Cognitive Processes in Moral Judgment

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Summary

The results of ten experiments investigating cognitive processes in moral judgment are presented in this thesis. The experiments examined two key questions relevant for theoretical issues in moral psychology. The first question asked: what is the role of effortful cognition in moral reasoning? And the second question asked: what cognitive processes are required for reasoning about the good moral actions of others, and are the same processes that underlie reasoning about moral violations recruited for reasoning about self-sacrificial actions?

The first series of experiments (Experiments 1 to 4) showed that completing a cognitively demanding task affects the moral judgments that people subsequently make. For example, after writing an essay without using words that contain the letters “a” or “n”, participants judged that causing harm to one person in order to save a larger group of people was less morally permissible compared to participants who had written essays with no constraints. The experiments also showed that this effect of cognitive fatigue on moral judgments can be eliminated by emphasising the good outcome. These novel findings corroborate predictions from one psychological theory of moral judgment, which proposes that people make deontological judgments against causing harm when they construct a simple mental model of the action and they make utilitarian judgments that favour the overall consequences when they use deliberative cognitive processes to construct complex mental models of action-outcome links.
This series of experiments showed not only that effortful cognition facilitates the constructions of complex mental models but also that it has a role in regulating the emotions people experience after making moral judgments. In two experiments (Experiments 3 and 4), participants felt worse about the judgments they made after they had completed a cognitively demanding task, even though their judgments about whether harm was permissible were the same as those made by participants who had completed a less cognitively demanding initial task. These results are argued to suggest a nuanced role for effortful cognition in moral judgment and some extensions to current theories are proposed in order to account for these findings.

The second issue that was examined was the cognitive processes involved in reasoning about self-sacrificial actions of others, and this issue was investigated in two series of experiments. The first of these series (Experiments 5 to 7) showed an asymmetric moral hindsight effect for good moral actions: participants judged that good actions should be taken more when they learned the outcome was good compared to before they learned about the outcome, but there was no such effect when the outcome was bad. Imagining how the outcome could have been worse had the action not been taken amplified judgments that it should have been taken, and imagining how the outcome could have turned out the same had the action not been taken diminished judgments that is should have been taken. These novel effects for good moral actions support predictions derived from theories about moral violations, for example that causal links between actions and outcomes are important for moral judgments.
Experiments 8 to 10 extended this moral hindsight effect for good actions to judgments of how inspiring or “morally elevating” good actions were judged to be, and further extended the effect to moral behaviour: participants were more likely to spontaneously engage in helping behaviour and more likely to donate money to charity after they had thought about times when good actions turned out well compared to times when good actions failed. The results suggest that moral elevation depends not only on the moral goodness of an action but also on its success.

Both series of experiments further contribute to the idea that domain-general reasoning processes, such as processes that underlie causal inferences and imagined alternatives, are influential in making judgments about moral contents as they are for thinking about non-moral contents. The novel effects of cognitive fatigue on moral judgments and hindsight about good moral actions have important implications for this debate, and for understanding how people make moral judgments in everyday life.
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# Table of Contents

**Chapter 1: Introduction** ................................................................. 15

- Domain Specific or Domain General Processes? ...................................... 19
- Effortful Cognition in Moral Reasoning .................................................. 24
- Reasoning about Good Moral Actions ..................................................... 35
- Aims ........................................................................................................ 41

**Chapter 2: Cognitive Fatigue and Moral Judgments** .............................. 43

- Utilitarian Judgments and Effortful Cognition ......................................... 44
- Sequential Task Designs .......................................................................... 48
- Pilot Study ................................................................................................ 50
- Experiment 1 .............................................................................................. 61
- Experiment 2 .............................................................................................. 73
- Experiment 3 .............................................................................................. 81
- Experiment 4 .............................................................................................. 89
- General Discussion .................................................................................... 98

**Chapter 3: Reasoning about Self-Sacrificial Actions** ............................. 106

- Outcome Knowledge ................................................................................ 107
- Imagined Alternatives and Hindsight ...................................................... 109
- Experiment 5 ............................................................................................ 112
- Experiment 6 ............................................................................................ 120
- Experiments 7A and 7B .......................................................................... 125
- General Discussion ................................................................................... 135

**Chapter 4: Moral Hindsight about Morally Elevating Memories** ............ 141

- Moral Elevation, Episodic Memories and Helping Behaviour .................. 142
- Good Actions, Imagined Alternatives and Future Behaviour .................. 143
List of Figures

Figure 2.1. Mean permissibility judgments to personal and impersonal dilemmas in the pilot study. Error bars represent the standard error of the mean.......................................................... 58

Figure 2.2. Average non-transformed latencies in milliseconds to respond to personal and impersonal dilemma questions. Error bars are the standard error of the mean. .......................................................... 60

Figure 2.3. Mean permissibility judgments to personal and impersonal dilemmas in Experiment 1. Error bars represent the standard error of the mean.......................................................... 68

Figure 2.4. Mean emotion judgments to dilemmas in Experiment 1. Error bars are the standard error of the mean. .......................................................... 70

Figure 2.5. Mean permissibility judgments to stories in Experiment 2. Error bars are the standard error of the mean. .......................................................... 77

Figure 2.6. Mean emotion judgments to stories in Experiment 2. Error bars are the standard error of the mean. .......................................................... 78

Figure 2.7. Mean permissibility judgments to personal and impersonal dilemmas in Experiment 3. Error bars are the standard error of the mean. .......................................................... 85

Figure 2.8. Mean emotion judgments to personal and impersonal dilemmas in Experiment 3. Error bars are the standard error of the mean. ............ 87

Figure 2.9. Proportion of sacrificial actions endorsed by participants in Experiment 4. Error bars are the standard error of the mean. ............ 95
Figure 2.10. Mean emotion judgments to dilemmas in Experiment 4. Error bars are the standard error of the mean. 

Figure 3.1. Mean agreement that the action should be taken in Experiment 5. Error bars are the standard error of the mean.

Figure 3.2. Mean agreement that the action is morally good in Experiment 5. Error bars are the standard error of the mean.

Figure 3.3. Mean agreement that the action should be taken in Experiment 6. Error bars are the standard error of the mean.

Figure 3.4. Mean agreement that the action is morally good in Experiment 6. Error bars are the standard error of the mean.

Figure 3.5. Mean agreement that the action should be taken in Experiment 7A. Error bars are the standard error of the mean.

Figure 3.6. Mean agreement that the action should be taken in Experiment 7B. Error bars indicate the standard error of the mean.

Figure 3.7. Mean agreement that the action is morally good in Experiment 7A. Error bars are the standard error of the mean.

Figure 3.8. Mean agreement that the action is morally good in Experiment 7B. Error bars are the standard error of the mean.

Figure 4.1. Types of counterfactuals created in Experiment 8. Error bars are the standard error of the proportion.

Figure 4.2. Proportion of participants who helped to pick up the pens in Experiment 8. Error bars are the standard error of the proportion.

Figure 4.3. Types of counterfactuals generated in Experiment 9. Error bars are the standard error of the proportion.
Figure 4.4. Proportion of participants who helped to pick up the pens in Experiment 9. Error bars are the standard error of the proportion. ... 161

Figure 4.5. Mean elevation ratings in Experiment 10. Error bars are the standard error of the mean. ......................................................... 167

Figure 4.6. Types of counterfactuals generated in Experiment 10. Error bars are the standard error of the proportion. ......................................... 169

Figure 4.7. Proportion of participants who were willing to donate some of their payment to charity in Experiment 10. Error bars are the standard error of the proportion. ................................................................. 171

Figure 4.8. Amount donated by participants who chose to donate some of their payment to charity in Experiment 10. Error bars are the standard error of the mean. ................................................................................. 172
List of Tables

Table 2.1. Means and Standard Deviations for Emotion Judgments to Actions

Judged Permissible in Experiment 1. .......................................................... 71

Table 2.2. Means and Standard Deviations for Emotion Judgments to Actions

Judged Permissible in Experiment 2. .......................................................... 79

Table 2.3. Means and Standard Deviations for Emotion Judgments to Actions

Judged Permissible in Experiment 3. .......................................................... 87

Table 2.4. Means and Standard Deviations for Emotion Judgments to Actions

Judged Permissible in Experiment 4. .......................................................... 97
Chapter 1: Introduction

People regularly make moral judgments in daily life, and debates about moral issues dominate national and international public discourse. For example, a referendum will take place in Ireland in 2018 about the permissibility of abortion. Pro-life campaigners claim that abortion causes harm to a life and it should be forbidden in all cases, whereas pro-choice activists claim that there are cases in which access to abortion has more positives than negatives. And morality is not solely concerned with judgments about harm; people often make moral judgments about the good actions of others. For example, in July 2017, the Guardian reported a story about beachgoers who formed a human chain in the sea in order to save a group of people from drowning, and readers responded with praise and admiration for the beachgoers’ actions (Luscombe, 2017). How do people judge whether moral actions should be taken? The aim of this thesis is to contribute to a better understanding of the cognitive processes that underlie these types of judgments. The 10 experiments reported in this thesis address several important questions, such as: does cognitive fatigue affect the types of moral judgments people make? Do outcomes of good actions affect whether people judge that they should be taken? And does imagining alternative outcomes influence peoples’ judgments? By answering these questions, the experiments contribute to debates about two key theoretical issues in moral psychology.

The first issue concerns the role of effortful cognition in moral judgments. Prominent theories in moral psychology tend to agree that automatic processes in the mind influence moral judgments, but they disagree
about the role of deliberative reasoning processes. Some researchers claim that
effortful cognition serves only to justify immediate intuitions, whereas others
claim that controlled processes can overturn emotional reactions and inform
judgments, and others propose that effortful cognition serves to construct more
complex mental models of moral dilemmas compared to intuitive processes
(e.g. Cushman, 2013; Crockett, 2013; Greene, Sommerville, Nystrom, Darley
& Cohen, 2001; Haidt, 2001). Experiments 1 to 4 in Chapter 2 inform this
debate by showing for the first time that exhausting cognitive resources with
cognitively demanding tasks alters the judgments participants subsequently
make to moral dilemmas, for example about whether it is permissible to cause
harm to one person in order to save a larger group of people. Specifically, the
results show that fatigued participants judge causing harm in this case to be less
permissible compared to non-fatigued participants. The experiments also show
that the effect of fatigue is eliminated when the outcome, that a greater number
of lives will be saved, is made salient. The results are argued to support the
idea that effortful cognition helps to construct more complete mental models of
the moral event being reasoned about, compared to less effortful cognition.
Competing theories for the role of effortful cognition in moral reasoning are
outlined in the second and third sections of this chapter.

The second issue deals with the cognitive processes that affect peoples’
judgments of good moral actions. Although some positive aspects of morality
have been well researched, such as the psychology of altruism, most of this
literature has been concerned with emotional precursors and reactions (e.g.
Batson & Shaw, 1991); underlying cognitive processes that inform peoples’
judgments of these acts have received little attention. Similarly, research on moral reasoning has focused primarily on peoples’ judgments about moral violations and bad moral actions (Bartels, Bauman, Cushman, Pizarro & McGraw, 2014). Experiments 5 to 7 in Chapter 3 examine whether knowledge about outcomes and imagining how those outcomes might have been different influences peoples’ judgments of good moral actions. The results show a moral hindsight effect for good actions that turn out well: people judge that good actions should be taken more so when they learn they have had good outcomes, but there is no effect on judgments when the outcome is bad. The results also show that imagining how the outcome could have been worse had the good action not been taken amplifies judgments that it should have been, and imagining how the outcome could have turned out the same even without the good action being taken diminishes the same judgments.

When people witness or learn about the good actions of others, they sometimes feel inspired, uplifted and motivated to act in good ways themselves, a phenomenon known as “moral elevation” (e.g. Haidt, 2001; Algoe & Haidt, 2009). Experiments 8 to 10 in Chapter 4 show that the moral hindsight effect for good actions affects moral elevation and subsequent behaviour: people are more morally elevated and are more likely to engage in helping behaviour if they think about times when good actions turned out well compared to times when good actions failed. It is argued that these original findings suggest a need to update the current conceptualisation of moral elevation, which does not account for the importance of outcomes. Moreover, it is argued that the results support the idea that people rely on cognitive
processing to connect actions to their outcomes when reasoning about good
moral events, in the same way that they do when people judge moral dilemmas.
The role of cognitive processes in reasoning about good moral actions is
considered further in the fourth section of this chapter.

The empirical chapters directly contribute to these two key issues of
effortful cognition in moral judgments and cognitive processes in reasoning
about good moral actions. They also contribute to the wider debate about
whether the cognitive processes that influence moral judgments are distinct
from or similar to the processes that underlie judgments in other domains. The
answer to this question has important implications for the focus of moral
psychology research: if the cognitive processes for moral judgment are distinct
from other sorts of reasoning, moral psychology is tasked with mapping the
domain-specific operations of the moral network, whereas if the cognitive
processes for moral and non-moral reasoning are similar, the task is to identify
how moral judgments are affected by these reasoning processes. This second
idea of domain-genericity for reasoning processes comprises an overarching
assumption in the designs of the experiments reported throughout this thesis:
paradigms used to assess reasoning about non-moral contents can be used to
understand how people reason about moral matters. The next section firstly
considers the broad question of whether moral reasoning relies on special
cognitive processes or ones that apply to reasoning more generally. It then
considers the more specific questions of whether moral reasoning requires
effortful cognitive processes, which is relevant for the experiments in Chapter
2, and whether moral reasoning about good moral actions depends on the same
cognitive processes as reasoning about bad actions, which is relevant for the experiments in Chapters 3 and 4.

**Domain Specific or Domain General Processes?**

One viewpoint in cognitive science proposes that special cognitive processes are recruited for reasoning about specific contents, for example that the “domain specific” processes recruited for reasoning about objects are wholly distinct from the processes recruited for reasoning about numbers (e.g. Gigerenzer & Todd, 1999; Spelke & Kinzler, 2007). An alternative view argues that the processes recruited for reasoning are similar across different sorts of contents, for example the “domain general” view of reasoning proposes that processes such as inhibiting impulsive responses and updating working memory underlie reasoning for different sorts of decisions and higher-order cognitive processes (e.g. Newell & Simon, 1972; Sanfey, Rilling, Aronson, Nystrom & Cohen, 2003). The idea that moral judgments rely on domain general reasoning processes is important for the following empirical chapters; the designs of the reported experiments assume that methods used in non-moral reasoning research can provide insight into how people reason about moral matters. In this section, theories and evidence for domain specific and domain general views of morality are outlined in order to show that the domain general view currently holds more empirical support.

**Domain Specificity in Moral Reasoning.** The domain specific view argues that evolution has equipped humans with innate heuristics and rules for dealing with everyday issues and these rules are exclusively applied to the
domain for which they were evolved. For example, one view of language is that people have an innate ability to acquire language and the processes recruited for language acquisition are unique and applied only to acquire language (Chomsky, 1986). The same specificity is argued to apply to moral matters, for example that cognitive predispositions have evolved for detecting potential cheaters, and that the processes recruited to judge moral issues are therefore specific to those contents (e.g. Cosmides, 1989; Cosmides & Tooby, 2000; see also Cheng & Holyoak, 1985).

One prominent domain specific theory in moral psychology has argued that humans have an innate predisposition for moral rules analogous to the predisposition they have for linguistic rules (Dwyer, Huebner & Hauser, 2010; Hauser, 2006; Mikhail, 2007, 2009, 2011). This theory of “universal moral grammar” proposes that there is a complex set of rules in the mind that function exclusively to judge moral matters. These rules are argued to be innate, accessed unconsciously and applied automatically (Mikhail, 2007).

The claim that any psychological concept is innate naturally relies on evidence from developmental research. The central evidence proposed to support the idea of innate moral rules shows that young children exhibit the same patterns of moral judgments as adults, even in situations that they presumably have not experienced before. For example, children sometimes use motives to distinguish between two acts that have the same outcome in the same way that adults do: they judge a boy who hurt his friend by throwing a ball to him more harshly if the harm was intended than if it was not (Nelson, 1980; see also Leslie, Knobe & Cohen, 2006; Hamlin, 2013).
However, in the experiment initially claimed to support the theory of universal moral grammar, only 28% of the young children showed this sensitivity to motives and many more children relied simply on any negative cue in the stories used to condemn the boy (Nelson, 1980). Moreover, children do not always demonstrate the same pattern of moral judgments as adults. A large body of research shows that in the often-used “footbridge” version of the trolley problem, in which the only way to prevent a runaway trolley from hitting and killing a group of five people is to push a large man into its path, adults judge killing the large man in this way to be morally impermissible. When the trolley can be diverted from the track that the five people are on to a track where there is only one man, as is the case in the “switch” version of the dilemma, adults judge causing the death of the man in this way to be much more permissible (e.g. Cushman, Young & Hauser, 2006; Greene et al., 2001; Mikhail, 2007). Children, however, judge acting in these dilemmas to be much more morally permissible than adults, and they do not distinguish between both versions of the dilemma in the way that adults do (e.g. Bucciarelli, 2015; see also Hannikainen, Machery & Cushman, 2017). Although there is other evidence for developmental trajectories of moral judgments (e.g. Hamlin, 2003), the domain-specific claim that innate rules are the sole drivers of moral judgments without scope for influence from overriding deliberative processes lacks sufficient support. As outlined in the next section, intuitions often interact with deliberative cognitive processes in moral reasoning.

**Domain Generality in Moral Reasoning.** An alternative view is that the cognitive mechanisms that underlie reasoning are the same across domains
but may be applied differently depending on the context. For example, one view is that people may rely on automatic responses such as heuristics or they may rely on effortful cognitive processes in a range of different domains (e.g. Kahneman, 2011). This domain general view claims that, for example, the types of heuristics or mental short-cuts that people rely on when reasoning about problems of logic and fact are similarly recruited for reasoning about moral issues (e.g. Sunstein, 2005; see also Greene & Haidt, 2002; Shenhav & Greene, 2010).

One such theory in moral psychology claims that reasoning about moral contents relies on the same sorts of processes as reasoning about concepts related to obligation and permission, that is, deontic reasoning, more generally (Bucciarelli, Khemlani & Johnson-Laird, 2008). In turn, deontic reasoning is proposed to rely on similar processes as other types of reasoning (Bucciarelli & Johnson-Laird, 2005; see also Johnson-Laird & Byrne, 1991). In other words, the mechanisms that underlie reasoning across domains are the same and what differs is the content being reasoned about.

