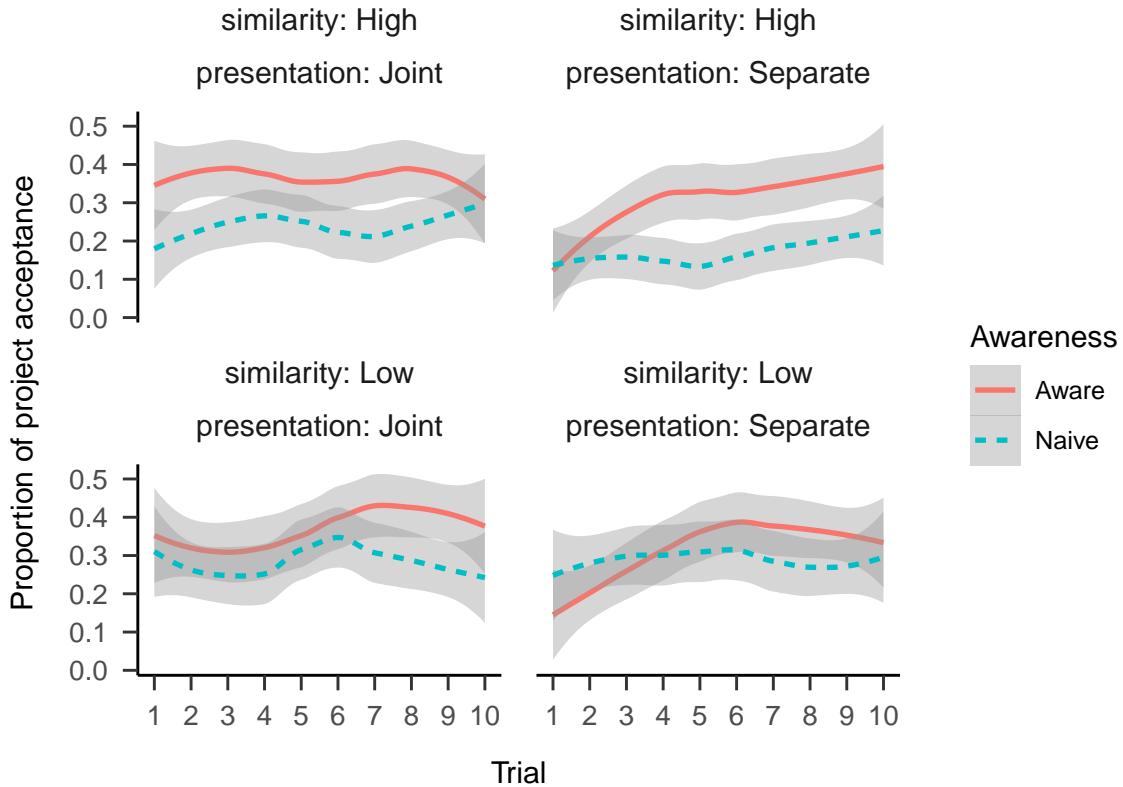
\*

Yes  No

[Continue](#)

Figure A.2: The outcome distribution of the 10 gambles used in Experiment 1.

A. Chapter 2 Appendix



**Figure A.3:** Proportion of project acceptance by trial, similarity, awareness, and presentation conditions. LOESS is used for smoothing over trials, and the shading represents 95% confidence intervals.

4694        **Samuelson (1963) Gambles** Participants saw the original Samuelson (1963)  
 4695        gamble, were asked whether they would accept 10 of that gamble, and whether they  
 4696        would accept those 10 given the associated outcome distribution. They then saw  
 4697        the same three questions, but using outcome magnitudes that were similar to the  
 4698        ones in the risky investment task. That is, \$100 million instead of \$100.

4699        **Redelmeier and Tversky (1992) Gambles** Participants saw the same  
 4700        three types of gambles (single, 10, and aggregated), but with the values from the  
 4701        gambles that were used by Redelmeier and Tversky (1992).

4702        **A.1.2 Results**

4703        **A.1.2.1 Trial-by-Trial Analysis**

4704        Figure A.3 shows proportions of project acceptance across all conditions and tri-  
 4705        als.

## A. Chapter 2 Appendix

### 4706 A.1.2.2 Outcome Distribution

4707 A paired-samples t-test was conducted to compare participants' decision to  
4708 invest in the 10 projects while seeing an aggregated distribution, and their deci-  
4709 sions to invest in the projects individually, without the distribution. Participants  
4710 invested in the 10 projects more when seeing the distribution both in the separate  
4711 presentation phase,  $t(197) = 5.48, p < .001, d_z = 0.50, 95\% \text{ CI } [0.31, 0.68]$ ; and in  
4712 the joint presentation phase,  $t(197) = 4.17, p < .001, d_z = 0.37, 95\% \text{ CI } [0.19, 0.56]$ .

4713 However, it was subsequently discovered that the code that generated this  
4714 distribution mistakenly flipped the outcome values. This means that although  
4715 it appeared from the distribution that the probability of loss was 0.09, the actual  
4716 probability of loss of the underlying values given the correct distribution was 0.26.  
4717 As such, even though Experiment 1 found an effect of distribution, it was unclear  
4718 if the effect was driven by participants actually accurately assessing the riskiness  
4719 of the individual gambles, and therefore showing a difference between the isolated  
4720 and aggregated gambles in a normative way.

## 4721 A.2 Experiment 2

### 4722 A.2.1 Method

#### 4723 A.2.1.1 Participants

4724 **A.2.1.1.1 Power Analysis** The power analysis was conducted using the `pwr`  
4725 package (Champely, 2020a), based on the presentation effect size from Experi-  
4726 ment 1, since it was the smallest effect. The analysis suggested that a minimum  
4727 sample size of 164 ( $41 \cdot 4$ ) was required for the presentation effect with an expected  
4728 power of at least 80%.

#### 4729 A.2.1.2 Materials

4730 **A.2.1.2.1 Follow-up** Figure A.4 shows the project number question. The  
4731 maximum value that they could enter was set to 20. Figures A.5 and A.6 ask  
4732 participants whether they are willing to take all or none of the projects; and how

## A. Chapter 2 Appendix

In total, how many projects did you just see?

projects

Continue

**Figure A.4:** Experiment 2 project number question.

many projects would they choose if they could pick randomly (maximum value was set to 20). Those in the distribution absent condition were asked the same questions, but without the distribution and its explanation.

### A.2.2 Results

#### A.2.2.1 Follow-up

**A.2.2.1.1 Project Number** Participants were asked how many projects they thought they saw. Figure A.7 shows that overall people correctly estimated the number of projects, with more accuracy for those in the aware condition.

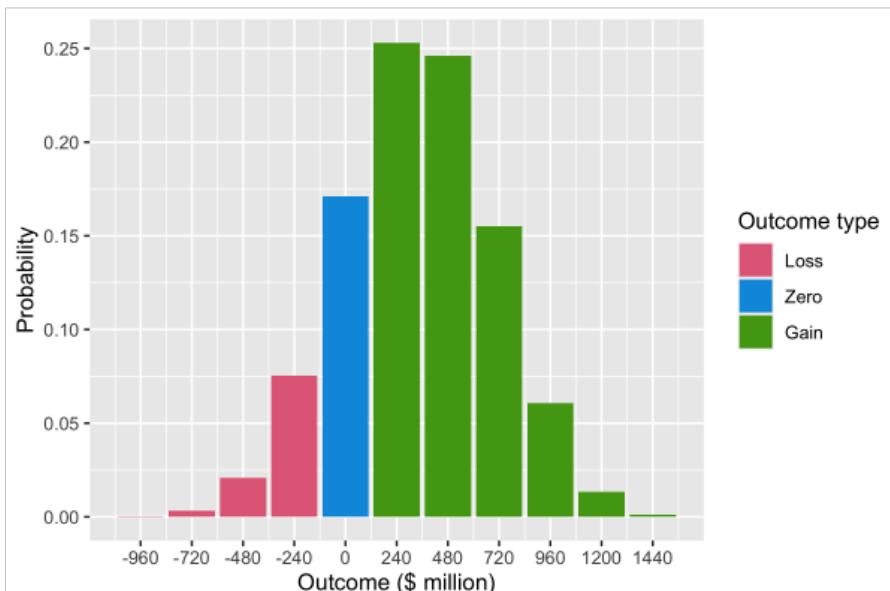
**A.2.2.1.2 Portfolio Choice - Binary** Participants were then asked if they would rather invest in all or none of the projects. As Figure A.8 shows, the difference between presentation conditions was not significant,  $\hat{\beta} = 0.15$ , 95% CI  $[-0.29, 0.60]$ ,  $z = 0.67$ ,  $p = .500$ . The awareness effect was also not significant,  $\hat{\beta} = 0.28$ , 95% CI  $[-0.17, 0.72]$ ,  $z = 1.21$ ,  $p = .225$ . However, those that saw a distribution chose to invest in all 10 projects significantly more (71.43%) than those that did not see a distribution (36.59%), .

**A.2.2.1.3 Portfolio Choice - Number** Subsequently, participants were asked how many projects they would invest in out of the 10 that they saw. As Figure A.9 shows, the difference between presentation conditions was not significant,  $d_s = 0.08$ , 95% CI  $[-0.35, 0.52]$ ,  $t(80) = 0.38$ ,  $p = .706$ . The awareness effect was also not significant,  $d_s = 0.09$ , 95% CI  $[-0.34, 0.53]$ ,  $t(79) = 0.42$ ,  $p = .678$ . However, those that saw a distribution chose to invest in significantly more projects than those that did not see a distribution,  $d_s = 0.60$ , 95% CI  $[0.15, 1.03]$ ,  $t(81) = 2.70$ ,  $p = .009$ .

## A. Chapter 2 Appendix

Below is the probability distribution of final outcomes if all projects were chosen.

The numbers on the x-axis (labelled 'Outcome') represent the final amounts of money possible if you chose to invest in all the projects. The numbers on the y-axis (labelled 'Probability') represent the likelihoods of each of the possible outcomes. Negative final outcomes (losses) are shown in red, positive final outcomes (gains) are shown in green, and a final outcome of zero (no loss or gain) is shown in blue.



Indicate below whether you would invest in the following:

**Consider all the projects you saw. If you had to choose between investing in all of them, or investing in none of them, which would you choose?**

**Figure A.5:** Experiment 2 binary portfolio question.

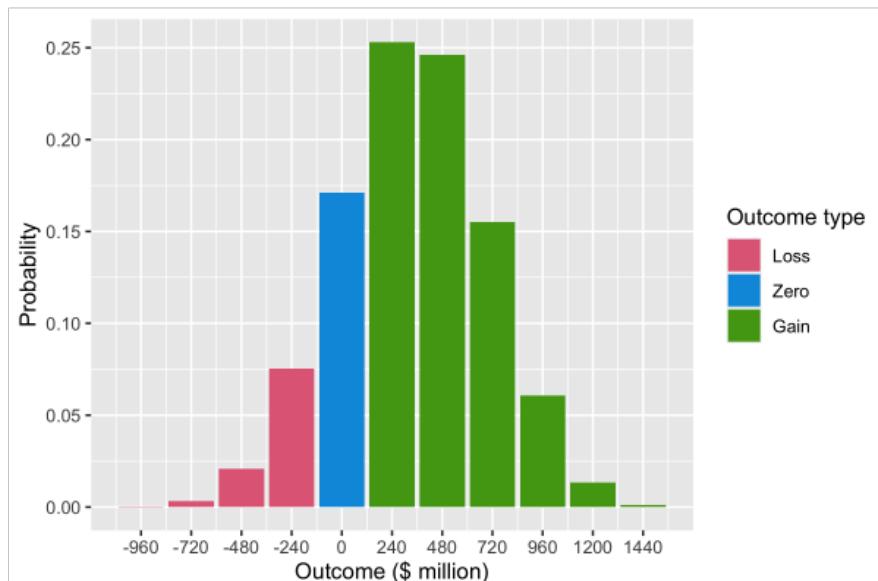
### 4755 A.2.2.2 Gambles

4756 Figures A.10 and A.11 show that the overall people seemed to prefer gam-  
bles with higher probabilities of gain, sometimes regardless of expected value or  
4757 value of the gain.  
4758

## A. Chapter 2 Appendix

Below is the probability distribution of final outcomes if all projects were chosen.

The numbers on the x-axis (labelled 'Outcome') represent the final amounts of money possible if you chose to invest in all the projects. The numbers on the y-axis (labelled 'Probability') represent the likelihoods of each of the possible outcomes. Negative final outcomes (losses) are shown in red, positive final outcomes (gains) are shown in green, and a final outcome of zero (no loss or gain) is shown in blue.



Indicate below whether you would invest in the following:

**The total number of projects you were shown is 10 . If you could choose to invest in a certain number of those 10 projects, how many would you invest in?**

projects

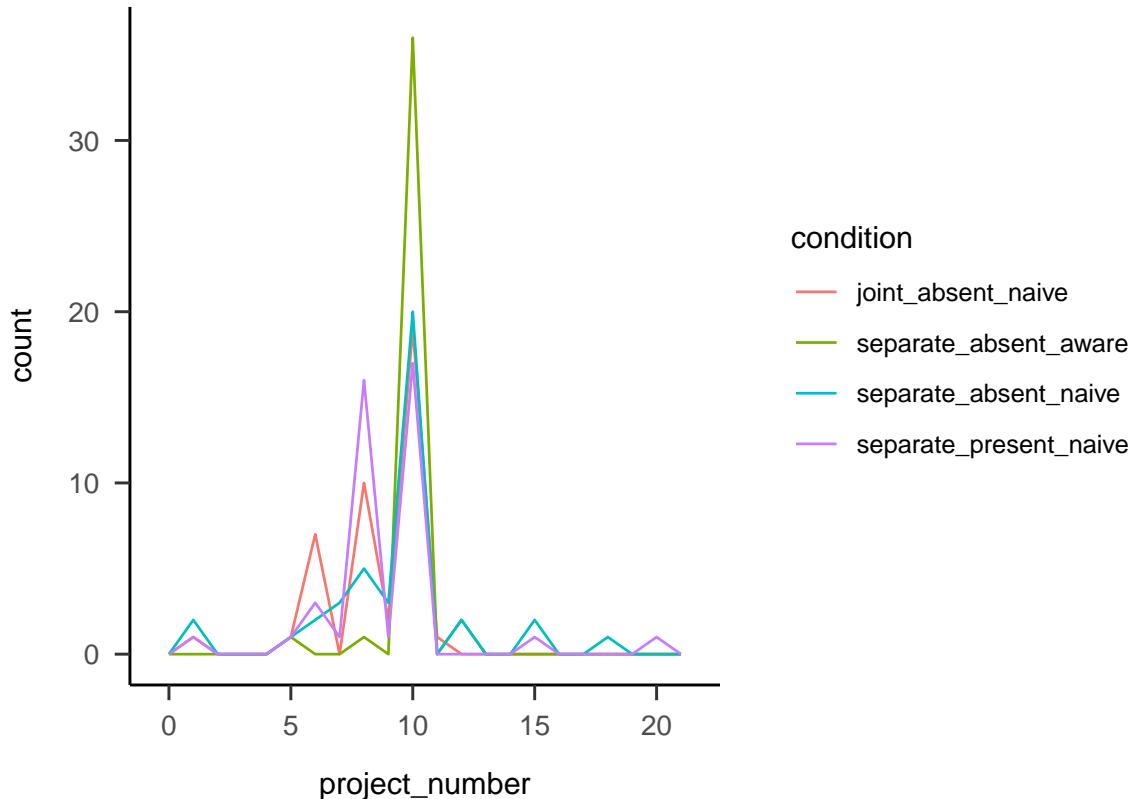
Continue

**Figure A.6:** Experiment 2 numerical portfolio question.

## 4759 A.3 Experiment 3

4760 Experiment 3 investigated the effect of similarity on project choice. The previ-  
4761 ous experiments did not counterbalance the project domain when displaying the 10  
4762 projects to participants. Experiment 3 used 10 different potential business domains  
4763 when constructing the project descriptions in order to reduce any potential effect  
4764 that the specific domain may have on people's choice. Therefore, Experiment 3

## A. Chapter 2 Appendix



**Figure A.7:** Number of projects participants reported seeing, by condition.

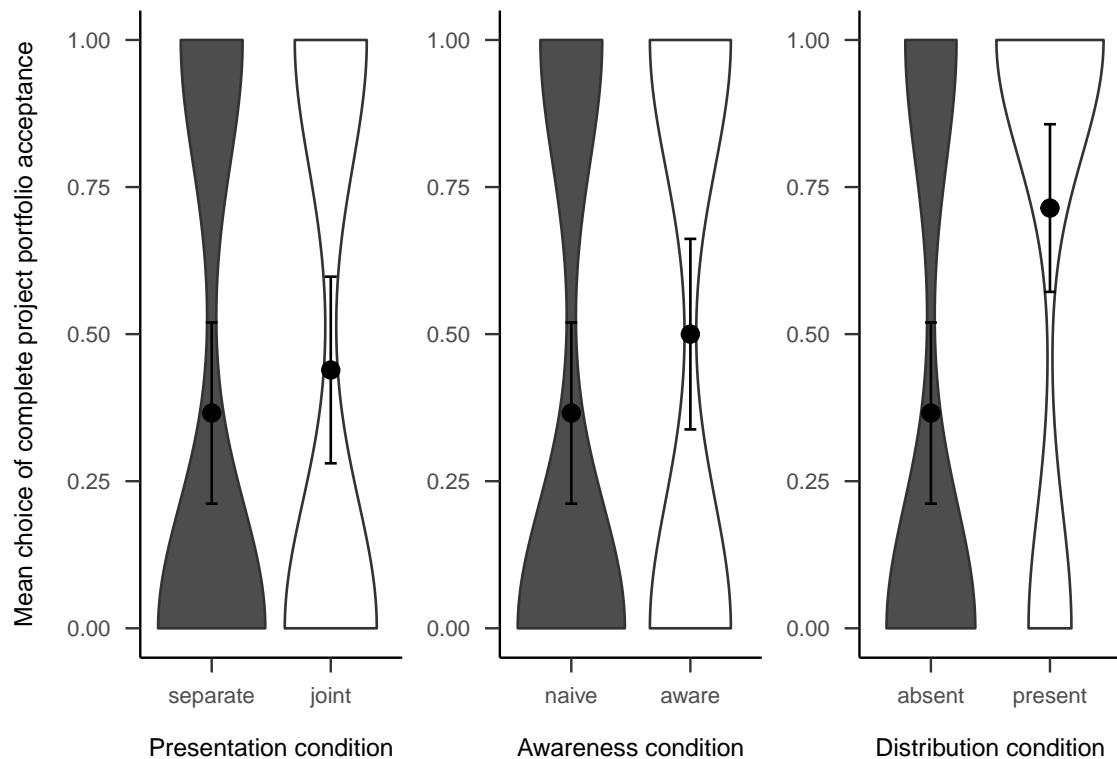
4765 again tested Hypothesis 2.3.

### A.3.1 Method

#### A.3.1.1 Participants

4766 Two hundred and sixty-six participants (127 female) were recruited from the  
4767 online recruitment platform Prolific. Participants were compensated at a rate of £5  
4768 an hour (Prolific is based in the UK). The average age was 39.56 years ( $SD = 8.77$ ,  
4769  $min. = 25$ ,  $max. = 71$ ). Participants reported an average of 5.64 years ( $SD = 6.45$ ,  
4770  $min. = 0$ ,  $max. = 40$ ) working in a business setting, and an average of 3.28 years  
4771 ( $SD = 4.92$ ,  $min. = 0$ ,  $max. = 30$ ) of business education. The mean completion  
4772 time of the task was 9.23 min ( $SD = 7.2$ ,  $min. = 1.41$ ,  $max. = 65.46$ ). Table A.1  
4773 shows the allocation of participants to the different conditions.  
4774

A. Chapter 2 Appendix



**Figure A.8:** Mean choice of investing in all 10 projects for the presentation, awareness, and distribution effects. Note, the condition on the left of each effect is the reference condition (separate presentation, naive awareness, distribution absent). As such, it is identical for the three effects.

**Table A.1:** Experiment 3 group allocation.

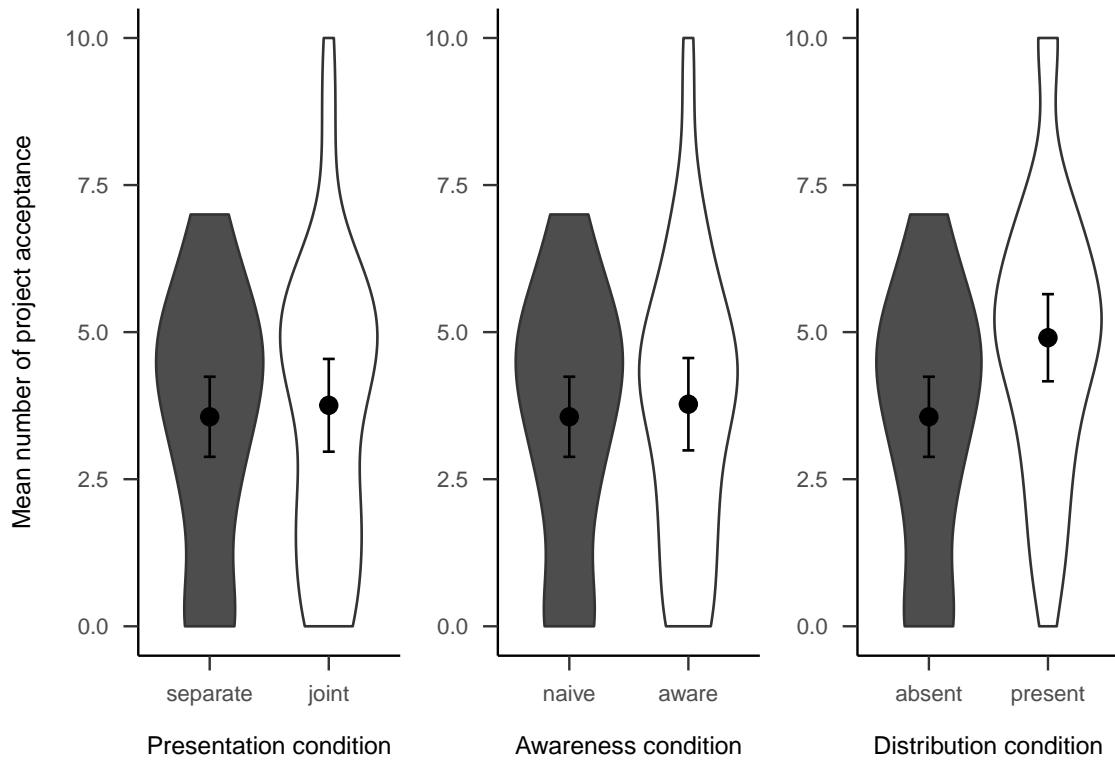
Similarity	N
High	133
Low	133
Total	266

4776 **A.3.1.2 Materials**

4777 **A.3.1.2.1 Instructions** Participants were shown the same instructions as in  
4778 Experiment 1 (see Section 2.2.1.2.1).

4779 **A.3.1.2.2 Risky Investment Task** Participants saw displays with the same  
4780 gamble values as those in Experiment 2 (see Section 2.3.1.2.2), but with some  
4781 changes in wording and sentence structure. The gamble information was the same,

## A. Chapter 2 Appendix



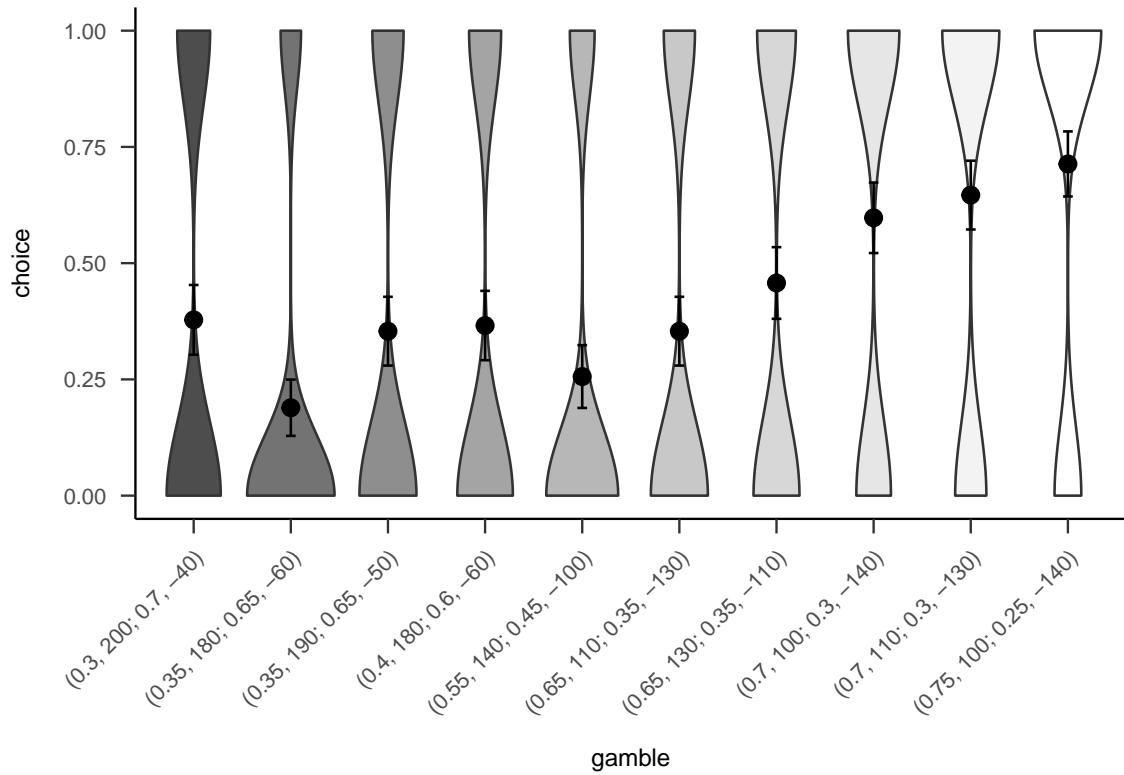
**Figure A.9:** Mean number of projects chosen in the follow-up for the presentation, awareness, and distribution effects. Note, the condition on the left of each effect is the reference condition (separate presentation, naive awareness, distribution absent). As such, it is identical for the three effects.

4782 but extra prose was added to describe the projects. Further, the order of the  
 4783 sentences was randomised, so that the descriptions would not appear so similar.  
 4784 See Figure A.12 for an example.

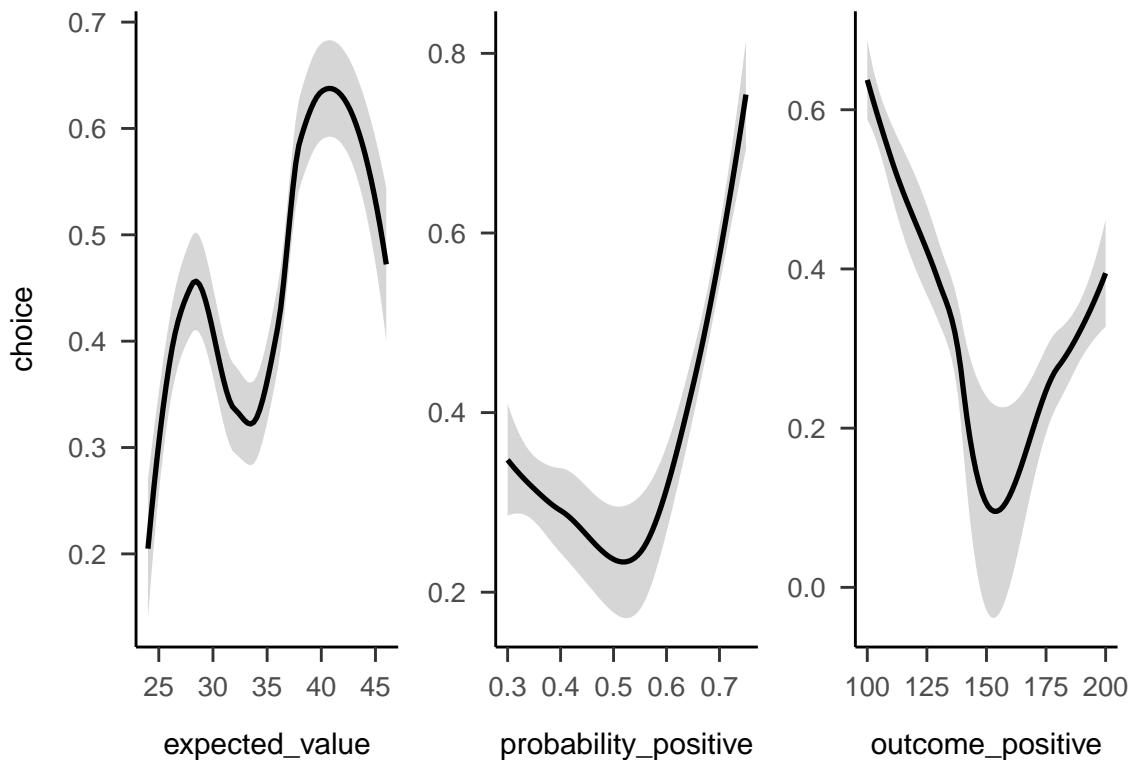
4785 The similarity manipulation was as in Experiment 1. However, project domain  
 4786 was varied so that in the high similarity condition participants saw one of ten  
 4787 project domains.

4788 **A.3.1.2.3 Follow-up** The follow-up questions were similar to those in Experi-  
 4789 ment 2 (see Section 2.3.1.2.3), except in the portfolio number question participants  
 4790 were also shown the total number of projects that they saw (10). Further, another  
 4791 question was added, asking how many projects participants were expecting to see  
 4792 at the beginning of the experiment (see Figure A.13).

A. Chapter 2 Appendix



**Figure A.10:** Mean project acceptance for the 10 gambles. The format of the labels indicates: (gain probability, gain value; loss probability, loss value).



**Figure A.11:** Mean project acceptance for the gambles' expected value, positive probability, and positive outcome.

## A. Chapter 2 Appendix

Indicate below whether you would invest in the following:

To summarise this investment, there is a 30% chance of gaining \$200 million (the forecasted revenue minus the cost amount) and a 70% chance of losing \$40 million. The company would make \$240 million if the forecasted concentration and quality of recoverable hydrocarbons at the site eventuates. The estimate for the anticipated chance of gain is based on a geological and seismic study of the site, and an analysis of previous similar sites. Refinera is a business in your company that proposes to construct an oil well project. Specifically, they want to establish an exploration site at an onshore location in Houston, US in order to see if the hydrocarbon supply is sufficient to establish a more permanent well. Refinera's research team has been investigating a possible site in an as yet unexplored area. Due to the location and size of the site, and consultant fees (e.g., geologists), they forecast the entire project to cost \$40 million (the loss amount).\*

Yes

No

Figure A.12: An example of a project display in Experiment 3.

At the begining of the experiment, before you saw any projects, how many projects did you expect to see?

project(s)

Figure A.13: Experiment 3 project expectation question.

### 4793 A.3.1.3 Procedure

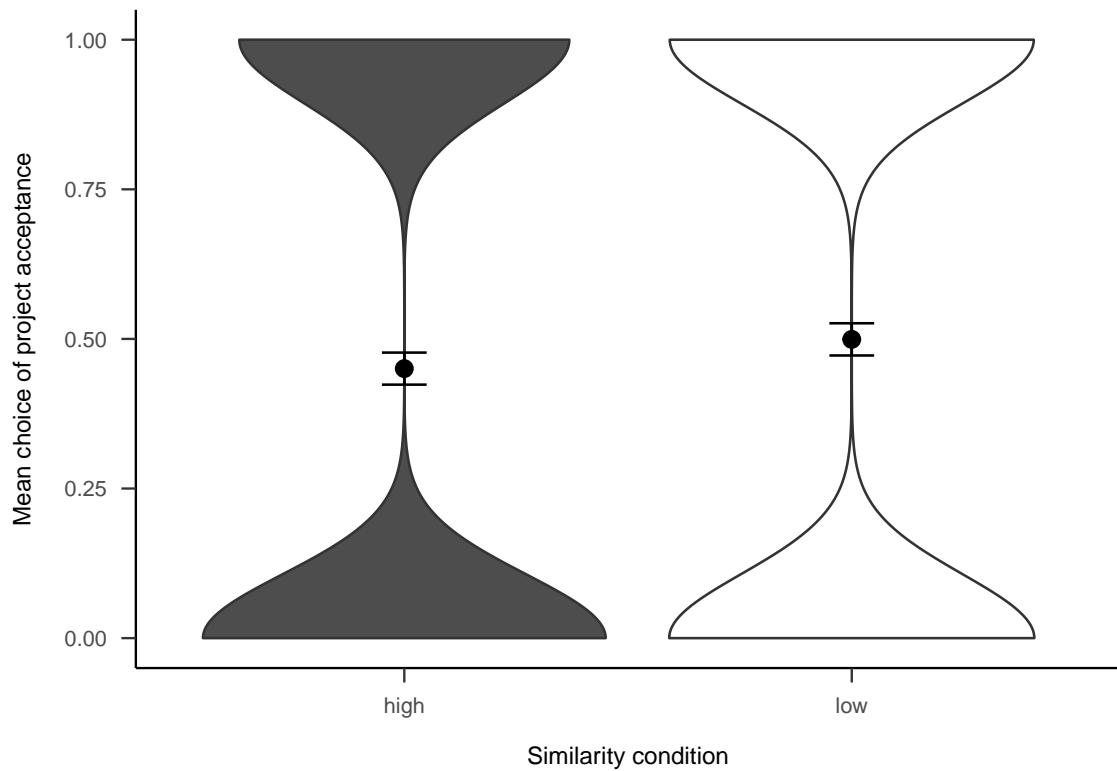
4794 Participants read the instructions and completed the risky investment task in  
4795 their respective conditions. After seeing the individual projects, participants were  
4796 then asked the four follow-up questions.

## 4797 A.3.2 Results

### 4798 A.3.2.1 Project Investment

4799 The project investment data were analysed as in Experiment 2 (see Section 2.3.2).  
4800 Figures A.14 and A.15 show the choice and proportion data, respectively. The

A. Chapter 2 Appendix



**Figure A.14:** Mean project acceptance for the similarity effect.

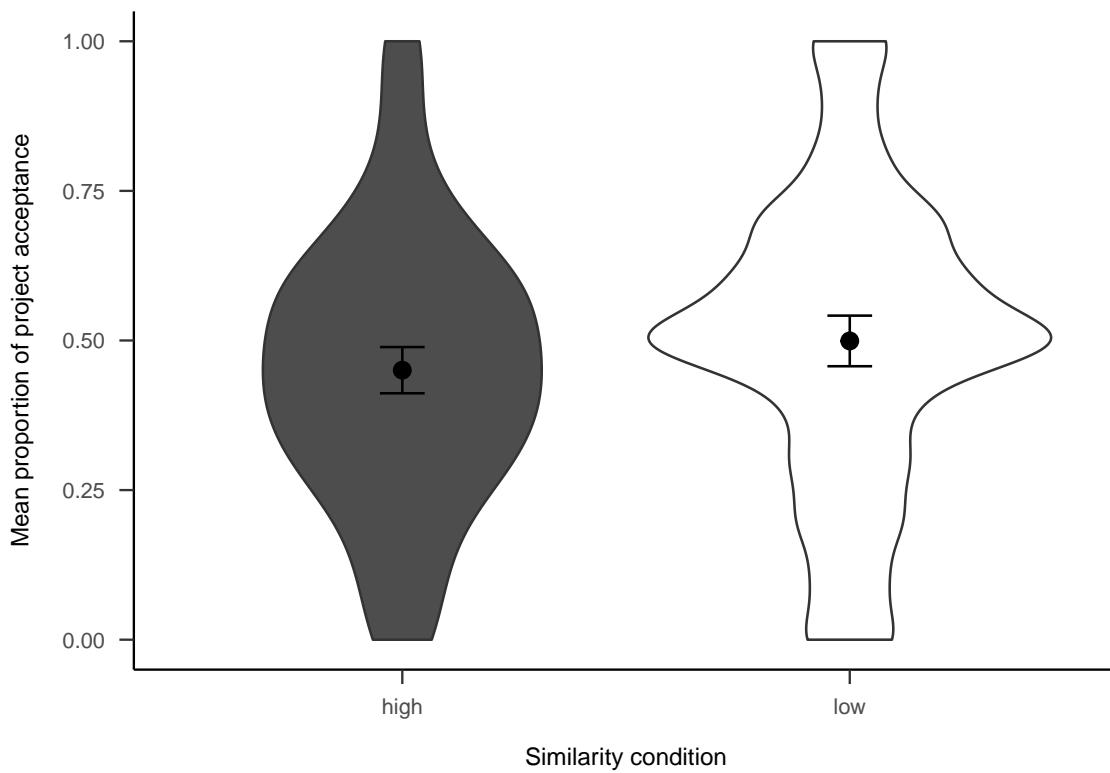
**Table A.2:** Logistic regression table of project acceptance by similarity and trial.