Specifically, this theory claims that people may rely on the same sorts of automatic heuristics, or “intuitions”, when making moral judgments as they do for non-moral reasoning, and in other instances they rely on the same sorts of effortful cognitive processes to consider more alternative possibilities when faced with a moral dilemma as they do for reasoning more generally. These processes are argued to operate in parallel with emotional processes; cognitive and emotional processes may both influence and be influenced by each other in the same way that they interact for other types of non-moral reasoning.
Hence, this “independent processes” account of moral reasoning subscribes to the domain-general view, and proposes that reasoning about moral contents should be susceptible to the same sorts of manipulations and heuristics as reasoning about non-moral contents.

A large body of evidence shows this prediction to be the case (e.g. Baron, 2010; Sunstein, 2005, 2010; see also Costa et al., 2014; Geipel, Hadjichristidis & Surian, 2015; Laham, Alter & Goodwin, 2009; Lombrozo, 2009; Paharia, Kassam, Greene & Bazerman, 2009). For example, the framing effect shows that people make different decisions depending on how options are presented (Tversky & Kahneman, 1986). In the classic example of this effect, participants are asked to choose between two treatments for 600 people who are affected by a deadly disease. The first treatment will save 200 lives and the second treatment has a 33% chance of saving all 600 people and 66% chance of saving no one. Presenting the options in this way highlights the number of lives saved and most people choose the first treatment. But when the options are presented in a way that highlights the numbers of lives that would be lost, for example that the first treatment will result in 400 people dying and the second treatment has a 33% chance that no one will die and 66% chance that all 600 people will die, people tend to choose the second treatment (Tversky & Kahneman, 1986). The framing effect has been evidenced in a number of domains, such as economic decision-making and judgments about health issues (e.g. Druckman, 2001; Levin & Gaeth, 1988; Lotto, Tasso, Gavaruzzi, Carnaghi & Rumiati; 2014; Tasso, Monaci, Trentin & Rosabianca, 2005). Importantly, the effect of framing is also evident when people make
moral decisions: people make different judgments to the trolley problems described previously depending on whether the emphasis is on the amount of people potentially killed or the amount of people potentially saved (Petrinovich & O’ Neill, 1996; see also Sachdeva & Medin, 2008; and for a review see Sinnott-Armstrong, 2008). In fact, the original disease problem could be considered to be a moral dilemma. This evidence shows that reasoning about moral contents is influenced by heuristics in the same way as reasoning about non-moral contents, and it is difficult to reconcile with the view that people rely exclusively on innate rules specific to moral matters.

The current evidence therefore supports the general idea that reasoning about moral matters relies, at least in part, on the same sorts of mechanisms as non-moral reasoning. The experiments reported in this thesis have further implications for this debate, and this issue is returned to in the following chapters. A related challenge requires identifying how those domain-general mechanisms operate when applied to moral contents. For example, does effortful reasoning help people to construct more complete mental models of moral problems compared to less effortful cognition, as it does when people reason about non-moral contents? The role of effortful cognition is a key question in moral psychology, and the experiments in Chapter 2 contribute to this debate.

**Effortful Cognition in Moral Reasoning**

A number of influential theories in cognitive science assume that there is a fundamental differentiation between two systems or processes in the mind
(e.g. Evans, 2009; Kahneman & Frederick, 2002; Kahneman, 2011; Stanovich & West, 2000). The characterisation of these processes varies, sometimes referred to as fast versus slow, intuitive versus deliberative, automatic versus controlled or simply System 1 versus System 2 (see Evans & Stanovich, 2013). Such “dual process” accounts have been especially influential in social cognition research (e.g. Chaiken & Trope, 1999; Smith & DeCoster, 2000), and have dominated discussions in moral psychology (e.g. Cushman, Young & Greene, 2010; Greene, 2009). As a result, although effortful reasoning was originally considered to be the core component of moral cognition (e.g. Kohlberg, 1976; Piaget, 1932), its role in moral judgment formation is now an open debate.

**Dual Processes in Moral Cognition.** This section outlines three prominent dual-process accounts of moral reasoning: (1) the social intuitionist model, (2) the default-interventionist account and (3) model-based accounts. All of these theories emphasise a role for automatic processing in response to moral dilemmas, but they disagree about the extent to which effortful cognition is recruited. Firstly, an outline is provided for how the current evidence rules out the first theory, which largely dismisses the role of controlled cognition in moral judgment. The focus then turns to the latter two theories, which accept that both intuitive processing and deliberative processing can influence moral judgments. It is argued that their primary disagreement is the nature of deliberative processing, with respect to how it impacts judgments and how it impacts emotions. The experiments in Chapter 2 aim to address some key issues in this disagreement.
**Social Intuitionist Model.** The social intuitionist model affords effortful reasoning a minimal role in moral judgment formation. It proposes that moral dilemmas give rise to a perception or intuition that is immediate, effortless and outside of conscious awareness. Moral judgments are driven by these intuitions and deliberative reasoning plays a role only if these judgments need to be rationalised post hoc. Reasoning that overturns initial intuitions is hypothesised to be extremely rare, occurring only when the initial intuition is weak and the capacity for effortful reasoning is high (Haidt, 2012). For example, when faced with the trolley problem described in the previous section, the social intuitionist model proposes that thinking about pushing the man in front of the trolley activates an immediate, negatively charged intuition that gives rise to negative affect and a judgment that it is impermissible to act. Deliberative reasoning processes are recruited only in order to justify this judgment or to persuade others that this judgment is morally correct (Haidt & Bjorklund, 2008).

Evidence for this model relies on cases in which people form a moral judgment but cannot provide conscious justifications; they are “morally dumbfounded” (e.g. Haidt, 2001; Haidt & Hersh, 2001; Paxton, Ungar & Greene, 2012). For example, when asked to judge whether it is acceptable for a brother and sister to engage in consensual sex while using multiple forms of birth control, people frequently judge this behaviour to be morally wrong. People appeal to perceived harmful consequences or purity violations to rationalise their judgment, and this judgment of wrongness persists even when challenged about the ostensible harmlessness of the act by the experimenter.
These persistent harm- and purity-based justifications are interpreted as evidence of dumbfounding because no harm was ostensibly done, since the siblings were using multiple forms of birth control, and “the fact that an act is disgusting does not make it wrong” (Haidt, Bjorklund & Murphy, 2000, p. 7). However, recent research has shown that the procedure of countering these justifications leads only to the appearance of dumbfounding. Participants often remain unconvinced by the counterarguments and hold steadfast in their harm- and purity-based concerns, or in other words, they remain consciously aware of the justifications for their judgments (Jacobson, 2012; Royzman, Kim & Leeman, 2015; see also Guglielmo, 2018). Moreover, people can readily access conscious justifications for many other types of moral judgments (e.g. Cushman et al., 2006).

Hence, the idea that moral judgments are exclusively intuition-driven lacks sufficient empirical support. Instead, the available evidence favours theories that afford effortful cognitive processes some sort of causal role in moral decision-making, in a similar way to the standard dual-processes approaches in other domains of reasoning (e.g. Evans & Stanovich, 2013). Moral psychology has two dominant approaches that subscribe to this dual-process framework: the first claims that conscious reasoning sometimes overturns immediate emotional reactions, the “default-interventionist” account, and the second claims that conscious reasoning helps to construct more complex mental models of the event being reasoned about compared to intuitive processes, the “model-based” accounts. The next sections outline both of these approaches in order to demonstrate that the current evidence cannot
sufficiently differentiate between the two, which is a central aim of the experiments reported in Chapter 2.

**Default-Interventionist Account.** According to the default-interventionist account of moral judgments, two neurally distinct processes underlie moral reasoning (e.g. Greene et al., 2001). One process is immediate and emotional, and it promotes an adherence to rules against moral violations in all cases; it favours “deontological” moral judgments. For example, when faced with the footbridge problem, the possibility of causing direct, bodily harm to another person activates the emotional system and leads to a judgment that causing such harm is impermissible (Greene et al., 2001; Greene, Nystrom, Engell, Darley & Cohen, 2004). A slower, more deliberate process may be recruited in the absence of emotional triggers, or it may overturn decisions made by the fast, emotional system. In these cases, a consideration of the overall utility of the consequences, or a “utilitarian” moral judgment, is favoured. For example, the deliberative system leads to the judgment that killing the man in the trolley problem is permissible because doing so maximizes the number of lives saved (e.g. Greene et al., 2004).

In support of this notion of a dissociation between emotion-based and cognitive systems, deontological judgments are linked with increased activation of brain areas associated with emotional processing, such as the amygdala, and utilitarian judgments are linked with increased activation of brain areas responsible for effortful cognition, such as the dorsolateral prefrontal cortex and the anterior cingulate cortex (e.g. Greene et al., 2001; Greene et al., 2004; Borg, Hynes, Van Horn, Grafton & Sinnott-Armstrong, 2006; Sarlo et al.,
2012; see also Ciaramelli, Muccioli, Lâdavas & di Pellegrino, 2007; Koenigs et al., 2007). The activation of dissociable brain areas, however, does not necessarily support the idea that effortful cognition *overturns* decisions made from emotional processes.

The default-interventionist account therefore relies heavily on reaction time data in order to show that utilitarian judgments depend on a slower process overturning an immediate one. Indeed, people sometimes take longer to make and justify utilitarian judgments, suggesting that they may require more deliberation than deontological judgments (Greene, Morelli, Lowenberg, Nystrom & Cohen, 2008; Paxton et al., 2012; Suter & Hertwig, 2011). This evidence is presented as strong support for the idea that utilitarian decisions arise from a slower, deliberative system overturning the initial, immediate aversion to causing harm that people experience, but a recent meta-analysis of reaction time studies has found no indication that slower reaction times are associated with utilitarian decisions (Baron & Gürçay, 2016; see also Bialek & De Neys, 2017; Gürçay & Baron, 2017; McGuire, Langdon, Coltheart & Mackenzie, 2009). In fact, some research has shown that people sometimes take longer to make deontological judgments against causing harm in trolley problems (Manfrinati, Lotto, Sarlo, Palomba & Rumiati, 2013). Although other evidence for the role of effortful cognition in utilitarian decision-making has been proposed to support the default-interventionist account, for example from cognitive load experiments, it is argued below that this evidence cannot differentiate between the default-interventionist and model-based accounts.
**Model-Based Accounts.** Other dual-process accounts have attempted to shift focus from the distinction between emotion and reason to mental representations of the dilemmas being reasoned about (e.g. Crockett, 2013; Cushman, 2013). Although minor differences between these accounts exist, they are considered together here as “model-based accounts” because they share the same core principles: these accounts distinguish between an intuitive process that leads to a judgment based on a mental representation of an action (e.g. pushing the man in front of the trolley) and another more effortful process that leads to judgments about an action based on the value assigned to its outcome (e.g. pushing the man in front of the trolley will save the five people). This latter process is more cognitively demanding because possible actions and their outcomes are simulated, or more complete mental models of the situation are constructed, whereas the former process is concerned with a rudimentary representation of the action (e.g. Cushman, 2013).

The idea that actions are sometimes judged in isolation from their consequences is supported by the literature on action aversion (e.g. Blair, Jones, Clark & Smith, 1997; Blair, White, Meffert & Hwang, 2013; Patil, 2015). People are physiologically averse to actions that typically cause physical harm even when the causal link to the usual outcome is severed, such as shooting another person with a fake gun (e.g. Cushman, Gray, Gaffey & Mendes, 2012; see also Gray & Wegner, 2009; Gray, Young & Waytz, 2012; Hannikainen, Miller & Cushman, 2017; Miller, Hannikainen & Cushman, 2014). When people think about sacrificing a moral value that they deem to be sacred or ‘protected’, they seem to be insensitive to the overall outcomes of that
sacrifice; even if the sacrifice results in greater overall utility for the value in question, people consider such acts to be impermissible. For example, people who deem the lives of animals to be morally protected are sometimes averse to allowing a small area of conservation land to be destroyed, even if doing so will prevent a larger area of land from being destroyed (Baron & Spranca, 1997). However, other research has shown that, although people seem insensitive to the outcome when the question highlights the required action (e.g., destroying conservation land), altering the focus towards the good outcome (e.g., saving a larger amount of conservation land) eliminates this bias (Bartels & Medin, 2007).

The notion that judging actions in light of their consequences requires more cognitive control is consistent with the neuroimaging evidence originally postulated as support for the default-interventionist account; brain areas associated with effortful cognition are associated with moral judgments proposed to result from complex models of the event (e.g. Greene et al., 2004). Moreover, this idea is consistent with research showing that conditions that encourage deliberative thought promote utilitarian judgments that harm is permissible if the benefits outweigh the losses. For example, people who perform better on tests that require considering more alternative possibilities rather than relying on intuitively appealing responses are more likely to make utilitarian judgments to trolley-type problems, but only if they are initially primed to consider more possibilities (e.g. Baron, Scott, Fincher & Metz, 2015; Paxton et al., 2012; Royzman, Landy & Leeman, 2014; see also Laham et al., 2009). Similarly, conditions that inhibit deliberative thinking reduce
propensities for utilitarian judgments, for example limiting available cognitive resources through working memory load decreases the frequency of utilitarian judgments to trolley problems (e.g. Conway & Gawronski, 2013; Trémolière, De Neys & Bonnefon, 2012).

The evidence that shows that effortful cognition is closely linked with utilitarian judgments is consistent with both the default-interventionist and the model-based accounts of moral reasoning, since both posit that utilitarian judgments rely on deliberative cognitive processes. In an effort to distinguish between these competing explanations, the experiments in Chapter 2 examine conditions in which effortful cognition is inhibited by using for the first time cognitive fatigue as the central manipulation. The experiments test whether cognitive fatigue differentially affects judgments that focus on the action, such as pushing the man, compared to judgments that make the link to the overall outcome explicit, such as doing so in order to save the larger group of people. The experiments also test a secondary issue of how effortful cognition might impact emotions when people make moral judgments.

**Emotion and Reasoning in Moral Judgments**

The three accounts of moral reasoning described in the previous section also disagree about how emotions and controlled cognition interact when people make moral judgments. The social-intuitionist model claims that automatic intuitions give rise to emotions; the default-interventionist account claims that emotions lead to immediate judgments, but they may be suppressed by deliberative reasoning; and the model-based accounts do not provide a clear
mechanism through which emotions might operate. Although there is some
agreement, the general idea in the literature is that emotions come into play
before deliberative cognition.

Indeed, some experiments show that priming different emotions can
impact the moral judgments people make. Inducing negative emotions such as
anger and disgust increases peoples’ condemnation of moral violations (e.g.
Ugazio, Lamm & Singer, 2012; Schnall, Haidt, Clore & Jordan, 2008; but see
Landy & Goodwin, 2015). When positive emotions such as mirth are induced,
people find moral violations to be more acceptable (Valdesolo & DeSteno,
2006; see also Strohminger, Lewis & Meyer, 2011). Although this research
shows that emotions can influence subsequent moral judgments, less
consideration has been given to the possibility that effortful cognition might
impact emotions following moral judgments.

The independent processes account of morality outlined in the previous
section proposes that emotional processes are independent of the intuitive and
deliberative processes that form judgments (Bucciarelli et al., 2008). It
suggests that, although emotions may influence cognitive processes and thereby
moral judgments, cognitive processes might also influence the emotions people
experience after making a moral judgment. Cognitive processes can be used to
regulate emotions that result from non-moral, everyday events: when people
think counterfactualy about how things could have been worse following a bad
outcome, for example doing poorly on an exam, they feel better about the real
outcome (Mandel, 2003; see also Gleicher et al., 1990; Roese, 1994; Roese &
Hur, 1997).
Relatively few studies have examined whether deliberative processes can influence emotions about moral contents. But research has shown that people can provide non-emotive justifications for their judgments about emotion-laden “personal” dilemmas like the footbridge problem and they can provide emotive justifications for their judgments about less emotive “impersonal” dilemmas like the switch problem, when primed to do so (Gubbins & Byrne, 2014). Moreover, when people think counterfactually about the typical choices to these dilemmas, that is, not acting in the personal dilemma and acting in the impersonal dilemma, they judge that the counterfactual option would have made them feel much worse (Tasso, Sarlo & Lotto, 2017; see also Pletti, Lotto, Tasso & Sarlo, 2016). These findings support the idea that cognitive processes may operate independently of emotional processes, and hence can both influence and be influenced by them. Another aim of the experiments in Chapter 2 is to examine the less-often researched claim that effortful cognition can influence emotions by testing whether fatigue affects how people feel about the moral judgments they make.

The experiments in Chapter 2 examine the effects of cognitive fatigue on reasoning about trolley-type problems as well as dilemmas in which a protected moral value other than human life is pitted against a larger amount of that value, such as the conservationism example outlined previously. The effects of fatigue are tested on judgments of how permissible it is to commit a moral violation in order to achieve a good outcome, and also how people feel about making such judgments. The experiments in Chapter 2 also make the unique contribution of examining not only reasoning about moral violations but
also reasoning about moral actions that elicit positive emotions, such as self-sacrificial moral acts, and this focus on morally good actions is pursued further in the experiments reported in Chapters 3 and 4. The next section highlights the need for research on how people judge the good actions of others, and presents some candidate cognitive processes that might underlie such reasoning.

**Reasoning about Good Moral Actions**

Experiments 5 to 10 in Chapters 3 and 4 examine some of the cognitive factors that underlie peoples’ judgments of self-sacrificial moral actions. Research on moral cognition has tended to focus on judgments about causing harm to others in order to prevent a greater amount of harm from occurring; trolley-type problems and dilemmas about protected moral values are pervasive (e.g. Bartels, 2008; Baron & Spranca, 1997; Greene et al., 2001; Mikhail, 2007; Ritov & Baron, 1999; Tanner & Medin, 2004). The ubiquity of these types of dilemmas is unsurprising, as harm is proposed to be the core foundation that defines whether a dilemma does indeed reflect a “moral” issue (Goodwin, 2017; Guglielmo, 2018; Schein & Gray, 2015; Turiel, 1983; but see also Graham, Haidt & Nosek, 2009). In addition, dilemmas that pit concern for the individual against greater good outcomes are argued to reflect most of the moral dilemmas people face every day (Greene, 2014). However the everyday moral judgments that people make are not only concerned with moral problems, as people often make judgments about the morally good behaviour of others. Despite this prominence, few studies have examined peoples’
judgments about the potentially self-sacrificial acts people sometimes make in order to help others (see Bartels et al., 2014; Monin, Pizarro, Beer, 2007).

**Judgments about Self-Sacrificial Actions.** Although the psychology of altruism and prosocial behaviour has been widely researched, most of this work is centred on emotional responses to the suffering of others (e.g. Batson & Shaw, 1991; Stich, Doris & Roedder, 2010) or personal motivations to help others (e.g. Carlo & Randall, 2002; Clary et al., 1998; Eisenberg & Shell, 1986; Eisenberg et al., 2002). Few studies have examined moral reasoning about good actions; the moral reasoning literature has primarily been concerned with moral violations. The few exceptions tend to highlight asymmetries between judgments of good and bad actions, for example actions that have foreseen bad side-effects are judged to be more intentional than those that have foreseen good side-effects (e.g. Knobe, 2003; Pizarro, Uhlmann & Salovey, 2003). But very few studies have been concerned with how people reason about the self-sacrificial actions of others.

In fact, just two studies have examined self-sacrifice in the often-used trolley-type dilemmas. When people are presented with a modified version of the switch dilemma in which the options were (1) flip a switch to direct the trolley towards another person, (2) flip a switch to direct the trolley towards themselves or (3) leave the trolley on its route to kill five individuals, participants are more likely to flip the switch than not, and they are equally likely to direct it towards the other person as towards themselves (Huebner & Hauser, 2011). Similarly, people judge sacrificing themselves to be as permissible as sacrificing another person in the switch dilemma, and are even
more likely to endorse sacrificing themselves in the footbridge version (Sachdeva, Iliev, Ekhtiari & Dehghani, 2015). Although some researchers discount these findings, questioning whether such radical acts of self-sacrifice would ever be conceivable outside of these laboratory-presented dilemmas (e.g. Huebner & Hauser, 2011), recent work on the phenomenon of moral elevation has demonstrated otherwise — even situations that involve jumping in front of a train to save another person.

For example, in July 2007, Wesley Autrey jumped in front of an oncoming train in order to save the life of a man who had fallen in front of it (Buckley, 2007). Such acts of moral excellence tend to gain attention in media reports and a nascent body of research has explored how people respond when they learn about such self-sacrificial acts (Haidt, 2000, 2003a, 2003b; Keltner & Haidt, 2003). When people witness or learn about these acts of moral excellence, they experience “moral elevation”, which is similar to being uplifted or inspired (e.g. Algoe & Haidt, 2009; Van de Vyver & Abrams, 2017). It is also characterised by positive thoughts about humanity and a desire to behave morally (e.g. Aquino, McFerran & Laven, 2011; Cox, 2010; Freeman, Aquino & McFerran, 2009; Lai, Haidt & Nosek, 2014; Schnall, Roper & Fessler, 2010; Siegel, Navarro & Thomson, 2015). Because research on self-sacrificial acts is relatively recent and the materials less established compared to research on moral violations, a brief overview of the methods used in elevation research was conducted (and for more details see Appendix 1.1). This review showed that the methods used to induce and measure moral elevation demonstrate considerable variability across studies (see also Pohling & Diessner, 2016).
More importantly, it showed that few studies have examined the underlying cognitive processes that influence peoples’ judgments of these self-sacrificial actions. The experiments in Chapters 3 and 4 apply insights and methods from research on moral violations and cognitive processes more generally to understand reasoning about these morally good actions. The next section presents some of these candidate cognitive processes.

**Cognitive Processes in Reasoning about Good Moral Actions.** The theories of morality outlined in the previous section tend to be conceptualised based on the cognitive processes that underlie reasoning about moral violations, but they rarely specify that their predictions apply only to these types of moral actions (e.g. Cushman, 2013; Greene et al., 2001; Haidt, 2001; cf. Bucciarelli et al., 2008). The experiments in Chapters 3 and 4 were motivated by these theories, and take as their starting point the idea that similar processes might underlie reasoning about self-sacrificial actions as those that underlie reasoning about moral violations and reasoning more generally. For example the model-based accounts described previously suggest that people might judge good actions differently if they mentally represent a basic model of the action compared to a more complex model that connects the action to its outcome (Crockett, 2013; Cushman, 2013). The first idea tested in Chapter 3 is that knowledge about outcomes influences peoples’ judgments about good moral actions, rather than good actions being judged on their own merit.

The idea that outcome knowledge might affect judgments about good moral actions parallels research in other domains. When people learn about outcomes to a variety of different types of actions, they judge that the outcome
is more predictable compared to before they learn about the outcome (e.g. Baron & Hershey, 1988; Christensen-Szalanski & Willham, 1991; Fischhoff, 1975). This ‘hindsight bias’ has been evidenced in, for example, judgments about political elections, medical decisions and legal verdicts (see Hawkins & Hastie, 1990). It has also been observed for moral risks: people judge a moral risk to have been less permissible when they learn that the risk has had bad consequences compared to when those consequences are a future possibility (Fleischhut, Meder & Gigerenzer, 2017). Whether outcome knowledge alters judgments about good moral actions is tested in Experiments 5, 6 and 7.

Another related possibility is that, when people witness or learn about a good deed, for example when they hear about Mr Autrey running in front of a train to save the other man, their judgments might be affected if they think about how the outcome could have been worse had Mr Autrey not acted. Imagined alternatives to reality, that is counterfactual thoughts, are known to affect a range of different sorts of judgments, including causal judgments and judgments about bad moral actions (e.g. McCloy & Byrne, 2002; Lench, Domsky, Smallman & Darbor, 2015). For example, if people think counterfactually about whether a bad moral agent could have acted differently following a moral violation, and their counterfactual thought affirms that they could have, people judge them to be more blameworthy for the bad outcome (Malle, Monroe & Guglielmo, 2014). Experiments 5, 6 and 7 also examine whether counterfactuals about how things could have gone differently affects peoples’ judgments of the action.
Counterfactuals not only influence the judgments people make, but they can also motivate future behaviour (see Byrne, 2016; Epstude & Roese, 2008). Imagining something that could have been done to prevent a bad outcome from occurring may prepare people for similar experiences in the future (e.g. Markman, Gavanski, Sherman & McMullen, 1993; Markman, McMullen & Elizaga, 2008). For example, near-accident reports from pilots that feature counterfactual descriptions about how things could have gone better are associated with explicit statements of intentions to act in ways that would lessen the risk of the same accident almost occurring again (Morris & Moore, 2000). Learning about good moral actions often motivates people to act morally in the future (e.g. Algoe & Haidt, 2009), and so one possibility is that counterfactual thoughts about how things could have been worse in the inspirational event may prompt people to intend to act better themselves in the future. The effects of outcomes and imagined alternatives on moral elevation and subsequent behaviour are tested in Experiments 8, 9 and 10.

Hence, the experiments in Chapters 3 and 4 test whether hindsight affects how people judge self-sacrificial actions and whether judgments are influenced by imagined alternatives. The rationale for testing these effects is based on research on moral violations, and also subscribes to the idea that reasoning about moral contents relies on similar processes as reasoning about non-moral matters. The experiments make unique contributions to the moral psychology literature by examining reasoning about self-sacrificial actions, and contribute to wider debates in the literature about the nature of the cognitive processes that underlie moral reasoning.
**Aims**

This chapter outlined prominent psychological theories of moral reasoning and many of the empirical findings that influenced their development and shape current understanding. The experiments reported in this thesis aim to test and extend different features of those theories. Five experiments (a pilot study and Experiments 1, 2, 3 and 4) are reported in Chapter 2 investigating the impact of cognitive fatigue on moral judgments. The experiments test whether cognitive fatigue affects reasoning about the permissibility of moral actions and how people feel about their judgments. The results relate to the debate about the role of effortful cognition in moral reasoning.

The remaining experiments, from Experiment 5 through to Experiment 10, test some candidate cognitive processes that might affect how people judge good moral actions. The experiments examined if knowing whether a good action succeeded or failed influences peoples’ judgments of that action, as well as how inspired they are and whether they attempt to emulate it. The results have implications for how the recent phenomenon of moral elevation is conceptualised, as well as for the broader field of moral psychology, which has tended to focus on moral violations.

The 10 experiments have further implications for the wider debate about whether processes recruited for moral reasoning are similar to the processes recruited for “domain general” reasoning or whether they comprise a special set of exclusively-moral processes. By testing the impact of fatigued general cognitive resources (Experiments 1 to 4) and the effects of outcome knowledge
and imagined alternatives (Experiments 5 to 10), the results contribute to the support for the domain general view. Moreover, the experiments test whether deliberative cognitive processes can impact the emotions people experience following bad moral actions, such as causing harm, and good actions, such as self-sacrificial acts to help others, thereby contributing to the idea that emotional processes may operate independently to, but interact with, cognitive processes.

This thesis aims to investigate psychological processes that underlie the moral judgments people make about the actions of others, especially relating to the moral permissibility of actions and how other people should act. The aim is not to contribute to philosophical claims about whether these judgments are indeed morally right, but to shed light on the processes people rely on to form their judgments. The results therefore have implications for understanding some of the processes that might lead people to judge the same moral actions differently in everyday life, and hence contribute to understanding the cognitive underpinnings of the moral debates that dominate current discourse. In the next chapter, the key aim is to understand how effortful cognitive processing influences peoples’ moral judgments.
Chapter 2: Cognitive Fatigue and Moral Judgments

People make different decisions when they are tired. For example, peoples’ preferences for risky gambles become more variable following a cognitively demanding task and consumers are more susceptible to marketing strategies when their cognitive resources are reduced (Levav, Heitmann, Herrmann & Iyengar, 2010; O’Dhaniel, Leong & Kurnianingsih, 2015; Pocheptsova, Amir, Dhar, Baumeister, 2009; see also Mani, Mullainathan, Shafir, & Zhao, 2013; Spears, 2010). People may make different decisions when they are cognitively fatigued because their limited cognitive resources have been exhausted and they no longer have sufficient capacity to allocate to new decisions (e.g., Baumeister & Heatherton, 1996; Schmeichel, Vohs & Baumeister, 2003), or because they experience a reluctance to engage in further effortful cognition (e.g., Inzlicht & Schmeichel, 2012).

The aim of the experiments in this chapter is to examine whether cognitive fatigue affects decision-making about moral contents. Do people judge violating a moral principle to achieve a good outcome, such as harming another person in order to save a larger group, differently when they are cognitively fatigued compared to when they are not? For example, do people think differently about the following dilemma if they have firstly completed a task that fatigues their cognitive resources compared to when they have completed an easier task?

You are the explosives expert for a company that has been hired to demolish a skyscraper. You are examining the last of the explosive charges when you notice a teenager below who
is about to accidentally detonate one of the charges out of sequence. This explosion will result in the building’s uncontrolled collapse onto you, the teenager, and the crowd of spectators. The teenager is several floors below you and cannot hear you because of the loud demolition noise. You realise that the only way to stop the teenager from detonating the charge is to drop a heavy cinder block on his head. This will crush his skull and kill him almost instantly but will prevent the out-of-sequence explosion.

Much of the research on moral violations distinguishes between deontological judgments that it is impermissible to violate a moral rule in all cases, for example that it is impermissible to cause harm to the teenager, and utilitarian judgments that violations are permissible if the benefits outweigh the costs, for example that it is permissible to cause harm to the teenager in order to save the crowd of spectators (e.g. Baron, 2017; Greene et al., 2001; Mikhail, 2007). The idea tested in this chapter is that cognitively fatigued participants will judge such moral violations to be less permissible than non-fatigued participants because utilitarian judgments require effortful cognitive processing (Crockett, 2013; Cushman, 2013; Greene et al., 2001).

**Utilitarian Judgments and Effortful Cognition**

Moral judgments about dilemmas require a consideration of both the action and the outcome and how they are linked (e.g. Elqayam, Wilkinson, Thompson, Over & Evans, 2017; Wiegmann & Waldmann, 2014). Moral principles, such as rules against causing harm, are important pillars of society,
and so the judgment that a moral violation is permitted in some instances is a serious one. Making such a judgment may require cognitive resources to weigh up the benefits of the outcome against the moral violation of the action, much more than focusing on just the violation. Hence, utilitarian judgments that a moral violation is permissible in some circumstances may depend on constructing a model that makes explicit links between the action and its outcome, which people may find cognitively demanding to do (e.g. Crockett, 2013; Cushman, 2013).

The idea that utilitarian decisions rely on effortful cognition is supported by dual-task cognitive load experiments. Peoples’ ability to carry out effortful executive functions, such as allocating attention and manipulating information in working memory, is limited (Baddeley, 1996, 2007; Smith & Jonides, 1999). One way to determine whether a specific task relies on deliberative reasoning is to exploit this limited capacity by dividing resources between two effortful tasks simultaneously; if responses are affected, it is assumed they typically require effortful cognition (e.g. Gilbert & Hixon, 1991; Lavie, Hirst, de Fockert & Viding, 2004; Ward & Mann, 2000). Inducing cognitive load in this way affects utilitarian judgments: people are less likely to judge that committing a moral violation to achieve a good outcome is permissible if they concurrently hold password-like character strings such as “n63#m1Q” or difficult dot patterns in their working memory (Conway & Gawronski, 2013; Trémolière et al., 2012). People also sometimes take longer to make utilitarian decisions under cognitive load (Greene et al., 2008).
Cognitive load tasks often require some sort of memorisation because working memory is closely tied to deliberative cognitive processing; higher working memory capacity is typically indicative of greater cognitive ability (Miyake & Shah, 1999; Smith & Jonides, 1999). Lending further support to the idea that utilitarian judgments require more working memory than deontological judgments, people who perform better on tasks designed to measure working memory tend to endorse more utilitarian responses compared to people who perform worse on working memory assessments (Moore, Clark & Kane, 2008). People high in working memory capacity also take longer to decide about moral violations that involve directly harming another person in order to achieve a greater overall outcome, possibly because they consider more alternative models of the dilemmas (Moore et al., 2008).

In a similar vein, the extent to which a person is likely to engage in effortful reasoning may depend on their individual cognitive style. The Rational-Experiential Inventory measures preferences for deliberative thinking using statements such as “Using logic usually works well for me in figuring out problems in my life” and other statements such as “I often go by my instincts when deciding on a course of action” to measure preference for intuitive thinking (Pacini & Epstein, 1999). Agreement to statements about deliberative reasoning is associated with utilitarian decisions that harm is permissible in order to save others (Conway & Gawronski, 2013). Analogously, intuitive thinkers are more likely to make deontological judgments that harm is not permissible, especially when dilemmas are presented using vivid language (Bartels, 2008; see also Amit & Greene, 2012; Ward & King, 2017).
findings that greater cognitive ability and deliberative cognitive styles are
linked to utilitarian judgments further support the idea that such judgments rely
on effortful cognition, and hence participant thinking style may be an important
factor to consider in moral reasoning research.

Although the evidence linking greater cognitive capacity with utilitarian
judgments is compelling, it is difficult to differentiate between the default-
interventionist claim that effortful cognitive processes overturn immediate
emotions and the model-based claim that effortful reasoning helps to construct
more complete models of the situation being reasoned about than intuitive
processing (e.g. Greene et al., 2001; Cushman, 2013). The aim of the
experiments in this chapter is to examine moral judgments when cognitive
resources are limited, and to distinguish between these competing theories by
focusing attention onto the action or deemphasising the action and focusing
attention onto the outcome. Previous research has shown that people are more
likely to make utilitarian judgments when options in a moral dilemma are
framed to highlight the beneficial outcomes compared to when the harmful
action is salient (e.g. Bartels & Medin, 2007; Petrinovich & O’ Neill, 1996;
Sachdeva & Medin, 2008). Although these findings have not been related to
model-based accounts of moral reasoning, one possibility is that such framing
effects occur because it is cognitively demanding to make the link from the
moral violation to the beneficial outcome, and that framing options to highlight
the outcome makes this link less cognitively effortful to construct. The
experiments in this chapter are the first to examine whether limiting cognitive
processing affects reasoning about moral dilemmas differently when the action
is made salient compared to when the outcome is highlighted. By examining whether effortful cognition impacts moral judgments differently when actions are highlighted compared to when outcomes are emphasised, the experiments test the key distinction between the default interventionist and model based accounts: the default interventionist account does not predict any difference in reasoning about both types of questions, whereas the model based accounts propose that people require effortful processes to incorporate information about outcomes when reasoning about moral violations, and hence they predict that the effects of limited cognitive resources should be evident when actions are highlighted but not when outcomes are. In addition, the experiments in this chapter use methods from a paradigm not yet applied to research on moral reasoning; they use sequential-task designs to limit cognitive resources.

**Sequential Task Designs**

Based on the same premise as cognitive load designs that effortful cognitive resources are limited, another way in which reasoning processes can be constrained is by employing a temporal load. Such experimental designs involve participants completing a cognitively demanding task prior to the key experimental task. They are based on the idea that when executive functions become exhausted, people are less reliant on deliberative processing when making decisions. Downstream effects on subsequent experimental tasks are proposed to demonstrate the extent to which effortful reasoning is recruited for those tasks (e.g., Baumeister & Heatherton, 1996; Muraven & Baumeister, 2000; Schmeichel et al., 2003). For example, tasks that involve logic and
reasoning, cognitive extrapolation and thoughtful reading comprehension are impaired after participants complete a cognitively demanding task, such as writing an essay without using words with the letters “a” or “n” (Schmeichel, 2007). Similar impairments have been found on other higher-order cognitive tasks, such as dynamic decision-making, prospective memory tasks and incongruent Stroop trials (Barber & Smit, 2014; Li, Nie, Zeng, Huntoon & Smith, 2013; Yam, 2015). Less complex tasks, such as general knowledge tests and simple recall tests, are unaffected (Schmeichel, 2007). Hence, if cognitive fatigue has similar effects on higher-order cognition as cognitive load manipulations, cognitive fatigue should affect the moral judgments people make in the same way that cognitive load does.