Term	$\hat{\beta}$	95% CI	$z$	$p$
Intercept	0.01	[−0.20, 0.22]	0.07	.944
Similarity1	-0.02	[−0.23, 0.18]	-0.22	.826
Project order	-0.02	[−0.05, 0.01]	-1.52	.127
Similarity1 × Project order	-0.02	[−0.05, 0.01]	-1.07	.284

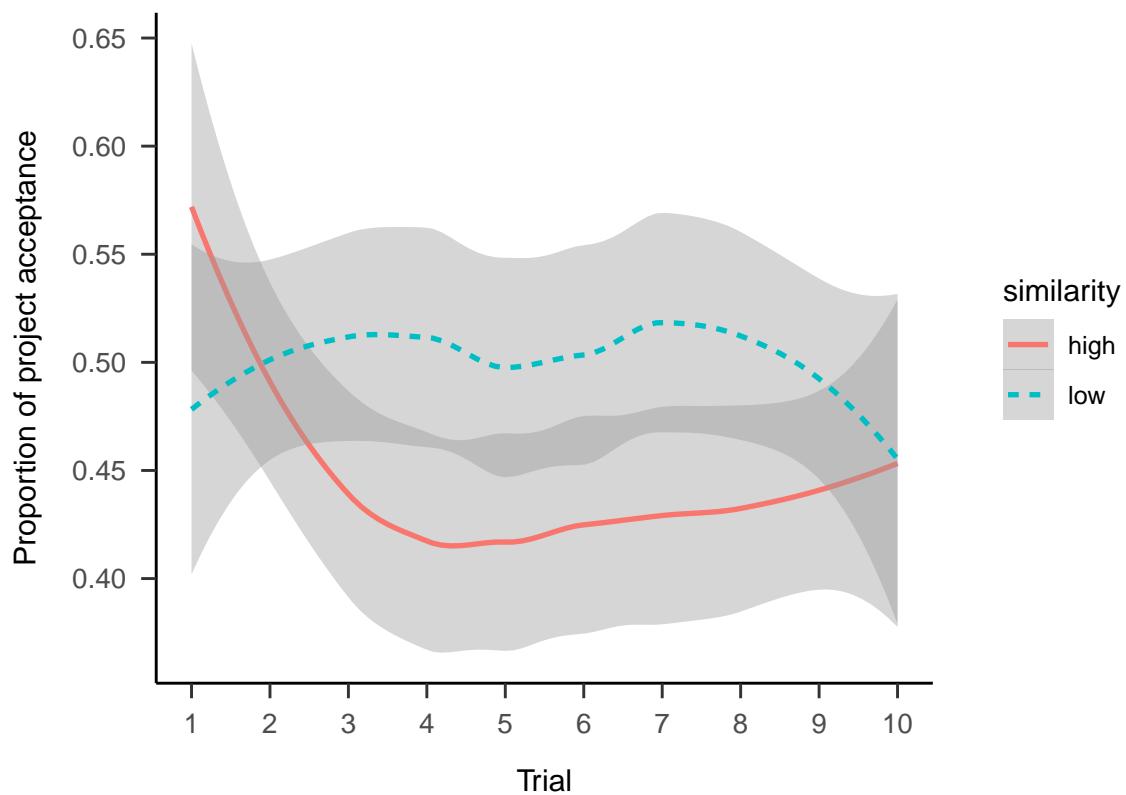
4801 difference between similarity conditions was not significant, both in the logistic  
 4802 regression  $b = 0.00$ , 95% CI  $[-0.18, 0.17]$ ,  $z = -0.04$ ,  $p = .966$ , and in the t-test,  
 4803  $d_s = -0.21$ , 95% CI  $[-0.45, 0.03]$ ,  $t(264) = -1.69$ ,  $p = .093$ .

4804 Further, Figure A.16 shows the choice data as a function of the order of the  
 4805 project in the sequence. As Table A.2 shows, there were no main effects or interac-  
 4806 tions.

A. Chapter 2 Appendix

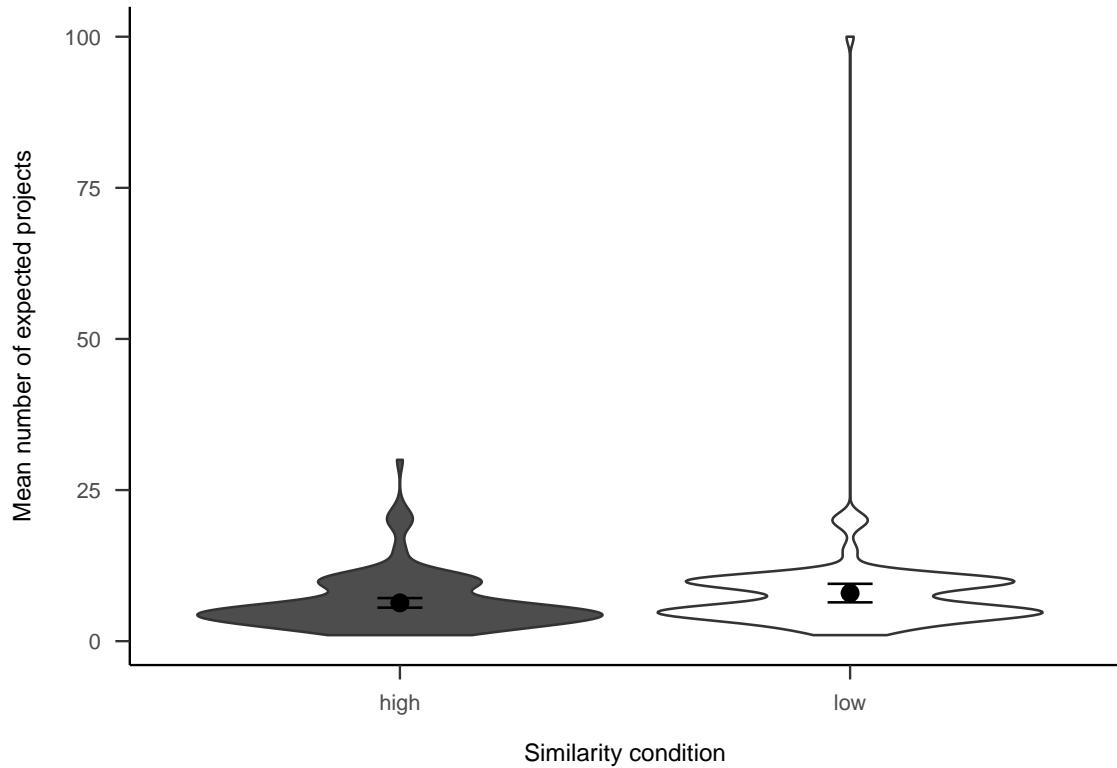


**Figure A.15:** Mean proportion of project acceptance for the similarity effect.



**Figure A.16:** Mean project acceptance by similarity and trial.

## A. Chapter 2 Appendix



**Figure A.17:** Number of projects participants expected to see, by similarity.

### 4807    A.3.2.2 Follow-up

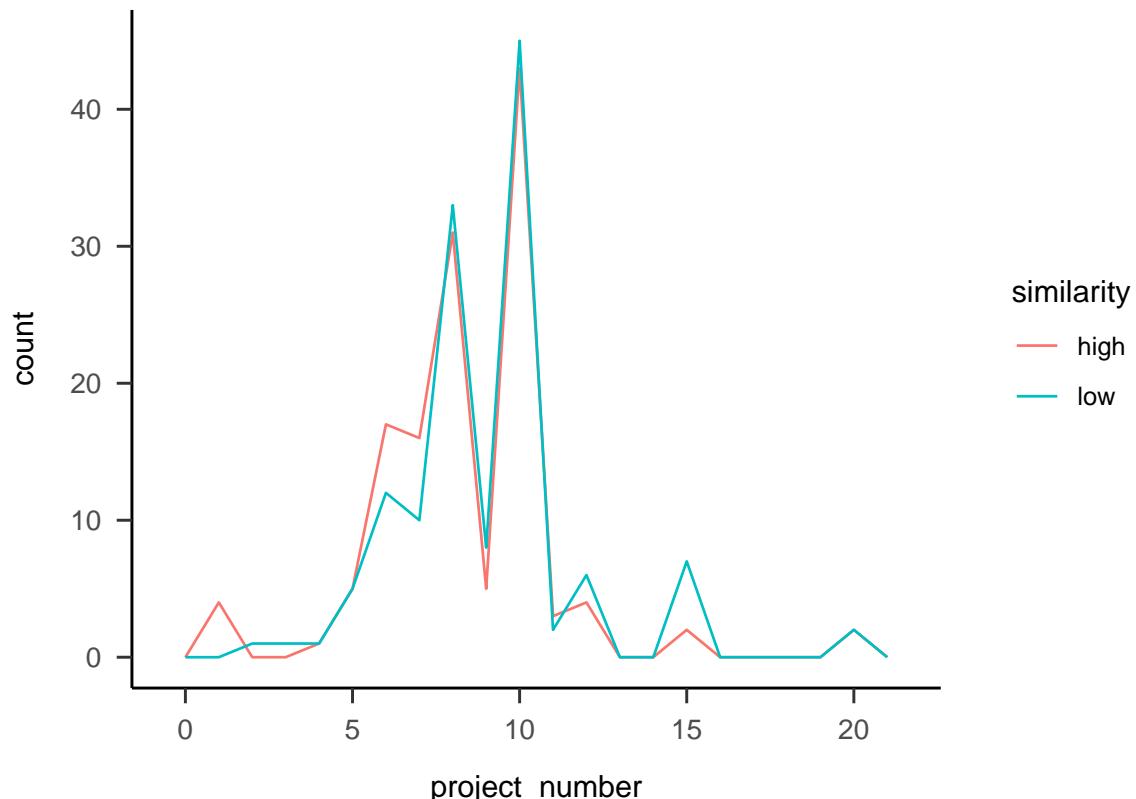
4808    **A.3.2.2.1 Project Expectation** Participants were asked how many projects  
4809    they expected to see. As Figure A.17 shows, the difference between similarity condi-  
4810    tions was not significant,  $d_s = -0.23$ , 95% CI [-0.47, 0.01],  $t(264) = -1.85$ ,  $p = .065$ .

4811    **A.3.2.2.2 Project Number** Participants were asked how many projects they  
4812    thought they saw. Figure A.18 shows that overall people correctly estimate the  
4813    number of projects.

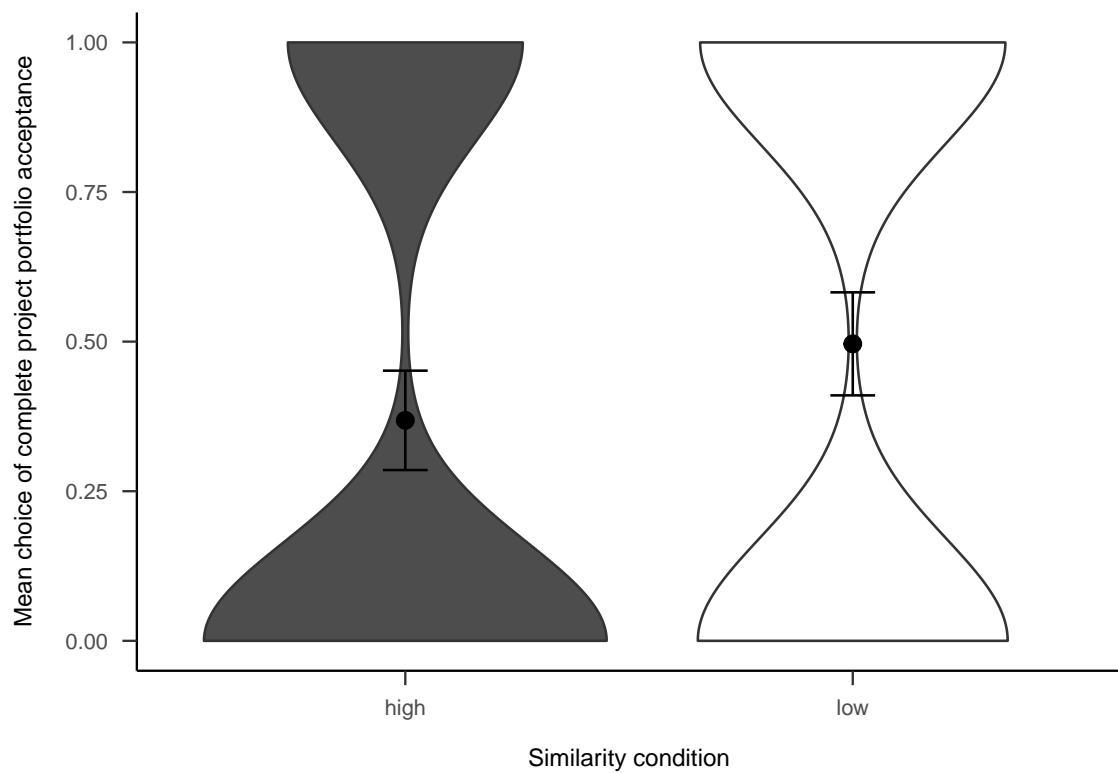
4814    **A.3.2.2.3 Portfolio Choice - Binary** Participants were then asked if they  
4815    would rather invest in all or none of the projects. As Figure A.19 shows, those in  
4816    the low similarity condition were significantly more likely to want to invest in all  
4817    of the projects,  $b = -0.26$ , 95% CI [-0.51, -0.02],  $z = -2.10$ ,  $p = .036$ .

4818    **A.3.2.2.4 Portfolio Choice - Number** Subsequently, participants were asked  
4819    how many projects they would invest in out of the 10 that they saw. As Figure A.20

A. Chapter 2 Appendix

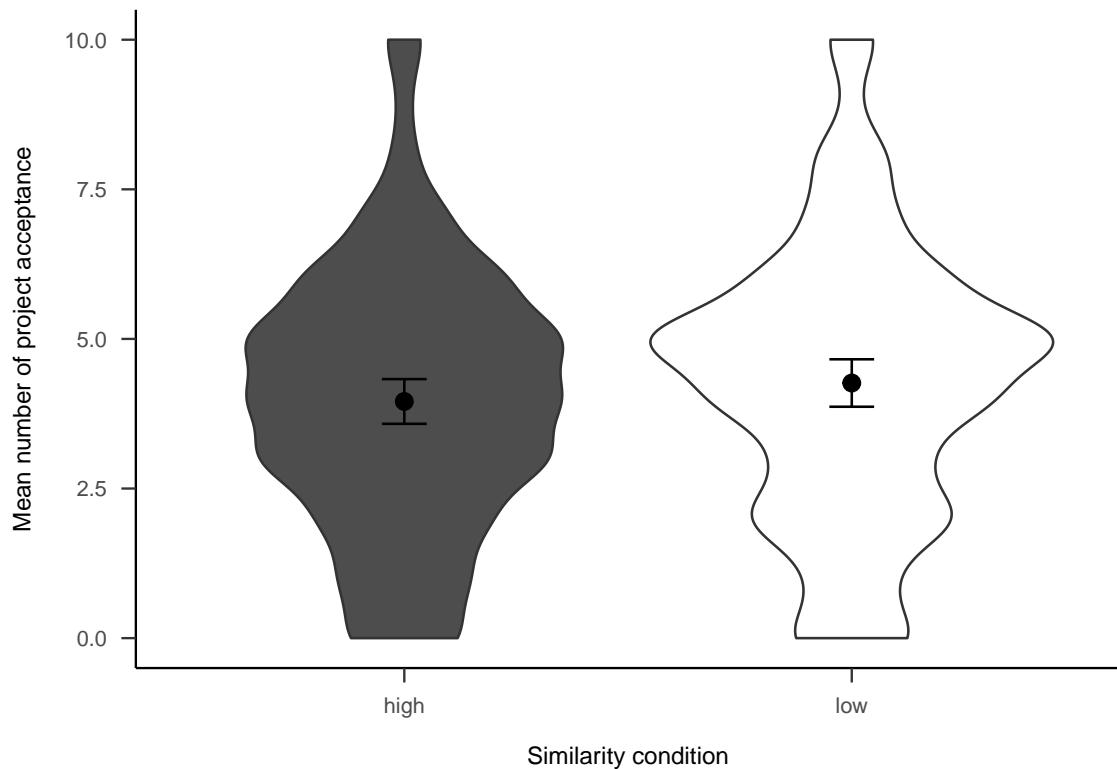


**Figure A.18:** Number of projects participants reported seeing, by similarity.



**Figure A.19:** Mean choice of investing in all 10 projects for the similarity effect.

## A. Chapter 2 Appendix



**Figure A.20:** Mean number of projects chosen in the follow-up for the similarity effect.

4820 shows, the difference between similarity conditions was not significant,  $d_s = -0.14$ ,  
4821 95% CI [-0.38, 0.10],  $t(264) = -1.12$ ,  $p = .264$ .

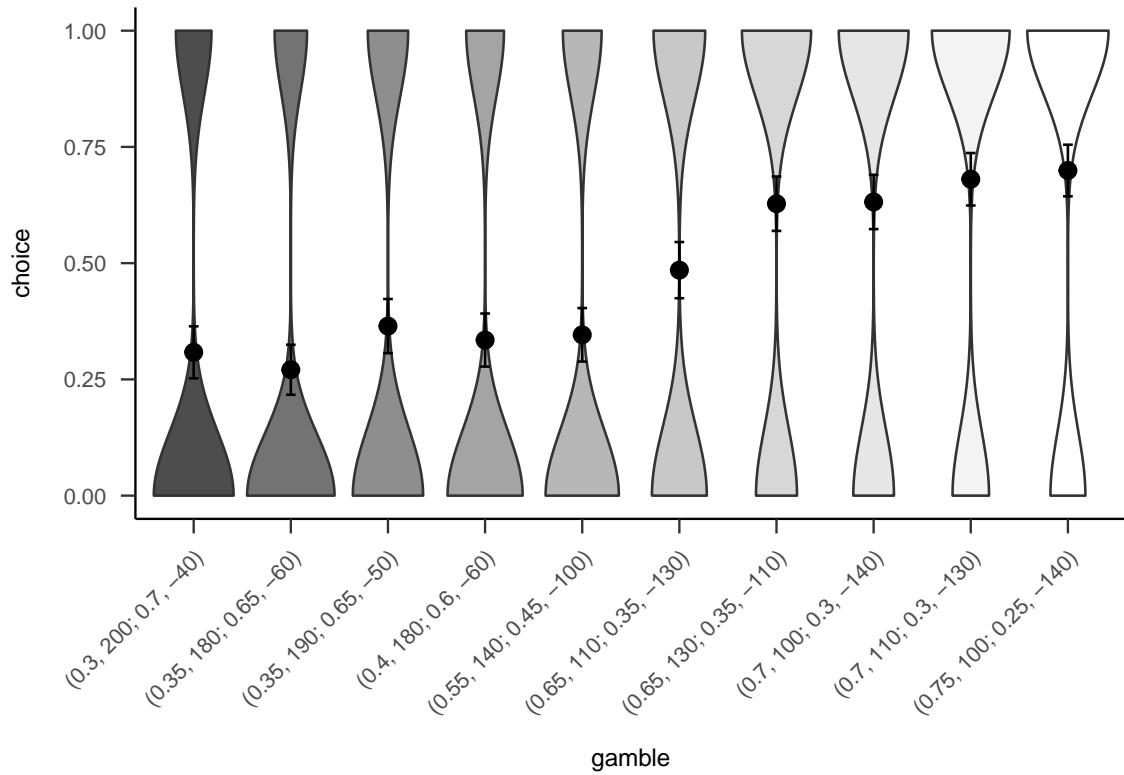
### 4822 **A.3.2.3 Gambles**

4823 Figures A.21 and A.22 show the overall people seemed to prefer gambles with  
4824 higher probabilities of gain, sometimes regardless of expected value or value of the  
4825 gain.

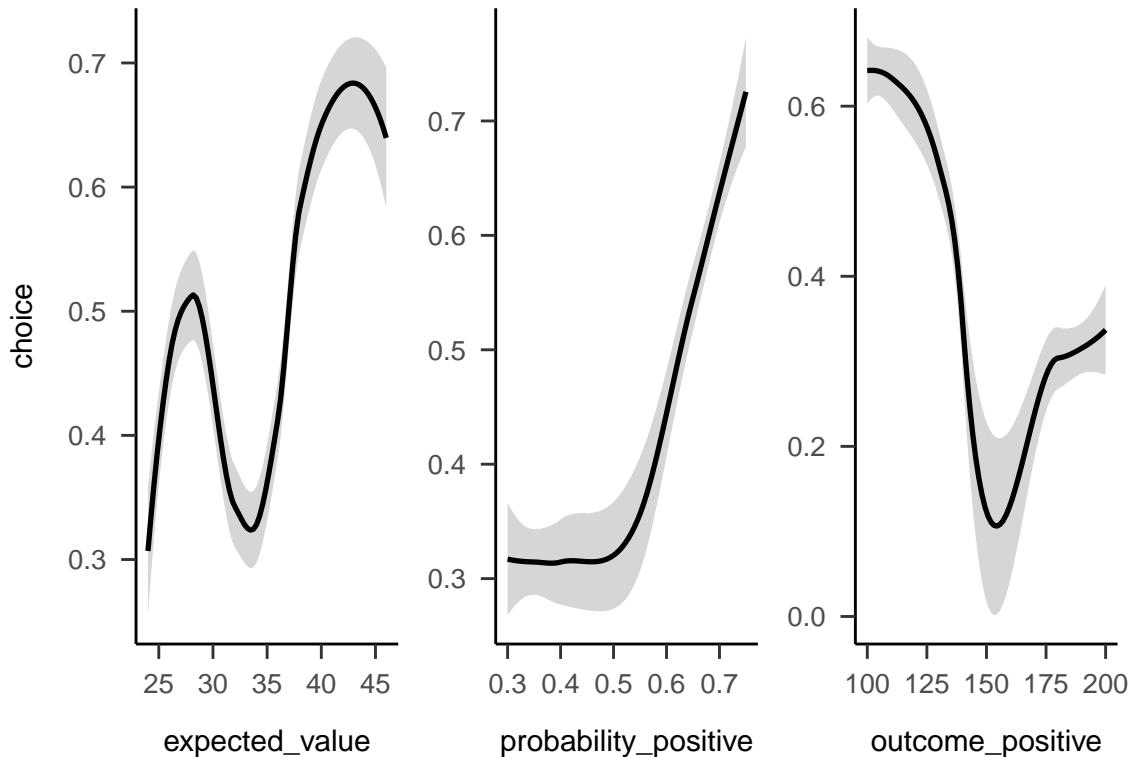
### 4826 **A.3.3 Discussion**

4827 Experiment 3 found some evidence for the effect of similarity on project choice,  
4828 but it was in the opposite direction to the one hypothesised. Specifically, the results  
4829 showed that when considering projects individually, participants' risk aversion did  
4830 not differ between similarity conditions, but when offered a portfolio of the projects,  
4831 those that saw the dissimilar projects were more likely to invest.

A. Chapter 2 Appendix



**Figure A.21:** Mean project acceptance for the 10 gambles. The format of the labels indicate: (gain probability, gain value; loss probability, loss value).



**Figure A.22:** Mean project acceptance for the gambles' expected value, positive probability, and positive outcome.

## A. Chapter 2 Appendix

4832 These results provide evidence for the naive diversification account expressed  
4833 above (see Section 2.2.3.3). Specifically, participants may really be naively diver-  
4834 sifying, but only when they are explicitly given an opportunity to do so. This  
4835 is similar to the multi-play effects because the question itself provides a sort of  
4836 choice bracketing. That is, the gambles are grouped together as a portfolio by the  
4837 question. Together, this suggests that people are not naively aggregating when  
4838 viewing gambles in isolation, but when the choices are bracketed explicitly, then  
4839 the choice seems to be driven by a naive diversification.

## 4840 A.4 Experiment 4

4841 Experiment 4 investigated the effect of awareness on project choice. Experi-  
4842 ment 1 found an effect of awareness in the trial-by-trial data that was not replicated  
4843 in Experiment 2. Above, this effect was explained through the law of small numbers:  
4844 people may have been anticipating less risky gambles towards the end of the set. As  
4845 such, the effect could be seen with more trials. Experiment 4 attempted to replicate  
4846 the effect from Experiment 1 with 20 projects. The *naive* condition attempted to  
4847 encourage participants to focus on projects one at a time and did not reveal the  
4848 total number of projects. The *aware* condition attempted to encourage participants  
4849 to think of all 20 projects. This was done by revealing the total number of projects  
4850 in the beginning of the task and by identifying at each project display its order in  
4851 the sequence. Experiment 4 again tested Hypothesis 2.4.

### 4852 A.4.1 Method

#### 4853 A.4.1.1 Participants

4854 Two hundred and sixty-six participants (110 female) were recruited from the  
4855 online recruitment platform Prolific. Participants were compensated at a rate of  
4856 £5 an hour (Prolific is based in the UK). The average age was 40.62 years ( $SD$   
4857 = 9.59,  $min.$  = 25,  $max.$  = 74). Participants reported an average of 7.45 years  
4858 ( $SD$  = 7.8,  $min.$  = 0,  $max.$  = 47) working in a business setting, and an average  
4859 of 5.52 years ( $SD$  = 7.27,  $min.$  = 0,  $max.$  = 48) of business education. The mean

## A. Chapter 2 Appendix

**Table A.3:** Experiment 4 group allocation.

Awareness	N
Aware	133
Naive	133
Total	266

Imagine that you are an executive in a large company composed of many individual businesses. You need to make decisions about projects that come across your desk.

As the executive, your pay will be determined by the performance of each investment.  
We want to know what choices you would actually make.

< Previous      Next >

**Figure A.23:** Instructions for those in the naive condition of Experiment 4.

4860 completion time of the task was 12.66 min ( $SD = 8.26$ ,  $min. = 1.48$ ,  $max. =$   
4861 53.47). Table A.3 shows the allocation of participants to the different conditions.

### 4862 A.4.1.2 Materials

4863 **A.4.1.2.1 Instructions** Participants were shown similar instructions to Ex-  
4864 periment 1 (see Section 2.2.1.2.1), except that the awareness manipulation was  
4865 incorporated into the text. Participants in the naive condition saw the instructions  
4866 in Figure A.23, and those in the aware condition saw the instructions in Figure A.24.

Imagine that you are an executive in a large company composed of many individual businesses. You need to make decisions about projects that come across your desk.

As the executive, your pay will be determined by the performance of your investments.  
We want to know what choices you would actually make.

There will be 20 projects that you will decide on this quarter.

< Previous      Next >

**Figure A.24:** Instructions for those in the aware condition of Experiment 4.

A. Chapter 2 Appendix

Below is a description of project 1 of 20.

Indicate below whether you would invest in the project:

The company would make \$240 million if the forecasted concentration and quality of recoverable hydrocarbons at the site eventuates. The estimate for the anticipated chance of gain is based on a geological and seismic study of the site, and an analysis of previous similar sites. To summarise this investment, there is a 55% chance of gaining \$125 million (the forecasted revenue minus the cost amount) and a 45% chance of losing \$115 million. Refinera's research team has been investigating a possible site in an as yet unexplored area. Due to the location and size of the site, and consultant fees (e.g., geologists), they forecast the entire project to cost \$115 million (the loss amount). Refinera is a business in your company that proposes to construct an oil well project. Specifically, they want to establish an exploration site at an onshore location in Houston, US in order to see if the hydrocarbon supply is sufficient to establish a more permanent well.\*

Yes

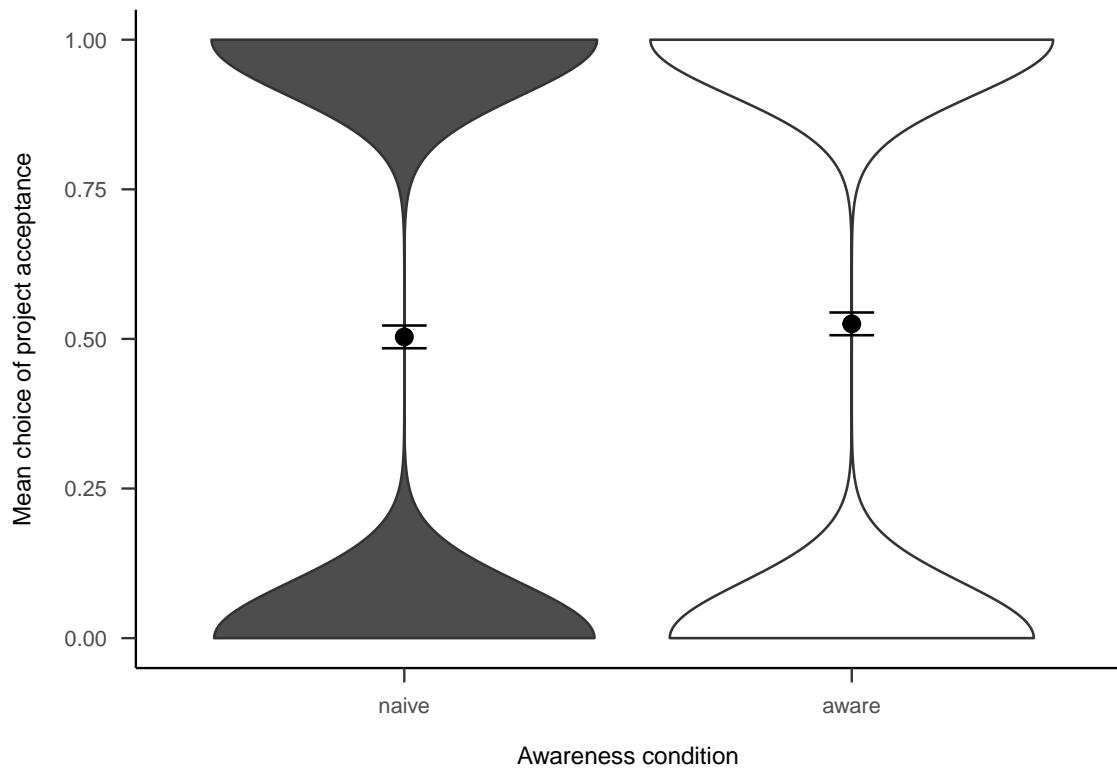
No

**Figure A.25:** An example of a project display in Experiment 4.

4867 **A.4.1.2.2 Risky Investment Task** Participants saw similar displays to those  
4868 in Experiment 3 (see Section A.3.1.2.2). However, here participants viewed 20  
4869 projects, so while the gamble constraints explained above were still applied, the  
4870 actual gamble values were different. Further, those in the aware condition saw  
4871 an added sentence that identified the number of the project they were currently  
4872 considering in the context of the total 20. See Figure A.25 for an example. Those  
4873 in the naive condition saw the same display without this sentence.

4874 **A.4.1.2.3 Follow-up** The follow-up questions were identical to those in Exper-  
4875 iment 3 (see Section A.3.1.2.3), except that the portfolio number question identified  
4876 the number of projects they saw as 20.

## A. Chapter 2 Appendix



**Figure A.26:** Mean project acceptance for the awareness effect.

### 4877 A.4.1.3 Procedure

4878 Participants read the instructions and completed the risky investment task in  
4879 their respective conditions. After seeing the individual projects, participants were  
4880 then asked the four follow-up questions.

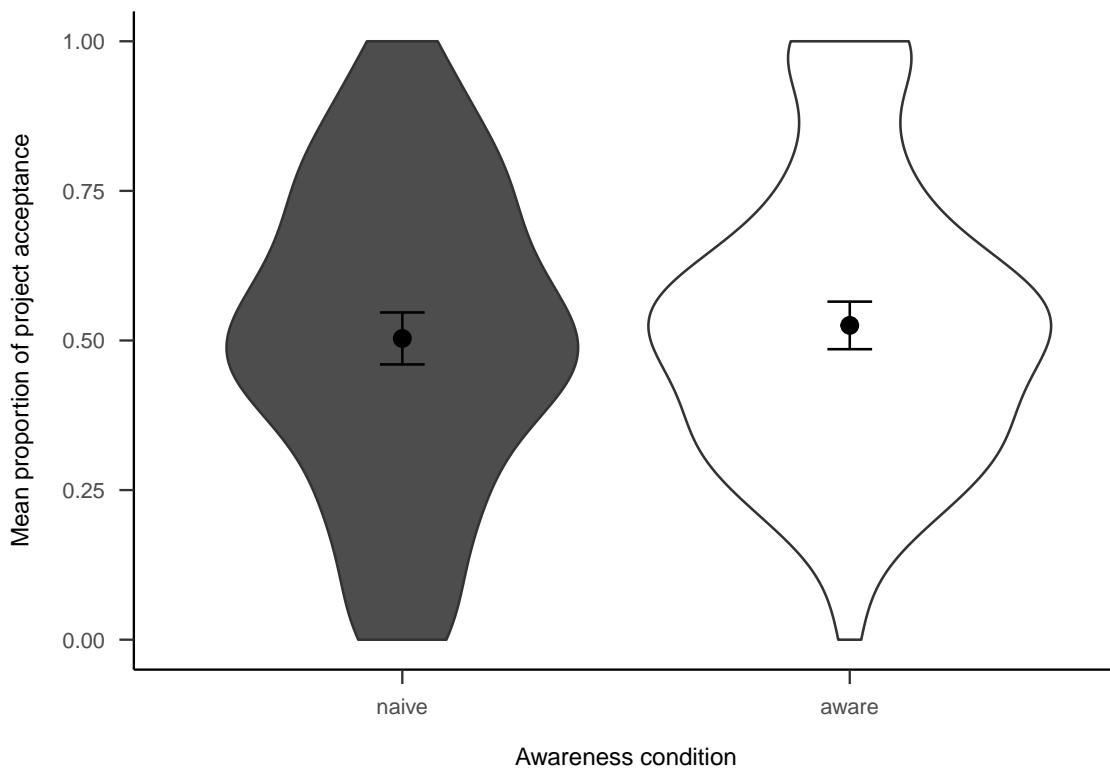
## 4881 A.4.2 Results

### 4882 A.4.2.1 Project Investment

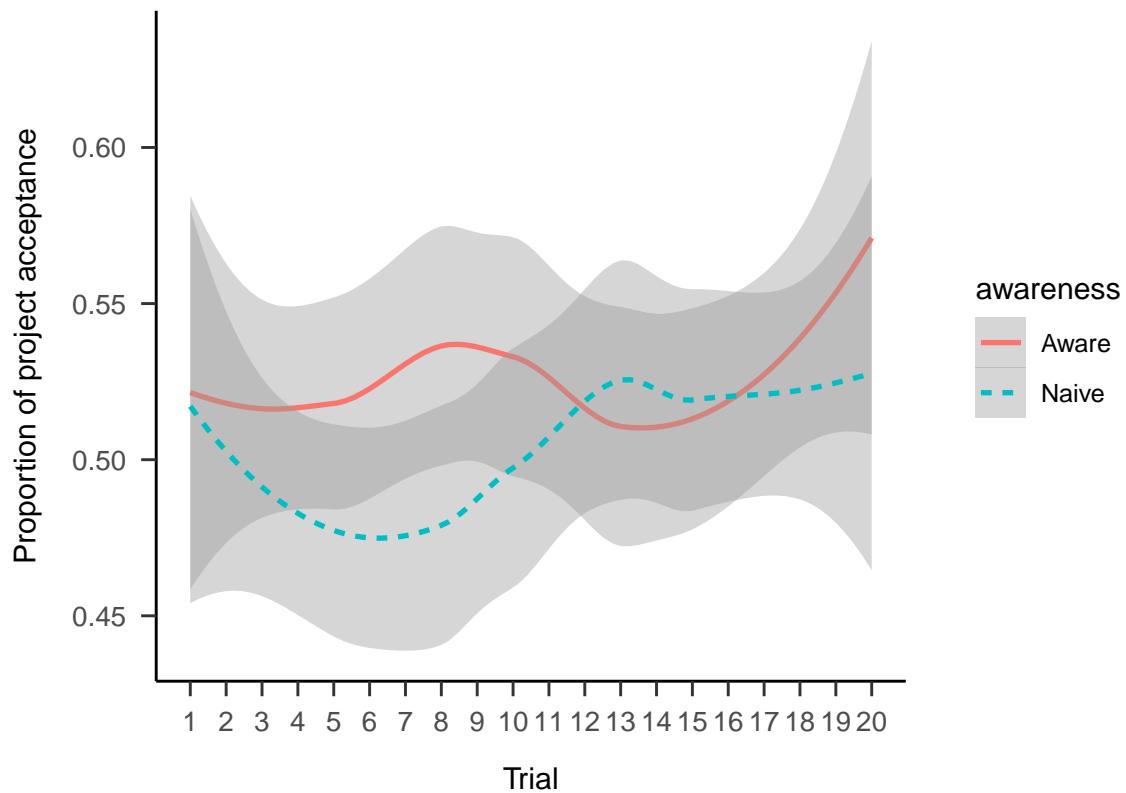
4883 The project investment data were analysed as in Experiment 2 (see Section 2.3.2).  
4884 Figures A.26 and A.27 show the choice and proportion data, respectively. The  
4885 difference between awareness conditions was not significant, both in the logistic  
4886 regression  $b = -0.05$ , 95% CI  $[-0.22, 0.13]$ ,  $z = -0.53$ ,  $p = .595$ , and in the t-test,  
4887  $d_s = -0.09$ , 95% CI  $[-0.33, 0.15]$ ,  $t(264) = -0.73$ ,  $p = .464$ .