Indeed, cognitive fatigue has been found to influence a range of ethical behaviours. After completing a difficult cognitive task, people are more likely to cheat for a small reward compared to people who complete easier initial tasks (Gino, Schweitzer, Mead & Ariely, 2011; see also Yam, Chen & Reynolds, 2014; Yam, 2015). Compared to non-fatigued participants, fatigued participants are also more likely to lie for their own benefit but less likely to lie if it only benefits others (Cantarero & Tilburg, 2014; Mead, Baumeister, Gino, Schweitzer & Ariely, 2009); they are less trusting in economic games and act less fairly (Ainsworth, Baumeister, Ariely & Vohs, 2014; Ren et al., 2014); they are more likely to rely on prejudicial stereotyping, and even more likely to allow racial biases to affect decisions of when to shoot potential criminals in simulated games (Govorun & Payne, 2006; Ma et al., 2013). Despite this
research on fatigue and moral behaviour, the impact of cognitive fatigue on the moral judgments people make has not yet been examined.

The aim of the experiments in this chapter was to expand on the research that shows that effortful cognition is recruited for moral reasoning and to elucidate some of the contours of the effect of reduced cognitive resources. Although reservations about sequential task-induced cognitive fatigue have been expressed in the ego depletion literature, for example with concerns that effect sizes can be very small (e.g. Carter & McCullough, 2014; Hagger et al., 2015), the tasks used for the experiments in this chapter address some of the primary concerns. The fatiguing tasks require breaking an established habit and manipulation checks were used to ensure that participants found them cognitively effortful (see Baumeister & Vohs, 2016; Dang, 2016; Hagger, Wood, Stiff & Chatzisarantis, 2010). Moreover, the aim of the experiments in this chapter was not to test claims made about the nature of fatigue, it may reduce the capacity people have to allocate resources to new decisions or people may experience a reluctance to engage in further effortful processing (e.g. Baumeister & Heatherton, 1996; Inzlicht & Schmeichel, 2012; Schmeichel et al., 2003). Rather, the aim was to use the sequential load method of depletion studies, analogous to the simultaneous load method of working memory studies, to reduce the availability of cognitive resources.

Pilot Study

The aim of this pilot study was to examine whether cognitive fatigue affects permissibility judgments to moral violations that have beneficial
outcomes. Some participants completed a cognitively demanding task, which required them to write an essay without using words with the letters “a” or “n”. This task recruits processes associated with controlled cognition, for example it requires frequent inhibition of the words that would typically be used when writing and the use of working memory to retrieve more appropriate ones (Schmeichel, 2007). Since English speakers are accustomed to using many words with the letters “a” and “n”, engaging in a writing task that prohibits such words requires breaking an ingrained habit and so should be sufficiently cognitively demanding to fatigue resources (Baumeister & Vohs, 2016). Other participants completed an easier version of the task, which required writing an essay but with no constraints on the words used. Mood was measured immediately after the writing task in case participants experienced negative emotions as a result of its difficulty, and because emotions can impact peoples’ judgments of moral dilemmas (e.g. Schmeichel, 2007; Schnall et al., 2008; Strohminger et al., 2011; Valdesolo & DeSteno, 2007).

Participants then made judgments about a series of moral dilemmas. Half of the dilemmas required causing direct, bodily harm to another person in order to save a larger group of people. For example, in the “personal” version of the Explosion dilemma described at the beginning of this chapter, the only way to prevent the teenager from causing the premature explosion is to drop a heavy cinder block on his head. The other half of the dilemmas described an indirect harm, for example in the “impersonal” version of the same dilemma, the teenager can be prevented from causing the explosion by flipping a switch that will cause him to be electrocuted (e.g. Greene et al., 2001; Moore et al.,
People tend to judge personal harms to be less permissible than impersonal ones (e.g. Cushman et al., 2006; Mikhail, 2007).

The use of these extreme dilemmas “trolley-type” dilemmas is ubiquitous in research on moral reasoning. Although people sometimes avoid emotionally aversive stimuli, such as high-threat health communications (e.g. Nestler & Egloff, 2010; see also Golman, Hagmann & Loewenstein, 2017), the use of these dilemmas carries the assumption that participants will attend to the problem presented to them. Some challenges to this assumption have been demonstrated, for example that some participants do not take the dilemmas seriously and instead find them humorous (e.g. Bauman et al., 2014).

However, in most cases, responses to dilemmas have been found to map onto formal philosophical theories of morality, such as utilitarianism and deontology, even those held by naïve reasoners. For example, individuals who score highly on measures of deontological outlooks tend to reject causing harm in the dilemmas whereas individuals who score highly on measures of utilitarianism tend to endorse causing harm in order to save a greater number of people (e.g. Lombrzo, 2009; see also Greene, 2014). Nonetheless, whether participants engage in the dilemmas is important to determine. To this end, an item to measure how participants felt about their judgments was included in the experiments conducted after the pilot study.

In the pilot experiment, the key judgment participants were asked for was normative, that is whether the action would be morally permissible, and they made their judgments on a scaled response format, from forbidden through permissible to obligatory, rather than a dichotomous one, to allow for a more
nuanced response (e.g. Kahane & Shackel, 2010; see also Verschueren, Schaeken & d’Ydewalle, 2005). It was hypothesised that, because actions in personal dilemmas are rarely judged to be permissible, fatigue would have a greater effect on impersonal dilemmas. It was also hypothesised that, because people tend to take longer to make moral judgments under cognitive load (Greene et al., 2008), fatigued participants would take longer to make their judgments compared to non-fatigued participants. Reaction times may also serve as an additional check to measure whether fatigued participants engage with the dilemmas; faster reaction times could indicate that they attend to the dilemmas less than non-fatigued participants.

Participants completed some additional experimental checks based on the previous literature. As outlined in the previous section, cognitive ability and style are associated with utilitarian responding, but measures of cognitive ability could be confounded with the fatigue manipulation; participants could perform poorly on measures of cognitive ability when fatigued or measures of cognitive ability could fatigue participants. In place of a cognitive ability measure, participants completed the Rationality-Experientialism Inventory as a measure of cognitive style at the end of the experiment (Pacini & Epstein, 1999). The Rational-Experientialism Inventory was used as a control check for cognitive style, in order to ensure that between-group differences could be attributed to the central fatigue manipulation and not any pre-existing differences in cognitive style between the groups. Lastly, they were asked how difficult they found each of the tasks in order to determine whether the fatigue task was cognitively effortful and whether it had any effects on metacognitive
perceptions of making moral judgments, for example whether fatigued participants find making such judgments to be more difficult than non-fatigued participants (e.g. Dang, 2016).

**Method**

**Participants.** The participants in the pilot study were 40 students (26 women, 14 men) from Trinity College Dublin. Sample size was calculated using G*Power 3.1 (Faul, Erdfelder, Lang & Buchner, 2007) on the basis of detecting a moderate to large effect size (found in laboratory cognitive depletion tasks; Hagger et al., 2010) and a high correlation, $r = .7$, between the repeated measures of personal and impersonal dilemmas, at 80% power. Undergraduate psychology students ($n = 33$) received course credits for participation. Their ages ranged from 18 to 61 years, with an average age of 24 years. Participants were assigned at random to either a fatigue condition ($n = 20$) or a no fatigue condition ($n = 20$) using the RAND function in Excel.

**Design and Materials.** The design was a 2 (condition: fatigue vs. no fatigue) x 2 (dilemma: personal vs. impersonal) mixed factorial one; condition was manipulated between-groups with dilemma type as the repeated measures factor. All participants firstly completed a writing task and were given the instructions (adapted from Schmeichel, 2007):

In the space provided below, please write a short essay about a trip you have taken. It may be a trip to the shop, to Cork or to another country – wherever! Please write until the experimenter asks you to stop.
The participants in the fatigue condition were given the further instruction:

Very important! Please do not use the letters *a* or *n* anywhere in your story. If you find yourself using words that have the letters *a* or *n* in them, please stop and try to find a different way to express yourself.

Participants were allocated 6 minutes for the task, which was timed from when they began writing. After completing the writing task, participants were given the Brief Mood Introspection Scale to assess the possible effect of the task on their mood (Mayer & Gashke, 1988).

Next, participants were informed that they would be presented with a series of hypothetical stories followed by a question asking for their judgment of each story’s resolution. Participants were asked to please disregard legality and base their judgments solely on their personal opinion. They were informed there were no right or wrong answers and there was no time limit (dilemmas and instructions from Moore et al., 2008). Participants read 8 moral dilemmas of different contents and 4 filler items. Each dilemma had a personal and impersonal version. In order to ensure participants each read 4 personal and 4 impersonal dilemmas and to control for content effects, two sets of 8 dilemmas were constructed and participants received one set at random. The order in which the dilemmas were presented was randomised by the computer program. Their task was to rate the moral permissibility of committing the harmful action, for example, “Killing the teenager in order to save yourself and the crowd of spectators is morally…” They made their judgments on a 7-point Likert scale from 1 “forbidden” to 7 “obligatory”. The length of time it took
participants to make this decision was recorded, but, because time-pressure can increase the likelihood of deontological responding (Suter & Hertwig, 2011), they were not made aware of this measure until the end of the experiment.

Participants lastly completed the Rational-Experiential Inventory (Pacini & Epstein, 1999), and they rated the difficulty of all tasks on a scale from 1 “not at all difficult” to 7 “extremely difficult”. They completed the tasks in the fixed order of writing task, mood scale, moral dilemmas, thinking style scale and difficulty ratings. This order was chosen as it was necessary to administer the writing task first, and to measure its impact on mood before the moral dilemmas were presented. The thinking style scale was administered after the moral dilemmas in order to reduce any priming effects the subscales might have on the primary measures of interest, and the difficulty ratings needed to be measured after all tasks had been completed. All materials are available in Appendix 2.1.

**Procedure.** Participants were tested individually in a quiet room. They completed the writing task, mood scale and difficulty ratings using pen-and-paper. The content of the written essays were assessed to ensure compliance with the instructions in each group; all participants were able to comply with the instructions. The dilemmas and thinking style scale were presented using a program written in SuperLab5.0 on a MacBook Pro. The moral dilemmas were presented in short paragraphs of several sentences. They appeared on screen paragraph by paragraph, with each paragraph joining the preceding one for each dilemma, and participants pressed the spacebar to move on to the next section. Their responses to the moral dilemmas were recorded using the
program, which also recorded their latency to respond to the question at the end of each dilemma. Latencies were recorded from the onset of the dilemma question until the participant made a response. In order to minimise any time delay between deciding and pressing the keys, stickers were used to label the keys “x” to “,” as the numbers 1 to 7. Participants were instructed to sit with their fingers resting on these keys and their thumbs on the spacebar. The experiment lasted about 30 minutes.

**Results and Discussion**

**Manipulation Checks.** Non-parametric statistics were used on the manipulation checks because the data were of ordinal quality, and parametric statistics were used on other analyses because the data were interval (provided relevant assumptions were met). Consistent with the view that the constrained writing task required more cognitive effort than the control task, a Mann-Whitney U test showed that participants in the fatigue condition rated it as more difficult \((Mdn = 7, IQR = 5.25-7)\) than non-fatigued participants rated the unconstrained task \((Mdn = 1, IQR = 1-2)\), \(U = 7.5, p < .001, r = .85\). A further Mann-Whitney U test showed that fatigued participants rated the moral judgments task to be more difficult \((Mdn = 5, IQR = 5-6)\) than non-fatigued participants did \((Mdn = 4, IQR = 2.25-5)\), \(U = 111, p = .014, r = .39\). These results suggest that the fatigue manipulation had an effect on later cognitive processing, implying that effortful cognition may be required for both types of tasks (the results from the mood scale and thinking style scale did not impact the key results; for brevity they are reported in Appendix 2.2).
Figure 2.1. Mean permissibility judgments to personal and impersonal dilemmas in Experiment 1. Error bars are the standard error of the mean.

Moral Judgments. Participants tended to judge that it was morally permissible to act, at about 4 on the scale of 1 to 7. To examine the influence of fatigue on moral judgments, a 2 (fatigue, no fatigue) x 2 (personal, impersonal) ANOVA with repeated measures on the second factor was conducted. There was a significant effect of fatigue, $F(1, 38) = 5.58, p = .023, \eta^2_p = .13$, as fatigued participants rated causing the death of one person in order to save a group of people to be less morally permissible than non-fatigued participants did, as Figure 2.1 shows. The effect of dilemma approached significance, $F(1, 38) = 3.85, p = .057, \eta^2_p = .09$, with causing death by personal harm rated as marginally less permissible than death by impersonal harm, in accordance with the literature (e.g. Greene et al., 2004). Although it
was expected that effects of fatigue would be more pronounced on impersonal dilemmas, there was no interaction, $F(1, 38) = 1.05, p = .313$; reduced cognitive resources had the same effect on both types of dilemmas.

**Response Latencies.** Latencies to respond were recorded from the onset of the dilemma question until the participant made a response. Outliers were trimmed on a by-participant, by-condition (i.e. dilemma type) basis, with short outliers defined as those below 1000ms and long outliers defined as those above the participant’s mean latency for that dilemma type plus 1.96 times their standard deviation (Ratcliff, 1993). Two long outliers were identified (0.06% of the data) and were replaced with each participant’s mean latency. Latencies to each dilemma type were averaged and then log$_{10}$-transformed in order to normalise the data.

Participants took about 11 seconds to make a decision. A 2 (fatigue, no fatigue) x 2 (personal, impersonal) ANOVA on log$_{10}$ latencies showed a main effect of fatigue, $F(1, 39) = 7.00, p = .012$, $\eta_p^2 = .16$, with fatigued participants taking significantly longer to make decisions compared to non-fatigued participants, as Figure 2.2 shows. There was no effect of dilemma type, $F < 1$, nor did the two factors interact, $F < 1$. As expected, participants who completed a cognitively demanding task took longer to make moral judgments than participants who initially completed an easier task.
Figure 2.2. Average non-transformed latencies in milliseconds to respond to personal and impersonal dilemma questions. Error bars are the standard error of the mean. Note that inferential analyses were conducted on log10-transformed latencies.

The same analysis was then conducted only on responses that favoured the utilitarian option (i.e. 4 or above on the scale). Note that, because some participants did not provide any responses above 4 on the permissibility scale, sample size and hence statistical power was much smaller for this analysis. Overall, the trend was the same but the effects did not reach significance: the same ANOVA on log_{10}-transformed “utilitarian” reaction times showed that fatigued participants ($n = 14$) tended to take longer to judge that both personal ($M = 11.16s, SD = 1.59s$) and impersonal ($M = 14.31s, SD = 4.87s$) harms were permissible compared to non-fatigued participants ($n = 16; M = 8.37s, SD = 1.64s; M = 7.60s, SD = 1.23s$, respectively), $F (1, 28) = 2.92, p = .098, \eta^2_p = \ldots$
.10. There was no effect of dilemma or interaction between the two factors, $F$s $< 1$.

**Summary.** The results from this pilot study established an effect of cognitive fatigue on moral judgments. Participants who completed a cognitively demanding task subsequently found reasoning about moral dilemmas to be more difficult and they judged the violation of a moral principle that leads to a beneficial outcome, such as harming another person to save a larger group, to be less permissible compared to participants who completed a less cognitively demanding task. They also took longer to make their judgments. The effect was observed for moral judgments that referred to both the action (e.g. “killing the teenager”) and its beneficial outcome (e.g. “saving the crowd of spectators”). The next experiment examines whether the effect of cognitive fatigue on moral judgments occurs for judgments that focus on the action alone compared to judgments that deemphasise the action and highlight the beneficial outcome.

**Experiment 1**

The aim of the experiment was to examine whether the effect of cognitive fatigue on moral judgments occurs because people sometimes construct a simple model that fails to explicitly link the action to its outcome when they are fatigued. Participants reasoned about judgments that focused on the action or judgments that focused on the outcome, and the responses from fatigued and non-fatigued participants were compared. It was expected that the
same effect of fatigue demonstrated in the pilot study would replicate for judgments that explicitly mention the action:

‘Killing the teenager in this case is morally…’

That is, that participants who are fatigued will tend to judge that a moral violation is less permissible than those who are not fatigued. However, when participants’ attention is explicitly directed to the outcome, fatigued participants should more readily construct a model that links the action to the outcome and they should judge that the action is as permissible as non-fatigued participants do. Participants in the “outcome” conditions were presented with a question that de-emphasised the action (by not repeating it, as was done in the pilot study) and emphasised instead its link to the outcome:

‘Doing this in order to save yourself and the crowd of spectators is morally…’

It was expected that the effect of cognitive fatigue on judgments would be diminished when participants made judgments about statements that explicitly highlighted the outcome. In addition, with each permissibility judgment, an emotion-based judgment was included, because fatigue can affect emotion regulation (e.g., Baumeister, Vohs & Tice, 2007; Hofmann, Rauch & Gawronski, 2007; Johns, Inzlicht & Schmader, 2008) and judgments to moral dilemmas are closely tied to negative emotions (e.g., Kahane, Everett, Earp, Farias & Savulescu, 2015; Koenigs, Kruepke, Zeier & Newman, 2011; Manfrinati et al., 2013; Szekely & Miu, 2015; Tasso et al., 2017; Valdesolo &
DeSteno, 2006). The experiment is the first to examine the effect that limiting cognitive resources has on how people feel about their moral judgments and tests the idea that deliberative cognitive processing may impact emotional processes following moral judgments (e.g. Bucciarelli et al., 2008); it was expected that fatigued participants would feel worse about their judgments.

Participants in Experiment 1 were recruited online, and so the cognitively demanding task was adapted to be used online. Participants in the fatigue condition were asked to quickly re-type a paragraph before being asked to re-type another paragraph as fast as possible without using the letter “e” or the spacebar. Since all participants were computer users and the first phase habituated fast typing using all keys, these constraints required inhibiting largely automatic impulses. Participants in the non-fatigued condition typed both paragraphs as normal (from Muraven, Pogarsky & Shmueli, 2006). Again, mood was measured immediately following the cognitively demanding task, and participants rated the difficulty of the tasks at the end of the experiment.

Method

Participants. The participants were 196 volunteers who completed the experiment on two online platforms, CrowdFlower and Prolific Academic.1 A further 28 participants were eliminated prior to analysis because English was

1 At the time of testing, Prolific Academic had a much smaller participant pool. Only 30 participants were recruited from this platform and they were randomly distributed across the four groups. Moreover, the participants recruited from both platforms were from the same English-speaking countries with no expected differences between the samples and, as such, platform was not used as a factor in the analyses.