4888 Further, Figure A.28 shows the choice data as a function of the order of the  
4889 project in the sequence. As Table A.4 shows, there were no main effects or interac-  
4890 tions.

A. Chapter 2 Appendix



**Figure A.27:** Mean proportion of project acceptance for the awareness effect.

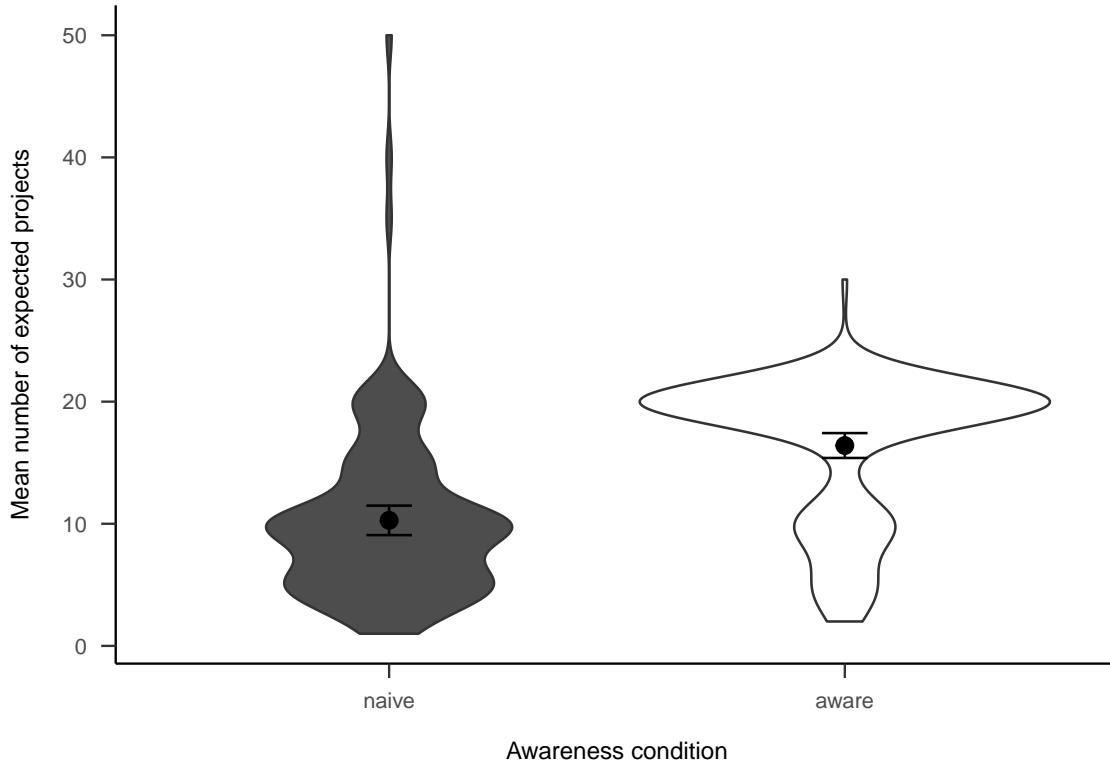


**Figure A.28:** Mean project acceptance by awareness and trial.

## A. Chapter 2 Appendix

**Table A.4:** Logistic regression table of project acceptance by awareness and trial.

Term	$\hat{\beta}$	95% CI	$z$	$p$
Intercept	-0.01	[-0.20, 0.17]	-0.12	.907
Awareness1	-0.10	[-0.28, 0.09]	-1.05	.293
Project order	0.01	[0.00, 0.02]	1.66	.096
Awareness1 × Project order	0.00	[-0.01, 0.01]	0.29	.775

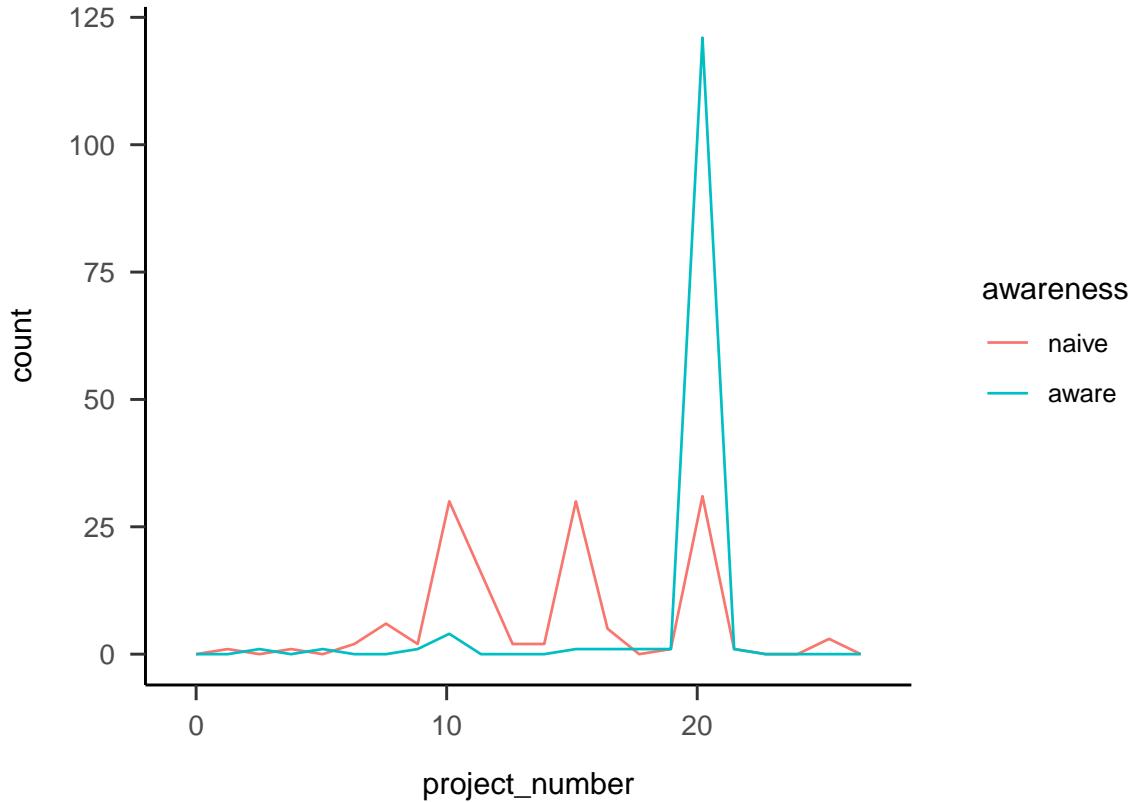


**Figure A.29:** Number of projects participants expected to see, by awareness.

### 4891 A.4.2.2 Follow-up

4892 **A.4.2.2.1 Project Expectation** Participants were asked how many projects  
 4893 they expected to see. Figure A.29 shows that those in the aware condition re-  
 4894 portedly expect to see more,  $d_s = -0.94$ , 95% CI [-1.19, -0.69],  $t(264) = -7.67$ ,  $p$   
 4895  $< .001$ . However, this is likely to be due to the fact that they were told how  
 4896 many projects there were.

## A. Chapter 2 Appendix



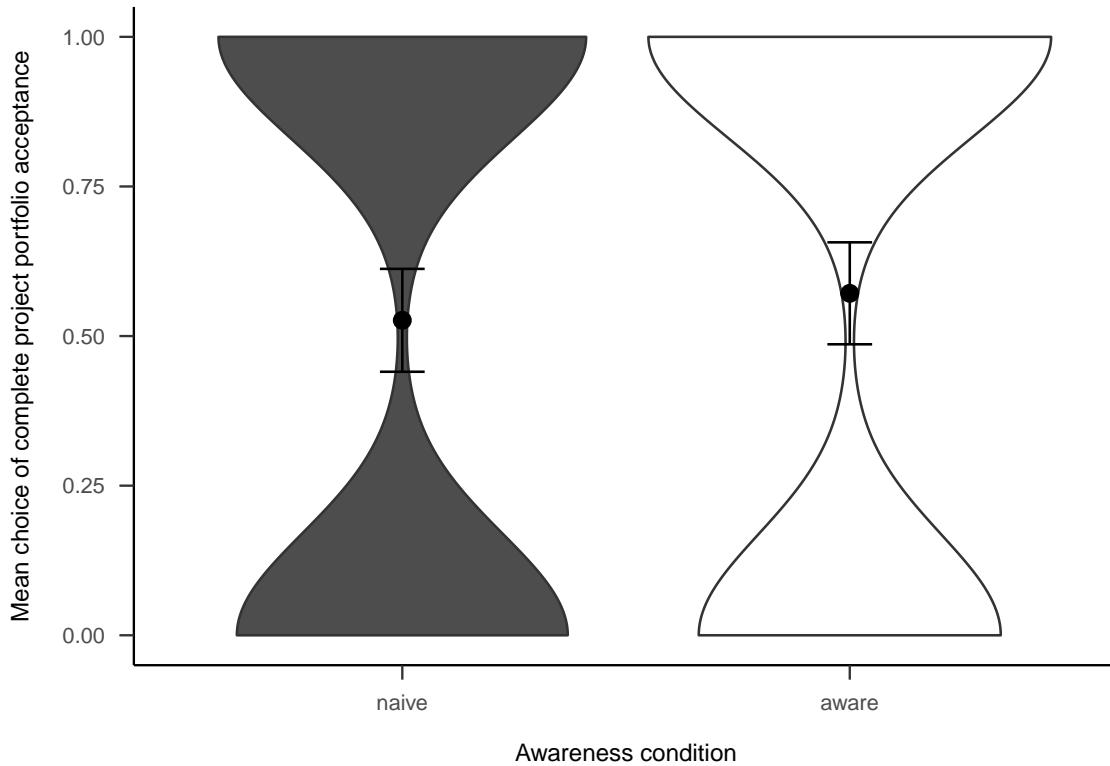
**Figure A.30:** Number of projects participants reported seeing, by awareness.

4897 **A.4.2.2.2 Project Number** Participants were asked how many projects they  
4898 thought they saw. Figure A.30 shows that overall people correctly estimated the  
4899 number of projects, with higher accuracy for those in the aware condition.

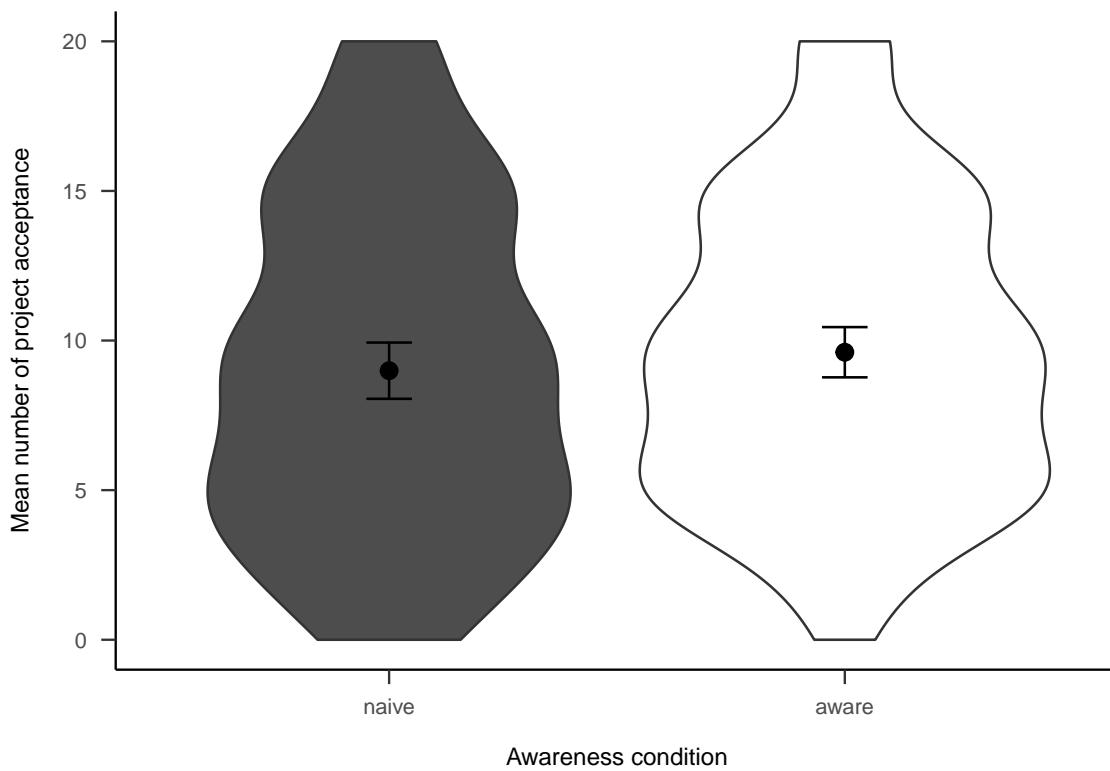
4900 **A.4.2.2.3 Portfolio Choice - Binary** Participants were then asked if they  
4901 would rather invest in all or none of the projects. As Figure A.31, there was no  
4902 significant difference between awareness conditions in wanting to invest in all of  
4903 the projects,  $b = -0.09$ , 95% CI  $[-0.33, 0.15]$ ,  $z = -0.74$ ,  $p = .460$ .

4904 **A.4.2.2.4 Portfolio Choice - Number** Subsequently, we asked participants  
4905 how many projects they would invest in out of the 20 that they saw. As Figure A.32  
4906 shows, the difference between awareness conditions was not significant,  $d_s = -0.12$ ,  
4907 95% CI  $[-0.36, 0.12]$ ,  $t(264) = -0.97$ ,  $p = .334$ .

A. Chapter 2 Appendix

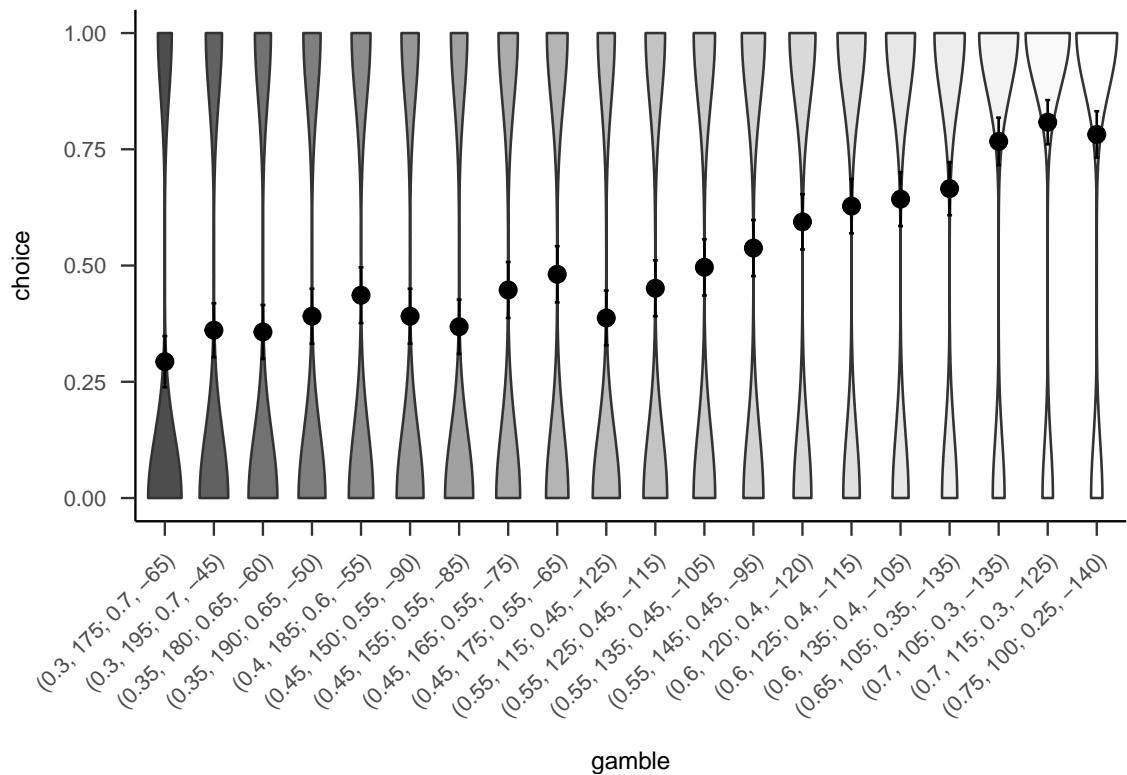


**Figure A.31:** Mean choice of investing in all 20 projects for the awareness effect.



**Figure A.32:** Mean number of projects chosen in the follow-up for the awareness effect.

## A. Chapter 2 Appendix



**Figure A.33:** Mean project acceptance for the 20 gambles. The format of the labels indicate: (gain probability, gain value; loss probability, loss value).

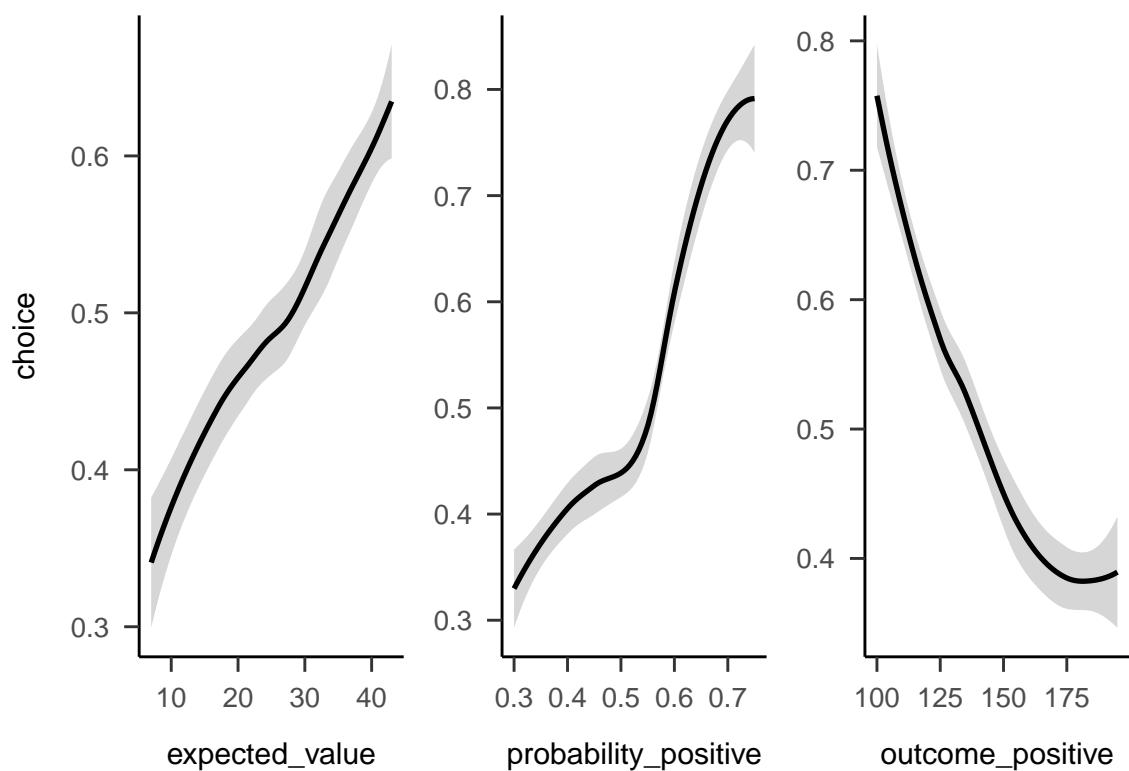
### 4908    A.4.2.3    Gambles

4909    Figures A.33 and A.34 show the overall people seemed to prefer gambles with  
 4910    higher probabilities of gain, sometimes regardless of expected value or value of the  
 4911    gain.

### 4912    A.4.3    Discussion

4913    Experiment 4 did not find evidence for Hypothesis 2.4. There was no significant  
 4914    effect of awareness on project choice by trial. Participants in the aware condition  
 4915    were expected to become less risk averse as they continued with the experiment  
 4916    if they were using a strategy similar to the law of small numbers. The fact  
 4917    that this effect was not replicated in Experiment 4 might mean that the finding  
 4918    in Experiment 1 was due to the specific gambles used in that experiment, or  
 4919    statistical chance.

*A. Chapter 2 Appendix*



**Figure A.34:** Mean project acceptance for the gambles' expected value, positive probability, and positive outcome.

# B

4920

4921

## Chapter 4 Appendix

4922

### Contents

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4923

4924	<b>B.1 Experiment 1</b> . . . . .	<b>196</b>
4925	B.1.1 Method . . . . .	197
4926	B.1.2 Results . . . . .	198
4927	B.1.3 Discussion . . . . .	204
4928	<b>B.2 Experiment 2</b> . . . . .	<b>206</b>
4929	B.2.1 Method . . . . .	206
4930	B.2.2 Results . . . . .	207
4931	<b>B.3 Experiment 3</b> . . . . .	<b>220</b>
4932	B.3.1 Method . . . . .	220
4933	B.3.2 Results . . . . .	223
4934	<b>B.4 Experiment 4</b> . . . . .	<b>224</b>
4935	B.4.1 Method . . . . .	226
4936	B.4.2 Results . . . . .	228
4937	B.4.3 Discussion . . . . .	231
4938	<b>B.5 Experiment 5</b> . . . . .	<b>231</b>
4939	B.5.1 Method . . . . .	232
4940	B.5.2 Results . . . . .	232
4941	B.5.3 Discussion . . . . .	234
4942	<b>B.6 Experiment 6</b> . . . . .	<b>234</b>
4943	B.6.1 Method . . . . .	235
4944	B.6.2 Results . . . . .	236
4945	B.6.3 Discussion . . . . .	238
4946	<b>B.7 Experiment 7</b> . . . . .	<b>238</b>
4947	B.7.1 Method . . . . .	238
4948	B.7.2 Results . . . . .	239
4949	B.7.3 Discussion . . . . .	240
4950	<b>B.8 Experiment 8</b> . . . . .	<b>241</b>

## B. Chapter 4 Appendix

4951	B.8.1 Method . . . . .	244
4952	B.8.2 Results . . . . .	250
4953	B.8.3 Discussion . . . . .	250

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4954  
4955

4956 This appendix contains supplementary materials and analyses for the three  
4957 experiments reported in Chapter 4. In addition, five related experiments are  
4958 reported. Experiment 4 was identical to Experiment 1, except that alignment  
4959 was manipulated within-subjects, it did not include a no NPV condition, and  
4960 there was no forecasting measure. Experiment 5 replicated Experiment 1, but  
4961 only tested the forecasting effect and did so with a sample that had investing  
4962 experience. Experiment 6 replicated Experiment 5 but with a larger sample size  
4963 and a lay sample. Experiment 7 attempted to facilitate a use of numerical reliability  
4964 through explicit hints. Experiment 8 tested both verbal and numerical reliability  
4965 effects in an all within-subjects design. However, unlike Experiment 3, the design  
4966 of Experiment 8 did not allow for a direct comparison of alignment conditions.  
4967

### B.1 Experiment 1

4968 In addition to the allocation measure, participants were also asked to rank the  
4969 projects and forecast their future returns. The ranking task was included before  
4970 the allocation task in order to encourage alignment and to have another measure of  
4971 participants' decision-making. The forecasting task was added (described further  
4972 below in Section B.1.1.1.2) in order to test whether the variance in people's forecasts  
4973 is affected by alignment and NPV reliability.  
4974

4975 **Hypothesis B.1.** All allocation effects will replicate in the ranking measure.

4976 **Hypothesis B.2.** All allocation effects will replicate in the forecasting mean  
4977 measure.

4978 In the forecasting measures, more alignable differences were expected to bring  
4979 about more certainty about forecasting decisions, since participants will have more  
4980 easily comparable information. As such, people's forecasting should be less variable

## B. Chapter 4 Appendix

4981 when comparing projects with alignable differences, than when comparing projects  
4982 with non-alignable differences.

4983 **Hypothesis B.3.** The standard deviation of participants' forecasts will be higher,  
4984 on average, in the low alignment condition than in the high alignment condition.

### 4985 B.1.1 Method

#### 4986 B.1.1.1 Materials

4987 **B.1.1.1.1 Instructions** Figures B.1, B.2, and B.3 show the instructions given  
4988 to those in the low NPV reliability, high NPV reliability, and no NPV condi-  
4989 tion, respectively.

4990 **B.1.1.1.2 Forecasting** Participants were asked to respond to a forecasting task  
4991 (adapted from Long et al., 2018), seen in Figure B.4. Participants were asked to  
4992 predict each project's rate of return after one month. This allowed to calculate  
4993 each participant's forecasting mean and standard deviation (the latter as inversely  
4994 proportional to forecasting precision).

4995 **B.1.1.1.3 Ranking** As shown in Figure B.5, participants were asked to rank  
4996 the projects in order of investment priority.

4997 **B.1.1.1.4 Confidence** As Figure B.6 shows, participants were asked to indi-  
4998 cate how confident they were about each of their allocation decisions on a scale  
4999 from 0 ("Not confident at all") to 100 ("Extremely confident").

5000 **B.1.1.1.5 Justification** As Figure B.7 shows, participants were asked to justify  
5001 their allocation decision in a free-response text-box.

## B. Chapter 4 Appendix

You will be shown information about a number of projects that a consumer products firm is considering to invest in. Some specific information about the product itself is provided. In addition to those numbers, you will find each project's net present value (NPV), which is the company's estimation of the future returns of the project. An NPV that is greater than 0 (zero) indicates that there is an expectation of profit. **The higher the NPV, the better the expectations for each project.** However, it is important to note that NPV is a very noisy measure relative to the other more specific measures because it relies on future forecasting. As such, **NPV is very unreliable and should be relied upon only as a last result; the specific project's measures should be used instead.**

We would like you to take the role of the manager in charge of capital allocation for the firm. This firm is specifically interested in investing in the development of high-end goods, so your valuations should reflect this. That is, even though there might be a market for the lower-end products in the descriptions that you will see, **you should be aiming to invest in the products with the highest objective value.** The features of the products that are listed matter because they reflect the direct value of the product, whereas financial measures such as NPV may reflect other factors, thus making it noisier, as mentioned above.

You will see a set of five different projects for which you must predict the investment returns after one month. For example, how likely is it that the project will return more than 9% after one month, how likely is it that the project will return 7% to 9%, etc.

You will also decide how to rank the projects in order of investment priority, and decide how to allocate the capital available for investment this year among the different projects. Note that this is not the operational budget (advertising, etc.), but rather the funds to be used for investment in developing the new products. You will do this by selecting a percentage value for each project, such that the budget is allocated completely among each set of projects.

**Figure B.1:** Experiment 1 low reliability instructions.

### 5002    B.1.2 Results

#### 5003    B.1.2.1 Ranking

5004    A mixed factorial ANOVA was conducted to investigate the effects of alignment  
5005    and verbally-instructed NPV reliability on participants' rankings of the target  
5006    project. As shown in Figure B.8, the alignment  $\times$  reliability level  $\times$  NPV in-  
5007    teraction was significant,  $F(6.62, 370.54) = 2.70$ ,  $p = .011$ ,  $\hat{\eta}_p^2 = .046$ . This  
5008    effect seems to be driven by the differences between the no NPV condition and  
5009    the conditions with NPV across the two alignment conditions. Specifically, in

## B. Chapter 4 Appendix

You will be shown information about a number of projects that a consumer products firm is considering to invest in. Some specific information about the product itself is provided. In addition to those numbers, you will find each project's net present value (NPV), which is the company's estimation of the future returns of the project. An NPV that is greater than 0 (zero) indicates that there is an expectation of profit. **The higher the NPV, the better the expectations for each project.** However, it is important to note that NPV is a very noisy measure relative to the other more specific measures because it relies on future forecasting. As such, NPV is very unreliable and should be relied upon only as a last result; the specific project's measures should be used instead. NPV is a very useful measure relative to the other more specific measures because it can be calculated regardless of the type of product. As such, **NPV is very reliable in most cases.**

We would like you to take the role of the manager in charge of capital allocation for the firm. This firm is specifically interested in investing in the development of high-end goods, so your valuations should reflect this. That is, even though there might be a market for the lower-end products in the descriptions that you will see, **you should be aiming to invest in the products with the highest objective value.**

You will see a set of five different projects for which you must predict the investment returns after one month. For example, how likely is it that the project will return more than 9% after one month, how likely is it that the project will return 7% to 9%, etc.

You will also decide how to rank the projects in order of investment priority, and decide how to allocate the capital available for investment this year among the different projects. Note that this is not the operational budget (advertising, etc.), but rather the funds to be used for investment in developing the new products. You will do this by selecting a percentage value for each project, such that the budget is allocated completely among each set of projects.

**Figure B.2:** Experiment 1 high reliability instructions.

5010 the low alignment condition, the linear NPV trend was significantly lower in the  
5011 no NPV condition than both the low reliability condition,  $M = -6.56$ , 95% CI  
5012  $[-10.26, -2.85]$ ,  $t(112) = -3.50$ ,  $p = .001$ , and the high reliability condition,  
5013  $M = -7.38$ , 95% CI  $[-10.83, -3.93]$ ,  $t(112) = -4.24$ ,  $p < .001$ . However, in  
5014 the high alignment condition, the linear NPV trend was only significantly lower  
5015 in the no NPV condition than the high reliability condition,  $M = -8.37$ , 95% CI  
5016  $[-11.85, -4.88]$ ,  $t(112) = -4.76$ ,  $p < .001$ , and not the low reliability condition,  
5017  $M = -1.71$ , 95% CI  $[-5.54, 2.13]$ ,  $t(112) = -0.88$ ,  $p = .380$ .

## B. Chapter 4 Appendix

You will be shown information about a number of projects that a consumer products firm is considering to invest in. Some specific information about the product itself is provided.

We would like you to take the role of the manager in charge of capital allocation for the firm. This firm is specifically interested in investing in the development of high-end goods, so your valuations should reflect this. That is, even though there might be a market for the lower-end products in the descriptions that you will see, **you should be aiming to invest in the products with the highest objective value**. The features of the products that are listed matter because they reflect the direct value of the product, whereas financial measures may reflect other factors.

You will see a set of five different projects for which you must decide how to rank in order of investment priority, and decide how to allocate the capital available for investment this year among the different projects. Note that this is not the operational budget (advertising, etc.), but rather the funds to be used for investment in developing the new products. You will do this by selecting a percentage value for each project, such that the budget is allocated completely among each set of projects.

**Figure B.3:** The instructions for the no NPV condition in Experiment 1.

Imagine that you have 100 points to assign to the following options for Project 1's rate of return on investment after one month. Assign points according to how likely you think each rate of return is.

	0	10	20	30	40	50	60	70	80	90	100
more than 9%	<input type="text"/>										0
7% to 9%	<input type="text"/>										0
5% to 7%	<input type="text"/>										0
3% to 5%	<input type="text"/>										0
1% to 3%	<input type="text"/>										0
-1% to -3%	<input type="text"/>										0
-5% to -7%	<input type="text"/>										0
-7% to -9%	<input type="text"/>										0
less than -9%	<input type="text"/>										0
<b>Total:</b>											<b>0</b>

**Figure B.4:** The forecasting task.

B. Chapter 4 Appendix

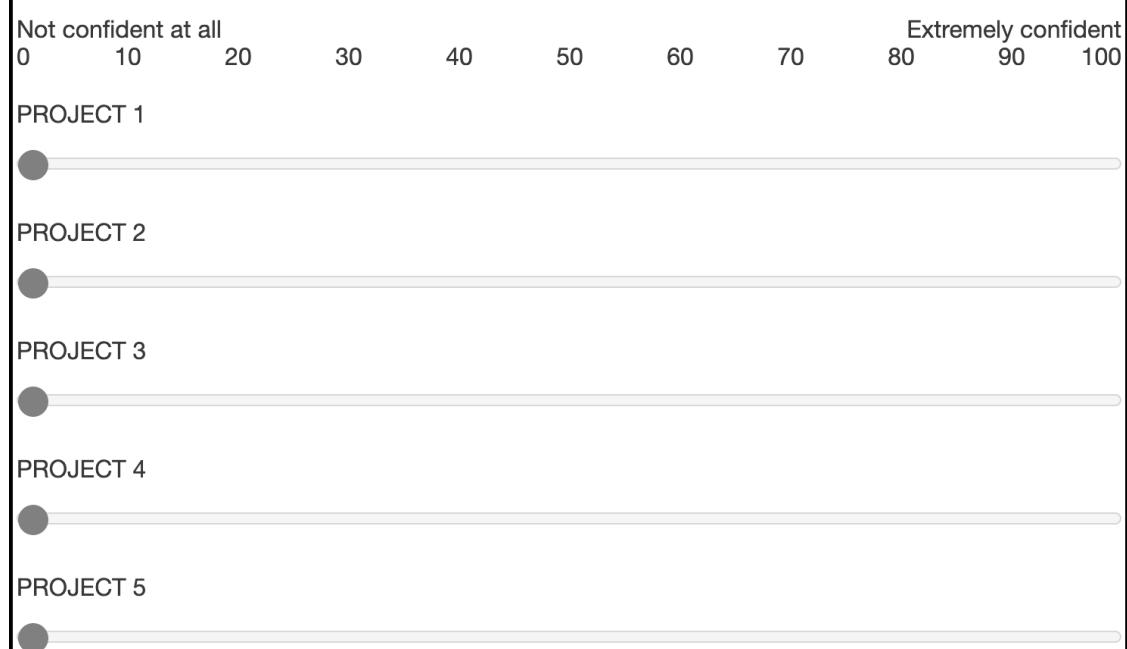
Rank the projects in order of investment priority (drag and drop).

Remember that you're trying to enter a high-quality market.

- 1 PROJECT 1
- 2 PROJECT 2
- 3 PROJECT 3
- 4 PROJECT 4
- 5 PROJECT 5

**Figure B.5:** The ranking task.

How confident are you in each of your decisions?