2 The counterfactuals were coded by 3 independent raters and inter-rater reliability was almost perfect for coding whether participants imagined a better or worse outcome, mean $\kappa = 0.89$;
not their first language \((n = 4)\), they had duplicate IP addresses \((n = 2)\), or they failed to carry out the instructions in the writing task to re-write the presented paragraph and not to type the letter “e” or use the spacebar key \((n = 22)\).

Sample size was initially calculated in the same way as the pilot study, that is, approximately 20 participants per cell (e.g., Hagger et al., 2010). However, following expert recommendations because of the concerns about effect sizes in the depletion literature, sample size was subsequently reset to approximately 50 participants per cell in line with recommendations by Simmons, Nelson and Simonsohn (2011); in fact the recalculated sample size made no difference to the results. There were 128 women and 63 men, 4 people who indicated their gender as something other than male or female and 1 who indicated a preference not to say. The average age was 33 years with a range from 18 to 69 years. Participation was restricted to a set of countries that speak English as a first language. Most of the participants were from the US \((n = 103)\), and the rest were from the UK \((n = 78)\), Ireland \((n = 8)\), Australia \((n = 4)\), New Zealand \((n = 2)\) and Canada \((n = 1)\). The experiment was programmed such that each participant had a .25 probability of being assigned to one of four groups: fatigued-action \((n = 51)\), fatigued-outcome \((n = 41)\), non-fatigued-action \((n = 46)\) and non-fatigued-outcome \((n = 58)\). Participants received a nominal payment in line with their platform norms: 25c (USD) on CrowdFlower and £1.50 (GBP) on Prolific Academic.

**Design and Materials.** The design was a 2 (condition: fatigue vs. no fatigue) x 2 (judgment: action vs. outcome) x 2 (dilemma: personal vs. impersonal) mixed-factorial design; condition and judgment were manipulated
between-groups and dilemma-type was the repeated measures factor.

Participants completed an online fatiguing task (adapted from Muraven et al., 2006, see Appendix 2.1). They were asked to re-type one 150-word paragraph taken from a statistics book as quickly as possible. Then they were asked to re-type a second paragraph. Participants in the fatigued group were told they were not to type the letter “e” or use the spacebar key, thus breaking a previously formed typing habit. Participants in the non-fatigued group were given no constraints. As noted above, 22 participants failed to follow the instructions and, based on an *a priori* decision, these participants were removed prior to any analyses. All remaining participants completed the typing task as per the instructions. After this task they completed the same mood scale as the pilot study.

Participants were presented with four of the moral dilemmas used in the pilot study (two personal and two impersonal) in randomised order. They judged how morally permissible the dilemma’s action was, on a scale from 1 “forbidden” to 7 “obligatory”. Half of the participants were given the judgment framed to highlight the action, for example “Killing the teenager in this case is morally…” and the other half were given the judgment framed to highlight the outcome, for example “Doing this in order to save yourself and the crowd of spectators is morally…” Participants were also asked “how bad would this decision make you feel?” They rated how they felt about their decision from 1 “not bad at all” to 7 “extremely bad”. As manipulation checks they also rated the difficulty of each of the tasks (all materials are available in Appendix 2.1, and the results from the mood and thinking style scales are in Appendix 2.2).
Procedure. The materials were presented using SurveyGizmo, presented on CrowdFlower or Prolific Academic to recruit participants. Each dilemma was presented on a single screen with the scale below it. The other tasks were presented on separate screens. The experiment took approximately 20 minutes to complete.

Results and Discussion

Manipulation Checks. Similar to the pilot study, the manipulation checks confirmed that participants in the fatigued conditions rated their typing task ($Mdn = 5, IQR = 5-6$) as more difficult than participants in the non-fatigued conditions ($Mdn = 3, IQR = 2-5$), Mann-Whitney $U = 2590.5, p < .001, r = .40$, and they also rated the moral judgment task as more difficult ($Mdn = 4, IQR = 3-6$) compared to the non-fatigued participants ($Mdn = 4, IQR = 1.25-5$), $U = 4004, p = .046, r = .14$. The manipulation checks suggest that the fatigue manipulation was indeed more cognitively effortful than the non-fatiguing task, and it may have had downstream effects on reasoning about the moral dilemmas since they were judged to be more difficult. Importantly, these results follow the pattern observed in the pilot study, albeit with smaller effects on both.

Moral Judgments. Participants tended to judge that it was morally permissible to act, about 4 on the scale, the same as the pilot study. To examine the influence of fatigue and judgment focus on moral judgments, a $2$ (fatigue, no fatigue) x $2$ (action, outcome) x $2$ (personal, impersonal) ANOVA with repeated measures on the last factor was conducted on moral
permissibility ratings. The results showed no main effect of fatigue, $F(1, 192) = 1.08, p = .299$, a main effect of judgment focus, $F(1, 192) = 9.61, p = .002$, $\eta_p^2 = .05$, as outcome focused judgments were judged more permissible than action focused ones, and a main effect of dilemma, $F(1, 192) = 5.75, p = .017$, $\eta_p^2 = .03$, as personal dilemmas were judged as less permissible than impersonal ones. Fatigue did not interact with either dilemma type, $F(1, 192) = 1.25, p = .266$, or judgment focus, $F < 1$, and dilemma did not interact with judgment focus, $F < 1$, but, importantly, there was a three-way interaction, $F(1, 192) = 13.64, p < .001$, $\eta_p^2 = .07$, as Figure 2.3 shows.

This three-way interaction was decomposed to test the hypothesised difference between fatigued and non-fatigued participants, with a Bonferroni-corrected alpha of .004 for the 12 comparisons. The decomposition showed that, as expected, the effects of fatigue were evident when the action was the focus of the question but not when the outcome was. Moreover, as was hypothesised for the pilot study, the effects of fatigue were more pronounced on impersonal dilemmas than personal ones.
Figure 2.3. Mean permissibility judgments to personal and impersonal dilemmas in Experiment 1. Error bars represent the standard error of the mean.

When the question highlighted the harmful action, fatigued and non-fatigued participants made similar judgments about personal dilemmas, $t(95) = 0.19$, $p = .847$, but fatigued participants judged impersonal dilemmas to be less permissible than non-fatigued participants did, and the effect was marginally significant on the Bonferroni-adjusted alpha, $t(95) = 2.74$, $p = .007$, $d = 0.56$.

When focus was directed to the outcome, there was no difference between fatigued and non-fatigued participants on personal dilemmas, $t(97) = 0.86$, $p = .392$, or impersonal dilemmas, $t(97) = 0.79$, $p = .434$.

Fatigued participants judged personal actions the same when the focus was on the action as the outcome, $t(90) = 1.52$, $p = .131$, as did non-fatigued participants, $t(102) = 2.57$, $p = .012$. Fatigued participants judged impersonal actions to be less permissible when the action was the focus compared to when
the outcome was, $t(90) = 3.73, p < .001, d = 0.79$, but non-fatigued participants judged impersonal actions the same in both cases, $t(102) = 0.12, p = .902$.

Non-fatigued participants also tended to make the well-documented judgment that the action was less permissible for personal dilemmas than impersonal ones, $t(45) = 3.99, p < .001, d = 0.59$, but fatigued participants showed no such effect, $t(50) = 1.13, p = .263$. When the outcome was the focus, there was no difference between personal and impersonal actions for fatigued participants, $t(40) = 2.04, p = .049$, or non-fatigued participants, $t(47) = 0.25, p = .802$.

Participants also rated how bad they felt about their moral judgments on a scale from 1 “not bad at all” to 7 “extremely bad”; on average, participants indicated that they felt bad about their judgments, at about 5.5 on the scale. An ANOVA of the same design as before on how participants felt about their judgments showed a main effect of fatigue, $F(1, 192) = 4.12, p = .044, \eta^2_p = .02$, as fatigued participants reported feeling worse about their judgments, and a main effect of dilemma type, $F(1, 192) = 4.74, p = .031, \eta^2_p = .02$, as participants felt worse about the dilemmas that described personal harms compared to impersonal harms. There was no effect of judgment focus, $F < 1$, and no interactions, all $Fs < 1$ except fatigue x dilemma $F(1, 192) = 1.44, p = .231$, as Figure 2.4 shows.
Emotion ratings were then analysed only for judgments that the action was permissible (4 to 7 on the scale). Sample sizes for this analysis are presented with the recalculated means and standard deviations in Table 2.1. An ANOVA of the same design as before on these emotion judgments showed no main effect of fatigue, $F (1, 125) = 1.26, p = .263$, the same main effect of dilemma, $F (1, 125) = 4.87, p = .029, \eta_p^2 = .04$, as participants felt worse about endorsing personal actions than impersonal ones, and no effect of focus, $F < 1$. There were no two- or three-way interactions, all $F$s < 1. Note that sample size was necessarily reduced for this analysis, as some participants had no judgments that were 4 or above on the scale, and so the results are interpreted with caution.
Table 2.1.

Means and Standard Deviations for Emotion Judgments to Actions Judged Permissible in Experiment 1.

<table>
<thead>
<tr>
<th>Action</th>
<th>Fatigue $(n = 27)$</th>
<th>No Fatigue $(n = 29)$</th>
<th>Fatigue $(n = 30)$</th>
<th>No Fatigue $(n = 43)$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Personal</td>
<td>5.74 (1.66)</td>
<td>5.24 (1.82)</td>
<td>5.73 (1.45)</td>
<td>5.62 (1.35)</td>
</tr>
<tr>
<td>Impersonal</td>
<td>5.54 (1.57)</td>
<td>5.21 (1.83)</td>
<td>5.50 (1.61)</td>
<td>5.24 (1.50)</td>
</tr>
</tbody>
</table>

**Summary.** The results show an effect of cognitive fatigue on judgments when actions are made salient, but not when outcomes are highlighted. This finding provides a more fine-grained analysis of the results from the pilot study. It was hypothesised that the effects of cognitive fatigue on moral judgments in the pilot study resulted from fatigued participants not constructing a model that linked the action to its beneficial outcome, or in other words they did not consider information about the outcome when making their judgments. Indeed, the effects of having completed a difficult writing task were evident only on action-focused judgments and were overcome when the outcome was made salient. Non-fatigued participants responded to action-focused questions in the same way as is typically found, they judged personal harms to be less permissible than impersonal harms, but fatigued participants judged both to be equally impermissible. Moreover, fatigued participants judged impersonal action-focused harms to be less permissible than non-fatigued participants did,
but there was no difference when the outcome was highlighted in the question. Note that there was no difference for personal harms, and this is likely because even non-fatigued participants typically judge these actions to be impermissible. The results corroborate the idea that participants who have engaged in a cognitively demanding task judge that the harmful action is impermissible because they tend not to construct a model of the events that explicitly links the harmful action to its beneficial outcome. When the action is deemphasized and their attention is directed to the outcome, they overcome this limitation.

The results from the emotion judgments showed that overall fatigued participants felt worse about the decisions they made, possibly suggesting that, in addition to facilitating more complex mental models of the dilemmas, effortful reasoning processes might also function to rationalise the judgments people make and hence regulate emotions. This finding supports the idea that cognitive evaluations can influence emotions (Bucciarelli et al., 2008), whereas most research to date has focused on how emotions impact judgments (e.g. Schnall et al., 2008; Strohminger et al., 2011; Valdesolo & DeSteno, 2006). However, this result is interpreted cautiously because the same result was not found when only emotion judgments to permissible judgments were analysed. This experiment showed that effortful cognition influences judgments about moral actions that people feel bad about, such as harming another person; the next experiment examines whether cognitive fatigue affects judgments about morally good actions.
Experiment 2

The aim of the experiment was to examine whether the effects of cognitive fatigue observed for judgments about moral violations extend to judgments about morally good deeds. People are morally elevated when they witness or learn about the noble or self-sacrificial actions of others, such as a man running in front of an oncoming train to save another man who has fallen in front of it (e.g., Algoe & Haidt, 2009; Freeman et al., 2009; Lai et al., 2014). Few studies have examined the cognitive processes that underlie reasoning about these self-sacrificial actions (for a review see Pohling & Diessner, 2016). This experiment tests the idea that when people make judgments about whether such morally elevating acts should be taken, they must also construct a model in which they link the self-sacrificial act to the beneficial outcome. Hence, it was hypothesised that the effect of fatigue on moral judgment will occur even when people reason about self-sacrificial morally good actions, but again that this effect would be overcome by prompting participants to consider the good outcome.

Participants in the fatigue conditions completed the same cognitively demanding task as the previous experiment and those in the no-fatigue conditions completed the easier task. All participants read stories in which someone did something good for someone else, for example they read a New York Times article about a man who ran in front of an oncoming train in order to save another man from being killed, and they judged how morally required this action was on the same scale as before (e.g. Lai et al., 2014). The questions participants were asked again highlighted either the action, for
example “…jumping in front of the train in this case…” or the beneficial outcome, for example “doing this to save Mr Hollopeter…” and it was hypothesised that cognitive fatigue would affect judgments focused on actions but not ones focused on outcomes.

**Method**

**Participants.** The participants were 187 volunteers who completed the study on the online platforms CrowdFlower and Prolific Academic. Prior to any data analysis, a further 6 participants were removed as English was not their first language and 19 were removed for failing to follow the instructions on the writing task. Sample size was calculated in the same way as the previous experiment. The participants were 115 women, 69 men and 3 participants who reported their gender as something other than male or female. Their average age was 35 years with a range from 18 to 72 years old. The participants were from the US \( (n = 101) \), the UK \( (n = 77) \), Australia \( (n = 4) \), Ireland \( (n = 2) \), New Zealand \( (n = 1) \), Canada \( (n = 1) \) and one American participant in Venezuela. They received the same nominal payments as the previous experiment. The experiment was again programmed such that each participant had a .25 probability of being assigned to one of four groups: fatigued-action \( (n = 46) \), fatigued-outcome \( (n = 46) \), non-fatigued-action \( (n = 49) \) and non-fatigued-outcome \( (n = 46) \).

**Materials, Design and Procedure.** The design and procedure were the same as the previous experiment. The fatigue task was the same as Experiment 1. The moral judgment materials were two newspaper articles in their original
form, as well as two modifications of them to create impersonal versions (see Appendix 2.1). Participants read one personal and one impersonal story, and they received one version of each of the stories (i.e., either Subway-Personal and Baseball-Impersonal or Subway-Impersonal and Baseball-Personal). The stories were presented in a different randomised order for each participant. Participants made the same moral judgments as the previous experiment using the same scale from 1 “forbidden” to 7 “obligatory”. They also judged how they felt about their decision in the same way as the previous experiment.

Participants completed several manipulation checks including the mood scale and difficulty ratings used in the previous experiments. They also completed an additional scale to measure how morally elevating the stories were, and this was constructed based on the review presented in Appendix 1.1. They were asked to indicate how much they experienced or were still experiencing the following emotions or thoughts while reading the story (on a 1-7 scale where 1 was “not at all” and 7 was “a lot”): (1) inspired, (2) there is still some good in the world, and (3) the person in the story has shown me how to be a better person. The results of mood and elevation scales are available in Appendix 2.2. They completed the tasks in the order: fatiguing task, mood scale, moral elevation scale, moral judgment task, difficulty ratings. All materials are available in Appendix 2.1.

Results and Discussion
Manipulation Checks. The same as Experiment 1, the manipulation checks confirmed that participants in the fatigued conditions rated their typing task ($Mdn = 5$, $IQR = 5-6$) as more difficult than participants in the non-fatigued conditions ($Mdn = 4$, $IQR = 2-5$), $U = 2538$, $p < .001$, $r = .54$, but this time they did not rate the moral judgment task as more difficult ($Mdn = 2$, $IQR = 1-3$) compared to the non-fatigued participants ($Mdn = 2$, $IQR = 1-3$), $U = 4034$, $p = .341$. The manipulation checks suggest that the fatigue manipulation was again more cognitively effortful than the control task, but there was no indication of downstream effects on metacognitive judgments of how difficult reasoning about self-sacrificial dilemmas was, unlike the moral violation dilemmas in the pilot study and Experiment 1.

Moral Judgments. Participants tended to judge that the act was morally required, at about 5 on the scale of 1 to 7. Although responses in this experiment were made on single-item Likert scales, parametric analyses were initially used to ease comparison between Experiment 1 and 2 and retained because the primary finding of interest required the combination of two Likert scales. To examine the influence of fatigue and judgment focus on moral judgments, a 2 (fatigue, no fatigue) x 2 (action, outcome) x 2 (personal, impersonal) ANOVA with repeated measures on the last factor was conducted on moral judgments. The results showed no main effect of fatigue, $F(1, 183) = 1.33$, $p = .250$, a main effect of judgment focus, $F(1, 183) = 26.72$, $p < .001$, $\eta_p^2 = .13$, as outcome focused judgments were judged to be more obligatory than action focused ones, and no main effect of dilemma, $F < 1$. Fatigue did not interact with dilemma type, $F(1, 183) = 3.08$, $p = .081$, but it did with
judgment focus, $F(1, 183) = 4.38, p = .038, \eta_p^2 = .02$. Dilemma did not interact with judgment focus, $F(1, 183) = 2.54, p = .113$, and there was no three-way interaction, $F(1, 193) = 1.49, p = .229$, as Figure 2.5 shows.

![Figure 2.5](image)

**Figure 2.5.** Mean permissibility judgments to stories in Experiment 2. Error bars are the standard error of the mean.

Although there was a three-way interaction in Experiment 1, dilemma type did not interact with fatigue and judgments focus in this experiment, and hence it may simply be the case that distinguishing between personal and impersonal actions is less important for good moral actions. Nonetheless, the interaction between fatigue and judgment focus lends support to the hypothesised effect. This interaction was decomposed with a Bonferroni-corrected alpha of .0125 for four comparisons and showed that fatigued participants tended to judge the action to be less obligatory for action-focused judgments compared to outcome focused ones, $t(90) = 5.50, p < .001, d = 1.16$, \ldots
but there was no such difference for non-fatigued participants, \( t(93) = 2.06, p = .043 \). Fatigued participants judged the action to be less obligatory than non-fatigued participants for action focused judgments, and the effect was marginal on the adjusted alpha, \( t(86.13) = 2.40, p = .018, d = 0.49 \), but there were no differences between the groups for outcome focused judgments, \( t(90) = 0.64, p = .525 \). The same as Experiment 1, the effects of fatigue were demonstrated on action-focused judgments but not outcome-focused ones.