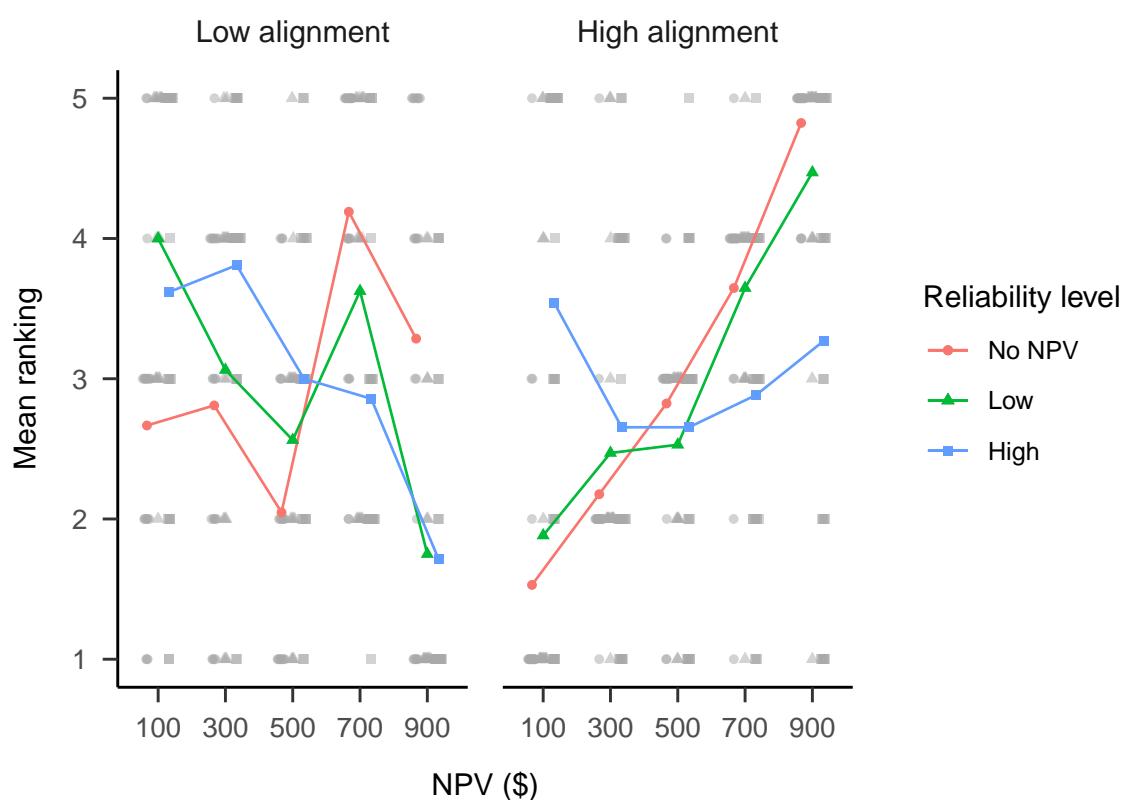


**Figure B.6:** The confidence task.

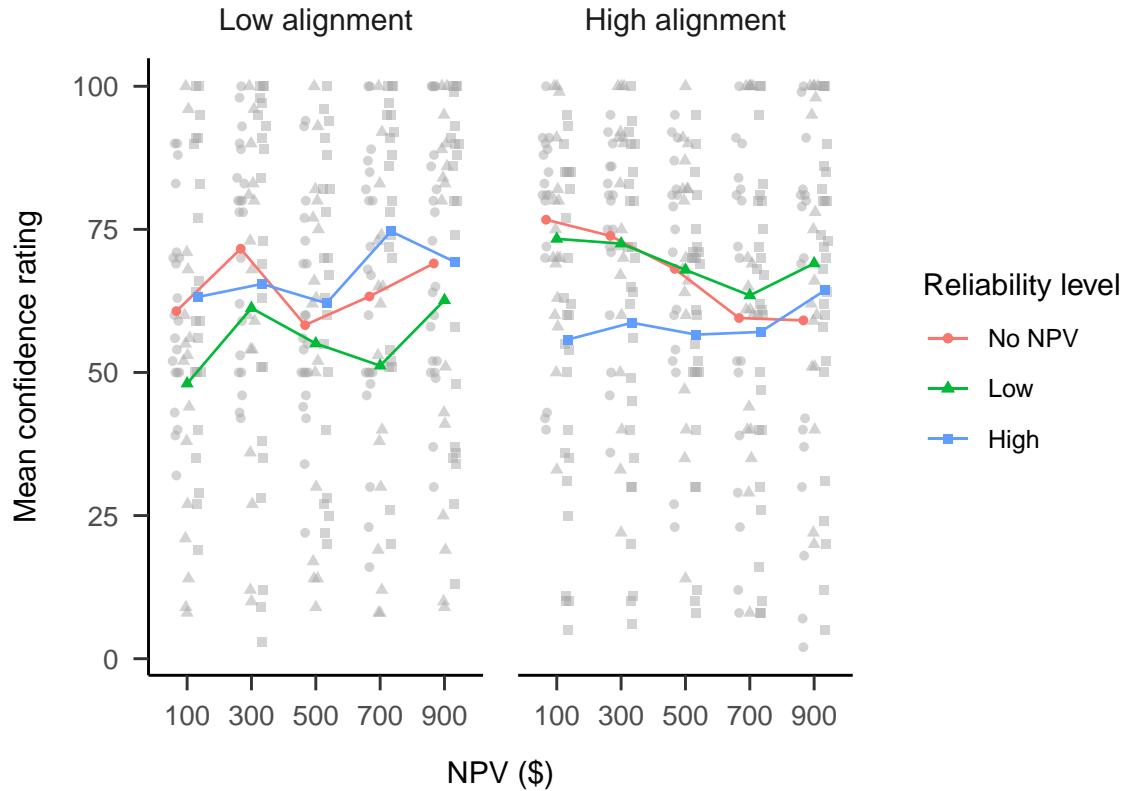
B. Chapter 4 Appendix



**Figure B.7:** The justification task.



**Figure B.8:** Mean ranking.



**Figure B.9:** Mean confidence.

### 5018 B.1.2.2 Confidence

5019 A mixed factorial ANOVA was conducted to investigate the effects of alignment  
 5020 and verbally-instructed NPV reliability on participants' confidence rating of their  
 5021 decisions. As shown in Figure B.9, the alignment  $\times$  reliability level  $\times$  NPV  
 5022 interaction was not significant,  $F(7.47, 418.08) = 1.26, p = .267, \eta_p^2 = .022$ .  
 5023 Contrary to the allocation and ranking data, in the low alignment condition,  
 5024 there were no significant differences in the linear NPV trend between the no  
 5025 NPV condition and low reliability condition,  $M = 10.73, 95\% \text{ CI } [-30.15, 51.61]$ ,  
 5026  $t(112) = 0.52, p = .604$ , nor the high reliability condition,  $M = 13.05, 95\%$   
 5027  $\text{CI } [-24.97, 51.07], t(112) = 0.68, p = .498$ . However, as above, in the high  
 5028 alignment condition, the linear NPV trend was significantly lower in the no NPV  
 5029 condition than the high reliability condition,  $M = 65.14, 95\% \text{ CI } [26.72, 103.57]$ ,  
 5030  $t(112) = 3.36, p = .001$ , and not the low reliability condition,  $M = 31.88, 95\%$   
 5031  $\text{CI } [-10.38, 74.14], t(112) = 1.49, p = .138$ .

5032     B. Chapter 4 Appendix

5032     B.1.2.3 Forecast Mean

5033     A mixed factorial ANOVA was conducted to investigate the effects of alignment  
5034     and verbally-instructed NPV reliability on participants' forecast means. As seen in  
5035     Figure B.10, the alignment  $\times$  reliability level  $\times$  NPV interaction was not significant,  
5036      $F(5.26, 142.10) = 1.89, p = .095, \hat{\eta}_p^2 = .066$ . However, the alignment  $\times$  NPV  
5037     interaction was significant,  $F(2.63, 142.10) = 2.89, p = .044, \hat{\eta}_p^2 = .051$ ; as well  
5038     as the reliability level  $\times$  NPV interaction,  $F(5.26, 142.10) = 7.91, p < .001, \hat{\eta}_p^2 =$   
5039      $.227$ . The simple effects appear to be as above. Specifically, in the low alignment  
5040     condition, the linear NPV trend was significantly lower in the no NPV condition  
5041     than both the low reliability condition,  $M = 0.19, 95\% \text{ CI } [0.09, 0.30], t(54) =$   
5042      $3.63, p = .001$ , and the high reliability condition,  $M = 0.16, 95\% \text{ CI } [0.04, 0.28]$ ,  
5043      $t(54) = 2.75, p = .008$ . However, in the high alignment condition, the linear NPV  
5044     trend was only significantly lower in the no NPV condition than the high reliability  
5045     condition,  $M = 0.22, 95\% \text{ CI } [0.11, 0.32], t(54) = 4.04, p < .001$ , and not the low  
5046     reliability condition,  $M = 0.08, 95\% \text{ CI } [-0.04, 0.21], t(54) = 1.30, p = .198$ .

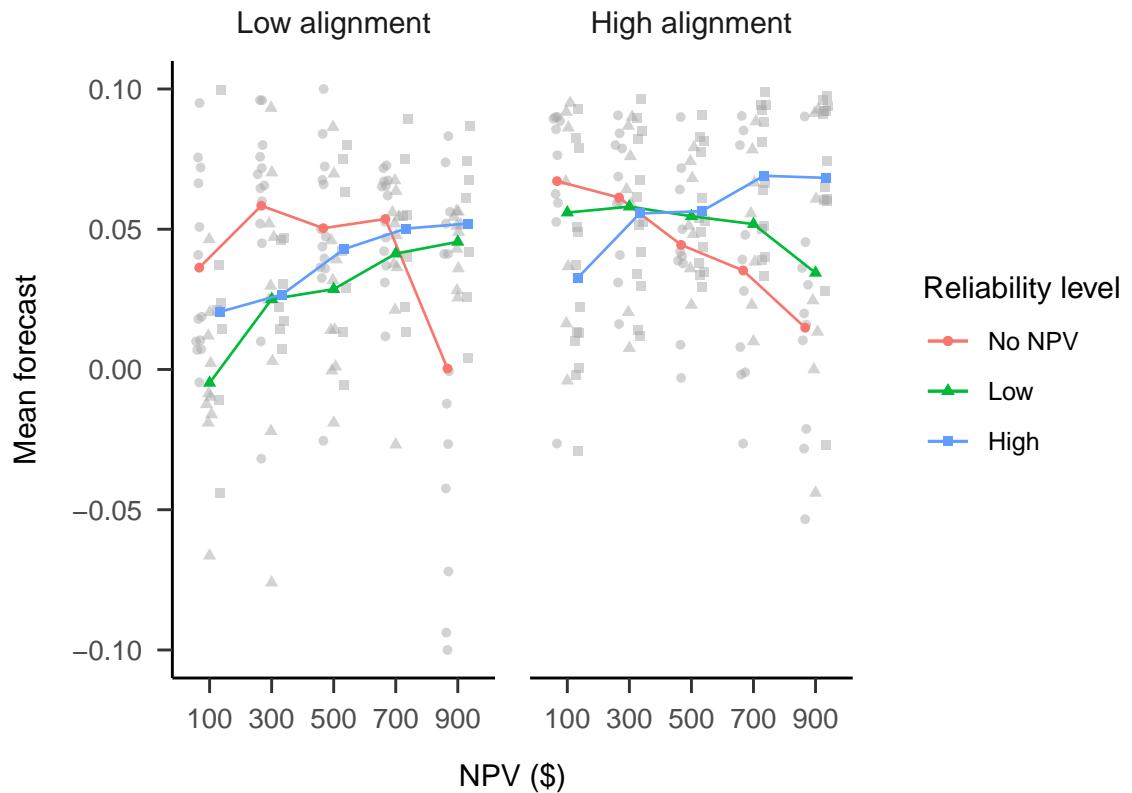
5047     B.1.2.4 Forecast SD

5048     A mixed factorial ANOVA was conducted to investigate the effects of alignment  
5049     and verbally-instructed NPV reliability on participants' forecast SDs. As seen in  
5050     Figure B.11, the alignment  $\times$  reliability level  $\times$  NPV interaction was significant,  
5051      $F(6.87, 185.42) = 2.91, p = .007, \hat{\eta}_p^2 = .097$ . However, none of the linear NPV  
5052     trends were significantly different from each other as above. Of relevance, the low  
5053     alignment condition on average had higher SDs than those in the high alignment  
5054     condition,  $F(1, 54) = 5.77, p = .020, \hat{\eta}_p^2 = .097$ .

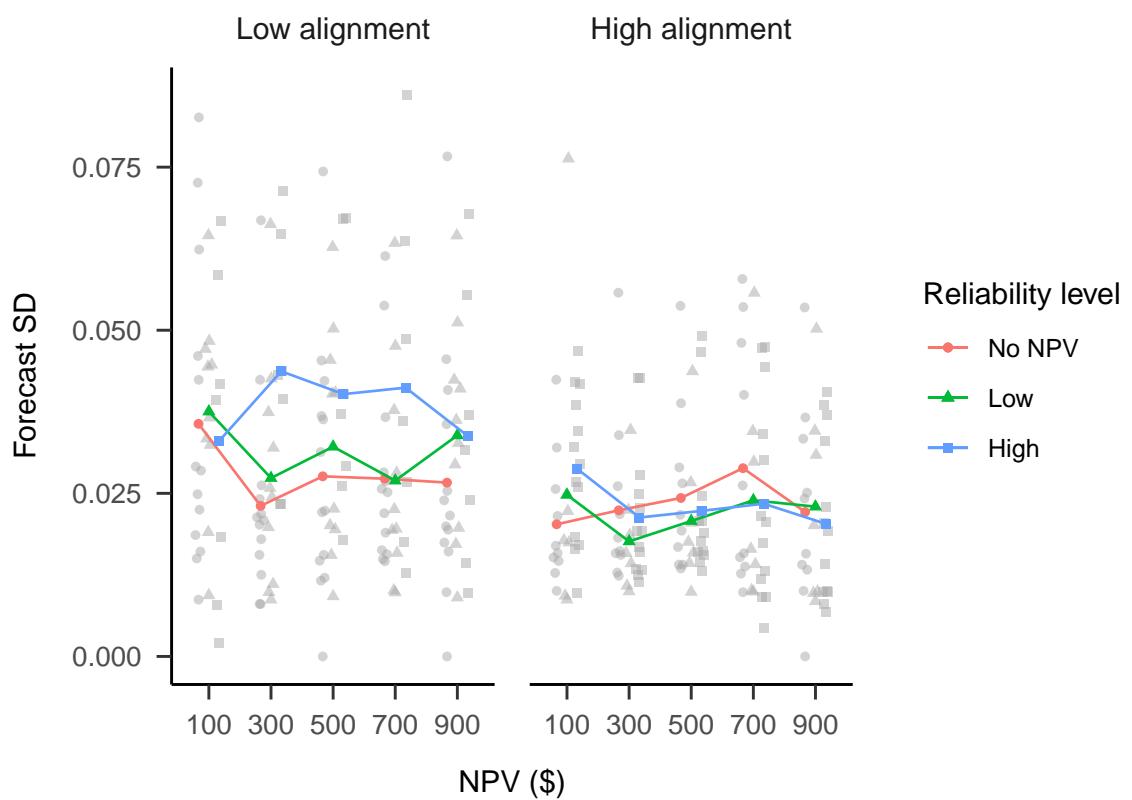
5055     B.1.3 Discussion

5056     Hypothesis B.4 was not supported, as there was no evidence of a main effect  
5057     of alignment on participants' confidence in their allocation decisions. Instead,  
5058     exploratory analyses showed that the difference in confidence between reliability  
5059     conditions is greater in the low alignment condition. This may reflect participants'

B. Chapter 4 Appendix



**Figure B.10:** Mean forecasts.



**Figure B.11:** Mean forecast SD.

## B. Chapter 4 Appendix

5060 difficulty in making sense of their choices when alignment was low, given more  
5061 confidence when assured of the reliability of NPV. In the high alignment condition,  
5062 on the other hand, regardless of reliability condition, they had a way of using  
5063 the reliability information. Further, confidence also seemed to increase more with  
5064 NPV, on average, more when projects were dissimilar, which provides evidence for  
5065 their reliance on NPV in this situation. There was limited evidence for the effect  
5066 of alignment on forecast variability. Experiments 5 and 6 attempted to replicate  
5067 this result with more participants.

## 5068 B.2 Experiment 2

### 5069 B.2.1 Method

#### 5070 B.2.1.1 Materials

5071 **B.2.1.1.1 Instructions** Figure B.12 shows the instructions.

5072 **B.2.1.1.2 NPV Test** Participants were given more extensive information about  
5073 NPV than in the previous experiment and were tested on their ability to calculate  
5074 simple averages from given numerical ranges, as shown in Figures B.13 and B.14.

5075 **B.2.1.1.3 NPV Knowledge Ratings** A similar design to Long et al. (2018,  
5076 Study 1) was used to test whether this sample may be overconfident in their  
5077 understanding on NPV. Therefore, participants were asked to rate their knowledge  
5078 of NPV in various points in the study (see the procedure in Section 4.3.1.3).  
5079 Figure B.15 shows an example of one such display.

## B. Chapter 4 Appendix

**Investment task**

You will be shown information about a number of projects that a consumer products firm is considering to invest in. Some specific information about the product itself is provided. In addition to those numbers, you will find each project's projected cash inflow for each year, and the net present value (NPV) that was calculated using those figures. The discount rate will always be 10% and the initial investment will always be \$5000. These are taken into account in the NPV calculations.

We would like you to take the role of the manager in charge of capital allocation for the firm. This firm is specifically interested in investing in the development of high-end goods, so your valuations should reflect this. That is, even though there might be a market for the lower-end products in the descriptions that you will see, **you should be aiming to invest in the products with the highest intrinsic quality.**

You will decide how to rank the projects in order of investment priority, and decide how to allocate the capital available for investment this year among the different projects. Note that this is not the operational budget (advertising, etc.), but rather the funds to be used for investment in developing the new products. You will do this by selecting a percentage value for each project, such that the budget is allocated completely among each set of projects.

Importantly, each page's set of five projects should be treated independently of the other pages' project sets.

**Figure B.12:** Experiment 2 instructions.

5080 **B.2.1.1.4 Variance Lecture** See below the slides for the variance lecture.

## 5081 **B.2.2 Results**

### 5082 **B.2.2.1 Ranking**

5083 A mixed factorial ANOVA was conducted to investigate the effects of NPV,  
5084 alignment, and numerical NPV reliability on participants' project rankings. Fig-  
5085 ure B.30 shows these data. The alignment  $\times$  reliability level  $\times$  NPV interaction  
5086 was not significant,  $F(3.00, 159.10) = 2.44, p = .066, \hat{\eta}_p^2 = .044$ . However, the  
5087 alignment  $\times$  NPV interaction was significant,  $F(3.31, 370.54) = 21.00, p < .001,$   
5088  $\hat{\eta}_p^2 = .158$ ; as well as the reliability amount  $\times$  NPV interaction,  $F(6.62, 370.54) =$   
5089  $9.73, p < .001, \hat{\eta}_p^2 = .148$ . As in the allocation data, the linear NPV trend did

## B. Chapter 4 Appendix

### Understanding NPV

Net Present Value (NPV) is used as a measure of a project's potential profitability. A positive value indicates that the project is profitable, while a negative value indicates that a project is not profitable.

When calculating NPV, the potential future cash inflows are converted to their "present values". This is important, because we know that an amount of money is more valuable in the present than it is in the future. The time value of money is accounted for by dividing each year's cash inflow by the discount rate. Finally, the sum of all the present values is deducted from the value of the initial investment.

To calculate the NPV you need the following components:

1. The cash inflow for each year of the project
2. The discount rate
4. The initial investment

Below is the generic formula for calculating NPV:

$$NPV = \frac{\text{Cash inflow for year 1}}{(1 + \text{discount rate})^1} + \frac{\text{Cash inflow for year 2}}{(1 + \text{discount rate})^2} + \frac{\text{Cash inflow for year 3}}{(1 + \text{discount rate})^3} \dots - \text{Initial investment}$$

Some of the time, it might be unclear exactly what the future cash inflow is, so it might be given as a range of possible values.

Below is an example of an NPV calculation with the discount rate calculations and initial investment already filled in. Notice that instead of a single cash inflow value, a range is provided (assume that the distribution is uniform). The value that should be used as the cash inflow is the mid point of these values. This is done by calculating the average of the two values.

For this session, you will get some practise in calculating NPV. However, we will give you the value that is in the denominator (the discount rate calculation) and the initial investment. All you need to do is calculate each year's cash inflow and enter them into the formula.

#### Example 1

$$NPV = \frac{[\text{range: } 1500 - 2500]}{1.1} + \frac{[\text{range: } 750 - 1250]}{1.21} + \frac{[\text{range: } 1875 - 3125]}{1.331} - 3000$$

Please calculate the mid-points for these ranges and type them in below:

Year 1 cash inflow	<input type="text"/>
Year 2 cash inflow	<input type="text"/>
Year 3 cash inflow	<input type="text"/>

**Figure B.13:** Experiment 2 NPV test.

B. Chapter 4 Appendix

The range for Year 1 was \$1500-\$2500.  
You calculated the Year 1 cash inflow to be \$2000.

The range for Year 2 was \$750-\$1250.  
You calculated the Year 2 cash inflow to be \$1000.

The range for Year 3 was \$1875-\$3125.  
You calculated the Year 3 cash inflow to be \$2500.

Therefore, NPV = \$1522.92

**Figure B.14:** Experiment 2 NPV test answers.

Please rate your knowledge of Net Present Value (NPV) on this 1-7 scale:

Shallow			Partial			Deep
1	2	3	4	5	6	7

NPV knowledge



**Figure B.15:** Experiment 2 NPV knowledge rating task.

NPV variance

**Figure B.16:** Variance lecture slide 1.

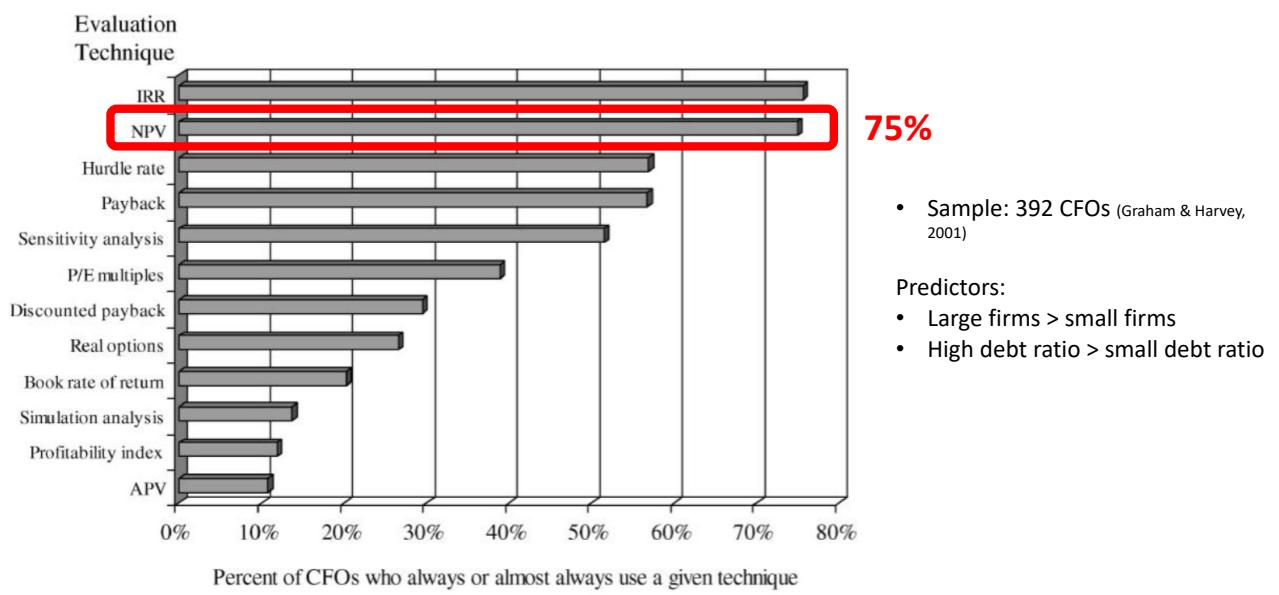
## B. Chapter 4 Appendix

### NPV

$$NPV = \frac{Year 1 \text{ inflow}}{(1+discount \% )^1} + \frac{Year 2 \text{ inflow}}{(1+discount \% )^2} + \frac{Year 3 \text{ inflow}}{(1+discount \% )^3} \dots - Initial \text{ investment}$$

**Figure B.17:** Variance lecture slide 2.

### NPV is used very frequently



**Figure B.18:** Variance lecture slide 3.

## The NPV paradox

- “Although the NPV method is criticized by both practitioners and academics, the traditional NPV calculation is by far the most commonly used tool for [exploration & production] project valuation.” (Willigers et al., 2017)
- “NPV is almost always applicable but is almost always wrong” (Fox, 2008)
- “the NPV rule as governing all capital budgeting decisions may not be appropriate” (Arya et al., 1998)

**Figure B.19:** Variance lecture slide 4.

## Consequences

- Researchers studied 174 cases of fraudulent financial reporting
  - Fraudulent “facts” vs “forecasts”
- Forecasts based on unreasonable accounting assumptions
  - Form 40% of fraud cases
  - Account for 44% of economic losses
- Total damages by fraudulent *facts*: US\$ 27 billion
- Total damages by fraudulent *forecasts*: US\$ 23 billion

**Figure B.20:** Variance lecture slide 5.

## NPV

$$NPV = \frac{\text{Year 1 inflow}}{(1+discount\%)^1} + \frac{\text{Year 2 inflow}}{(1+discount\%)^2} + \frac{\text{Year 3 inflow}}{(1+discount\%)^3} \dots - \text{Initial investment}$$

Where do these cash inflows come from?

Figure B.21: Variance lecture slide 6.

“It’s impossible to forecast most projects’ actual cash flows accurately” (Myers, 1984)

Figure B.22: Variance lecture slide 7.

## Forecasting is error-prone

- Future forecasts tend to be overly-optimistic
  - For longevity
  - For relationships
  - When dopamine is increased
  - In animal behaviour
- Executives are similarly overly-optimistic
  - In stock market returns
  - For firm earnings

**Figure B.23:** Variance lecture slide 8.

## Forecasting is error-prone

- CFO survey between 2001-2011
- *Over the next year, I expect the annual S&P 500 return will be:*
  - *There is a 1-in-10 chance the actual return will be less than \_\_\_\_%.*
  - *I expect the return to be: \_\_\_\_%.*
  - *There is a 1-in-10 chance the actual return will be greater than \_\_\_\_%.*
- 13,346 estimates

**Figure B.24:** Variance lecture slide 9.

## Forecasting is error-prone

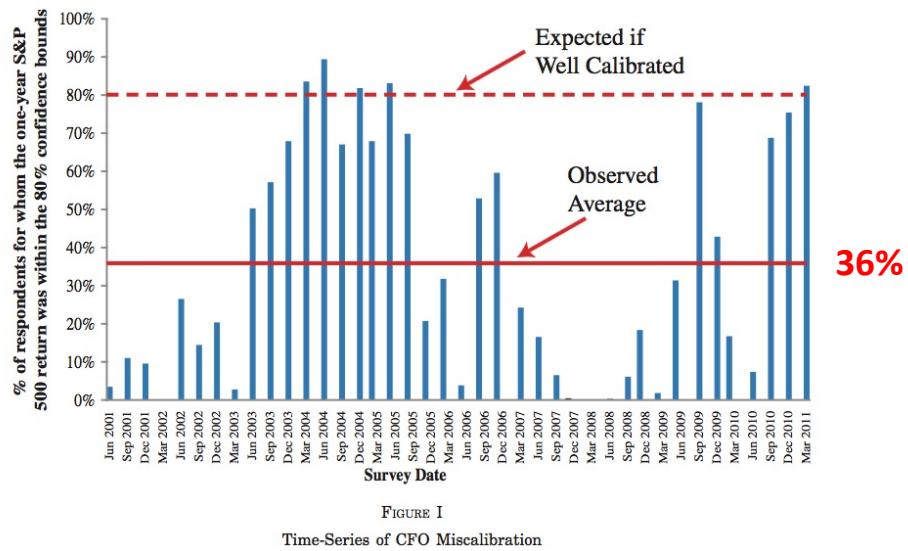


Figure B.25: Variance lecture slide 10.

## Paying attention to variance

- Ranges are frequently used for forecast estimates
  - 80% of the time between 2002-2010
- Taking account of variance increases forecasting accuracy

Figure B.26: Variance lecture slide 11.

## Paying attention to variance - Example

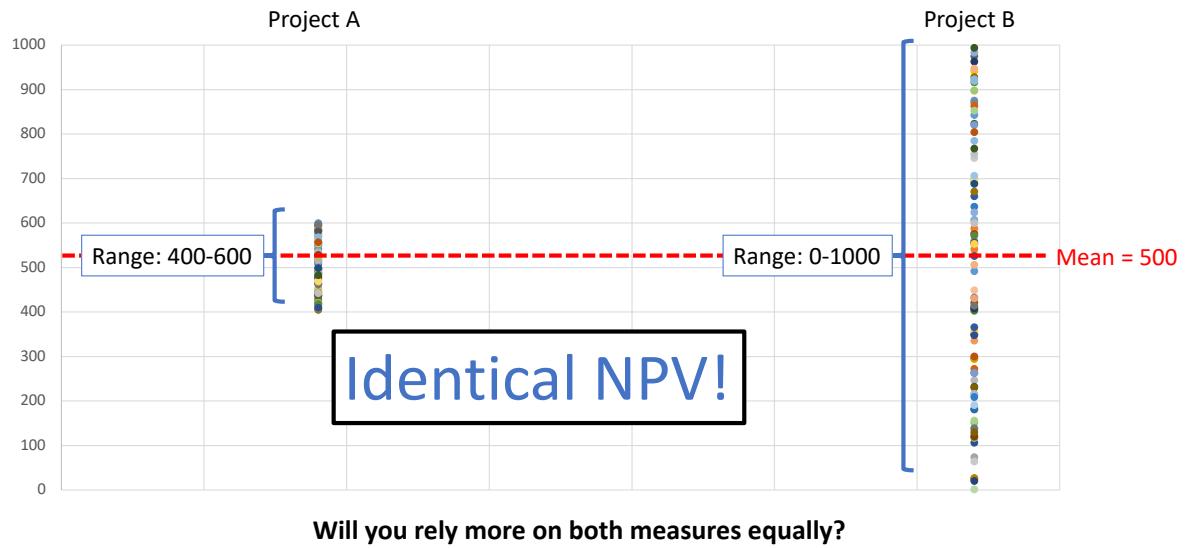


Figure B.27: Variance lecture slide 12.

## Summary

- NPV is used a lot, but criticised by some
- The costs of poor forecasting are potentially high
- NPV relies on forecasting
- Executives may underestimate forecast variance

Figure B.28: Variance lecture slide 13.

## Bottom line

- Pay attention to cash inflow variance
- Not all NPVs are created equal
  - NPV based on more variance should be weighted less than other measures

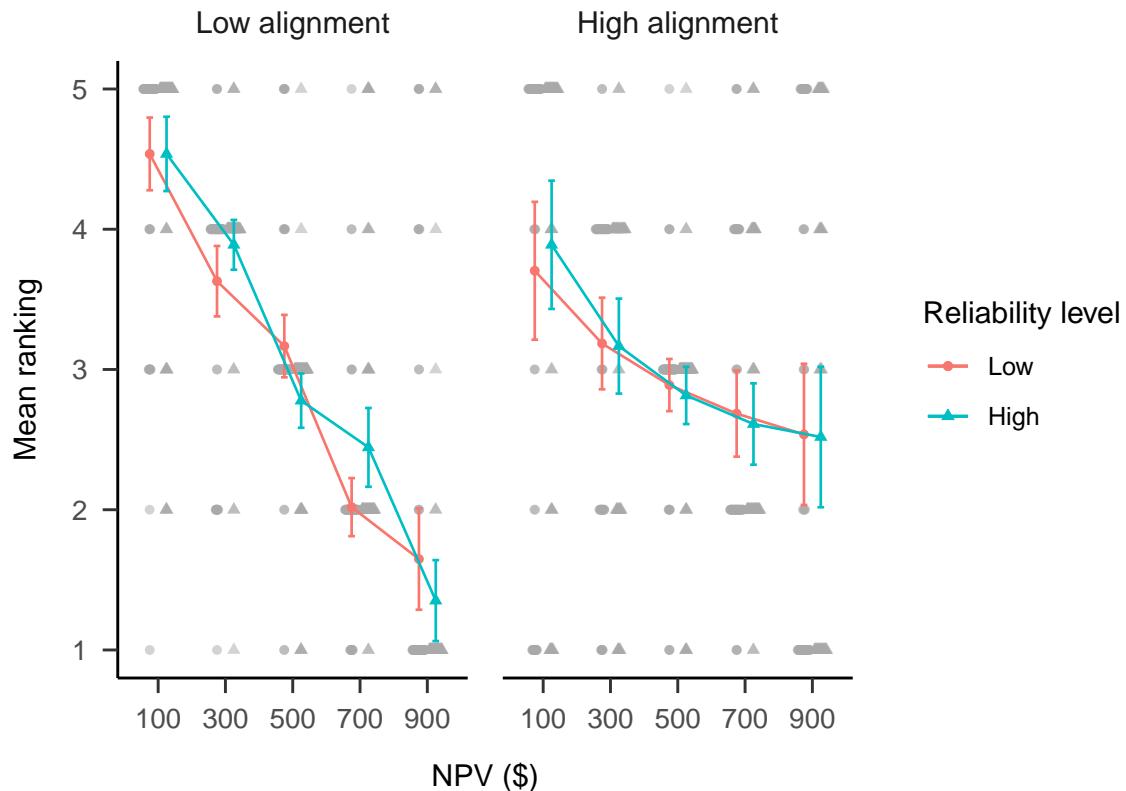
**Figure B.29:** Variance lecture slide 14.

5090 not differ between reliability level condition in neither the low alignment condi-  
5091 tion,  $\Delta M = 0.43$ , 95% CI  $[-0.77, 1.63]$ ,  $t(53) = 0.71$ ,  $p = .480$ , nor the high  
5092 alignment condition,  $\Delta M = 0.46$ , 95% CI  $[-0.92, 1.84]$ ,  $t(53) = 0.67$ ,  $p = .504$ .  
5093 However, averaging over reliability level, the linear NPV trend was higher in the  
5094 low alignment condition than in the high alignment condition,  $\Delta M = -4.54$ , 95%  
5095 CI  $[-6.39, -2.68]$ ,  $t(53) = -4.91$ ,  $p < .001$ .

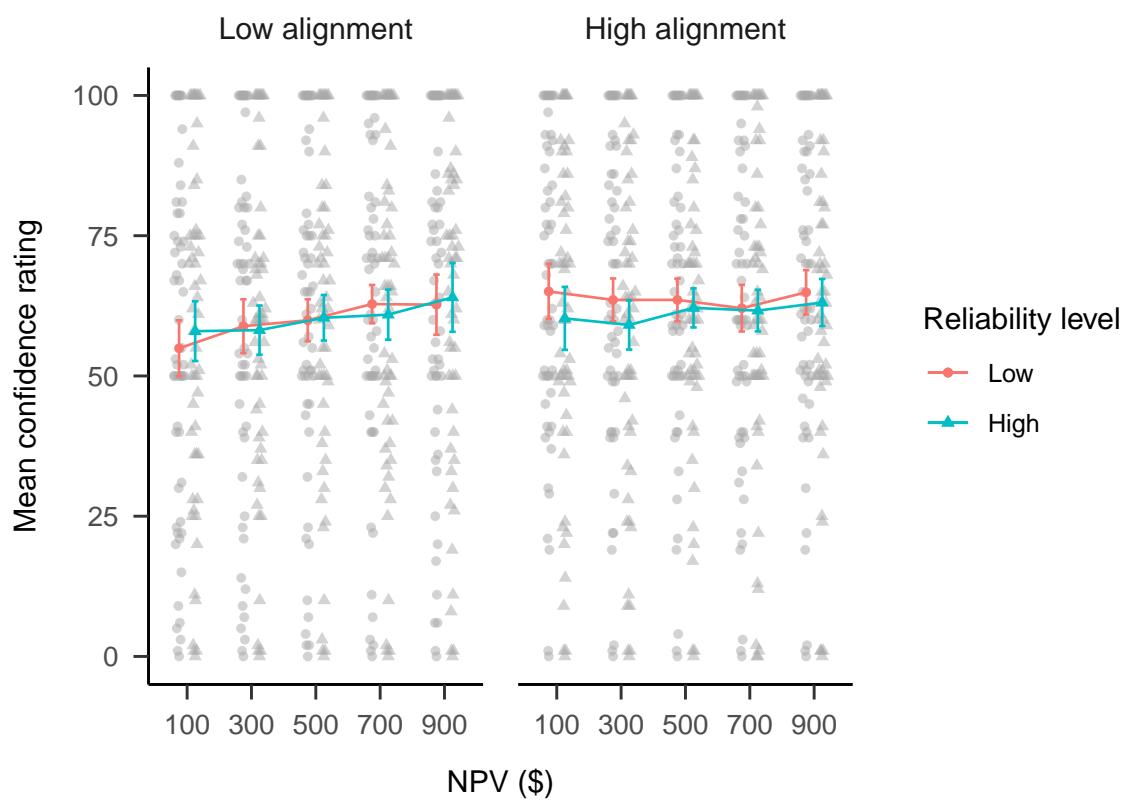
5096 **B.2.2.2 Confidence**

5097 A mixed factorial ANOVA was conducted to investigate the effects of NPV,  
5098 alignment, and numerical NPV reliability on participants' confidence ratings. Fig-  
5099 ure B.31 shows these data. Only the main effect of NPV was significant,  $F(2.62, 139.08) =$   
5100 2.97,  $p = .041$ ,  $\hat{\eta}_p^2 = .053$ .

B. Chapter 4 Appendix



**Figure B.30:** Mean ranking.



## B. Chapter 4 Appendix

### 5101 B.2.2.3 Variance Lecture

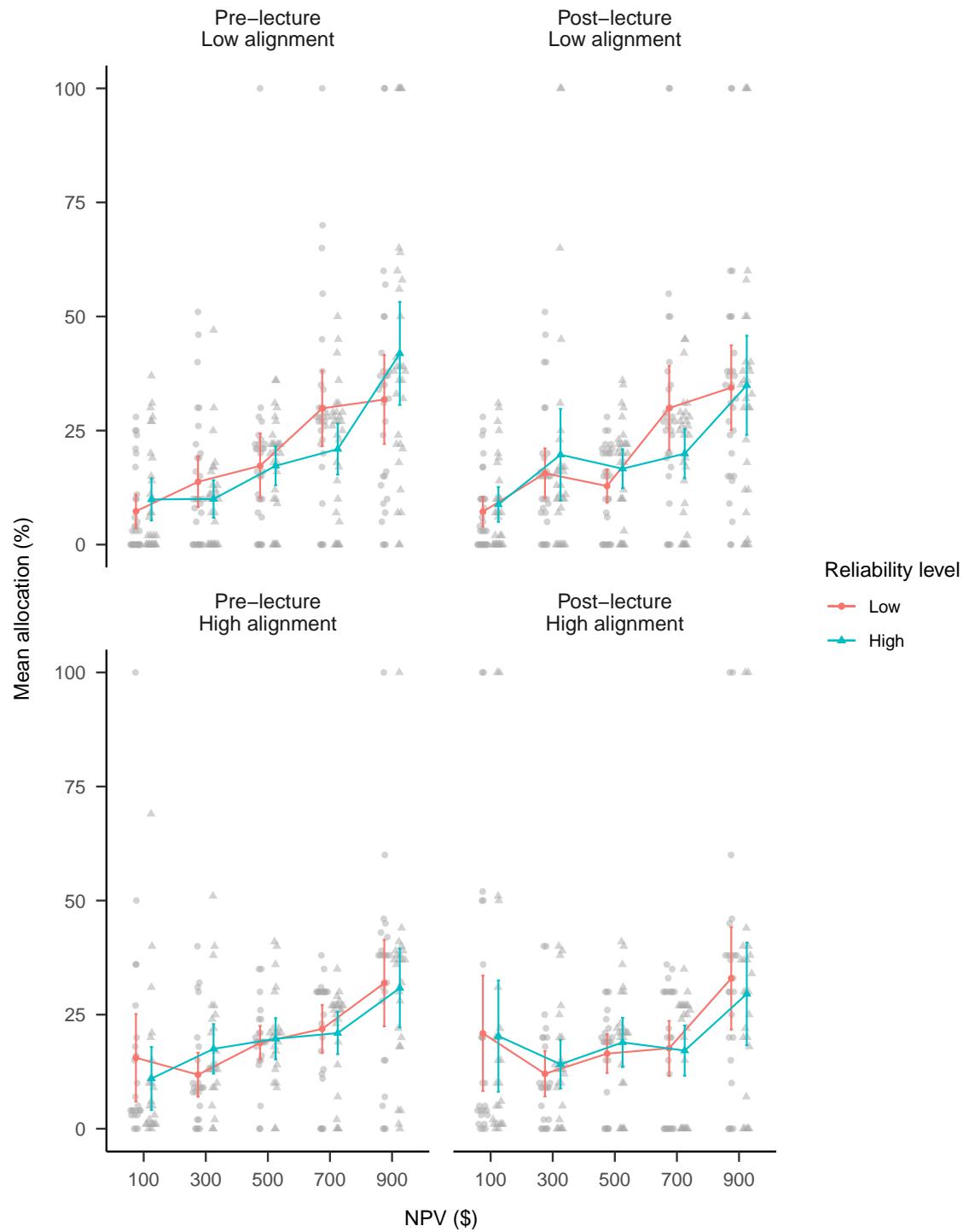
5102 The allocation and ranking data show that participants were affected by the  
5103 similarity of options, but were not affected by variance information. After the  
5104 main task of this experiment, participants were shown a short lecture about the  
5105 importance of variance information when making allocation decisions. They were  
5106 then presented with half of their previous allocations and gave them an opportunity  
5107 to amend their allocations. It was hypothesised that participants will be more  
5108 sensitive to variance after the educational intervention.

5109 A mixed factorial ANOVA was conducted to investigate the effects of phase on  
5110 participants' project allocations. As shown in Figure B.32, the four-way interaction  
5111 was not significant,  $F(2.56, 133.09) = 1.74, p = .169, \hat{\eta}_p^2 = .032$ . Further, the  
5112  $NPV \times \text{phase} \times \text{reliability level}$  interactions were not significant for either the low  
5113 alignment condition,  $\Delta M = 4.43, 95\% \text{ CI } [-23.71, 32.58], t(52) = 0.32, p = .753$ ;  
5114 or the high alignment conditions,  $\Delta M = -11.92, 95\% \text{ CI } [-43.39, 19.55], t(52) =$   
5115  $-0.76, p = .451$ . In the low alignment condition, the linear NPV trend (averaged  
5116 over reliability level) was significantly weaker after the lecture, compared with the  
5117 linear NPV trend before the lecture,  $\Delta M = -12.85, 95\% \text{ CI } [-24.08, -1.62]$ ,  
5118  $t(52) = -2.30, p = .026$ . However, this comparison was not significant in the  
5119 high alignment condition,  $\Delta M = -6.37, 95\% \text{ CI } [-18.93, 6.18], t(52) = -1.02,$   
5120  $p = .313$ . These results suggest that participants did not become better informed  
5121 by NPV numerical reliability after the variance lecture. There was, however, some  
5122 reduction in reliance on NPV overall when projects were dissimilar.

### 5123 B.2.2.4 NPV Knowledge

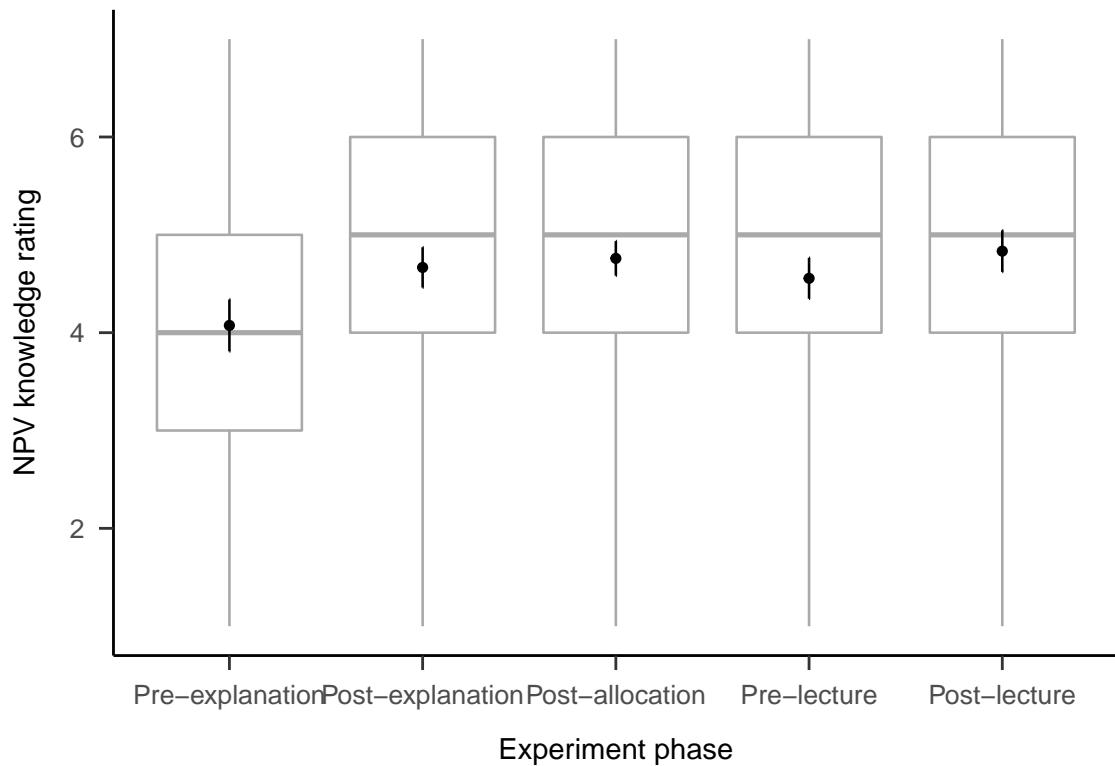
5124 A repeated-measures ANOVA was conducted to investigate the effects of ex-  
5125 periment phase condition on participants' NPV knowledge rating. Figure B.33  
5126 shows these data. The main effect of phase was significant,  $F(2.43, 128.59) = 7.80$ ,  
5127  $p < .001, \hat{\eta}_p^2 = .128$ . The post-explanation rating was significantly higher than  
5128 the pre-explanation rating,  $\Delta M = -0.59, 95\% \text{ CI } [-0.92, -0.26], t(53) = -5.07$ ,

B. Chapter 4 Appendix



**Figure B.32:** Mean allocation by NPV, reliability level, alignment, and phase.

## B. Chapter 4 Appendix



**Figure B.33:** Mean NPV knowledge rating.

<sup>5129</sup>  $p < .001$ . However, there were no significant differences in rating between any  
<sup>5130</sup> of the later phases.

## <sup>5131</sup> **B.3 Experiment 3**

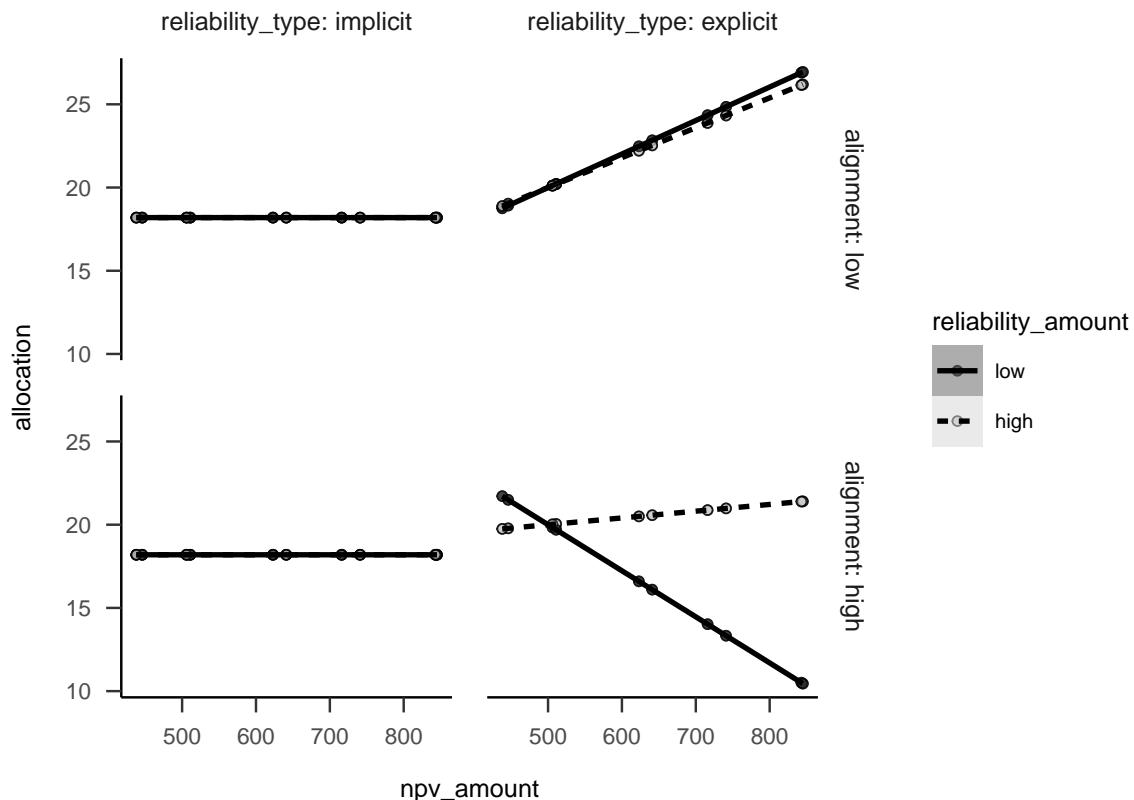
<sup>5132</sup> Figure B.34 shows the simulated hypothesised effects for Experiment 3. These  
<sup>5133</sup> effects were constructed as a composite of Experiment 1 data (without the no  
<sup>5134</sup> NPV condition) for the verbal reliability type condition, and data from a pilot  
<sup>5135</sup> study (see Appendix B.8) for the numerical reliability type condition. Variance  
<sup>5136</sup> was removed to see the effects clearer.

### <sup>5137</sup> **B.3.1 Method**

#### <sup>5138</sup> **B.3.1.1 Participants**

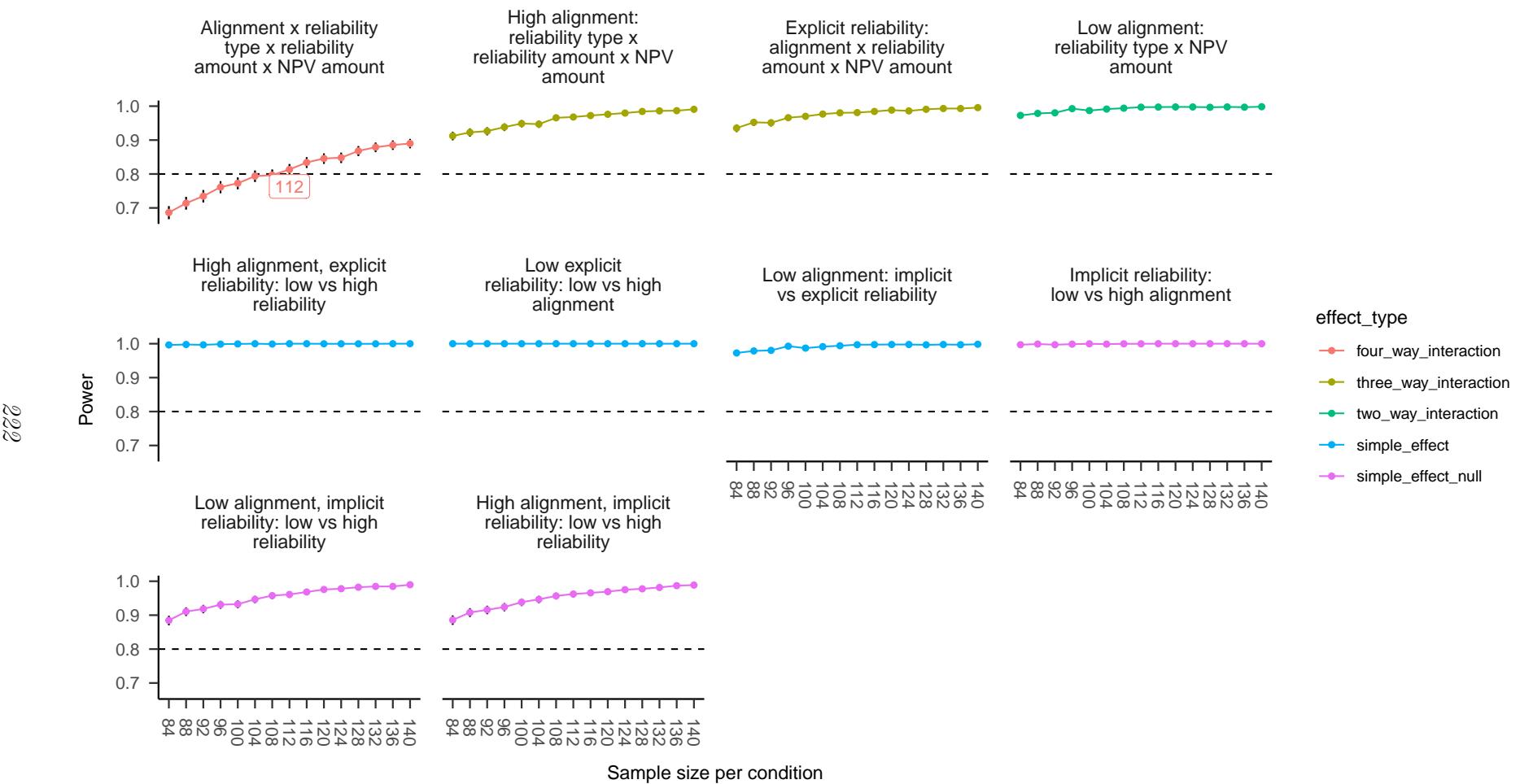
<sup>5139</sup> **B.3.1.1.1 Power Analysis** A power analysis was conducted through simula-  
<sup>5140</sup> tion of the effects hypothesised in Experiment 3 (and the simple effects implied by  
<sup>5141</sup> them). The simulated data used the same regression coefficients as Experiment 2 for

## B. Chapter 4 Appendix



**Figure B.34:** Experiment 3 predicted data.

the explicit condition, no effects for the implicit condition (as shown in Figure B.34), and the intercept and residual variance of Experiment 2. The null effects were analysed using the two one-sided tests (TOST) procedure, or *equivalence* testing (Lakens et al., 2018), and setting the smallest effect size of interest to the smallest difference that leads to a significant equivalence between low and high implicit reliability for low alignment in Experiment 8 (see Appendix B.8). Figure B.35 shows the resulting power curve. The analysis suggests a total sample size of 448 ( $112 \cdot 4$ ).



**Figure B.35:** Alignment Experiment 3 power curve. Labels indicate lowest sample size above 80% power.

## B. Chapter 4 Appendix

### 5149 B.3.1.2 Materials

5150 **B.3.1.2.1 Instructions** Figures B.36 and B.37 show the instructions for the  
5151 verbal and numerical reliability conditions, respectively.

5152 **B.3.1.2.2 Interstitial Display** Figure B.38 shows an example of an interstitial  
5153 display.

## 5154 B.3.2 Results

### 5155 B.3.2.1 Allocation

5156 The three-way interaction (reliability level  $\times$  NPV  $\times$  reliability type) in the high  
5157 alignment condition was significant,  $\Delta M = 35.43$ , 95% CI [20.74, 50.12],  $t(444) =$   
5158 4.74,  $p < .001$ . The NPV  $\times$  reliability type (averaging over reliability level) in  
5159 the low alignment condition was significant,  $\Delta M = 11.48$ , 95% CI [0.19, 22.77],  
5160  $t(444) = 2.00$ ,  $p = .046$ . The association between allocation and NPV for those in  
5161 the explicit low reliability condition was significantly stronger for those in the low  
5162 alignment condition, than for those in the high alignment condition,  $\Delta M = 35.68$ ,  
5163 95% CI [22.27, 49.09],  $t(444) = 5.23$ ,  $p < .001$ . The linear NPV trend for those in  
5164 the low alignment condition was significantly stronger for those in the explicit reli-  
5165 ability condition, than for those in the implicit reliability condition (averaging over  
5166 reliability level),  $\Delta M = 11.48$ , 95% CI [0.19, 22.77],  $t(444) = 2.00$ ,  $p = .046$ . The  
5167 linear NPV trend for those in the implicit reliability condition was not significantly  
5168 “equivalent” between those in the low and high reliability conditions for both those  
5169 in the low alignment  $\Delta M = 1.64$ , 95% CI [-8.74, 12.03],  $t(444) = 0.31$ ,  $p = .620$   
5170 and high alignment conditions  $\Delta M = -1.21$ , 95% CI [-11.59, 9.18],  $t(444) = 0.22$ ,  
5171  $p = .589$ . However, this is likely to be because the “lowest effect size of interest”  
5172 estimate originated from an analysis used before data collection that was different  
5173 to the one that one used after data collection. Specifically, a univariate linear  
5174 model was originally used (treating NPV as a continuous predictor), whereas the  
5175 data were ultimately analysed using a multivariate linear model (treating NPV  
5176 as a repeated measures factor). In the numerical reliability condition, a pilot

B. Chapter 4 Appendix

Imagine that you are a CEO of a large company composed of many individual businesses.

You will be shown information about a number of projects that your company is considering to invest in. Each project is independent of the others. Some specific information about the project itself is provided. In addition to those numbers, you will find each project's net present value (NPV), which is the company's estimation of the future returns of the project. An NPV that is greater than 0 (zero) indicates that there is an expectation of profit. The higher the NPV, the better the expectations for each project.

For each project, you will see an NPV, alongside a statement of whether NPV is considered to be a reliable (or an unreliable) metric for that project. There are usually a range of plausible NPV outcomes, so when NPV is considered to be "reliable" this means that the range of possible values is relatively narrow (indicating high confidence in the estimate). Conversely, when NPV is considered to be "unreliable", this means that the range of possible values is relatively wider (indicating low confidence in the estimate).

Your task is to rank the projects in order of investment priority and decide how to allocate the available budget (as a percentage) between them.

Test yourself on the above instructions. If Project A has an NPV of \$100, and Project B has an NPV of \$200, write in the following text box the name of the project that has a greater expectation of profit: Project

Continue

**Figure B.36:** Experiment 3 verbal reliability instructions.

5177 experiment (see Appendix B.8) suggested that the linear NPV trend would be  
5178 equivalent between those in the low and high alignment conditions, averaged over  
5179 reliability level. However, the test of equivalence was not significant,  $\Delta M = 15.19$ ,  
5180 95% CI [3.90, 26.48],  $t(444) = 2.64$ ,  $p = .996$ .

5181 **B.4 Experiment 4**

5182 Experiment 4 further investigated the effects of alignment and verbal NPV  
5183 reliability information on capital allocation decisions. Experiment 4 used the same  
5184 methodology as in Experiment 1 (see Section 4.3.1), except for two main changes.  
5185 First, the alignment conditions were manipulated within subjects. Second, the no  
5186 NPV condition in the NPV reliability variable was removed.

B. Chapter 4 Appendix

Imagine that you are a CEO of a large company composed of many individual businesses.

You will be shown information about a number of projects that your company is considering to invest in. Each project is independent of the others. Some specific information about the project itself is provided. In addition to those numbers, you will find each project's net present value (NPV), which is the company's estimation of the future returns of the project. An NPV that is greater than 0 (zero) indicates that there is an expectation of profit. The higher the NPV, the better the expectations for each project.

For each project, you will see a range of possible NPVs alongside a 'midpoint'. The range literally represents the range of plausible outcomes (a uniform distribution), but the midpoint is the best guess, and hence is the same as a single NPV. That is, all values within the range are equally likely, but the midpoint is still the best guess because it is the value that is closest to all the other values.

Your task is to rank the projects in order of investment priority and decide how to allocate the available budget (as a percentage) between them.

Test yourself on the above instructions. If Project A has an NPV of \$100, and Project B has an NPV of \$200, write in the following text box the name of the project that has a greater expectation of profit: Project

Continue

Figure B.37: Experiment 3 numerical reliability instructions.

You will now see project display #1. It is important that you pay attention and read through the task carefully.

To show that you are reading and paying attention, please click on the following checkbox **before** clicking on "Continue":

Continue

Figure B.38: An example of an interstitial display in Experiment 3.

## B. Chapter 4 Appendix

**Table B.1:** Experiment 4 group allocation.

Reliability level of net present value (NPV)	N
High	34
Low	37
Total	71

5187 The results of Experiment 1 were expected to replicate (see Section 4.3.2).  
5188 Specifically, it was expected that in the high alignment condition, participants  
5189 will be able to respond to each reliability condition, whereas, in the low alignment  
5190 condition, they will rely more on NPV regardless of reliability condition.

5191 In addition to the all-project allocation data analysed above, analyses for just  
5192 the “target project” are also reported. This refers to allocation of capital to the  
5193 project that had the highest NPV, but the lowest value on concrete measures in-  
5194 trinsic to the actual product (e.g., the capacity of a laptop in gigabytes). Therefore,  
5195 a higher allocation value indicated a higher reliance on NPV. Further, the method  
5196 and analyses for the confidence measure are also reported.

5197 **Hypothesis B.4.** Participants will be more confident about their decisions in the  
5198 high alignment condition than in the low alignment condition.

### 5199 B.4.1 Method

#### 5200 B.4.1.1 Participants

5201 Seventy-one participants (44 female) were recruited from the online recruitment  
5202 platform Prolific. Participants were compensated at a rate of £5 an hour (Prolific  
5203 is based in the UK). The average age was 33.27 years ( $SD = 10.21$ ,  $min. =$   
5204 18,  $max. = 65$ ). Table B.1 shows the allocation of participants to the different  
5205 conditions. The two alignment conditions (low and high) were presented within  
5206 subjects and the order of their presentation was randomised. Further, NPV was  
5207 varied within subjects.

## B. Chapter 4 Appendix

You will be shown information about a number of projects that a consumer products firm is considering to invest in. Some specific information about the product itself is provided. In addition to those numbers, you will find each project's net present value (NPV), which is the company's estimation of the future returns of the project. An NPV that is greater than 0 (zero) indicates that there is an expectation of profit. **The higher the NPV, the better the expectations for each project.** However, it is important to note that NPV is a very noisy measure relative to the other more specific measures because it relies on future forecasting. As such, **NPV is very unreliable and should be relied upon only as a last result; the specific project's measures should be used instead.**

We would like you to take the role of the manager in charge of capital allocation for the firm. This firm is specifically interested in investing in the development of high-end goods, so your valuations should reflect this. That is, even though there might be a market for the lower-end products in the descriptions that you will see, **you should be aiming to invest in the products with the highest objective value.** The features of the products that are listed matter because they reflect the direct value of the product, whereas financial measures such as NPV may reflect other factors, thus making it noisier, as mentioned above.

You will see a set of five different projects in each page, and for each set you must decide how to allocate the capital available for investment this year among the different projects. Note that this is not the operational budget (advertising, etc.), but rather the funds to be used for investment in developing the new products. You will do this by selecting a percentage value for each project, such that the budget is allocated completely among each set of projects. Critically, treat each set of projects as independent of one another; one page's project set allocation does not impact another page's allocation.

**Figure B.39:** Experiment 4 low reliability instructions.

### 5208 B.4.1.2 Materials

5209 The project display, allocation task, and confidence task were the same as in  
5210 Experiment 1 (see Section 4.2.1.2).

5211 **B.4.1.2.1 Instructions** Participants were shown similar instructions to Experi-  
5212 ment 1 (see Section 4.2.1.2.1), except for the addition of references to the multiple  
5213 displays and the removal of an explanation about the forecasting task. Figures B.39  
5214 and B.40 show the instructions for each NPV reliability condition.

## B. Chapter 4 Appendix

You will be shown information about a number of projects that a consumer products firm is considering to invest in. Some specific information about the product itself is provided. In addition to those numbers, you will find each project's net present value (NPV), which is the company's estimation of the future returns of the project. An NPV that is greater than 0 (zero) indicates that there is an expectation of profit. **The higher the NPV, the better the expectations for each project.** NPV is a very useful measure relative to the other more specific measures because it can be calculated regardless of the type of product. As such, **NPV is very reliable in most cases.**

We would like you to take the role of the manager in charge of capital allocation for the firm. This firm is specifically interested in investing in the development of high-end goods, so your valuations should reflect this. That is, even though there might be a market for the lower-end products in the descriptions that you will see, **you should be aiming to invest in the products with the highest objective value.**

You will see a set of five different projects in each page, and for each set you must decide how to allocate the capital available for investment this year among the different projects. Note that this is not the operational budget (advertising, etc.), but rather the funds to be used for investment in developing the new products. You will do this by selecting a percentage value for each project, such that the budget is allocated completely among each set of projects. Critically, treat each set of projects as independent of one another; one page's project set allocation does not impact another page's allocation.

**Figure B.40:** Experiment 4 high reliability instructions.

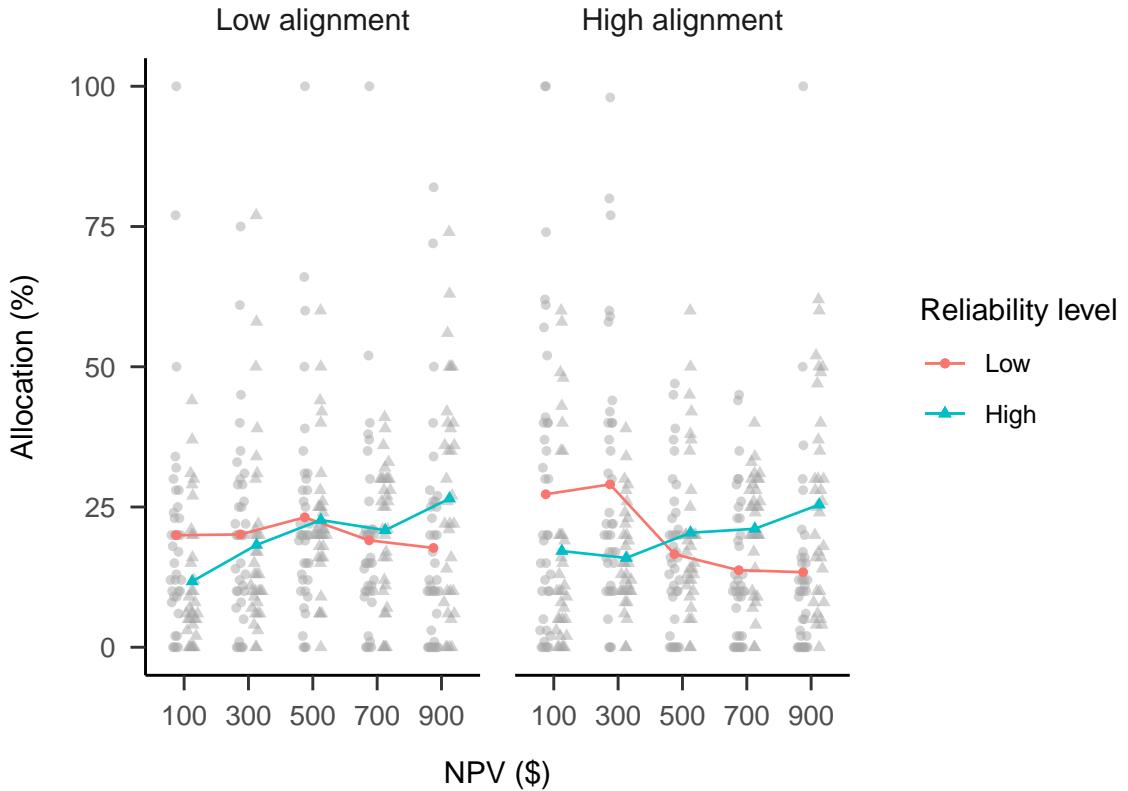
### 5215 B.4.1.3 Procedure

5216 The procedure was the same as in Experiment 1, except that there were no  
5217 forecasting or ranking tasks.

## 5218 B.4.2 Results

5219 A mixed factorial ANOVA was conducted to investigate the effects of alignment,  
5220 verbal NPV reliability, and NPV on participants' project allocations. As seen in  
5221 Figure B.41, the alignment  $\times$  reliability level  $\times$  NPV interaction was not significant,  
5222  $F(3.64, 250.93) = 1.71, p = .153, \hat{\eta}_p^2 = .024$ . This is most likely due to the fact  
5223 that the reliability level  $\times$  NPV interaction was significant in the high alignment  
5224 condition,  $\Delta M = -64.82, 95\% \text{ CI } [-102.70, -26.93], t(69) = -3.41, p = .001$ ,  
5225 the low alignment condition,  $\Delta M = -37.74, 95\% \text{ CI } [-70.92, -4.56], t(69) =$

## B. Chapter 4 Appendix



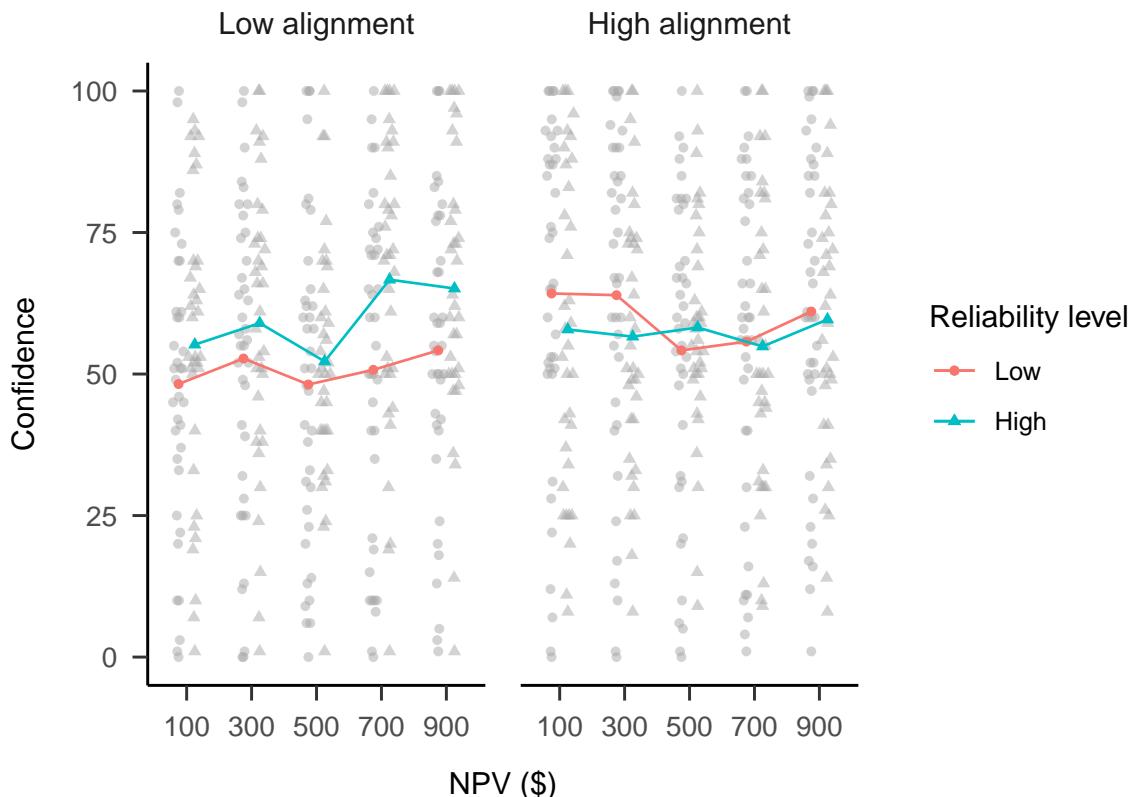
**Figure B.41:** Mean project allocation in Experiment 4. Error bars represent 95% confidence intervals based on the multivariate model. Note that this mixed factorial design does not allow for using confidence intervals to make inferences by “eye” across conditions.

5226  $-2.27$ ,  $p = .026$ , as well as averaging over alignment conditions,  $F(2.98, 205.65) =$   
 5227  $4.90$ ,  $p = .003$ ,  $\hat{\eta}_p^2 = .066$ . Despite this, the alignment  $\times$  NPV interaction was  
 5228 significant,  $F(3.64, 250.93) = 3.19$ ,  $p = .017$ ,  $\hat{\eta}_p^2 = .044$ , such that the linear trend  
 5229 of NPV was stronger in the low alignment,  $\Delta M = 13.28$ , 95% CI  $[-3.31, 29.87]$ ,  
 5230  $t(69) = 1.60$ ,  $p = .115$  than in the high alignment condition,  $\Delta M = -10.67$ , 95%  
 5231 CI  $[-29.62, 8.27]$ ,  $t(69) = -1.12$ ,  $p = .265$ . However, neither of these trends  
 5232 were individually significant.

### 5233 B.4.2.1 Confidence

5234 A mixed factorial ANOVA was conducted to investigate the effects of alignment,  
 5235 verbal NPV reliability, and NPV on participants’ confidence in their allocations.  
 5236 As shown in Figure B.42, the difference between alignment conditions was not  
 5237 significant,  $F(1, 69) = 2.76$ ,  $p = .101$ ,  $\hat{\eta}_p^2 = .038$ . However, the reliability  $\times$

B. Chapter 4 Appendix



**Figure B.42:** Mean confidence. Error bars represent 95% confidence intervals based on the multivariate model. Note that this mixed factorial design does not allow for using confidence intervals to make inferences by “eye” across conditions.

5238 alignment interaction was significant, as well as the  $NPV \times$  alignment interaction.  
 5239 An exploratory analysis was conducted of the relevant simple effects for each  
 5240 interaction, applying a Šidák correction to the p values for each effect. None  
 5241 of the simple effects were significant after the correction.

5242 The raw mean differences indicated that there was a greater difference be-  
 5243 tween reliability conditions in the low alignment condition,  $\Delta M = -8.83$ , 95% CI  
 5244  $[-17.84, 0.18]$ ,  $t(69) = -1.95$ ,  $p = .055$  compared to the high alignment condition,  
 5245  $\Delta M = 2.37$ , 95% CI  $[-8.65, 13.40]$ ,  $t(69) = 0.43$ ,  $p = .669$ . Further, there was a  
 5246 stronger linear trend of NPV in the low alignment condition,  $\Delta M = 18.70$ , 95% CI  
 5247  $[-0.87, 38.26]$ ,  $t(69) = 2.44$ ,  $p = .067$  compared to the high alignment condition,  
 5248  $\Delta M = -6.40$ , 95% CI  $[-26.84, 14.04]$ ,  $t(69) = -0.80$ ,  $p = .891$ .

## B. Chapter 4 Appendix

### B.4.3 Discussion

Experiment 4 found evidence for most of the hypotheses. As per Hypothesis 4.4, laypeople responded appropriately to verbal reliability instructions in the high alignment condition. Contrary to Hypothesis 4.5, however, participants also did this in the low reliability condition. That is, regardless of the type of project display, participants tended to use NPV more when they were told that it was reliable and tended to use it less when they were told that it was unreliable. Further, there was no evidence that this effect was depended on alignment condition, contrary to Hypothesis 4.3. However, the linear NPV trend was higher in the high than low alignment condition, when averaging over reliability level, as predicted in Hypothesis 4.2. This suggests that overall participants still make more use of NPV information when it is hard to compare between projects.