![Figure 2.6](image)

*Figure 2.6.* Mean emotion judgments to stories in Experiment 2. Error bars are the standard error of the mean.

Participants indicated that they did not feel bad about their moral judgments, at an average of about 2 on the scale. An ANOVA of the same design as the previous one on the emotion ratings showed no main effect of fatigue, \( F < 1 \), or dilemma, \( F < 1 \), but a main effect of judgment focus, \( F(1, \)
183) = 5.23, $p = .023$, $\eta_p^2 = .03$, as people felt worse when they made judgments that focused on the action compared to judgments that focused on the outcome, as Figure 2.6 shows. There was no interaction between fatigue and dilemma, $F (1, 183) = 2.33, p = .129$, or judgment focus, $F < 1$. Dilemma did not interact with judgment focus, $F < 1$, and there was no three-way interaction, $F < 1$.

The pattern was the same when the analysis was re-run only on “obligatory” responses (4 to 7 on the scale). There was no effect of fatigue or dilemma, $Fs < 1$, the effect of focus was in the same direction as before, $F (1, 170) = 6.44, p = .012$, $\eta_p^2 = .04$, and there were no interactions, all $Fs < 1$ except fatigue x dilemma $F (1, 170) = 2.06, p = .154$, as Table 2.2 shows.

Table 2.2.

Means and Standard Deviations for Emotion Judgments to Actions Judged Permissible in Experiment 2.

<table>
<thead>
<tr>
<th>Action</th>
<th>Fatigue</th>
<th>No Fatigue</th>
<th>Fatigue</th>
<th>No Fatigue</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$(n = 41)$</td>
<td>$(n = 45)$</td>
<td>$(n = 45)$</td>
<td>$(n = 43)$</td>
</tr>
<tr>
<td>Personal</td>
<td>2.44 (1.42)</td>
<td>2.71 (1.64)</td>
<td>1.93 (1.18)</td>
<td>2.33 (1.48)</td>
</tr>
<tr>
<td>Impersonal</td>
<td>2.59 (1.48)</td>
<td>2.60 (1.78)</td>
<td>2.04 (1.41)</td>
<td>2.07 (1.20)</td>
</tr>
</tbody>
</table>

Summary. The results show an effect of fatigue for judgments about self-sacrificial actions: fatigued participants judged morally good actions to be less obligatory when the judgment focused on the self-sacrifice compared to
when it focused on the beneficial outcome, but there was no effect for non-fatigued participants. Similar to Experiment 1, the effects of cognitive fatigue were demonstrated when participants reasoned about action-focused questions, but were overcome when information about the outcome was made explicit.

There were some differences between the findings from Experiment 1 and 2. There was no effect of whether the action described was “personal” or “impersonal”, but this distinction had not yet been tested for self-sacrificial actions and was used in this experiment in order to limit the differences between Experiments 1 and 2. The results suggest that the nature of the action may be less important for good moral actions compared to moral violations, but the general finding of an effect of cognitive fatigue on reasoning about moral actions remained essentially the same. Another difference was that there was no indication of an effect of fatigue on emotion judgments for self-sacrificial actions, and one possible explanation is that judgments about these good actions may require less justification in order for participants to evaluate them positively.

The main result from this experiment, that fatigue affects reasoning about actions unless outcomes are emphasised, corroborates the idea that participants who have engaged in cognitively demanding tasks construct a model of the events that does not explicitly consider the action in light of its outcome, whether it is a morally good self-sacrificial action or a moral violation. Fatigued participants overcome this limitation when their attention is drawn to the outcome. The effects of fatigue reported in the experiments thus far have resulted from an experimental task; the next experiment examines
whether the effect of cognitive fatigue on moral judgments is observed following an everyday situation that requires cognitive effort, such as an evening lecture that requires extensive sustained attention.

**Experiment 3**

The aim of the experiment was to examine moral reasoning in participants who experienced cognitive fatigue after an ordinary everyday event (see Inzlicht, 2016). The participants in this experiment were enrolled in an evening statistics module for which lectures occurred twice per week for two semesters. They were tested during the first part of the second semester. All participants were tested in the evening to rule out the possibility of time-of-day effects (e.g. Kouchaki & Smith, 2014). Some students were tested at the end of a break period after the first part of the two-hour lecture, and the rest were tested at the end of another lecture before they had taken a break (note that both lectures involved different students). It was expected that students would be cognitively fatigued after the lecture because of the sustained allocation of attention that it required, but students tested after just completing one hour and having taken a break would be less fatigued (Baumeister et al., 2007; Fischer, Langner, Birbaumer & Brocke, 2008; Hagger et al., 2010; Schmeichel & Vohs, 2009). Participants made judgments about the same sort of moral violation dilemmas as the pilot study and Experiment 1.

**Method**
Participants. The participants were 62 students at Trinity College Dublin who were attending an evening postgraduate certificate course in statistics. They were 35 women and 25 men (and 2 did not disclose their gender), aged 22 to 71 years old with an average age of 32 years. Because the moral judgment questions were the same as the pilot study, sample size was initially calculated in the same way, but to allow for a potentially smaller effect outside of laboratory conditions it was increased to approximately 30 per group. Participants were offered the opportunity to enter a draw for a €50 shopping voucher in return for participating, and 38 participants opted to enter the draw. Participants in the fatigue group \((n = 33)\) took part immediately after completing a 2-hour statistics lecture, which had a five-minute break after the first hour, and participants in the non-fatigued group \((n = 29)\) took part after completing the first hour of a two-hour lecture, after a break between the first part of the lecture and the second.

Design and Materials. The design was a 2 (condition: fatigue vs. no fatigue) \(\times\) 2 (dilemma: personal, impersonal) mixed-factorial design; condition was manipulated between-groups and dilemma-type was the repeated measures factor, the same as the pilot study. As a manipulation check, participants completed a short, 4-item questionnaire. The items asked were:

1. Thinking back on today, how many times did you make a decision that involved exerting willpower?

\[
\begin{array}{ccccccc}
1 & 2 & 3 & 4 & 5 & 6 & 7 \\
None at all today & & & & & & A great many decisions today
\end{array}
\]

2. How much of the previous lecture did you pay attention to?

\[
\begin{array}{ccccccc}
1 & 2 & 3 & 4 & 5 & 6 & 7 \\
\end{array}
\]
And the last question was taken as the key indicator of fatigue.

After they completed the questionnaire, participants were told the experiment was ready to begin. They were presented with four dilemmas (two personal and two impersonal; see Appendix 2.1) in random order. The content was again counterbalanced across the experiment and no participant received two versions of the same content. The task was the same as the pilot experiment: participants were asked to judge how morally permissible committing the action in the dilemma was in order to achieve the outcome, for example, “Killing the teenager in order to save yourself and the crowd of spectators is morally…” and they judged how permissible this was from 1 “forbidden” to 7 “obligatory”. Participants were also asked how they felt about each decision and they gave their responses on a 7-point scale from 1 “not bad at all” to 7 “extremely bad”. On the final page, they were asked how difficult they found making judgments to the four dilemmas on a scale from 1 “not at all” to 7 “extremely”. All materials are available in Appendix 2.1.

**Procedure.** The materials were presented in booklets and participants completed them in the lecture theatre at their own pace. They completed the manipulation check measures first, then the moral dilemmas, and for each
moral dilemma they first made a permissibility judgment and then an emotion judgment. Lastly, they rated how difficult they found making moral judgments. The experiment took about 10 minutes.

Results and Discussion

Manipulation Checks. The manipulation checks confirmed that participants in the fatigued group ($Mdn = 5, IQR = 4-6$) reported feeling more tired than those in the non-fatigued group ($Mdn = 4, IQR = 4-5$), $U = 327, p = .041, r = .26$, but the size of the effect was much smaller compared to the previous experiments ($rs = .85, .40, .54$, respectively). Unlike Experiments 1 and 2, there was no difference in how difficult the fatigued participants rated making moral judgments ($Mdn = 5, IQR = 4-6.5$) compared to non-fatigued participants ($Mdn = 6, IQR =3.5-6.5$), $U = 477, p = .983$. The fatigued and non-fatigued groups reported similar levels of attention paid during the lecture, ($Mdn = 4, IQR = 3-5; Mdn = 5, IQR = 3.25-5.75$, respectively), $U = 384.5, p = .246$, and similar levels of difficulty paying attention ($Mdn = 4, IQR = 3-6; Mdn = 4, IQR = 3.25-5$, respectively), $U = 381, p = .229$, suggesting similar levels of baseline fatigue before attending the lecture. (Note that one participant in the non-fatigued group did not complete the attention and mental tiredness questions, and so was excluded from these analyses).

Moral Judgments. The participants judged the action to be permissible, at about 4 on the scale from 1 “forbidden” to 7 “obligatory”, which is the same as the pilot study and Experiment 1. A 2 (fatigue, no fatigue) x 2 (personal, impersonal) ANOVA with repeated measures on the second factor was
conducted on permissibility judgments. Unlike the pilot study, there was no main effect of fatigue, $F(1, 60) = 1.96, p = .167$. There was also no main effect of dilemma, $F(1, 60) = 3.30, p = .074$. The two variables did not interact, $F < 1$, as Figure 2.7 shows.

![Figure 2.7](image)

**Figure 2.7.** Mean permissibility judgments to personal and impersonal dilemmas in Experiment 3. Error bars are the standard error of the mean.

An ANOVA of the same design was conducted on judgments of how badly each moral decision made participants feel. Similar to Experiment 1, there was a main effect of fatigue, $F(1, 60) = 5.44, p = .023, \eta_p^2 = .08$, as fatigued participants felt worse about their decisions than non-fatigued participants. Unlike Experiment 1, there was no effect of dilemma, $F(1, 60) =
2.82, \( p = .098 \), but the two variables interacted, \( F(1, 60) = 10.05, p = .002, \eta_p^2 = .14 \), as Figure 2.8 shows.

Contrasts to decompose the interaction with a Bonferroni-adjusted alpha of .0125 showed that non-fatigued participants felt worse about personal dilemmas than impersonal ones, \( t(28) = 2.93, p = .007, d = 1.11 \), but fatigued participants showed no such difference and felt equally bad about their decisions to personal and impersonal dilemmas, \( t(32) = -1.26, p = .217 \).

Fatigued and non-fatigued participants felt equally bad about personal dilemmas, \( t(60) = 0.44, p = .664 \), but fatigued participants felt worse than non-fatigued participants about impersonal dilemmas, \( t(45.68) = 3.60, p = .001, d = 0.90 \) (on Welch’s t-test because the data failed Levene’s test for homogeneity of variance, \( F = 7.49, p = .008 \)).
Figure 2.8. Mean emotion judgments to personal and impersonal dilemmas in Experiment 3. Error bars are the standard error of the mean.

Similar to the previous analyses of emotion judgments, the same analysis was then conducted for only “utilitarian” responses (permissibility judgments of 4 to 7 on the scale). A 2 (fatigue, non-fatigue) x 2 (personal, impersonal) ANOVA on these responses showed that there was no main effect of fatigue, $F(1, 42) = 1.16, p = .289$, and no main effect of dilemma, $F < 1$, but the same interaction was found, $F(1, 42) = 4.70, p = .036, \eta_p^2 = .02$, as Table 2.3 shows.

Table 2.3.

<table>
<thead>
<tr>
<th></th>
<th>Fatigue</th>
<th>No Fatigue</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$(n = 22)$</td>
<td>$(n = 22)$</td>
</tr>
<tr>
<td>Personal</td>
<td>6.11 (1.34)</td>
<td>5.77 (1.54)</td>
</tr>
<tr>
<td>Impersonal</td>
<td>6.07 (1.17)</td>
<td>5.07 (1.53)</td>
</tr>
</tbody>
</table>

Decomposing this interaction with a Bonferroni-corrected alpha of .0125 for 4 comparisons showed that fatigued participants felt worse about endorsing impersonal harms than non-fatigued participants, and the effect was marginal on the adjusted alpha, $t(44) = 2.49, p = .017, d = 0.75$, but fatigued participants reported feeling as bad as non-fatigued ones about personal harms, $t(58) = 0.92, p = .363$. Fatigued participants felt equally bad about endorsing
personal and impersonal harms, $t(21) = 0.89, p = .386$, and there was no
difference for non-fatigued participants on only the utilitarian responses, $t(21) = 2.17, p = .042, d = .46$. Note again that sample size was necessarily reduced
for this analysis because some participants made no judgments about 4 on the
permissibility scale.

**Summary.** The results show no effect of fatigue on participants’ moral
d Judgments, but fatigue affected emotion judgments to the dilemmas.

Participants who completed an everyday cognitively demanding task, attending
a two hour evening statistics lecture, subsequently judged harmful actions to be
as permissible as participants who completed a non-fatiguing — or at least a
less cognitively demanding — task, attending one hour of the evening statistics
lecture. However, the results show an effect of fatigue on emotion judgments
as the fatigued participants reported that they felt very bad about their
judgments, and they failed to discriminate between personal and impersonal
dilemmas in the way that non-fatigued participants did. The fatigued
participants felt worse than the non-fatigued participants about their decisions
for impersonal dilemmas when they endorsed causing harm. There was no
effect on personal dilemmas, possibly due to a ceiling effect as even non-
fatigued participants reported feeling extremely bad about these dilemmas.

These results echo the findings on action-focused permissibility judgments in
Experiment 1.

One potential issue with this experiment is that the difference between
fatigued and non-fatigued participants was not as great as in the previous
experiments; the difference in cognitive demand between one hour and two
hours of a statistics lecture might not have been as great as the difference between completing the experimental fatigue tasks and the easier tasks used previously. However, the manipulation checks showed that there was indeed a difference in how fatigued the participants felt, albeit a smaller one compared to the previous experiments. To consider the issue of everyday effects of fatigue on moral judgments from another angle, the next experiment examines whether there are comparable effects of fatigue on violations that involve less extreme moral values, such as conservationism.

**Experiment 4**

The aim of Experiment 4 was to examine the effects of fatigue when participants made moral judgments about the sacrifice of less extreme moral values, rather than life-and-death trolley-type dilemmas, in order to test whether effortful cognitive processes are required for these sorts of dilemmas (see Bauman, McGraw, Bartels & Warren, 2014; Kahane & Shackel, 2010). The participants completed the same fatiguing task as the pilot study, because the effect of this manipulation was larger compared to the other fatigue manipulations, and were then presented with moral dilemmas of the following sort (from Ritov & Baron, 1999):

A logging company has the rights to 1,000 square miles of old-growth forest, home to many species of endangered plants and animals. The company is willing to trade this land for 100 square miles of similar land, part of a national park and home to some (different) species of endangered plants and animals. You can give this smaller area to the company and make the
larger area into a national park. The logging company will destroy all trees (and inhabitants) in whichever area it owns.

Participants were asked whether they would carry out the action in the question,

In this scenario, you were asked if you would trade 100 square miles of land (which would be consequently destroyed) in order to save the 1000 square miles of land. Would you trade the smaller section of land for the larger section?

and they were asked how bad they would feel about making such a trade, in the same way as the previous experiments. Participants were also asked for the maximum sacrifice they would make of the value at risk, for example “What is the largest area of land you would trade the logging company for the 1,000 square miles of forest it currently owns?” — but because this question asked for a numerical response and hence was different to the questions asked in the other experiments, its results are reported in Appendix 2.2.

The dilemmas are based on a moral value, for example animal conservation, which may be “protected” for an individual, that is, a value that the individual considers to be morally sacred (e.g., Baron & Spranca, 1997; Tanner & Medin 2004). “Protected moral value” dilemmas can be considered to be a less severe form of the life-and-death harm dilemmas used in the previous experiments because they pit acting to cause the loss of some smaller amount of a moral value against allowing the loss of a larger amount. In other words, participants are required to judge whether the utilitarian action that maximises the overall outcome is permissible to perform. People typically do
not endorse sacrificing moral values they consider to be protected, at least when the question is posed in a way that highlights the sacrifice and they judge sacrificing “unprotected” values to be more permissible (e.g., Ritov & Baron, 1999; Bartels & Medin, 2007; Sachdeva & Medin, 2008). In order to ensure that at least some values were unprotected in this experiment, four ostensibly non-moral dilemmas were included that involved, for example, a trade-off between maintaining 100 sporting grounds and maintaining 1000.

The hypothesis was that participants with fatigued cognitive resources would be less willing to sacrifice a protected value, even though such an action would lead to an overall better outcome, in the same way that fatigued participants are less willing to sacrifice one person in order to save many people compared to non-fatigued participants, as Experiment 1 showed. It was also hypothesised that they would feel worse about sacrificing a protected value compared to non-fatigued participants, in the same way that participants felt worse about sacrificing one person in Experiment 3. (The same as the pilot study, participants’ reaction time to respond to the dilemma question was also measured, but for brevity the results are reported in Appendix 2.2).

Method

Participants. The participants were 40 students from Trinity College Dublin who received course credits for taking part. There were 31 women and 9 men aged 18 to 47 years, with an average age of 24 years. The same fatiguing task was used as the pilot study, and so sample size was determined in the same way. Participants were assigned at random in the same way as the
pilot study to the fatigued condition \((n = 20)\) or the non-fatigued condition \((n = 20)\).

**Design and Materials.** The design was a 2 (condition: fatigued vs. non-fatigued) x 2 (status: protected vs. non-protected) mixed-factorial one; condition was manipulated between-groups and status was the repeated measures factor. Participants firstly completed the same writing task as the pilot study: in the fatigue condition, participants had to write about a trip they had taken without using words with the letters “a” or “n” and in the non-fatigued condition there were no constraints. After this, they completed the same mood scale as before.

Participants were then presented with four moral dilemmas about realistic everyday moral values (animal conservation, medical treatment funds, racism and famine-relief) and four non-moral contents (about making noise, farming equipment, fixing roads and repairing sports pitches; see Appendix 2.1). As a materials check, whether these dilemmas were based on “protected moral values” was assessed. For each item, participants were asked to choose one of the following four options (from Baron & Spranca, 1997; Bartels, 2008):

*The loss of endangered animal species*

1. I don’t care about this.
2. This is acceptable, if the benefits are great enough.
3. I can imagine situations in which the benefits are great enough, nonetheless I consider this to be unacceptable.
4. I cannot imagine any case in which this is acceptable.