Hypothesis B.4 was not supported, as there was no evidence of a main effect of alignment on participants' confidence in their allocation decisions. Instead, exploratory analyses showed that the difference in confidence between reliability conditions was greater in the low alignment condition. This may reflect participants' difficulty in making sense of their choices when alignment was low, given more confidence when assured of the reliability of NPV. In the high alignment condition, on the other hand, regardless of reliability condition, they had a way of using the reliability information. Further, confidence also seemed to increase more with NPV, on average, more when projects were dissimilar, which provides evidence for their reliance on NPV in this situation.

### B.5 Experiment 5

Experiment 5 further investigated the effects of alignment and explicit NPV presence information on forecasting. The goal of this experiment was to replicate the forecasting results of Experiment 1, but with a sample that has investing experience. As before, the hypothesis was that people's forecasting would be less

## B. Chapter 4 Appendix

**Table B.2:** Experiment 5 group allocation.

Project alignment	Reliability level of net present value (NPV)	N
High	Absent	19
High	Present	17
Low	Absent	14
Low	Present	10
Total		60

variable when comparing projects with alignable differences, than when comparing projects with non-alignable differences.

### B.5.1 Method

#### B.5.1.1 Participants

Sixty participants (2 female) were recruited from Reddit. Participants were compensated with a virtual Gold Award, which gives the recipient a week of a premium version of Reddit and 100 virtual coins. The average age was 28.17 years ( $SD = 8.73$ ,  $min. = 16$ ,  $max. = 61$ ). Table B.2 shows the allocation of participants to the different conditions.

#### B.5.1.2 Materials

**B.5.1.2.1 Risky Investment Task** The only task that was used was the forecasting task used in Experiment 1, except that it was fixed by adding the relevant percentage intervals that were left out in Experiment 1, seen in Figure B.43.

#### B.5.1.3 Procedure

The procedure was the same as in Experiment 1, except participants only completed the forecasting task.

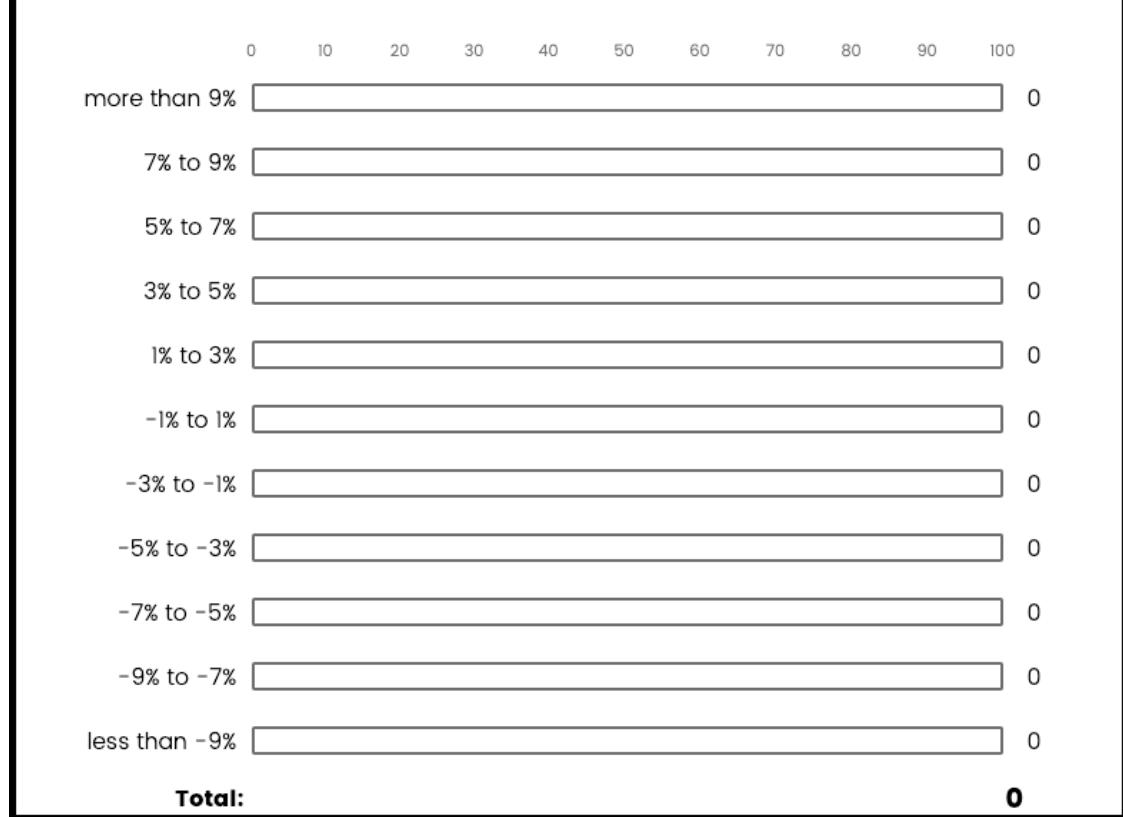
### B.5.2 Results

#### B.5.2.1 Forecast Mean

A mixed factorial ANOVA was conducted to investigate the effects of alignment and NPV presence on participants' forecasts. As shown in Figure B.44, the align-

## B. Chapter 4 Appendix

Imagine that you have 100 points to assign to the following options for Project 1's rate of return on investment after one year. Assign points according to how likely you think each rate of return is.



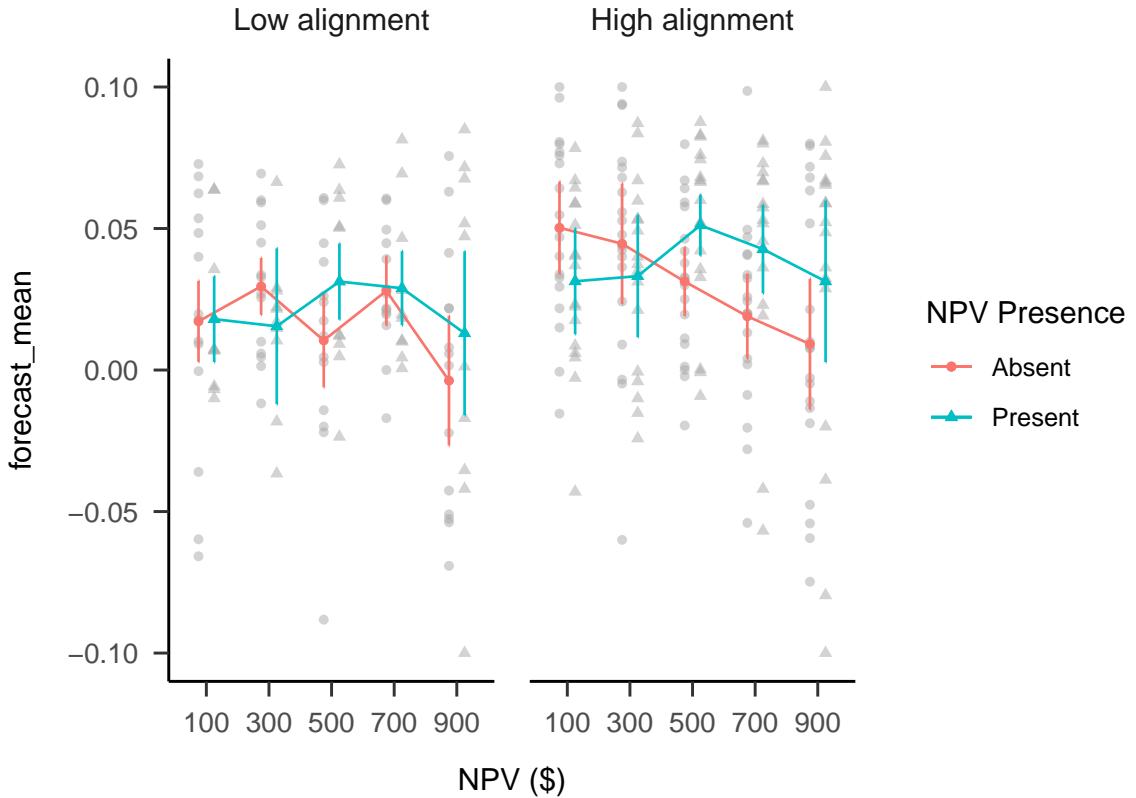
**Figure B.43:** An example of the forecasting task in Experiment 5.

ment  $\times$  reliability level  $\times$  NPV interaction was not significant,  $F(2.75, 154.16) = 0.72$ ,  $p = .531$ ,  $\hat{\eta}_p^2 = .013$ . Despite this, as in the previous experiments, the interaction between the linear NPV trend and NPV presence was significant in the high alignment condition,  $M = -0.12$ , 95% CI  $[-0.21, -0.02]$ ,  $t(56) = -2.50$ ,  $p = .015$ , but not in the low alignment condition,  $M = -0.05$ , 95% CI  $[-0.16, 0.07]$ ,  $t(56) = -0.81$ ,  $p = .424$ .

### 5302 **B.5.2.2 Forecast SD**

5303 A mixed factorial ANOVA was conducted to investigate the effects of alignment  
5304 and NPV presence on participants' forecast SDs. As shown in Figure B.45, there  
5305 were no significant differences between alignment conditions,  $F(1, 56) = 0.41$ ,  
5306  $p = .522$ ,  $\hat{\eta}_p^2 = .007$ . The alignment  $\times$  reliability level  $\times$  NPV interaction was

## B. Chapter 4 Appendix



**Figure B.44:** Mean forecasts.

not significant,  $F(2.99, 167.18) = 1.27, p = .287, \hat{\eta}_p^2 = .022$ . However, as above, the interaction between the linear NPV trend and NPV presence was significant in the high alignment condition,  $M = 0.02, 95\% \text{ CI } [0.00, 0.04], t(56) = 2.06, p = .045$ , but not in the low alignment condition,  $M = 0.01, 95\% \text{ CI } [-0.02, 0.03], t(56) = 0.38, p = .709$ .

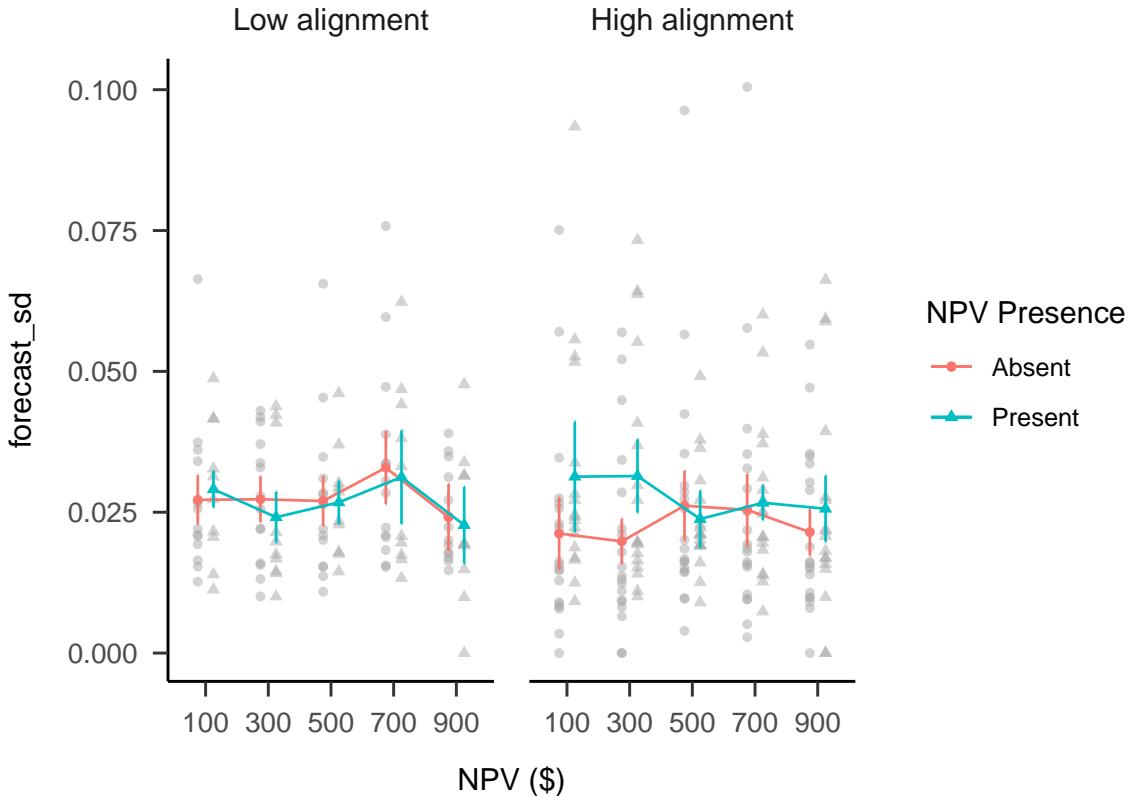
### B.5.3 Discussion

Experiment 5 found that people with some investing experience responded to alignable information in the form of NPV when it is given, but did not show the same effect of alignment on forecast SD that was seen in Experiment 1.

## B.6 Experiment 6

Experiment 6 further investigated the effects of alignment and NPV Presence information on forecasting. Experiment 5 did not clearly replicate the forecasting

B. Chapter 4 Appendix



**Figure B.45:** Mean forecast SD.

5319 results of Experiment 1, potentially due to low power, so this experiment collected  
 5320 a much larger sample size. As before, it was hypothesised that people's forecasting  
 5321 would be less variable when comparing projects with alignable differences, than  
 5322 when comparing projects with non-alignable differences.

5323 **B.6.1 Method**

5324 **B.6.1.1 Participants**

5325 Three hundred and eighty-nine participants (170 female) were recruited from  
 5326 the online recruitment platform Prolific. Participants were compensated at a rate  
 5327 of £5 an hour (Prolific is based in the UK). The average age was 32.39 years ( $SD$   
 5328 = 11.89,  $min.$  = 18,  $max.$  = 75). Table B.3 shows the condition allocation.

5329 **B.6.1.2 Materials**

5330 The materials were the same as in Experiment 5.

## B. Chapter 4 Appendix

**Table B.3:** Experiment 6 group allocation.

Project alignment	Reliability level of net present value (NPV)	N
High	Absent	97
High	Present	87
Low	Absent	101
Low	Present	104
Total		389

### 5331 **B.6.1.3 Procedure**

5332 The procedure was the same as in Experiment 5.

## 5333 **B.6.2 Results**

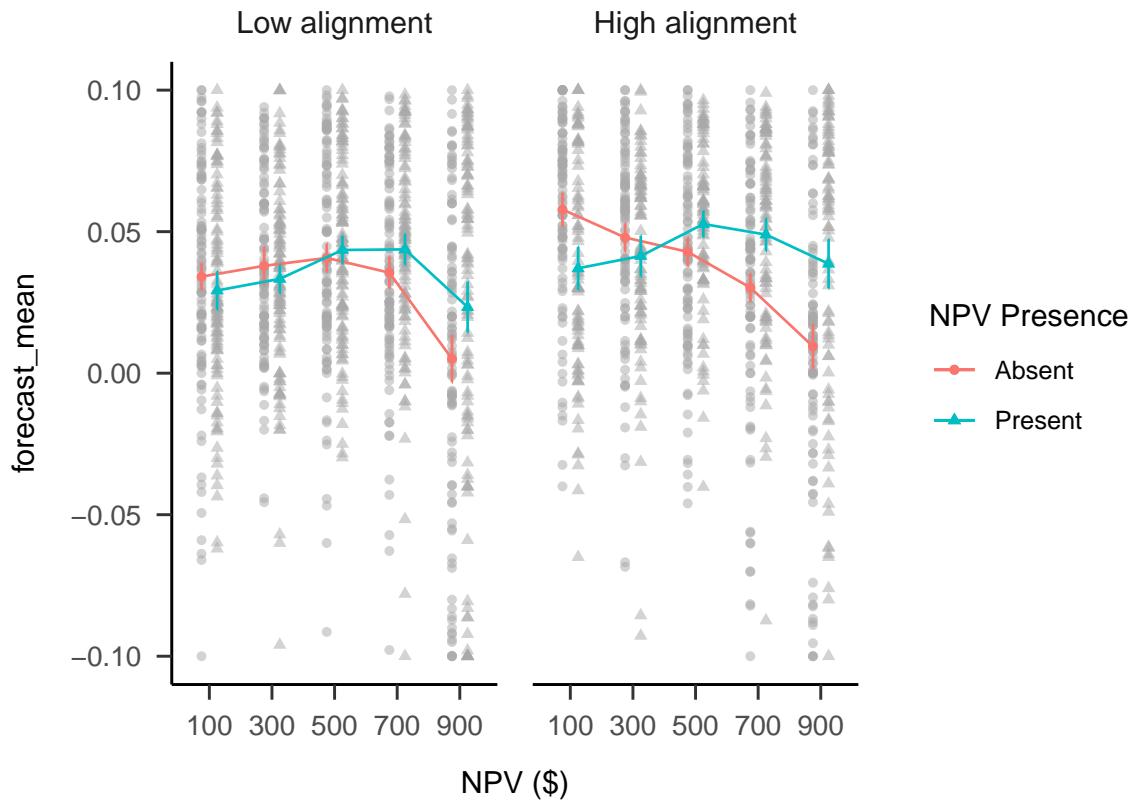
### 5334 **B.6.2.1 Forecast Mean**

5335 A mixed factorial ANOVA was conducted to investigate the effects of alignment  
5336 and NPV presence on participants' forecasts. As shown in Figure B.46, the align-  
5337 ment  $\times$  reliability level  $\times$  NPV interaction was significant,  $F(3.08, 1, 186.45) =$   
5338 3.13,  $p = .024$ ,  $\hat{\eta}_p^2 = .008$ . As in the previous experiments, the interaction  
5339 between the linear NPV trend and NPV presence was significant in both the  
5340 high alignment condition,  $M = -0.13$ , 95% CI  $[-0.16, -0.09]$ ,  $t(385) = -6.57$ ,  
5341  $p < .001$ , and in the low alignment condition,  $M = -0.06$ , 95% CI  $[-0.09, -0.02]$ ,  
5342  $t(385) = -3.28$ ,  $p = .001$ .

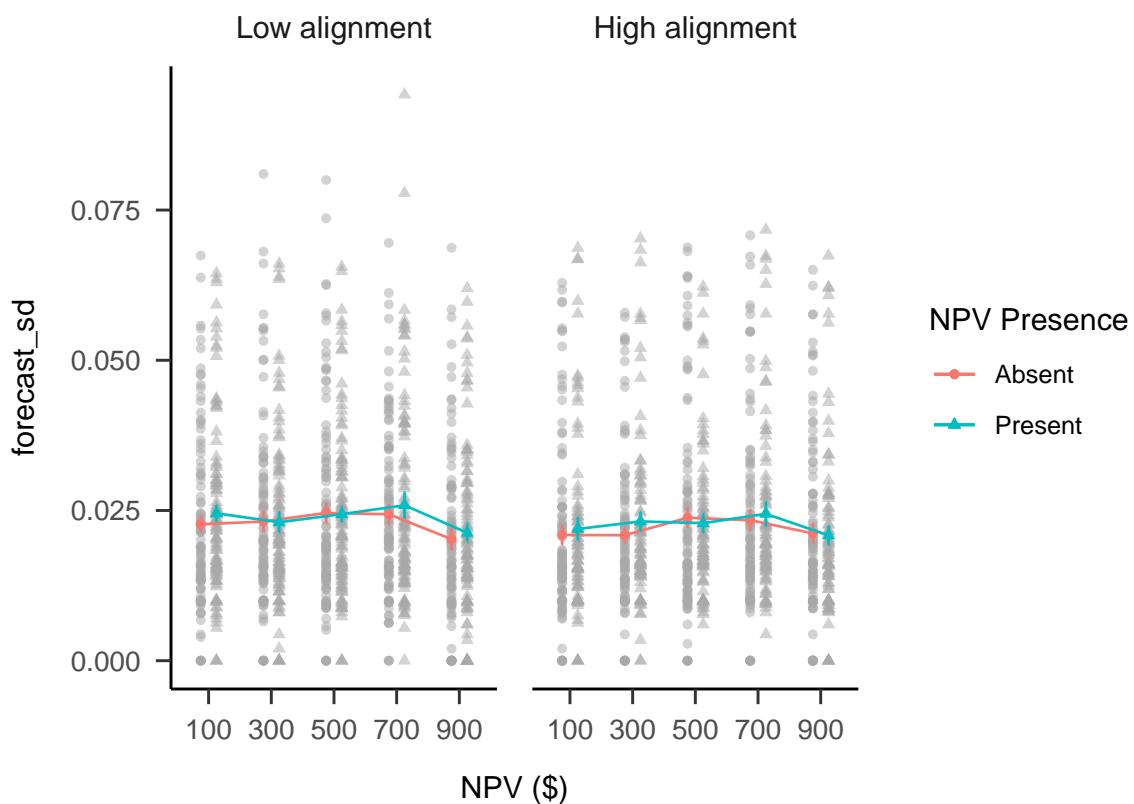
### 5343 **B.6.2.2 Forecast SD**

5344 A mixed factorial ANOVA was conducted to investigate the effects of alignment  
5345 and NPV presence on participants' forecast SDs. As shown in Figure B.47, the  
5346 alignment  $\times$  reliability level  $\times$  NPV interaction was not significant,  $F(3.45, 1, 328.06) =$   
5347 0.82,  $p = .496$ ,  $\hat{\eta}_p^2 = .002$ . The main effect of alignment was not significant,  
5348  $F(1, 385) = 0.64$ ,  $p = .424$ ,  $\hat{\eta}_p^2 = .002$ .

B. Chapter 4 Appendix



**Figure B.46:** Mean forecasts.



**Figure B.47:** Mean forecast SD.

## B. Chapter 4 Appendix

### B.6.3 Discussion

5349      Experiment 6 did not replicate the effect of alignment on forecast SD seen in  
5350      Experiment 1. However, participants still seemed to pay attention to the task, as  
5351      seen in their higher forecasts for the high NPV project when NPV was present.  
5352

## B.7 Experiment 7

5353      Experiment 7 investigated potential ways to facilitate people's use of variance  
5354      in capital allocation. Arguably, people's decisions should depend on variance, espe-  
5355      cially with a small set of projects. That is, when considering between two potential  
5356      measures to use for capital allocation, measures with narrow ranges should be relied  
5357      upon more than those with wider ranges. As such, this experiment presented  
5358      participants with the same capital allocation scenario as in Experiment 2, but  
5359      only in low numerical reliability displays. Experiment 7 varied both the variance  
5360      associated with NPV, and the extent to which participants were explicitly hinted  
5361      to use the variance information. It was predicted that participants' allocations  
5362      would be more likely to be informed by variance when told explicitly to do so  
5363      with increased salience for variance, than when only salience is increase, or when  
5364      no hint is given.  
5365

### B.7.1 Method

#### B.7.1.1 Participants

5366      Seventy-nine participants (35 female) were recruited from the online recruitment  
5367      platform Prolific. Participants were compensated at a rate of £5 an hour (Prolific  
5368      is based in the UK). The average age was 31.15 years ( $SD = 11.11$ ,  $min. = 16$ ,  $max.$   
5369      = 71). Table B.4 shows the allocation of participants to the different conditions.  
5370  
5371

#### B.7.1.2 Instructions

5372      As shown in Figure B.48, participants in the no hint condition saw the same  
5373      instructions as in Experiment 1. As shown in Figure B.49, those in the salience  
5374

## B. Chapter 4 Appendix

**Table B.4:** Experiment 7 group allocation.

Hint	Variance	N
Hint salience	High	11
Hint salience	Low	11
No hint	High	9
No hint	Low	13
Salience only	High	19
Salience only	Low	16
Total		79

5375 only condition saw the instructions along with a sentence that drew attention to  
5376 the *Cash inflow range* row. As shown in Figure B.50, those in the salience + hint  
5377 condition saw the instructions along with a specific description of how to use the  
5378 variance information in their allocation decisions.

### 5379 B.7.1.3 Project Display

5380 The project displays were the same as Experiment 2 (see Figure B.51).

### 5381 B.7.1.4 Procedure

5382 Participants read the instruction page as per their hint condition, and then  
5383 proceeded to complete one set of ranking and allocations.

## 5384 B.7.2 Results

### 5385 B.7.2.1 Allocation

5386 A mixed factorial ANOVA was conducted to investigate the effects of hint and  
5387 NPV variance on participants' allocations. As shown in Figure B.52, none of the  
5388 interactions or main effects were significant.

### 5389 B.7.2.2 Ranking

5390 A mixed factorial ANOVA was conducted to investigate the effects of hint and  
5391 NPV variance on participants' project rankings. As shown in Figure B.53, only the  
5392 main effect of NPV was significant,  $F(2.03, 148.33) = 7.59, p = .001, \hat{\eta}_p^2 = .094$ .

## B. Chapter 4 Appendix

### Allocation task

You will be shown information about a number of projects that a consumer products firm is considering to invest in. Some specific information about the product itself is provided.

In addition to those numbers, you will find each project's projected cash inflow for the first year (the money that it is expected to generate), and the net present value (NPV) that was calculated using those figures. It is usually unclear exactly what the future cash inflow is, so instead of a single cash inflow value, it will be given as a range (assume that all the values in that range are equally likely). Also assume that all the other elements that are required to calculate NPV (i.e., the discount rate and initial investment) are identical for all projects.

We would like you to take the role of the manager in charge of capital allocation for the firm. This firm is specifically interested in investing in the development of high-end goods, so your valuations should reflect this. That is, even though there might be a market for the lower-end products in the descriptions that you will see, **you should be aiming to invest in the products with the highest intrinsic quality.**

You will decide how to rank the projects in order of investment priority, and decide how to allocate the capital available for investment this year among the different projects. Note that this is not the operational budget (advertising, etc.), but rather the funds to be used for investment in developing the new products. You will do this by selecting a percentage value for each project, such that the budget is allocated completely among each set of projects.

**Figure B.48:** Instructions for the no hint condition.

### B.7.3 Discussion

5393      Experiment 7 found that explicitly telling participants how to use variance  
5394      information to inform their allocations did not help them do so. However, there was  
5395      an increased reliance on NPV with more hints in the ranking data. This suggests  
5396      that the hint manipulations potentially simply increase participants' attention to  
5397      NPV. It is possible that the study was under-powered, as there was substantial  
5398      variance in both the allocation and ranking data. Future work should attempt to  
5399      replicate this experiment with a larger sample.  
5400

## B. Chapter 4 Appendix

### Allocation task

You will be shown information about a number of projects that a consumer products firm is considering to invest in. Some specific information about the product itself is provided.

In addition to those numbers, you will find each project's projected cash inflow for the first year (the money that it is expected to generate), and the net present value (NPV) that was calculated using those figures. It is usually unclear exactly what the future cash inflow is, so instead of a single cash inflow value, it will be given as a range (assume that all the values in that range are equally likely). Also assume that all the other elements that are required to calculate NPV (i.e., the discount rate and initial investment) are identical for all projects.

We would like you to take the role of the manager in charge of capital allocation for the firm. This firm is specifically interested in investing in the development of high-end goods, so your valuations should reflect this. That is, even though there might be a market for the lower-end products in the descriptions that you will see, **you should be aiming to invest in the products with the highest intrinsic quality.**

You will decide how to rank the projects in order of investment priority, and decide how to allocate the capital available for investment this year among the different projects. Note that this is not the operational budget (advertising, etc.), but rather the funds to be used for investment in developing the new products. You will do this by selecting a percentage value for each project, such that the budget is allocated completely among each set of projects.

**Pay special attention to the cash inflow ranges as they are important to the decision making process.**

**Figure B.49:** Instructions for the salience only condition.

## B.8 Experiment 8

5401      Experiment 8 tested the alignment and reliability effects found in the previous  
5402 experiments, while addressing their limitations. Experiments 1 and 4 found a verbal  
5403 reliability effect. That is, laypeople allocated more capital to a high NPV project,  
5404 depending on how reliable they were told NPV was as a measure. Experiment 2  
5405 found a lack of a numerical reliability effect. That is, business students allocated  
5406 an equivalent amount of capital to projects associated with a high variance NPV,  
5407 as projects with a low NPV. Testing these two effects in two different populations  
5408

## B. Chapter 4 Appendix

### Allocation task

You will be shown information about a number of projects that a consumer products firm is considering to invest in. Some specific information about the product itself is provided.

In addition to those numbers, you will find each project's projected cash inflow for the first year (the money that it is expected to generate), and the net present value (NPV) that was calculated using those figures. It is usually unclear exactly what the future cash inflow is, so instead of a single cash inflow value, it will be given as a range (assume that all the values in that range are equally likely). Also assume that all the other elements that are required to calculate NPV (i.e., the discount rate and initial investment) are identical for all projects.

We would like you to take the role of the manager in charge of capital allocation for the firm. This firm is specifically interested in investing in the development of high-end goods, so your valuations should reflect this. That is, even though there might be a market for the lower-end products in the descriptions that you will see, **you should be aiming to invest in the products with the highest intrinsic quality.**

You will decide how to rank the projects in order of investment priority, and decide how to allocate the capital available for investment this year among the different projects. Note that this is not the operational budget (advertising, etc.), but rather the funds to be used for investment in developing the new products. You will do this by selecting a percentage value for each project, such that the budget is allocated completely among each set of projects.

**Pay special attention to the cash inflow ranges, because they imply the extent to which you should be relying on that particular NPV.** NPVs with higher variance (greater cash inflow ranges) should be relied upon less. For instance, imagine two NPVs, one with a future cash flow range of \$100-\$1900 (range of  $\pm 90\%$  around the average), and one with a range of \$900-\$1100 (range  $\pm 10\%$  around the average). The average of each range is the same (\$1000), and yet the first estimate is more uncertain than the second. As such, with the first estimate, other factors should be used more in the decision making than the NPV, while with the second estimate, the NPV can be relied on more confidently. In general, ranges of less than 10% either way of the average are considered very low variance, and those more than 80% are considered very high variance.

**Figure B.50:** Instructions for the salience + hint condition.

B. Chapter 4 Appendix

	Project 1	Project 2	Project 3	Project 4	Project 5
Product	Laptop	Laptop	Laptop	Laptop	Laptop
RAM (GB)	4	8	32	2	16
Hard drive (GB)	500	750	2000	250	1000
Resolution (px)	900	1080	1440	768	1200
Processor (Ghz)	2.4	3.2	3.8	1.6	3.6
Cash inflow range for Year 1 (\$)	\$5,890-\$6,510	\$5,738-\$6,342	\$5,244-\$5,796	\$6,137-\$6,783	\$5,538.50-\$6,121.50
NPV (\$)	\$636.36	\$490.91	\$18.18	\$872.73	\$300.00

Figure B.51: The projects display.

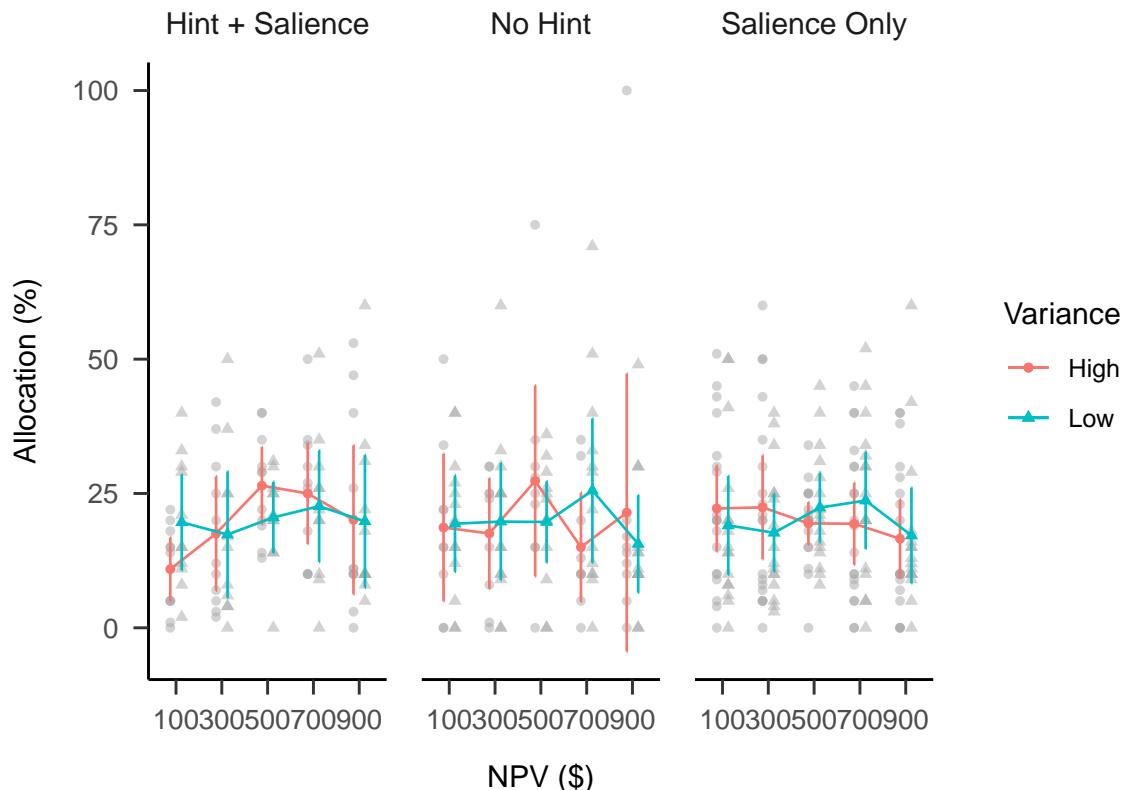
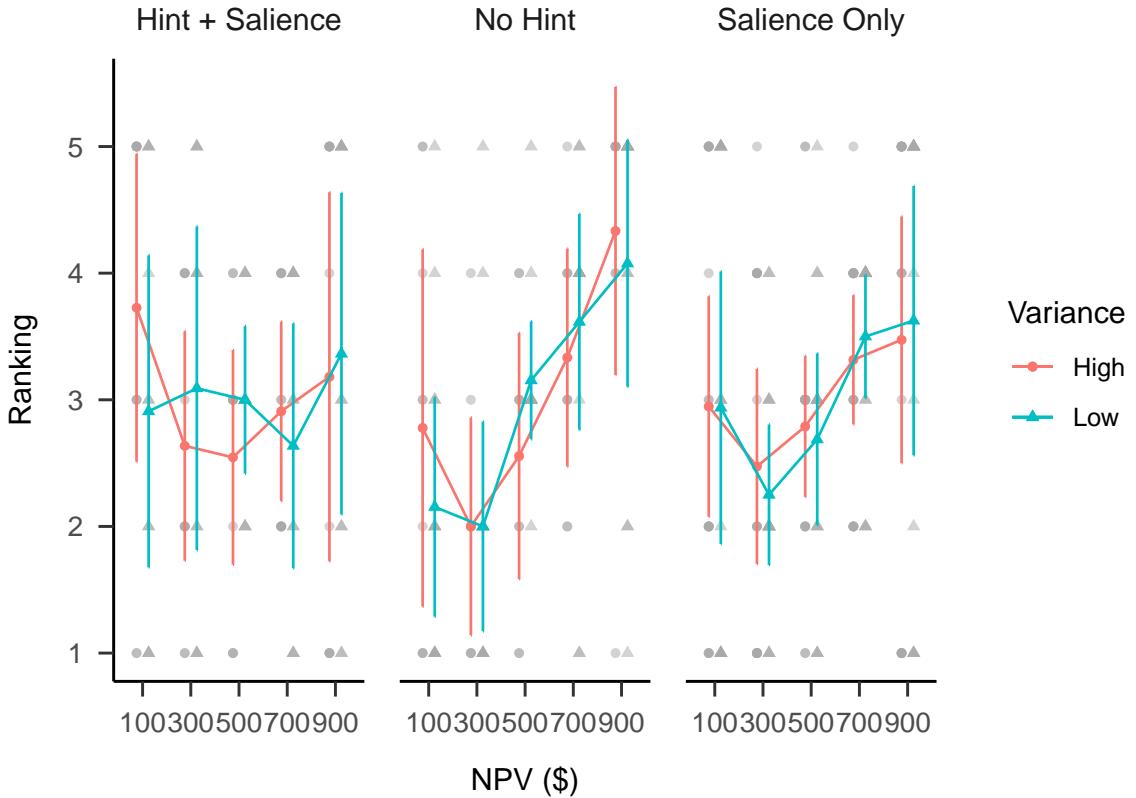


Figure B.52: Mean allocation.

## B. Chapter 4 Appendix



**Figure B.53:** Mean ranking.

5409 did not account for potential expertise effects. As such, Experiment 8 tested both  
 5410 effects with a naive sample. Further, Experiment 8 used projects whose features  
 5411 more clearly indicate their profitability, and included more project domains.