Participants who chose options 3 or 4 were scored as having a “protected” value for that item, and those who chose 1 or 2 were scored as
“unprotected”. To ensure that they thought carefully about each issue, participants were first asked to think of acceptable counterexamples (from Baron & Leshner, 2000):

For many of the items, people say: "This is not acceptable, no matter how great the benefits." Possibly, in some of these cases, people could imagine potential benefits great enough to justify the action if they tried to do so. For each of the following items, please try to imagine and write (briefly) some benefit that would make this item acceptable. If you can imagine several answers, write the one that is most realistic. If you simply cannot imagine any answer, say so.

Each item was then presented within a dilemma. In each dilemma, a large amount of the value was at risk (e.g. 1000m$^2$ of conservation land) and participants were asked whether they would sacrifice a smaller amount (e.g. 100m$^2$ of conservation land) in order to save the former. (Latencies to respond to this question and the question about the maximum sacrifice that they would make are reported in Appendix 2.2). Lastly, they were asked how bad their decision would make them feel from 1 “not bad at all” to 7 “extremely bad”. When finished with the moral dilemmas, participants completed the Rational-Experientialism Inventory as a measure of thinking style, and then rated the difficulty of the preceding tasks from 1 “not at all difficult” to 7 “extremely difficult” (see Appendix 2.1). Again, the Rational-Experientialism Inventory was used as a check for cognitive style.

**Procedure.** The procedure was the same as the pilot study. Participants were tested individually in a quiet room. They completed the writing task,
mood scale and difficulty ratings using pen-and-paper. The protected values assessment, dilemmas and thinking style tasks were presented using a program written in SuperLab5.0 on a MacBook Pro. The experiment lasted about 40 minutes.

Results and Discussion

**Manipulation Checks.** Consistent with the previous experiments, the writing task in the fatigue condition was rated as more difficult ($Mdn = 6, IQR = 6-6$) than the control writing task ($Mdn = 2, IQR = 1-2.75$), $U = 7.50, p < .001, r = .84$, suggesting that the fatigue manipulation was effective. Unlike the pilot study and Experiment 1 but the same as Experiments 2 and 3, fatigued participants did not find making judgments about the moral dilemmas to be more difficult ($Mdn = 5, IQR = 5-6$) compared to non-fatigued participants ($Mdn = 5, IQR = 4-6$), $U = 198.00, p = .955$ (nor did they find generating counterexamples to be more difficult, ($Mdn = 4, IQR = 3-4.75; Mdn = 4, IQR = 3-5$, respectively), $U = 158.50, p = .247$).

As a materials check, a 2 (fatigue, no fatigue) x 2 (moral content, non-moral content) ANOVA on the proportion of items judged to be “protected” showed that participants tended to judge the moral contents, such as conservationism, as protected (56%) whereas they judged the non-moral contents, such as maintaining sporting grounds, as unprotected (92%), $F (1, 38) = 147.90, p < .001, \eta_p^2 = .80$. There was no effect of fatigue, $F < 1$, or interaction between fatigue and content, $F (1, 38) = 2.56, p = .118$. Subsequent
analyses were conducted using whether the value was protected or unprotected as the repeated measures factor.

![Figure 2.9](image)

**Figure 2.9.** Mean proportion of sacrificial actions endorsed by participants in Experiment 4. Error bars are the standard error of the mean.

**Moral Judgments.** The first judgment participants had to make to the dilemmas was whether they would endorse sacrificing a smaller amount of the value in question in order to save a larger amount. In general, participants endorsed committing the action about 85% of the time. A 2 (fatigue, no fatigue) x 2 (protected, non-protected) ANOVA with repeated measures on the second factor on the proportion of “yes” responses showed no main effect of fatigue, $F(1, 37) = 1.20, p = .281$, and, unlike the previous literature, there was no effect of protected status, $F(1, 37) = 1.27, p = .267$, and the two factors did not interact, $F < 1$, as Figure 2.9 shows. (Note that one participant in the
fatigued group rated no value as “protected” and so was excluded from analyses.)

Participants also rated how their decision made them feel from 1 “not bad at all” to 7 “extremely bad”. In general, they rated that they would feel quite bad about trade-offs, approximately 4 on the scale. An ANOVA of the same design as before showed that fatigued participants felt worse about their decisions than non-fatigued participants, \( F(1, 37) = 15.52, p < .001, \eta^2_p = .30 \), and participants felt worse about judgments that involved protected values compared to unprotected ones, \( F(1, 37) = 46.16, p < .001, \eta^2_p = .56 \), as Figure 2.10 shows. The two factors did not interact, \( F < 1 \).

![Figure 2.10. Mean emotion judgments to dilemmas in Experiment 4. Error bars are the standard error of the mean.](image-url)
The same analysis only on affect ratings in response to endorsing the trade-off showed the same pattern: fatigued participants felt worse about endorsing both protected and non-protected trade-offs compared to non-fatigued participants, \( F(1, 35) = 10.66, p = .002, \eta_p^2 = .23 \), and participants felt worse about sacrificing protected compared to non-protected values, \( F(1, 35) = 25.88, p < .001, \eta_p^2 = .43 \). The two factors did not interact, \( F < 1 \), as Table 2.4 shows. (Note that a further two participants were excluded from this analysis because they did not endorse the sacrifice for any protected values.)

Table 2.4.

*Means and Standard Deviations for Emotion Judgments to Actions Judged Permissible in Experiment 4.*

<table>
<thead>
<tr>
<th></th>
<th>Fatigue ((n = 18))</th>
<th>No Fatigue ((n = 19))</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Protected</strong></td>
<td>5.19 (0.99)</td>
<td>4.10 (1.33)</td>
</tr>
<tr>
<td><strong>Unprotected</strong></td>
<td>4.04 (1.21)</td>
<td>3.12 (0.93)</td>
</tr>
</tbody>
</table>

**Summary.** The results show no effect of fatigue on judgments about everyday moral values, but fatigue affected emotion judgments to the dilemmas, similar to Experiment 3. Participants who completed a difficult writing task endorsed trading a small amount of a protected value in order to save a larger amount as often as participants who completed an easier writing
task. Unlike previous literature, whether the value was protected or not had no effect on decisions (cf. Baron & Spranca, 1997), but participants reported feeling worse about endorsing the utilitarian action for protected values compared to values that were not protected. Moreover, fatigued participants reported feeling worse about endorsing utilitarian judgments compared to non-fatigued participants.

This finding that fatigued participants felt worse about endorsing the sacrifice of a moral value in order to protect a larger amount of that value mirrors the finding from Experiment 3 that fatigued participants felt worse about endorsing the sacrifice of one person in order to save a larger group of people. These findings complement recent research that implicates cognitive processes in the emotions people predict they will experience following moral decisions (e.g. Tasso et al., 2017) and are the first to suggest a role for effortful cognition in regulating emotions following moral judgments. This nuanced role for effortful cognitive processing suggests that it has a greater influence in moral reasoning than it is afforded in dominant theories, and the next section considers this contribution in more detail.

**General Discussion**

The experiments in this chapter employed for the first time a sequential task design to examine the impact of reduced resources for effortful cognition on moral reasoning. The research question addressed in this chapter was whether completing a cognitively demanding task affects subsequent moral judgments. The general hypothesis was that participants would be less likely to
make utilitarian judgments that harm is permissible in order to achieve a greater overall outcome after completing a cognitively demanding task, compared to participants who completed easier initial tasks. In this section, the results are discussed and their implications for the debate on the role of effortful cognition in utilitarian moral judgment are elaborated.

Participants in the pilot study responded to questions that mentioned both the action and the outcome, for example “killing the teenager in order to save the crowd of spectators”. The results showed for the first time that, after a cognitively demanding task, participants judged moral violations in order to achieve good outcomes to be less permissible than participants who initially completed a less demanding task. In Experiment 1, the questions highlighted either the action, for example “killing the teenager in this case” or deemphasised the action and highlighted the outcome, for example “doing this in order to save the crowd of spectators”. Fatigued participants judged moral violations to be less permissible for action-focused questions compared to non-fatigued participants, but this difference was eliminated for outcome-focused questions.

In addition to being the first experiments to examine cognitive fatigue and moral judgments using a sequential task design, the results from the pilot study and Experiment 1 provide novel evidence that helps to explain the impact that disrupting cognitive resources has on moral reasoning. Increasing cognitive load by engaging inhibition processes has previously been found to decrease the likelihood of utilitarian judgments (Conway & Gawronski, 2013; Trémolière et al., 2012; Trémolière & Bonnefon, 2014). The pilot study
showed that the same effect occurs after participants complete a cognitively demanding task, and Experiment 1 made the unique contribution of showing that fatiguing cognitive resources affects reasoning about committing moral violations, but this effect is offset if reasoners are explicitly prompted to consider the good outcomes those actions will have. Theoretically, the results support the idea that, when reasoning about moral dilemmas, people may rely on simple models that focus on the action or they may construct more complex models that link the action to its outcome. The effect of cognitive fatigue demonstrated in Experiment 1 shows that reasoning about actions in light of their outcomes requires more cognitive resources than making judgments based on a model of just the action (Crockett, 2013; Cushman, 2013).

Moreover, Experiment 2 demonstrated that similar effects are evidenced for reasoning about morally good acts when cognitive processes are taxed. Non-fatigued participants judged self-sacrificial actions the same when they were prompted to consider the self-sacrificial nature of the action compared to when they were prompted to consider the good outcome; in both cases they tended to judge that it was morally obligatory to act. When fatigued participants were prompted to consider the self-sacrificial nature of the act, for example “jumping in front of the train”, they judged acting to be less morally required compared to non-fatigued participants. Similar to Experiment 1, when fatigued participants were prompted to explicitly consider the outcomes of acting, they judged the actions the same as non-fatigued participants. The results again corroborate the idea that mental representations that link moral
actions to their outcomes may be cognitively demanding to spontaneously generate.

The results from Experiment 2 provide the unique contribution of examining the impact of cognitive fatigue on morally good actions. Little research to date has examined how people reason about self-sacrificial actions (see Pohling & Diessner, 2016), but the results from Experiment 2 are consistent with the idea that cognitive resources are required in order to spontaneously link morally good actions to their outcomes and, failing that, people are less likely to judge such acts to be morally required. Novel predictions were derived from theories that have largely been concerned with moral violations (e.g. Crockett, 2013; Cushman, 2013), and the experiment shows that theories of morality that highlight the importance of representations of actions and action-outcome links are beneficial for explaining not only moral harm dilemmas but also situations that focus on positive morality. The experiment provides a fruitful starting point for research on the cognitive processes that underlie how people reason about morally noble deeds (and this idea is pursued in Chapters 3 and 4).

The results from Experiments 3 and 4, however, suggest that the relationship between cognitive effort and moral judgment is more complex than has been previously detailed. Experiment 3 showed that, following an everyday cognitively demanding activity, moral judgments are unaffected but fatigued people feel worse about judging a moral violation to be permissible. Similarly, Experiment 4 showed that, when the dilemmas are about less severe outcomes, for example about harm to some value other than human life,
judgments are unaffected but fatigued people feel worse about judging that a small amount of some value should be sacrificed in order to save a larger amount. This nuanced interplay between cognitive processes and emotional experiences for moral judgments provides a novel contribution to the literature, and supports the idea that, in addition to emotions impacting cognitive processing, cognitive processing can affect emotions people have about moral dilemmas (e.g. Bucciarelli & Danielle, 2015; Bucciarelli et al., 2008). Together, the results from the experiments presented in this chapter make some original contributions to the understanding of the role of effortful cognition in moral reasoning.

**Implications: Effortful Cognition in Moral Reasoning.** Effortful cognition has well-established links with utilitarian judgments that moral violations are permissible if they have net beneficial outcomes (e.g. Bartels, 2008; Conway & Gawronski, 2013; Greene et al., 2001; Moore et al., 2008). This body of research sufficiently rules out the idea that moral judgments rely solely on pre-conscious intuitions (e.g. Haidt, 2001), and so this section instead focuses on differentiating between two competing, dual-process explanations. The first “default-interventionist” explanation is that moral judgments depend on two competing systems, whereby one system is immediate and emotional and promotes adherence to rules against moral violations in all cases, but can sometimes be overturned by a slower, more deliberative system that is focused on consequences, and this explanation has dominated moral psychology research (e.g. Greene et al., 2004; Paxton et al., 2012). The second “model-based” explanation is that utilitarian judgments require constructing a complex
model that links actions to their outcomes, and doing so requires more cognitive effort than relying on a simple model that focuses on the action (e.g. Crockett, 2013; Cushman, 2013).

The results presented in this chapter are not entirely compatible with predictions from the default-interventionist account. Primarily, this model would predict that cognitive fatigue would lead to a decrease in utilitarian judgments, which was indeed observed, but the finding that this deficit could be overcome without restoring cognitive resources poses some difficulty for the theory; it was sufficient to prompt a consideration of the outcomes, as Experiments 1 and 2 showed. Hence, the results suggest that mentally representing both the action and the outcome in the dilemma are important for judgments, and that cognitive resources are required in order to spontaneously judge aversive actions in light of their good outcomes. Although these results could be reconciled with the default-interventionist model, it is insufficiently detailed to predict these findings. It is argued here that the model-based account holds more specific predictive power. The results from this chapter support the idea that controlled reasoning is responsible for model-based simulations of actions and their relations to outcomes and that the simulations that incorporate outcome information lead to utilitarian judgments (e.g. Crockett, 2013; Cushman, 2013; see also Johnson-Laird & Byrne, 2002; Sloman & Lagnado, 2015).

The results from Experiments 3 and 4 also point to a role for effortful cognition in post-decisional emotions. Whereas emotions have been argued to be a central component of moral decision-making, much of the research has
focused on the effects that emotions have on judgments (e.g. Strohminger et al., 2011). Comparatively few studies have examined how cognitive processes might affect the emotions that people experience following moral judgments (but see Gubbins & Byrne, 2014; Szekely & Miu, 2015). Recent research, however, has shown that thinking about the counterfactual alternatives after endorsing the deontological option to a personal dilemma (that is, thinking about having chosen the utilitarian option) amplifies negative emotions. This finding was proposed to suggest that deontological options are chosen in order to limit anticipated negative affect from the utilitarian choice (Tasso et al., 2017).

Although this suggestion was argued to support the default-interventionist account, it is also compatible with the model-based dual-process account, because it is suggested that reasoners engage in cognitive processes to contemplate different models of the choices they face. The results from this chapter complement this view. Experiments 3 and 4 showed that cognitive fatigue increased negative affect, or prevented it from being regulated, but did not impact judgments, and one possibility is that participants did not anticipate the negative affect they would experience by considering different models of the dilemmas. Moreover the results from Experiment 4 showed that fatigue affected how participants felt about non-moral dilemmas as well as moral ones, supporting the idea that domain-general processes are applied to moral contents rather than moral reasoning relying solely on domain-specific faculties. Proposed mechanisms that may underlie this increase in negative affect are discussed in the Effortful Cognition section of Chapter 5.
Conclusion. The results show a novel cognitive fatigue effect on moral judgments, and provide a more fine-grained analysis of the effect of reduced cognitive resources on moral judgments than has been demonstrated in previous experiments: effortful cognitive processes are required in order to reason about moral actions in light of their outcomes. The results also make the novel contribution of demonstrating a similar mechanism applying to reasoning about moral violations as reasoning about good moral actions. The idea that judgments about good moral actions may rely on some of the same cognitive processes as judgments about moral violations is pursued in Chapter 3.
Chapter 3: Reasoning about Self-Sacrificial Actions

In July 2017, two young boys were playing in the sea when they were caught by a riptide and began to struggle. Their family tried to help but were caught by the riptide, too. There were no lifeguards on duty at the time. In response, fellow beachgoers formed a human chain that stretched from the shore to where the family were struggling, allowing each family member to be passed along to the beach where paramedics were waiting. In the end, the family survived and were relatively unharmed (Luscombe, 2017). When people read a news article about this story they responded favourably to the beachgoers, as people tend to do when they witness or learn about the morally good actions of others (e.g. Algoe & Haidt, 2009). It is well established that people feel uplifted and inspired by moral excellence and are often motivated to act in morally good ways themselves (e.g. Cox, 2010; Diessner et al., 2013; Freeman et al., 2009; Schnall & Roper, 2012). Less is known, however, about the cognitive factors that are important for peoples’ positive judgments of such self-sacrificial acts. For example, does it matter that the family survived the rescue attempt in order for readers to have judged that the beachgoers should have tried to save them? One aim of the experiments in this chapter was to examine whether outcome knowledge influences peoples’ judgments of morally good actions, or in other words whether there is a hindsight effect for morally good actions.
Outcome Knowledge

The literature on moral violations shows that peoples’ judgments are affected by the outcomes of those actions. For example, judgments of negligent behaviour are often influenced by its consequences (e.g. Gino, Shu & Bazerman, 2010; Mazzocco, Alicke & Davis, 2004). When a doctor chooses to prescribe a patient a less effortful remedy rather than a more thorough but time-consuming one, people judge the doctor more harshly if the patient suffers negative side-effects than if the remedy is successful (Gino et al., 2010; see also Baron & Hershey, 1988).

Although the intended outcome of negligent behaviour may be ambiguous, for example people may think that the negligent doctor believed that the less effortful remedy would be successful, similar results are found when intentions are clear, for example with accidental bad outcomes (e.g. Cushman, Dreber, Wang & Costa, 2009). When Cynthia drives through a leaf pile at the side of the road, most people judge that she should be punished if they learn that two children who were hiding in the leaves were killed, but if there are no children in the leaf pile most people judge that Cynthia deserves no punishment (Martin & Cushman, 2016). Moreover, when the outcome is intended to be bad but instead is not, people judge that agents should be blamed and punished less. For example, people judge a teenager who threw bricks from an overpass attempting to hit traffic passing underneath more harshly when he is successful and causes injury to a driver than when he fails and causes no injury (e.g. Lench et al., 2015).
One possible explanation for the effect of outcomes on moral judgments is that an emotional reaction to the outcome influences judgments, because emotions are proposed to drive moral judgments in many cases (e.g. Greene et al., 2001). Put another way, bad outcomes may lead people to experience a negative emotional reaction that overshadows any consideration of the agent’s intention. However such an explanation is limited to unintended bad outcomes, such as accidental harms, and the outcomes of moral violations, such as throwing bricks from an overpass to try to cause harm. One aim of the experiments in this chapter was to examine whether outcome effects occur for good actions that lead to the intended, good outcome and good actions that lead to an unintended, bad outcome.