### B.8.1 Method

#### B.8.1.1 Participants

5414 Fifty-two participants (33 female) were recruited from both the online recruit-  
 5415 ment platform Prolific and a cohort of psychology undergraduates at The University  
 5416 of Sydney. Participants from Prolific were compensated at a rate of £5 an hour  
 5417 (Prolific is based in the UK), and participants from the undergraduate sample were  
 5418 compensated with course credit. The average age was 24.46 years ( $SD = 7.77$ ,  
 5419  $min. = 18$ ,  $max. = 68$ ). Participants reported an average of 2.63 years ( $SD$   
 5420 = 4.16,  $min. = 0$ ,  $max. = 25$ ) working in a business setting, and an average  
 5421 of 0.81 years ( $SD = 1.39$ ,  $min. = 0$ ,  $max. = 5$ ) of business education. The  
 5422 mean completion time of the task was 35.57 min ( $SD = 71.96$ ,  $min. = 7.36$ ,

## B. Chapter 4 Appendix

5423  $\max.$  = 511.74). All conditions were presented within-subjects: alignment (low  
5424 and high), NPV reliability type (numerical and verbal), NPV (low and high), and  
5425 NPV reliability level (low and high).

### 5426 B.8.1.2 Materials

5427 **B.8.1.2.1 Instructions** Participants saw instructions similar to the previous  
5428 experiments.

5429 **B.8.1.2.2 Project Display** Participants saw and responded to four webpage  
5430 displays. At the top of each display was a text preamble, and underneath this  
5431 a table that contained project descriptions. The two columns to the right of  
5432 each description contained text boxes for participants to enter a value for the  
5433 project ranking and budget allocation. Alignment was manipulated by asking  
5434 participants to either compare between each of the project pairs (high alignment),  
5435 or across all eight projects in the display (low alignment). For instance, in the high  
5436 alignment display, participants had to compare between two railway projects, and  
5437 then separately between two logistics projects, etc. However, in the low alignment  
5438 display, participants had to compare railway projects to logistics projects directly.  
5439 This was manipulated within-subjects, such that project descriptions were identical  
5440 across alignment conditions and only the type of comparison (and the associated  
5441 preamble text) varied.

5442 Figures B.54, B.55, B.56, B.57 show the four conditions that participants saw  
5443 (counterbalanced). Each description provided the name of the business involved  
5444 in the project, the type of project, three specific features of the project, an NPV,  
5445 and an indication of reliability (either numerical through ranges or verbal through  
5446 explicit labels).

5447 The value of each type of reliability was also manipulated. Explicit reliability  
5448 was manipulated by varying whether participants were told that a project pair was  
5449 in an industry in which NPV is considered a reliable or unreliable measure. Implicit  
5450 reliability was manipulated by presenting NPVs alongside numerical ranges instead

## B. Chapter 4 Appendix

For the following set of projects, the budget is shared among all eight projects.

The total budget is \$400 million.

Therefore, the sum of allocations for all the projects should be 400 and the rankings will be between 1 and 8.

<p>Business name: FreightCog.          - Investment: <u>railway</u>.          - Predicted project features:            - Railway lines built: 5 a decade.            - Number of seats filled by paying customers at peak hour: 2000.            - Time before the train carriages will need to be serviced: 12 years.          - NPV: \$128 million.          (In this particular industry, NPV is a <b>reliable</b> predictor of project success.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Railmont.          - Investment: <u>railway</u>.          - Predicted project features:            - Railway lines built: 3 a decade.            - Number of seats filled by paying customers at peak hour: 1200.            - Time before the train carriages will need to be serviced: 7 years.          - NPV: \$974 million.          (In this particular industry, NPV is a <b>reliable</b> predictor of project success.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Pharmacore.          - Investment: <u>pharmaceutical</u>.          - Predicted project features:            - Pills pressed: 180000 an hour.            - Shelf life: 12 months.            - Probability of symptom reduction after a week: 54%.          - NPV: \$952 million.          (In this particular industry, NPV is an <b>unreliable</b> predictor of project success.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Biotechly.          - Investment: <u>pharmaceutical</u>.          - Predicted project features:            - Pills pressed: 300000 an hour.            - Shelf life: 20 months.            - Probability of symptom reduction after a week: 90%.          - NPV: \$194 million.          (In this particular industry, NPV is an <b>unreliable</b> predictor of project success.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Pressbloom.          - Investment: <u>national newspaper</u>.          - Predicted project features:            - Newspapers printed: 30000 a day.            - Number of weekly advertisers: 48.            - Ink that is not discarded due to impurities: 3000L a day</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>

**Figure B.54:** Experiment 8 low alignment, verbal reliability display. Cropped for space (full display had eight projects).

## B. Chapter 4 Appendix

For the following set of projects, the budget is shared among all eight projects.

The total budget is \$400 million.

Therefore, the sum of allocations for all the projects should be 400 and the rankings will be between 1 and 8.

<p>Business name: Dinerly.          - Investment: <u>restaurant chain</u>.          - Predicted project features:            - Restaurants established: 9 a year.            - Number of reservations on a Saturday night: 100.            - Positive reviews: 40 a month.          - NPV: \$27-339 million.          (Midpoint: \$183.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Savoro.          - Investment: <u>restaurant chain</u>.          - Predicted project features:            - Restaurants established: 5 a year.            - Number of reservations on a Saturday night: 60.            - Positive reviews: 24 a month.          - NPV: \$137-1689 million.          (Midpoint: \$913.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Altchip.          - Investment: <u>microchip</u>.          - Predicted project features:            - Microchips produced: 2400 an hour.            - Usable semiconductor yield after testing: 36%.            - Compatible PCs in the market: 48%.          - NPV: \$143-1761 million.          (Midpoint: \$952.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Microxy.          - Investment: <u>microchip</u>.          - Predicted project features:            - Microchips produced: 4000 an hour.            - Usable semiconductor yield after testing: 60%.            - Compatible PCs in the market: 80%.          - NPV: \$29-359 million.          (Midpoint: \$194.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Enfuel.          - Investment: <u>oil well</u>.          - Predicted project features:            - Oil extracted: 1200L an hour.            - Time the machinery lasts before requiring maintenance: 4 years.            - Probability of finding oil: 50%</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>

**Figure B.55:** Experiment 8 low alignment, numerical reliability display. Cropped for space (full display had eight projects).

## B. Chapter 4 Appendix

For the following set of projects, the budget is split up evenly between each industry pair, i.e., projects with the same type of "Investment".

The total budget is \$400 million. Therefore, the sum of allocations in each pair should be 100 and the rankings will be between 1 and 2.

<p>Business name: Erectic.</p> <ul style="list-style-type: none"> <li>- Investment: <u>high-rise construction</u>.</li> <li>- Predicted project features:           <ul style="list-style-type: none"> <li>- High-rises built: 5 a year.</li> <li>- Probability that the builders complete construction within a month of the due date: 42%.</li> <li>- Number of tenant expressions of interest: 60.</li> <li>- NPV: \$913 million.</li> </ul> </li> </ul> <p>(In this particular industry, NPV is an <b>unreliable</b> predictor of project success.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Refit.</p> <ul style="list-style-type: none"> <li>- Investment: <u>high-rise construction</u>.</li> <li>- Predicted project features:           <ul style="list-style-type: none"> <li>- High-rises built: 8 a year.</li> <li>- Probability that the builders complete construction within a month of the due date: 70%.</li> <li>- Number of tenant expressions of interest: 100.</li> <li>- NPV: \$183 million.</li> </ul> </li> </ul> <p>(In this particular industry, NPV is an <b>unreliable</b> predictor of project success.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Pressbloom.</p> <ul style="list-style-type: none"> <li>- Investment: <u>national newspaper</u>.</li> <li>- Predicted project features:           <ul style="list-style-type: none"> <li>- Newspapers printed: 30000 a day.</li> <li>- Number of weekly advertisers: 48.</li> <li>- Ink that is not discarded due to impurities: 3000L a day.</li> </ul> </li> </ul> <p>(In this particular industry, NPV is a <b>reliable</b> predictor of project success.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Grown Media.</p> <ul style="list-style-type: none"> <li>- Investment: <u>national newspaper</u>.</li> <li>- Predicted project features:           <ul style="list-style-type: none"> <li>- Newspapers printed: 50000 a day.</li> <li>- Number of weekly advertisers: 80.</li> <li>- Ink that is not discarded due to impurities: 5000L a day.</li> </ul> </li> </ul> <p>(In this particular industry, NPV is a <b>reliable</b> predictor of project success.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: FreightCog.</p> <ul style="list-style-type: none"> <li>- Investment: <u>railway</u>.</li> <li>- Predicted project features:           <ul style="list-style-type: none"> <li>- Railway lines built: 5 a decade.</li> </ul> </li> </ul>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>

**Figure B.56:** Experiment 8 high alignment, verbal reliability display. Cropped for space (full display had eight projects).

## B. Chapter 4 Appendix

For the following set of projects, the budget is split up evenly between each industry pair, i.e., projects with the same type of "Investment".

The total budget is \$400 million. Therefore, the sum of allocations in each pair should be 100 and the rankings will be between 1 and 2.

<p>Business name: Enfuel.</p> <ul style="list-style-type: none"> <li>- Investment: <u>oil well</u>.</li> <li>- Predicted project features:           <ul style="list-style-type: none"> <li>- Oil extracted: 1200L an hour.</li> <li>- Time the machinery lasts before requiring maintenance: 4 years.</li> <li>- Probability of finding oil: 54%.</li> </ul> </li> <li>- NPV: \$916-1012 million.</li> </ul> <p>(Midpoint: \$964.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Refinera.</p> <ul style="list-style-type: none"> <li>- Investment: <u>oil well</u>.</li> <li>- Predicted project features:           <ul style="list-style-type: none"> <li>- Oil extracted: 2000L an hour.</li> <li>- Time the machinery lasts before requiring maintenance: 7 years.</li> <li>- Probability of finding oil: 90%.</li> </ul> </li> <li>- NPV: \$182-202 million.</li> </ul> <p>(Midpoint: \$192.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Altchip.</p> <ul style="list-style-type: none"> <li>- Investment: <u>microchip</u>.</li> <li>- Predicted project features:           <ul style="list-style-type: none"> <li>- Microchips produced: 2400 an hour.</li> <li>- Usable semiconductor yield after testing: 36%.</li> <li>- Compatible PCs in the market: 48%.</li> </ul> </li> <li>- NPV: \$143-1761 million.</li> </ul> <p>(Midpoint: \$952.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Microxy.</p> <ul style="list-style-type: none"> <li>- Investment: <u>microchip</u>.</li> <li>- Predicted project features:           <ul style="list-style-type: none"> <li>- Microchips produced: 4000 an hour.</li> <li>- Usable semiconductor yield after testing: 60%.</li> <li>- Compatible PCs in the market: 80%.</li> </ul> </li> <li>- NPV: \$29-359 million.</li> </ul> <p>(Midpoint: \$194.)</p>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>
<p>Business name: Solgistics.</p> <ul style="list-style-type: none"> <li>- Investment: <u>shipping logistics</u>.</li> <li>- Predicted project features:           <ul style="list-style-type: none"> <li>- Packages shipped: 480 a week.</li> <li>- Number of packages that do not spend time in a bottleneck: 240 a day.</li> <li>- Average accuracy of shipments: 57%.</li> </ul> </li> </ul>	Project ranking: <input type="text"/>	Budget allocation: \$ <input type="text"/>

**Figure B.57:** Experiment 8 high alignment, numerical reliability display. Cropped for space (full display had eight projects).

## B. Chapter 4 Appendix

5451 of verbal reliability information about them, and varying whether the range was  
5452 high or low. Both of these were manipulated within-display, such that NPV was  
5453 reliable for four projects in each display, and NPV was unreliable for the other four.

5454 Each project had an associated NPV, which was crossed with each project pair's  
5455 intrinsic features. That is, each pair had one project with a high NPV and low  
5456 intrinsic feature values, and one project with a low NPV and high intrinsic feature  
5457 values. As such, a reliance on NPV was inferred if participants allocated the high  
5458 NPV project more capital, or a reliance on the intrinsic features if participants  
5459 allocated the low NPV project more capital.

### 5460 B.8.1.3 Procedure

5461 Participants viewed the instructions and then completed the ranking and allo-  
5462 cation tasks in the four sets of project descriptions. The order of the display was  
5463 counterbalanced, and the order of the project pairs on each page was randomised.

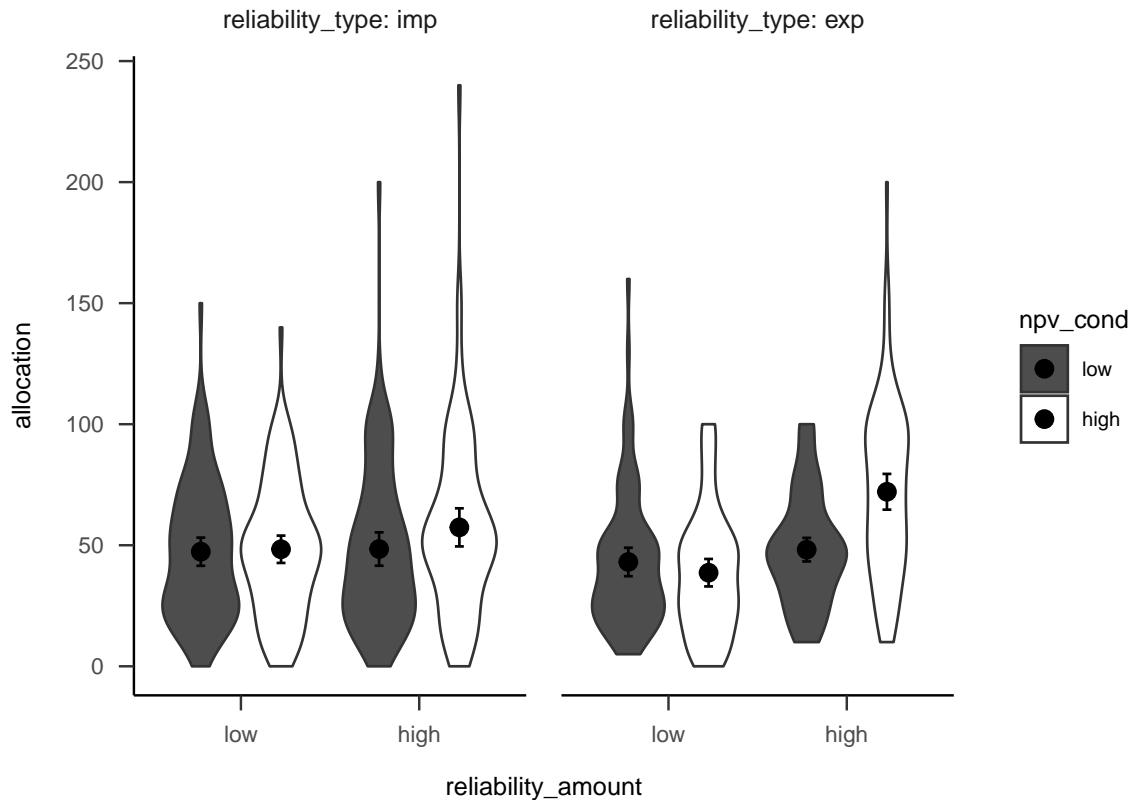
## 5464 B.8.2 Results

5465 A mixed factorial ANOVA was conducted to investigate the effects of alignment  
5466 and NPV reliability type on participants project allocations. A direct comparison of  
5467 the two alignment conditions was not possible due to the different allocation input  
5468 scales, so the NPV reliability level  $\times$  NPV interaction was tested separately in each  
5469 alignment condition (see Figures B.58 and B.59). This interaction was significant  
5470 for both the high alignment condition,  $F(1, 51) = 27.81, p < .001, \hat{\eta}_p^2 = .353$ ;  
5471 and the low alignment condition,  $F(1, 51) = 7.63, p = .008, \hat{\eta}_p^2 = .130$ . However,  
5472 there was a significant effect of NPV in the low verbal reliability condition in high  
5473 alignment,  $\Delta M = 18.69, 95\% \text{ CI } [2.87, 34.52], t(113.10) = 3.17, p = .012$ ; but not  
5474 in low alignment,  $\Delta M = 6.04, 95\% \text{ CI } [-9.24, 21.32], t(121.35) = 1.06, p > .999$ .

### 5475 B.8.3 Discussion

5476 Experiment 8 found that when variance was presented verbally, participants  
5477 allocated according to the reliability information, for both low and high alignment

## B. Chapter 4 Appendix

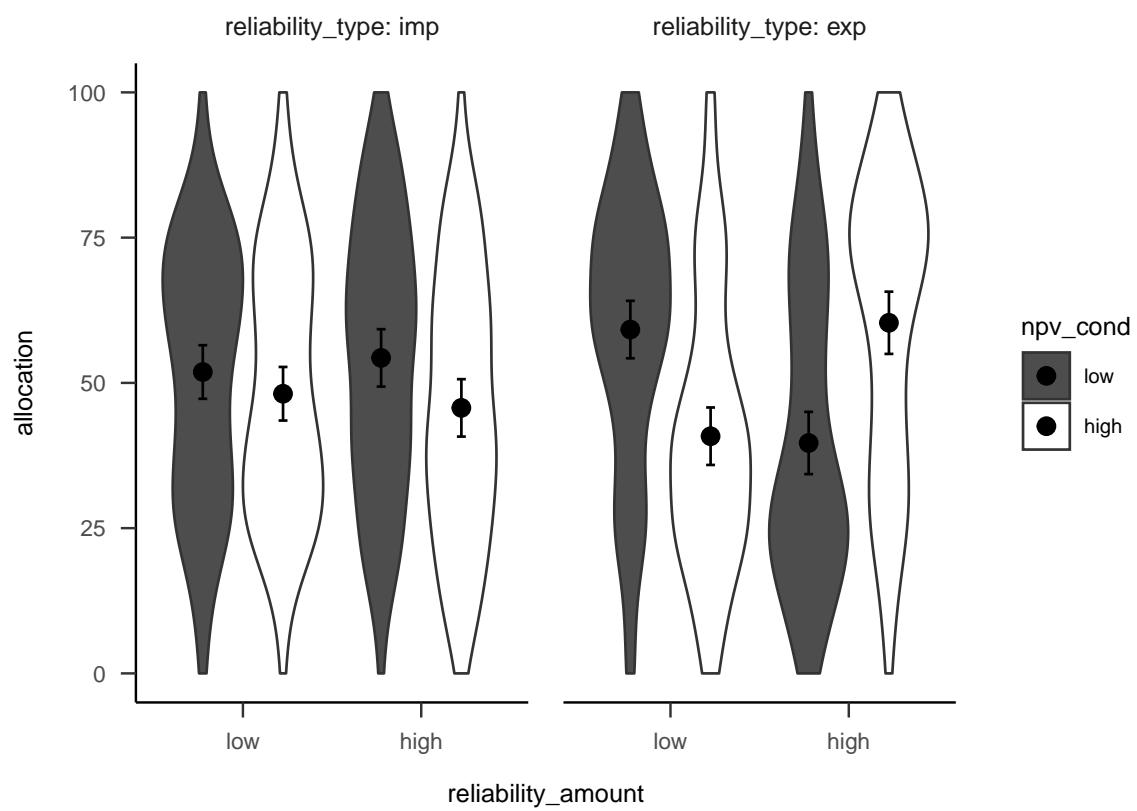


**Figure B.58:** Mean project allocation, for the low alignment condition. Error bars represent 95% confidence intervals.

5478 conditions. When variance was presented numerically, there were no differences  
 5479 in allocations, for both low and high alignment conditions. Further, there was an  
 5480 effect of NPV in low reliability for the high alignment condition, but not the low  
 5481 alignment condition. This effect shows that people still relied on NPV more than  
 5482 they should when comparing across dissimilar projects.

5483 This experiment shows that similar to the previous experiments, when con-  
 5484 trolling for presentation and domain, people still find it easier to allocate capital  
 5485 based on explicit reliability information when projects are comparable. However,  
 5486 due to the difference in scale across alignment conditions, a direct alignment effect  
 5487 was more difficult to test than with the previous experiments. Further, similar to  
 5488 Experiment 2, Experiment 8 showed that people without much business experience  
 5489 also struggle to use range information in capital allocation to such an extreme extent  
 5490 that they do not seem to be using any coherent allocation strategy.

B. Chapter 4 Appendix



**Figure B.59:** Mean project allocation, for the high alignment condition. Error bars represent 95% confidence intervals.

# C

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## Chapter 6 Appendix

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### Contents

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5494

5495	<b>C.1 Experiment 1</b> . . . . .	<b>253</b>
5496	C.1.1 Method . . . . .	255
5497	C.1.2 Results . . . . .	259
5498	<b>C.2 Experiment 2</b> . . . . .	<b>262</b>
5499	C.2.1 Method . . . . .	266
5500	C.2.2 Results . . . . .	269

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5502

5503

5504     This appendix contains supplementary materials and analyses for the two ex-  
5505     periments reported in Chapter 6.

5506

### C.1 Experiment 1

5507

       Below are hypotheses that were tested, but were not sufficiently relevant for  
5508     Chapter 6 to be reported in the main text.

5509

**Hypothesis C.1—Allocation similarity manipulation check for negative**  
5510     **anecdote.** For negative anecdotes, allocations for the anecdote only low similarity  
5511     condition will be higher than those in the anecdote only high similarity condition.

### C. Chapter 6 Appendix

5512 **Hypothesis C.2—Relationship between allocation and perceived similarity for negative anecdote.** In the negative valence condition, the correlation  
5513 between allocation and similarity rating will be negative  
5514

5515 **Hypothesis C.3—Relationship between allocation and specific-relevance for negative anecdote.** In the negative valence condition, there will be no correlation  
5516 between allocation and specific-relevance rating in the low similarity condition,  
5517 but a negative correlation in the high similarity condition.  
5518

5519 After the allocation task, participants were asked to rate the relevance of the  
5520 anecdote to the target project. It was predicted that those that saw only an anecdote  
5521 would be more influenced by the similarity of the anecdote than those that saw  
5522 an anecdote as well as statistics. Therefore, the following hypotheses were tested:

5523 **Hypothesis C.4.** The similarity effect on specific relevance will be greater in the  
5524 anecdote only condition than in the anecdote + statistics condition.

5525 **Hypothesis C.5.** The similarity effect on specific relevance will be greater in  
5526 the statistics + anecdote condition than in the anecdote + enhanced statistics  
5527 condition.

5528 Further, participants were asked to rate the relevance of the anecdote to other  
5529 projects in the same industry. It was predicted that those that saw only an anecdote  
5530 would be more influenced by the similarity of the anecdote than those that saw an  
5531 anecdote as well as statistics. Therefore, the following hypotheses are tested:

5532 **Hypothesis C.6.** The similarity effect on general relevance will be greater in the  
5533 anecdote only condition than in the anecdote + statistics condition.

5534 **Hypothesis C.7.** The similarity effect on general relevance will be greater in  
5535 the statistics + anecdote condition than in the anecdote + enhanced statistics  
5536 condition.

## C. Chapter 6 Appendix

### C.1.1 Method

#### C.1.1.1 Participants

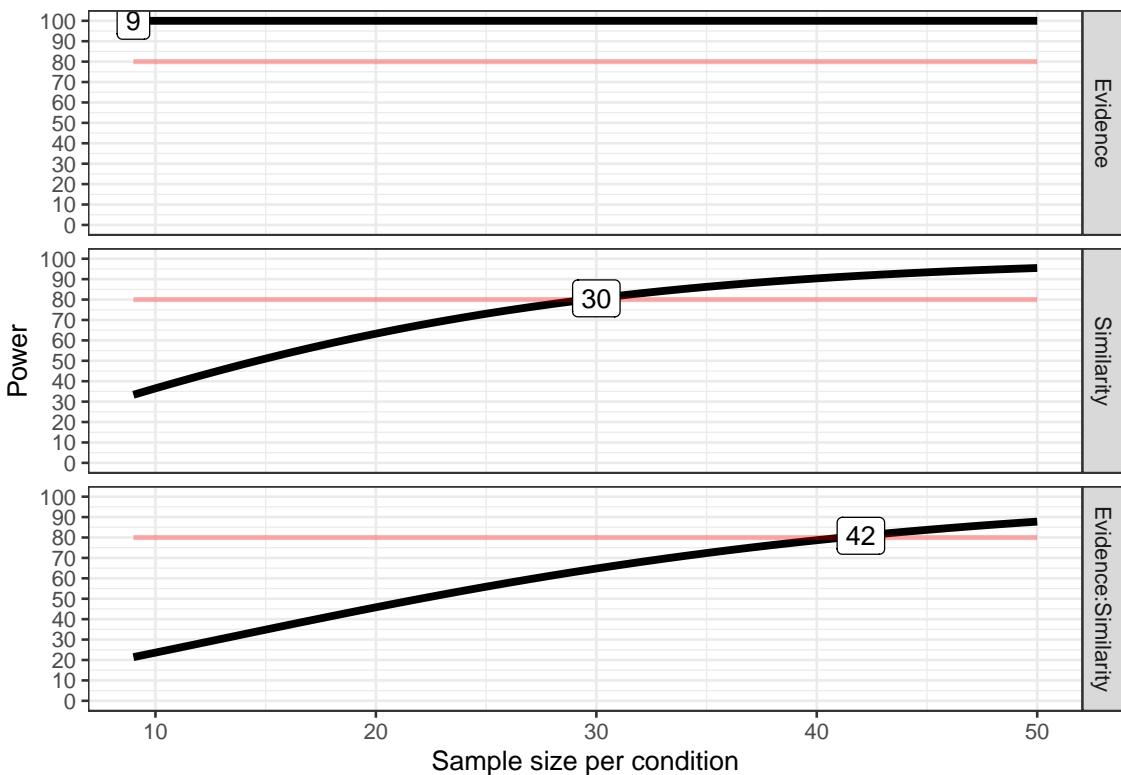
5537     **C.1.1.1.1 Power Analysis** The sample size for Experiment 1 was determined  
5538     by conducting power analyses using the `Superpower` package (Lakens & Caldwell,  
5539     2019). The package uses experimental design, and predicted means and standard  
5540     deviation, to conduct a priori power calculations. Data from Wainberg (2018),  
5541     Jaramillo et al. (2019), and Hoeken and Hustinx (2009, Study 3) was used to  
5542     determine realistic means and standard deviations for the evidence and similarity  
5543     factors. According to the power functions, the resulting sample size is assumed to  
5544     allow for an expected power of at least 80%.

5545     Data from Wainberg (2018) were used to determine the predicted means for the  
5546     anecdote conditions. Specifically, the values for the high similarity condition were  
5547     taken from the anecdote & statistics, anecdote & enhanced statistics, and statistics  
5548     only conditions for the corresponding anecdote conditions. This was done because  
5549     in Wainberg (2018) the anecdote was always of a similar case. Wainberg (2018)  
5550     did not use an anecdote only condition, but Wainberg et al. (2013) did and found  
5551     no significant differences between the anecdote only condition and the anecdote &  
5552     statistics condition. As such, the same mean value was used for both conditions.

5553     It was hypothesised that there will only be an effect of similarity for the anecdote  
5554     only and anecdote & statistics conditions. As such, the data from Hoeken and  
5555     Hustinx (2009, Study 3) were used to determine the corresponding mean values for  
5556     the low similarity condition. Specifically, each predicted mean was multiplied by  
5557     the Cohen's  $d_z$  of the similarity effect in Hoeken and Hustinx (2009, Study 3).

5558     To determine the predicted standard deviation, the data from Jaramillo et al.  
5559     (2019) Experiment 2 and Hoeken and Hustinx (2009, Study 3) were re-analysed to  
5560     determine the coefficient of variation (CV) of each condition. Each CV was then  
5561     converted to a standard deviation value in the relevant scale by multiplying the  
5562     mean of the CV values by the predicted means from above.

### C. Chapter 6 Appendix



**Figure C.1:** Power curves for the similarity and anecdote effects.

Imagine you are a executive in a multi-business company and that you are presented with two projects to potentially invest in. Your job is to decide how to allocate the capital available in your budget between these two projects.

In a moment you will see a table that details the two target projects and relevant information about them.

**Figure C.2:** Experiment 1 general instructions. The two boxes were split between two separate web-pages.

5565 As shown in Figure C.1, the power analysis suggested that a minimum sample  
 5566 size of 294 ( $42 \cdot 7$ ) is required for the interaction effect with an expected power  
 5567 of at least 80%.

#### 5568 C.1.1.2 Method

5569 **C.1.1.2.1 Instructions** Figure C.2 shows the general instructions all participants received, and Figures C.3, C.4, C.5, and C.6 show the condition-specific instructions.

### C. Chapter 6 Appendix

Managers often find it useful to consult with previous case studies before making important decisions. As well as seeing the two target projects, you will also be provided with an example of a failed project with some information that was available just before the company decided to invest in it. Further, you are also provided with an analysis of this investment decision after it became clear that the project will not meet its expected return on investment.

**Figure C.3:** Experiment 1 specific instructions for those in the anecdotes only condition.

Managers often find it useful to consult with previous case studies before making important decisions. As well as seeing the two target projects, you will also be provided with an example of a failed project with some information that was available just before the company decided to invest in it. Further, you are also provided with an analysis of this investment decision after it became clear that the project will not meet its expected return on investment.

As a part of the relevant information that will be provided for each target project, you will be provided with measures of overall reliability and Net Present Value (NPV). The NPV is the company's estimation of the future returns of the project. An NPV that is greater than 0 (zero) indicates that there is an expectation of profit. The higher the NPV, the better the expectations for each project. Both these measures were collected as part of a research study conducted by an international consulting company that aggregated data from thousands of other projects in relevant industries.

Note that the project in the case study was included in the research study, so its features are subsumed in the aggregated data.

**Figure C.4:** Experiment 1 specific instructions for those in the anecdote & statistics condition.

5572 **C.1.1.2.2 Allocation Task** A horizontally integrated company is one which  
5573 is made up of multiple businesses that operate in similar markets, and may have  
5574 previously been competitors (Gaughan, 2012a). A vertically integrated company,  
5575 on the other hand, is one which is made up of multiple business than operate in  
5576 the same market, but in different levels of the supply chain (Gaughan, 2012b). A  
5577 centralised organisational structure is one in which a company decisions tend to  
5578 come from a specific business unit or leader, whereas a decentralised structure is

### C. Chapter 6 Appendix

Managers often find it useful to consult with previous case studies before making important decisions. As well as seeing the two target projects, you will also be provided with an example of a failed project with some information that was available just before the company decided to invest in it. Further, you are also provided with an analysis of this investment decision after it became clear that the project will not meet its expected return on investment.

As a part of the relevant information that will be provided for each target project, you will be provided with measures of overall reliability and Net Present Value (NPV). The NPV is the company's estimation of the future returns of the project. An NPV that is greater than 0 (zero) indicates that there is an expectation of profit. The higher the NPV, the better the expectations for each project. Both these measures were collected as part of a research study conducted by an international consulting company that aggregated data from thousands of other projects in relevant industries.

Note that the project in the case study was included in the research study, so its features are subsumed in the aggregated data.

Alongside its results, the research study also encouraged managers to use 'scientific thinking'.

Scientific thinking can be characterized as a process of objectively analyzing information about a given topic. A scientific thinker is one who very carefully considers the quality of each piece of information so as not to be unduly swayed by insignificant and/or less significant facts.

Progress in science is generally achieved via the deliberate process of obtaining quantifiable evidence through observation and/or experimentation. The scientific method requires that experimental and observational findings be reproducible and cautions against drawing strong conclusions from any single study or observation. You may recall from statistics that this scientific principle is consistent with the fact that small samples of observations tend to have a higher probability of error while larger samples tend to be more accurate. Scientific knowledge is therefore based on an accumulation of carefully designed studies or observations which lend support to a given assertion.

**Figure C.5:** Experiment 1 specific instructions for those in the anecdote & enhanced statistics condition.

### C. Chapter 6 Appendix

As a part of the relevant information that will be provided for each target project, you will be provided with measures of overall reliability and Net Present Value (NPV). The NPV is the company's estimation of the future returns of the project. An NPV that is greater than 0 (zero) indicates that there is an expectation of profit. The higher the NPV, the better the expectations for each project. Both these measures were collected as part of a research study conducted by an international consulting company that aggregated data from thousands of other projects in relevant industries.

**Figure C.6:** Experiment 1 specific instructions for those in the statistics only condition.

one in which decisions can be made by separate units or people independently (Kenton, 2021).

**C.1.1.2.3 Follow-up** Figure C.7 shows the follow-up questions.

## C.1.2 Results

### C.1.2.1 Allocation

A two-way ANOVA was conducted to investigate the interaction of similarity (low and high) and anecdote conditions (anecdote only, statistics & anecdote, anecdote & enhanced statistics). The main text reports the more relevant interaction that excludes the enhanced statistics condition. There was a main effect of anecdote type,  $F(2, 238) = 14.47, p < .001, \hat{\eta}_p^2 = .108$ ; and a main effect of similarity,  $F(1, 238) = 38.91, p < .001, \hat{\eta}_p^2 = .141$ . However, the interaction was not significant,  $F(2, 238) = 2.16, p = .118, \hat{\eta}_p^2 = .018$ . The difference between the anecdote only condition and the anecdote & enhanced statistics condition was not significant,  $M = -9.24, 95\% \text{ CI } [-22.00, 3.51], t(238) = -1.43, p = .155$ .

### C.1.2.2 Manipulation Check

Figure C.8 shows participants' ratings of the similarity of the anecdote to the target project. As intended, participants in the high similarity condition rated the anecdote as more similar to the target project than those in the low similarity condition,  $F(1, 238) = 27.01, p < .001, \hat{\eta}_p^2 = .102$ .

### C. Chapter 6 Appendix

Please answer the following:

Follow up

On a scale of 1 to 6, how similar do you think the Refinera project (the case study) is to the Enfuel project (the target oil project)? A choice of 1 indicates low similarity, and 6 indicates high similarity.

Justify your answer:

On a scale of 1 to 6, how relevant do you think the information about the Refinera project is for determining whether to invest in the Enfuel project? A choice of 1 indicates low relevance, and 6 indicates high relevance.

Justify your answer:

On a scale of 1 to 6, how relevant do you think the information about the Refinera project is for determining whether to invest in *any* oil well project? A choice of 1 indicates low relevance, and 6 indicates high relevance.

Justify your answer:

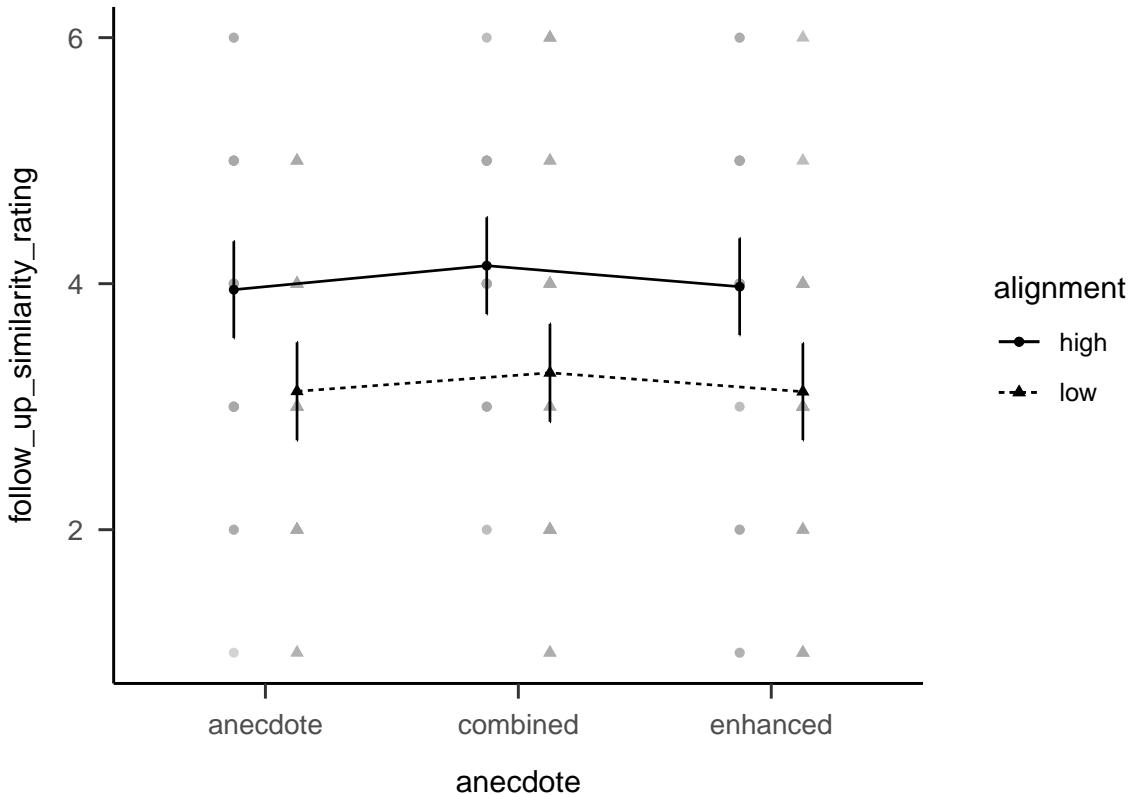
**Figure C.7:** Follow-up questions in Experiment 1.

#### 5598 C.1.2.3 Follow-up

5599 Figure C.9 shows participants' ratings of the specific relevance question. There  
5600 was no significant effect of evidence type  $F(2, 238) = 0.96, p = .383, \hat{\eta}_p^2 = .008$ ; or  
5601 similarity,  $F(1, 238) = 1.54, p = .216, \hat{\eta}_p^2 = .006$ . The interaction was also not sig-  
5602 nificant, `r_results_anecdotes_1$relevance_specific$anecdote_alignment`.

5603 Figure C.10 shows participants' ratings of the general relevance question. There  
5604 was no main effect of similarity,  $F(1, 238) = 3.32, p = .070, \hat{\eta}_p^2 = .014$ , or interac-  
5605 tion of similarity and evidence type, `r_results_anecdotes_1$relevance_general$anecdote_align`  
5606 However, there was an unexpected main effect of evidence type,  $F(2, 238) = 3.80,$

C. Chapter 6 Appendix



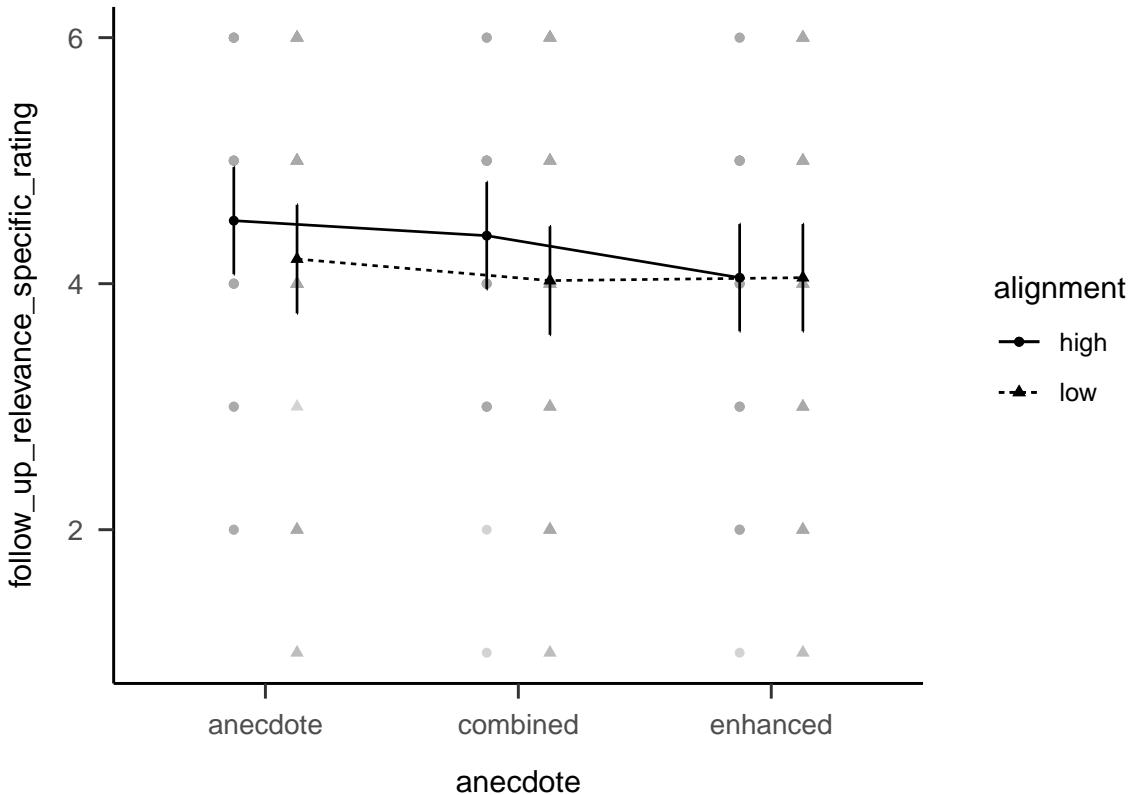
**Figure C.8:** Mean similarity rating of Project A (the target project) to the anecdote. Error bars represent 95% confidence intervals.

5607  $p = .024$ ,  $\hat{\eta}_p^2 = .031$ . A contrast analysis with Bonferroni correction revealed that  
 5608 the anecdote only condition was rated significantly higher than the anecdote &  
 5609 statistics condition,  $\Delta M = 0.58$ , 95% CI [0.06, 1.10],  $t(238) = 2.71$ ,  $p = .022$ .  
 5610 However, the difference between the two anecdote & statistics conditions was not  
 5611 significant,  $\Delta M = -0.39$ , 95% CI [-0.90, 0.13],  $t(238) = -1.81$ ,  $p = .212$ .

5612 Regression analyses were conducted to determine the relationship between al-  
 5613 locations and the follow-up ratings of similarity and relevance. As shown in Fig-  
 5614 ure C.11, similarity ratings were negatively correlated to allocations,  $b = -3.53$ ,  
 5615 95% CI [-5.70, -1.37],  $t(242) = -3.21$ ,  $p = .002$ . Finally, as shown in Figure C.12  
 5616 similarity ratings were positively correlated to specific relevance ratings,  $b = 0.30$ ,  
 5617 95% CI [0.17, 0.43],  $t(242) = 4.59$ ,  $p < .001$ .

5618 Participants' justifications for the ratings were not analysed, so are not reported.

C. Chapter 6 Appendix



**Figure C.9:** Mean rating of how relevant participants thought the anecdote was to Project A (the target project). Error bars represent 95% confidence intervals.

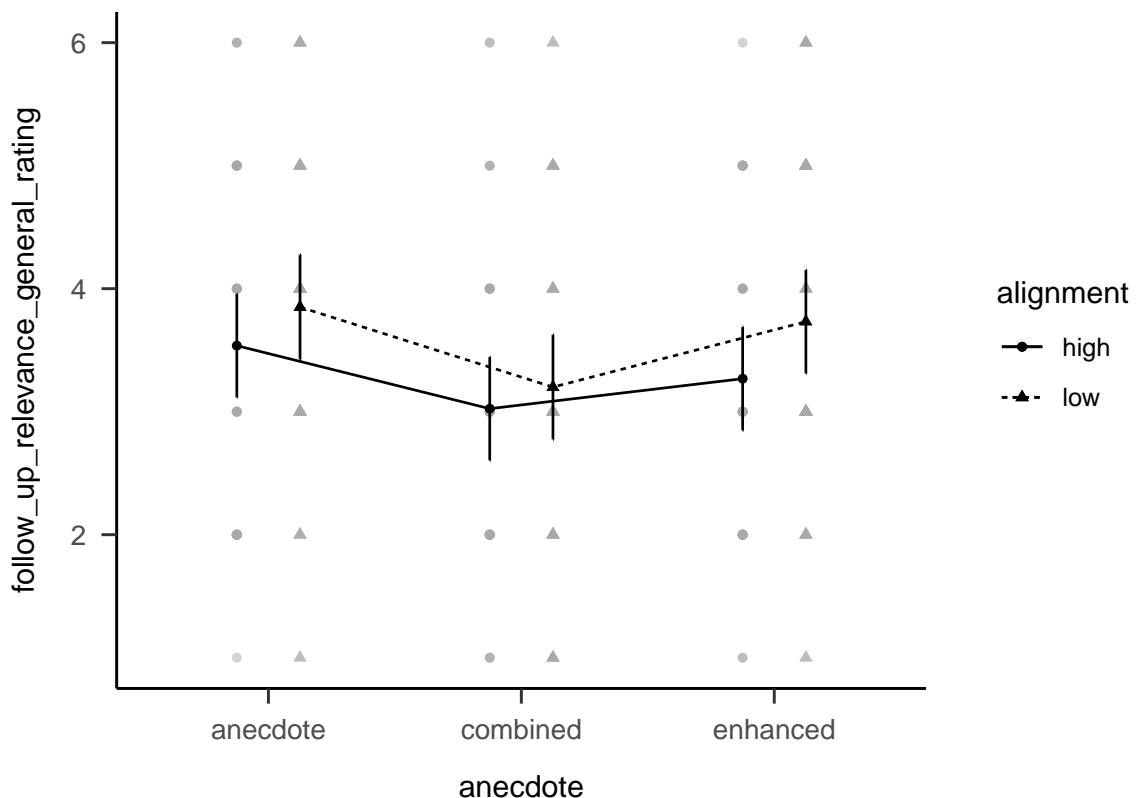
## 5619 C.2 Experiment 2

5620 Figures C.13 and C.14 show the simulated data for the negative and positive  
 5621 valence conditions, respectively. These figures are different from the equivalent  
 5622 figures in the main text. Here, the same statistics only value was used for both  
 5623 valence conditions, whereas in the main text the relevant values for each condition  
 5624 were used. Further, the main text reports the difference score from the relevant  
 5625 statistics only values, whereas here the raw means are shown.

5626 **Hypothesis C.8—Allocation similarity manipulation check for positive**  
 5627 **anecdote.** For positive anecdotes, allocations for the anecdote only high similarity  
 5628 condition will be higher than those in the anecdote only low similarity condition.

5629 The rating effects found in Experiment 1 were expected to replicate in the  
 5630 Experiment 2 negative valence condition. The reverse effects were expected to be  
 5631 found in the positive valence condition.

C. Chapter 6 Appendix



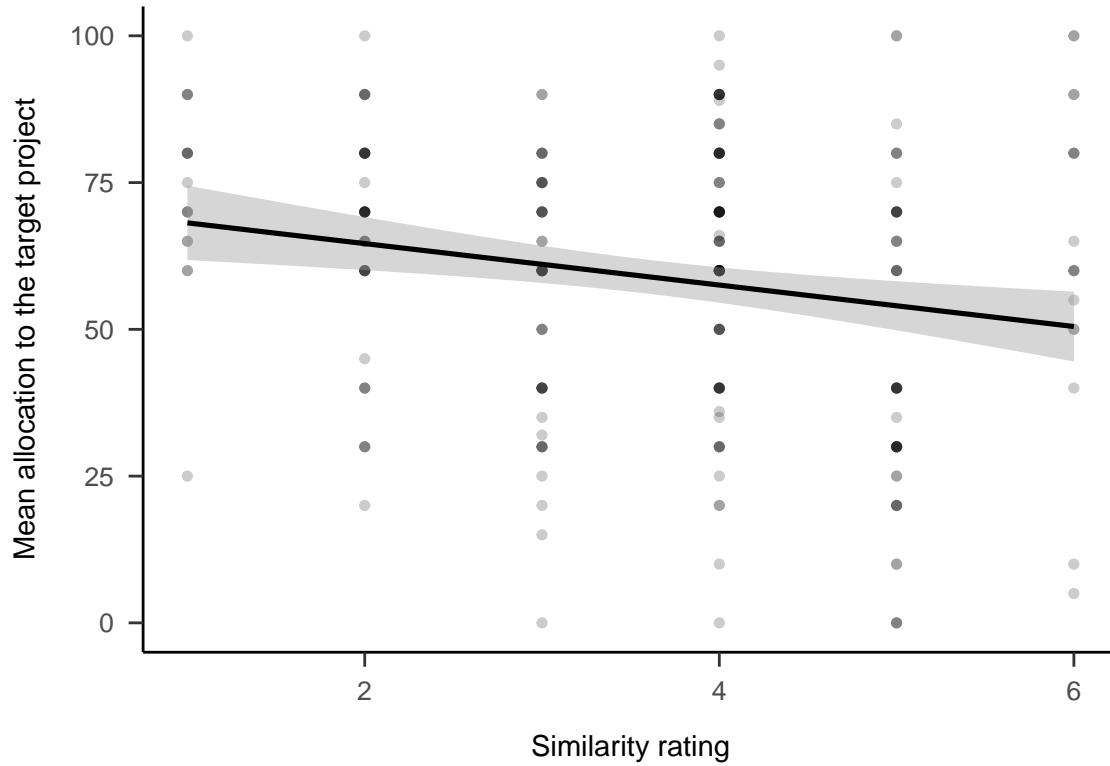
**Figure C.10:** Mean rating of how relevant participants thought the anecdote was to other oil projects. Error bars represent 95% confidence intervals.

5632 **Hypothesis C.9—Relationship between allocation and perceived simi-**  
 5633 **larity for positive anecdote.** In the positive valence condition, the correlation  
 5634 between allocation and similarity rating will be positive

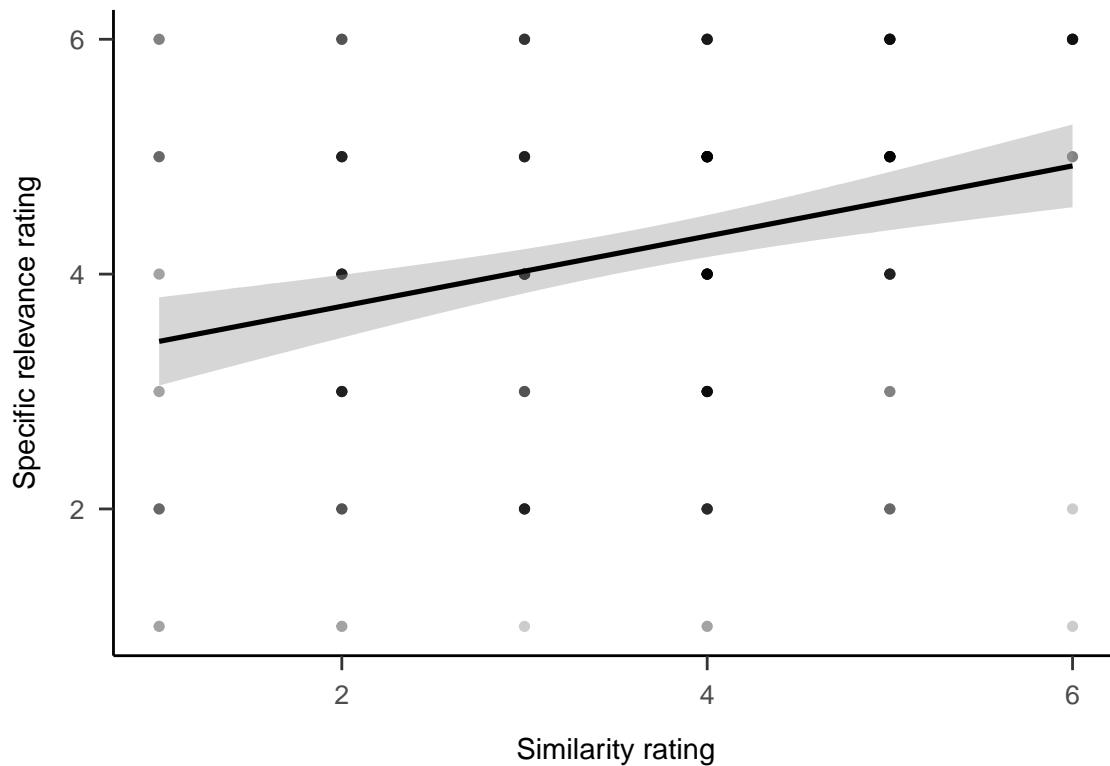
5635 **Hypothesis C.10—Relationship between allocation and specific-relevance**  
 5636 **for positive anecdote.** In the positive valence condition, there will be no correla-  
 5637 tion between allocation and specific-relevance rating in the low similarity condition,  
 5638 but a positive correlation in the high similarity condition.

5639 **Hypothesis C.11—Relationship between allocation and general-relevance**  
 5640 **for positive anecdote.** There will be no significant correlations between alloca-  
 5641 tion and general-relevance rating

C. Chapter 6 Appendix

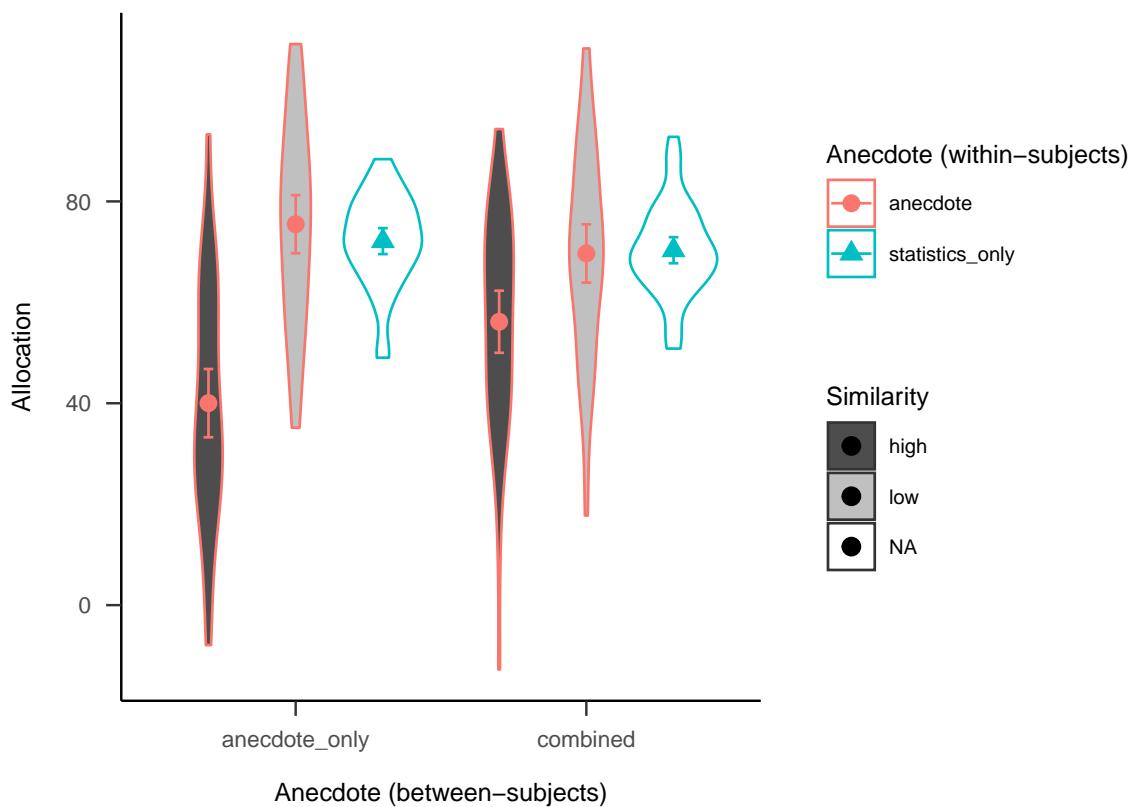


**Figure C.11:** Mean allocation to the target project by similarity rating. The shading represents 95% confidence intervals.

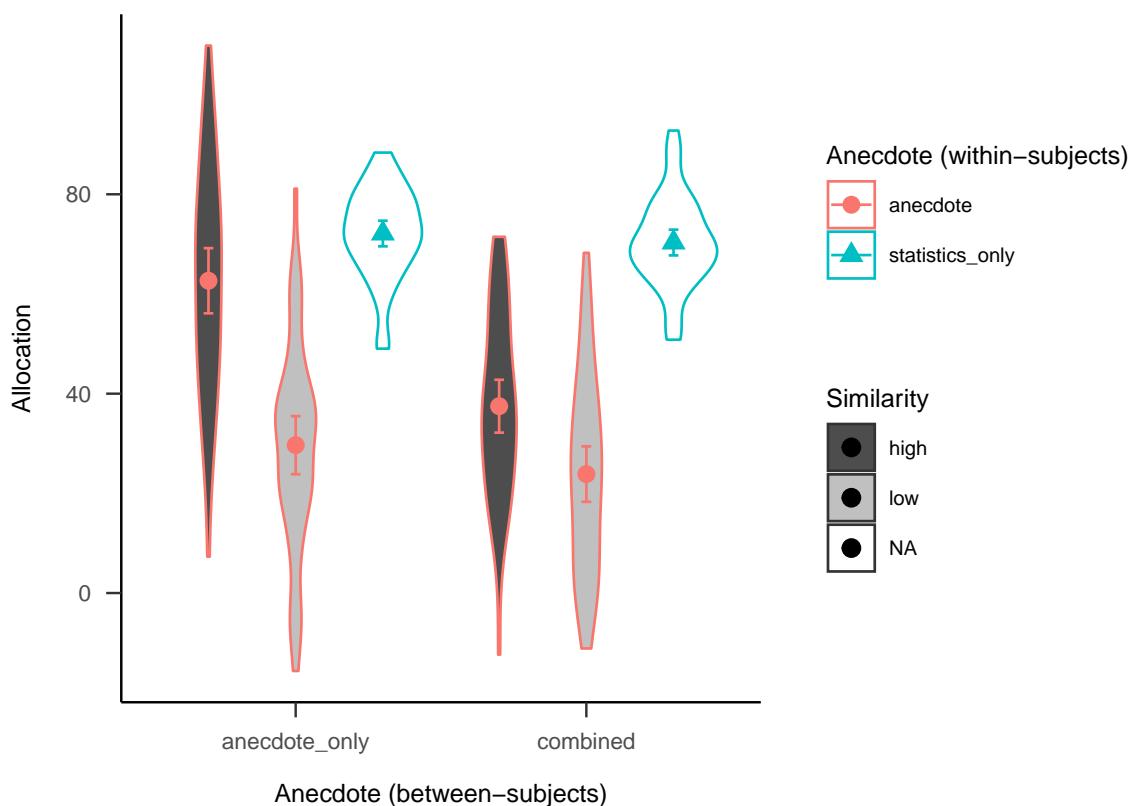


**Figure C.12:** Rating of how relevant participants considered the anecdote to the target project, by similarity rating. The shading represents 95% confidence intervals.

C. Chapter 6 Appendix



**Figure C.13:** Anecdotes Experiment 2 predicted data for the negative valence condition



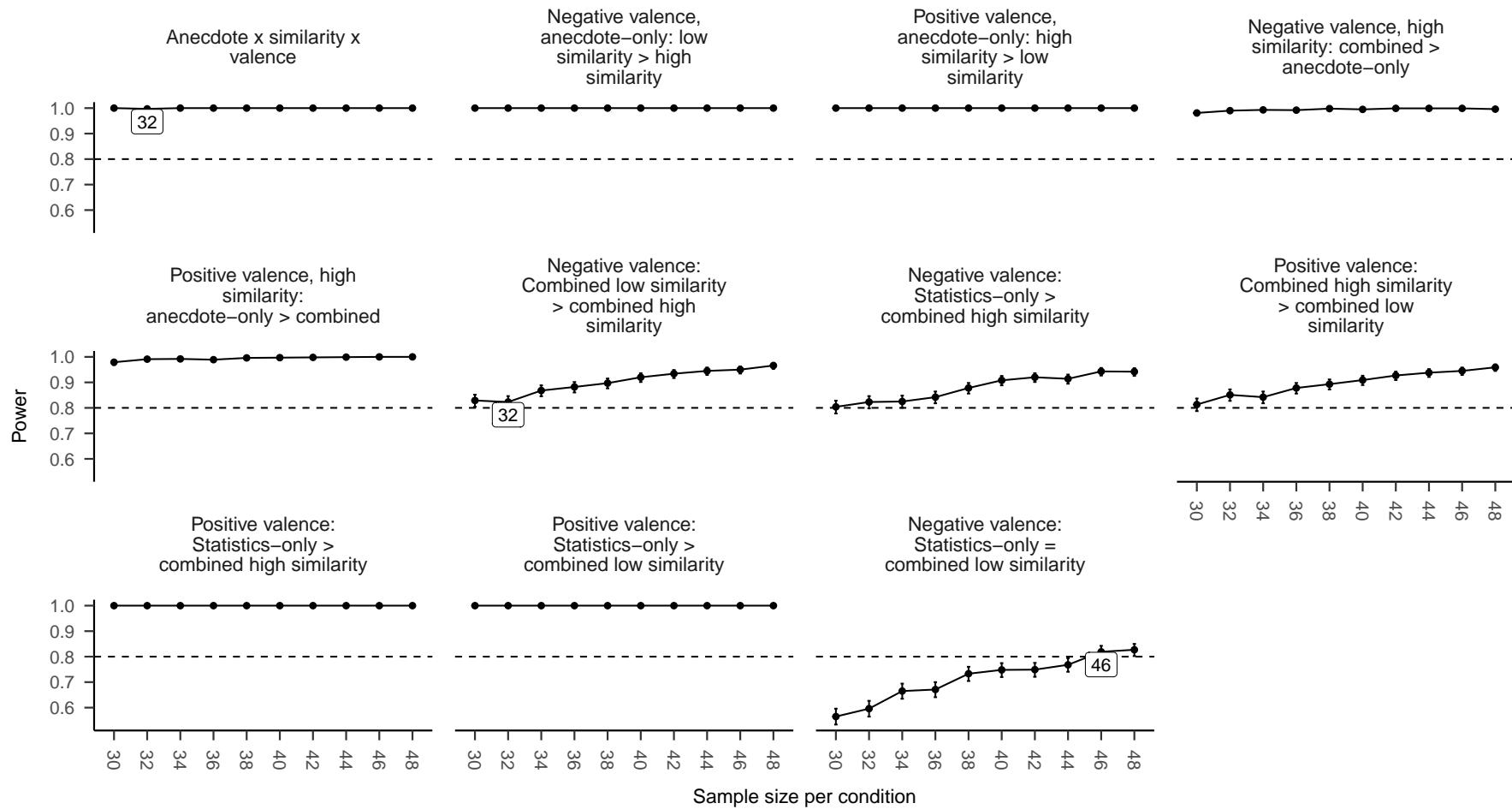
**Figure C.14:** Anecdotes Experiment 2 predicted data for the positive valence condition

## C. Chapter 6 Appendix

### C.2.1 Method

#### C.2.1.1 Participants

5642     **C.2.1.1.1 Power Analysis** A power analysis was conducted through simula-  
5643     tion of the effects implied by the hypotheses in Experiment 2. Data were simulated  
5644     with the same mean values as Experiment 1 for the effects that were previously  
5645     significant (i.e., similarity, statistics, and interaction effects), and no effect for the  
5646     differences that were non-significant (as shown in Figures C.13 and C.14). The null  
5647     effect was analysed using the two one-sided tests (TOST) procedure, or *equivalence*  
5648     testing (Lakens et al., 2018), and setting the smallest effect size of interest to the  
5649     smallest difference that leads to a significant equivalence between the combined  
5650     low similarity and statistics only conditions in Experiment 1. Figure C.15 shows  
5651     the results of this analysis, which suggested a total sample size of 92 ( $46 \times 2$ ).  
5652  
5653



**Figure C.15:** Anecdotes Experiment 2 power curve. Labels indicate lowest sample size above 80% power.

## C. Chapter 6 Appendix

**Instructions**

Imagine you are an executive in a multi-business company and that you are presented with two projects to potentially invest in. Your job is to decide how to allocate the capital available in your budget between these two projects.

In total, you will see five of these project pairs (across five separate web pages). Each page will also contain relevant information about the projects.

Test yourself on the above instructions: How many pairs of projects will you see?

project pairs

[Continue](#)

**Figure C.16:** General instructions for Experiment 2.

### 5654 C.2.1.2 Materials

5655 **C.2.1.2.1 Instructions** Figure C.16 shows the general instructions all participants received, and Figures C.17, C.18, and C.19 show the condition-specific instructions.

5658 **C.2.1.2.2 Allocation Task** The following were counterbalanced: (a) project variation (five latin square variations), which is the association of each display 5659 content with each within-subject condition; and (b) anecdote variation (two variations), which is the association of each project display and being either the target or 5660 5661 5662 comparison project. Table column order and project display order were randomised.

5663 **C.2.1.2.3 Follow-up Questions** Figure C.20 shows an example of the follow-up questions.

5665 **C.2.1.2.4 Interstitial Display** Figure C.21 shows an example of one of the 5666 interstitial displays.

### C. Chapter 6 Appendix

#### Instructions

Managers often find it useful to consult with previous case studies before making important decisions. As well as seeing the two target projects, you will also be provided with an example of a failed project with some information that was available just before the company decided to invest in it. This project was randomly chosen from a pool of thousands of projects. Others rated the similarity of all the case studies to the below target project based on dimensions such as the overall money invested, the quality of the proposal, the experience of the managers that proposed it, and the specific operations that were required. This case study was found to be, on average, as similar to the target as the others in the sample. Further, you are also provided with an analysis of this investment decision after it became clear that the project will not meet its expected return on investment.

**Figure C.17:** Experiment 2 specific instructions for those in the anecdotes only condition.

## 5667 C.2.2 Results

### 5668 C.2.2.1 Allocation

5669 **C.2.2.1.1 Similarity Manipulation Check** The similarity manipulation worked  
5670 as expected, with the negative anecdote only low similarity condition being allo-  
5671 cated significantly more than those in the high similarity condition,  $\Delta M = 26.98$ ,  
5672 95% CI [18.12, 35.84],  $t(186.55) = 6.01$ ,  $p < .001$ . For positive anecdotes, partici-  
5673 pants allocated more to the high similarity condition than those in the low similarity  
5674 condition,  $\Delta M = -22.62$ , 95% CI [-31.48, -13.77],  $t(186.55) = -5.04$ ,  $p < .001$

### 5675 C.2.2.2 Ratings

5676 **C.2.2.2.1 Similarity Manipulation Check** Evidence for the similarity ma-  
5677 nipulation working was also seen in the rating data. Participants rated anecdotes  
5678 in the high similarity condition as more similar to the target than those in the low  
5679 similarity condition,  $F(1, 94) = 48.36$ ,  $p < .001$ ,  $\hat{\eta}_p^2 = .340$ .

5680 **C.2.2.2.2 Allocation is Influenced by Perceived Similarity** As hypothe-  
5681 sised, allocation was influenced by perceived similarity. That is, in the negative  
5682 valence condition, there was a negative correlation between allocation and similarity

### C. Chapter 6 Appendix

**Instructions**

Managers often find it useful to consult with previous case studies before making important decisions. As well as seeing the two target projects, you will also be provided with an example of a failed project with some information that was available just before the company decided to invest in it. This project was randomly chosen from a pool of thousands of projects. Others rated the similarity of all the case studies to the below target project based on dimensions such as the overall money invested, the quality of the proposal, the experience of the managers that proposed it, and the specific operations that were required. This case study was found to be, on average, as similar to the target as the others in the sample. Further, you are also provided with an analysis of this investment decision after it became clear that the project will not meet its expected return on investment.

As a part of the relevant information that will be provided for each target project, you will be provided with measures of overall reliability and Net Present Value (NPV). The NPV is the company's estimation of the future returns of the project. An NPV that is greater than 0 (zero) indicates that there is an expectation of profit. The higher the NPV, the better the expectations for each project. Both these measures were collected as part of a research study conducted by an international consulting company that aggregated data from thousands of other projects in relevant industries.

Note that the project in the case study was included in the research study, so its features are subsumed in the aggregated data.

**Figure C.18:** Experiment 2 specific instructions for those in the combined condition.

**Instructions**

As a part of the relevant information that will be provided for each target project, you will be provided with measures of overall reliability and Net Present Value (NPV). The NPV is the company's estimation of the future returns of the project. An NPV that is greater than 0 (zero) indicates that there is an expectation of profit. The higher the NPV, the better the expectations for each project. Both these measures were collected as part of a research study conducted by an international consulting company that aggregated data from thousands of other projects in relevant industries.

**Figure C.19:** Experiment 2 specific instructions for those in the statistics only condition.

### C. Chapter 6 Appendix

**Follow-up**

On a scale of 1 to 7, how similar do you think the Dinerly project (the case study) is to the Savoro project (the restaurant chain target project)? A choice of 1 indicates low similarity, and 7 indicates high similarity.

On a scale of 1 to 7, how relevant do you think the information about the Dinerly project is for determining whether to invest in the Savoro project? A choice of 1 indicates low relevance, and 7 indicates high relevance.

On a scale of 1 to 7, how relevant do you think the information about the Dinerly project is for determining whether to invest in *any* restaurant chain project? A choice of 1 indicates low relevance, and 7 indicates high relevance.

Justify your answer:

Press the button below to continue.

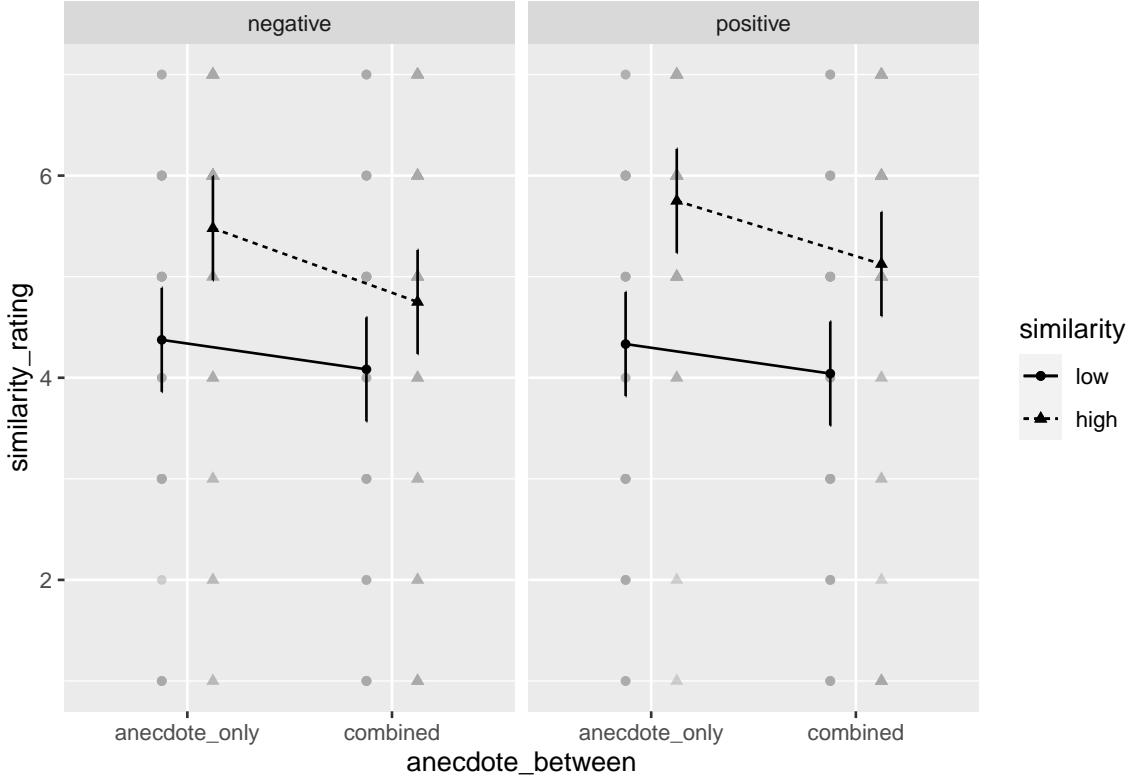
**Figure C.20:** An example of one of the follow-up question displays in Experiment 2.

You will now see project display #1. Please consider this display independently from all the other displays. That is, your allocation should be informed only by the instructions and project descriptions that are on the same webpage.

It is important that you pay attention and read through the task carefully. To show that you are reading and paying attention, please click on the following checkbox **before** clicking on "Continue":

**Figure C.21:** An example of an interstitial display in Experiment 2.

### C. Chapter 6 Appendix



**Figure C.22:** Mean similarity rating of Project A (the target project) to the anecdote. Error bars represent 95% confidence intervals.

rating,  $\Delta M = 0.34$ , 95% CI  $[-3.72, 4.39]$ ,  $t(376) = 0.16$ ,  $p = .870$ . However, in the positive valence condition, there was a positive correlation between allocation and similarity rating,  $\Delta M = 2.86$ , 95% CI  $[-1.47, 7.18]$ ,  $t(376) = 1.30$ ,  $p = .195$ .

#### C.2.2.2.3 The Relationship Between Allocation and Specific-Relevance

**Depends on Similarity** In the negative valence condition, there was no significant difference between the slopes of the high and low similarity conditions,  $M = -2.02$ , 95% CI  $[-6.44, 2.41]$ ,  $t(376) = -0.90$ ,  $p = .371$ . In the low similarity condition, allocation and specific-relevance rating were not correlated,  $\Delta M = 1.01$ , 95% CI  $[-1.21, 3.22]$ ,  $t(376) = 0.90$ ,  $p = .371$ , as in the low similarity condition,  $\Delta M = -1.01$ , 95% CI  $[-3.22, 1.21]$ ,  $t(376) = -0.90$ ,  $p = .371$ .

In the positive valence condition, there was no significant difference between the slopes of the high and low similarity conditions,  $M = 4.25$ , 95% CI  $[-0.20, 8.70]$ ,  $t(376) = 1.88$ ,  $p = .061$ . In the low similarity condition, allocation and specific-

*C. Chapter 6 Appendix*

5696 relevance rating were not correlated,  $\Delta M = -2.12$ , 95% CI [-4.35, 0.10],  $t(376) =$   
5697  $-1.88$ ,  $p = .061$ , as in the low similarity condition,  $\Delta M = 2.12$ , 95% CI [-0.10, 4.35],  
5698  $t(376) = 1.88$ ,  $p = .061$ .

5699 **C.2.2.2.4 People do not Consider General-Relevance in Their Alloca-  
5700 tion** There were no significant correlations between allocation and general-relevance  
5701 rating.

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