Another possible explanation for the effects of outcomes is that knowledge about the outcome leads people to re-assess the agent’s action and judge whether they acted reasonably in the first place, for example whether it was reasonable for Cynthia to risk driving through a pile of leaves without knowing what could be lying underneath (e.g. Baron & Hershey, 1988; Martin & Cushman, 2016). When people have knowledge about an outcome they tend to believe they would have predicted that outcome all along, that is, judgments of actions can be biased by a “hindsight effect” (e.g. Fischhoff, 1975; Roese & Vohs, 2012). For example, people judge that a riot was more probable after they learn it had occurred compared to when it is a future possibility (Fischhoff, 1975). Hindsight can affect permissibility judgments of moral risks, such that people judge a moral risk to have been less permissible to take if it has had negative consequences compared to when these consequences are a future
possibility (Fleischhut et al., 2017; see also Christensen-Szalanski & Willham, 1991; Hawkins & Hastie, 1990; Oeberst & Goeckenjan, 2016). The hindsight bias shows that people may re-assess judgments of actions in light of their outcomes, perhaps through constructing causal links that connect actions to outcomes. This idea is further supported by the effects that imagined counterfactual alternatives to reality have on judgments.

**Imagined Alternatives and Hindsight**

Counterfactual thoughts about how things could have been different help to identify various sorts of relations, such as causal and intentional relations (e.g. Byrne, 2016; Lagnado, Gerstenberg & Zultan, 2013; Rasga, Quelhas & Byrne 2016; Roese & Epstude 2017; Walsh & Byrne 2007). When people reason about a moral event, they may mentally represent the possibility of the action and its outcome occurring. When they imagine how things could have been different, they may represent the possibility that the action did not occur and hence the outcome did not occur, and comparing this counterfactual to reality emphasises the causal relation between the action and its outcome (e.g., Byrne 2005; McCloy & Byrne 2002; McElney & Byrne 2006). For example, when people learn about a runner who lost a race by a small margin after experiencing negative side effects from a painkiller, they judge that the painkiller caused her to lose the race. Their causal judgments are amplified when they learn that another athlete took an alternative painkiller and did not experience negative side effects, but their causal judgments are diminished
when they learn that the alternative painkiller also caused side effects (e.g. McCloy & Byrne, 2002).

Counterfactuals can have a similar influence on moral judgments. Imagined alternatives that change a crime victim’s behaviour but do not undo the crime, that is “semi-factual” alternatives, result in higher ascriptions of blame to the assailant. For example if people think “even if Beth had refused to travel with Brad, he would have still attacked her”, they judge Brad more harshly than when they do not imagine the semi-factual. In contrast, counterfactuals that change the victim’s behaviour and also undo the crime, for example “if only Beth refused to travel with Brad, she would not have been attacked”, result in higher ascriptions of blame to the victim (Branscombe, Owen, Garstka & Coleman, 1996; see also Malle et al., 2014). Similar effects are evidenced for failed attempts at causing harm. For example, peoples’ condemnation of the teenager who, after throwing a brick from the overpass, did not manage to cause the intended harm, is amplified if they imagine a counterfactual alternative about how things might have turned out worse, for example “if a car had been passing underneath at the right moment, the driver would have been injured” (e.g. Lench et al., 2015; Parkinson & Byrne, 2017).

Counterfactuals facilitate causal judgments and they amplify negative judgments of moral violations, suggesting that moral judgments may result from mental representations of actions and outcomes and can be influenced by the perceived causal link between the two. However, one issue with this explanation is that semi-factual “even-if” alternatives, in which the antecedent is altered but the outcome remains the same, have no effect on condemnations
of failed attempts to harm. For example, the semi-factual statement that “even if a car had been passing underneath, the driver would not have been injured” has no effect on peoples’ condemnation of the brick-throwing teenager (Parkinson & Byrne, 2017). Because semi-factuals reduce the probability of perceiving a causal relationship between actions and outcomes, they should diminish negative moral judgments about bad outcomes (e.g. McCloy & Byrne, 2002). Since semi-factuals that describe the same outcome occurring have no effect and counterfactuals that describe a worse outcome occurring amplify negative judgments, it may be the case that counterfactuals simply shift focus onto a worse outcome and the negative evaluation of this outcome drives judgments. But the absence of a semi-factual diminishment effect may instead result from some sort of floor effect; it may be the case that reasoners are simply unwilling to be any more lenient in their judgments of an agent who attempted to cause harm. Because judgments of morally good actions should be evaluated positively, they should not exhibit such a floor effect and hence they allow for a fairer test of whether semi-factuals can influence moral judgments. Such a test was the second aim of the experiments reported in this chapter. A semi-factual diminishment effect would support the idea that causal relations between moral actions and their outcomes influence moral judgments (e.g. Cushman, 2013).

Hence, the experiments reported in this chapter had two central aims: the first was to establish whether hindsight influences moral judgments of good actions, that is whether the outcome of a good action affects judgments about that action; and the second was to establish whether counterfactuals amplify
positive judgments of good actions in the same way that they amplify negative judgments of bad actions, and whether semi-factuals diminish positive judgments of good actions in the same way that they diminish causality judgments. Affirmative support for both of these aims would be consistent with the idea that mental representations of action-outcome links underlie moral judgments.

Experiment 5

The aim of this experiment was to determine whether a hindsight effect occurs for good actions that are successful, that is ones that have good outcomes, and whether imagined alternatives influence peoples’ judgments of successful good actions. Participants were presented with a vignette that described an agent, Ann, acting in a self-sacrificial way in order to save a child, Jill, from being hit by a truck. The vignette was inspired by the “Subway” story used frequently in the moral elevation literature in which a man runs in front of a train to save another man from being hit (e.g. Lai et al., 2014; see also Experiment 2). Note that the vignette was chosen at random from a larger pool of vignettes with varying contents; pre-tests showed that there were no differences in responses across contents. Participants firstly read:

Ann is waiting for a bus with her son. Nearby a young child, Jill trips over an uneven pavement stone and stumbles into the road and falls down near the traffic lights at the busy intersection. Just then, a very large truck drives through the intersection. Ann rushes to help, she can see that the truck driver cannot see Jill struggling on the ground and will not
be able to stop in time. She decides that the only way to help is to run into the road and hold Jill down so they both lie still as the truck passes over them. She knows that there is a terrible risk that the truck could crush them both.

Before learning about the outcome, participants rated their agreement to two statements that (1) “Ann should run into the road to help Jill” and (2) “Ann’s action is morally good”. They then read that there was a good outcome:

Ann ran into the road and held Jill down. When the truck drove on and passersby got to them, Jill had sustained only minor bumps and bruises from the truck.

And they read an ending statement that either restated the facts:

The police examined the videos of the incident later and observed Ann rushing to help and Jill sustaining injuries from the truck.

or presented a counterfactual alternative:

The police examined the videos of the incident later and observed that if Ann had not rushed to help, Jill’s injuries would have been worse.

or a semi-factual alternative:

The police examined the videos of the incident later and observed that even if Ann had not rushed to help, Jill’s injuries would have been the same.
They then rated their agreement to the same two statements as before, “Knowing what I know now, I believe Ann should have run into the road to help Jill” and “Knowing what I know now, I believe Ann’s action was morally good”. It was hypothesised that, if there is a moral hindsight effect for good actions, reading that there was a good outcome would amplify peoples’ positive moral judgments compared to before they knew about the outcome. It was further hypothesised that, if the causal relationship explanation is accurate, agreement to the statements should be amplified by the counterfactual statement and diminished by the semi-factual one. Participants also completed a short moral elevation scale and had the opportunity to purportedly donate half of their small experimental reimbursement to future research, in order to determine whether elevation and helping behaviour are affected by hindsight and imagined alternatives.

Method

Participants. The participants were 154 volunteers (69 women, 85 men) who were recruited from the online platform Prolific Academic. Their ages ranged from 18 to 72 years, with an average of 33 years. A further 17 participants were eliminated prior to data analysis because they failed to correctly answer two questions designed to test whether they were paying attention (during the experiment they were asked to select option 1 on a 1-7 scale to confirm they were paying attention, and at the end they were asked to select the names of the two characters from the story out of a set of 5 names); exclusion based on these responses was decided a priori. Sample size was set
to 50 participants per condition to detect a small to medium effect. As it was expected that some participants might fail attention check questions, a sample of 170 participants was set on Prolific Academic. Participation was restricted to users from countries that speak English as their first language: the participants were from the United States ($n = 96$), the United Kingdom ($n = 40$), Canada ($n = 13$), Ireland ($n = 3$), New Zealand ($n = 1$) and Australia ($n = 1$). Each participant had a .33 probability of being assigned to each condition; in the end there were 59 participants in the factual condition, 53 participants in the counterfactual condition, and 42 participants in the semi-factual condition. They were paid 50p (GBP) for taking part.

**Design and Materials.** The design was a 3 (ending: factual vs. counterfactual vs. semi-factual) x 2 (judgment: pre-outcome vs. post-outcome) mixed-factorial design; ending was manipulated between-groups and judgment type was the repeated measures factor. Each participant was presented with the story about Jill falling in front of a truck and Ann rushing to help. Participants were given a story ending that contained a re-statement of the facts, a counterfactual statement or a semi-factual statement. They made two pre-outcome judgments, about whether Ann should run into the road to help Jill and whether the action is morally good, and they made the same judgments again after learning the outcome. They provided their judgments on a 7-point scale, with 1 labelled “agree” and 7 labelled “disagree”. Participants also completed a 6-item elevation scale, which asked how much they experienced the following emotions or thoughts while reading the story: (1) inspired, (2) uplifted, (3) people are really good, (4) the world is full of kindness and
generosity, (5) I need to do more to help other people and (6) I want to be more like Ann. They made their judgments on a scale from 1 “not at all” to 7 “a lot”. Lastly, participants were presented with the option to donate half of their pay to future research as a measure of helping behaviour, they were told:

We [the researchers] are currently trying to fundraise for future research on how people think about the kind of actions you have just read about.

And they could choose to donate half of their 50p to future research or decline. At the end of the experiment, all participants were informed that this question was simply an additional measure in the study and that they would be paid the full 50p. All materials are available in Appendix 3.1.

**Procedure.** The participants were presented with a link to the experiment, which was hosted on SurveyGizmo. Each set of judgment questions was presented on a separate screen, as were the elevation questions and the helping behaviour measure. The experiment lasted approximately 5 minutes.

**Results and Discussion**

**Moral Judgments.** For clarity, the scales were reverse-scored such that higher scores indicate stronger agreement with the statements. Because each judgment was made on a 1-7 Likert scale, and hence were of ordinal quality, the data were analysed using non-parametric statistics (but note that graphing the means and standard errors more clearly represented the data compared to medians and interquartile ranges, which are available in
Appendix 3.2). Since comparing a 3 x 2 mixed design using non-parametric statistics requires 5 initial comparisons (one Kruskal-Wallis H test for judgments across the groups before learning the outcome and another for judgments after, and three Wilcoxon Signed Ranks tests to compare the before and after measures within each group), the alpha value was set to .01 for these comparisons.

The first Kruskal-Wallis H test on judgments that the action should be taken before participants learned the outcome showed that participants in the three conditions gave similar baseline agreements, $\chi^2(2, N = 154) = 2.03, p = .362$. The same analysis on judgments after learning the outcome showed that there were significant differences between the three conditions, $\chi^2(2, N = 154) = 21.98, p < .001, \eta^2 = .14$. Mann-Whitney U tests to compare the 3 conditions on these scores, with a further corrected alpha of .003, showed that there was no difference between the factual and counterfactual conditions on agreement that the action should be taken after participants learned the outcome, $U = 1502.50, p = .708$, but participants in the factual condition agreed more strongly that it should have been taken compared to participants in the semi-factual condition, $U = 634, p < .001, r = .43$, as did participants in the counterfactual condition, $U = 601, p < .001, r = .40$, as Figure 3.1 shows. Wilcoxon Signed Ranks tests to compare agreement before and after learning about the outcome for all three groups showed that agreement was significantly amplified after learning the outcome for the factual group, $Z = 4.04, p < .001, r = .37$, and the counterfactual group, $Z = 4.55, p < .001, r = .
.44, and significantly diminished for the semi-factual group, \( Z = 2.68, p = .007, r = .29. \)

![Figure 3.1](image)

**Figure 3.1.** Mean agreement that the action should be taken in Experiment 5. Error bars are the standard error of the mean.

The same approach was taken to analyse judgments that the action was morally good. There was no difference between the groups on judgments before participants learned the outcome, \( \chi^2 (2, N = 154) = 4.78, p = .092, \) or after, \( \chi^2 (2, N = 154) = 2.26, p = .323. \) Within-group comparisons showed that there was no difference after learning the outcome compared to before for participants who read the factual statement, \( Z = 1.19, p = .234, \) or the semi-factual statement, \( Z = 2.13, p = .033, \) but participants who read the counterfactual statement agreed more strongly that it was a morally good action, \( Z = 3.78, p < .001, r = .37, \) as Figure 3.2 shows.
Figure 3.2. Mean agreement that the action is morally good in Experiment 5. Error bars are the standard error of the mean.

Elevation and Helping. The 6 elevation items showed very high reliability, Cronbach’s $\alpha = .91$, and were averaged to give each participant a composite elevation score. A one-way ANOVA comparing the three conditions on elevation scores showed that participants in the factual ($M = 4.72, SD = 0.18$), counterfactual ($M = 4.39, SD = 0.18$) and semi-factual ($M = 4.78, SD = 0.17$) conditions were elevated by the story to a similar extent, $F(2, 151) = 1.37, p = .258$. Approximately 28% ($n = 43$) of participants opted to donate half of their pay to future research, and there was no association between the factual (27.1%) counterfactual (28.3%) and semi-factual (28.6%) conditions and whether the participant donated, $\chi^2 < 1$.

Summary. The results show that participants’ judgments that a good action should be taken were amplified after participants learned the outcome
was good and read a restatement of the facts, consistent with a hindsight effect for good moral actions. Moreover, judgments that a good action should be taken were amplified after participants learned about the good outcome and read a counterfactual that the outcome would have been worse if the action were not taken, and participants agreed more strongly that the action was morally good in this case. Judgments that the action should be taken were diminished when participants learned that there was a good outcome and read a semi-factual statement that it would have turned out the same even if the action were not taken. The ending statement had no effect on the experience of elevation or on subsequent helping behaviour. The next experiment tests whether this pattern is the same when the good action fails.

**Experiment 6**

The counterfactual amplification effect and the absence of a semi-factual diminishment effect on judgments about bad moral agents, for example the brick-throwing teenagers, were demonstrated on failed attempts at harm, that is, situations in which the outcome is not the one that was intended (e.g. Lench et al., 2015). In this experiment, participants made judgments about failed attempts at helping, in which Ann again attempts to prevent Jill from being injured by the truck but this time the outcome is bad: Jill sustains serious life-threatening injuries from the truck. It was hypothesised that a moral hindsight effect would again be evident such that participants would agree that the good action should have been taken less so when they learned that the outcome was bad. It was also hypothesised that the counterfactual worse
alternative would again amplify judgments and the semi-factual alternative would diminish judgments, in the same way as Experiment 5.

**Method**

**Participants.** The participants were 145 volunteers (71 women, 74 men) who were recruited from the online platform Prolific Academic. Their ages ranged from 18 to 67 years, with an average of 31 years. A further 17 participants were eliminated prior to data analysis because they failed to correctly answer the same two attention-check questions as Experiment 5. Sample size was set in the same way as Experiment 5. Participation was restricted to users who had not taken part in the previous experiment and were from countries that speak English as their first language: they were from the United States ($n = 79$), the United Kingdom ($n = 52$), Canada ($n = 11$), Ireland ($n = 1$), New Zealand ($n = 1$) and Australia ($n = 1$). Again each participant had a .33 probability of being assigned to each condition; in the end there were 43 participants in the factual condition, 66 in the counterfactual condition, and 36 in the semi-factual condition. Participants were paid 50p (GBP) for taking part.

**Design, Materials and Procedure.** The design and procedure were the same as Experiment 5. The same materials were used; the only difference was that the outcome was bad:

> When the truck drove on and passersby got to them, Jill had sustained serious life threatening injuries from the truck.

**Results and Discussion**
**Moral Judgments.** The same approach was taken to analyse the judgments as Experiment 5. Again, a Kruskal-Wallis H test on judgments that the action should be taken before participants learned about the outcome indicated similar baseline agreements between the groups, $\chi^2 < 1$. The same test after participants learned the outcome showed a significant difference between the groups, $\chi^2 (2, N = 145) = 23.48, p < .001, \eta^2 = .16$. Mann-Whitney U comparisons, again with a further corrected alpha of .003 for the three comparisons, showed that there was no difference between participants who read the factual statement and the counterfactual statement on the corrected alpha, $U = 1055.5, p = .02$, but participants who read the factual statement agreed marginally more strongly than participants who read the semi-factual statement, $U = 504, p = .007, r = .30$, and participants who read the counterfactual statement agreed significantly more strongly compared to participants who read the semi-factual statement, $U = 532.5, p < .001, r = .47$, which is the same as Experiment 5. Comparing judgments within each group using Wilcoxon Signed-Rank tests, there was no difference before and after learning the outcome for participants in the factual condition, $Z = 1.59, p = .111$, but again agreement was amplified for participants in the counterfactual condition, $Z = 4.45, p < .001, r = .39$, and diminished for participants in the semi-factual condition, $Z = 3.54, p < .001, r = .42$, as Figure 3.3 shows.
Figure 3.3. Mean agreement that the action should be taken in Experiment 6. Error bars are the standard error of the mean.

For judgments that the action was morally good, there was no difference between the conditions before learning the outcome, $\chi^2 < 1$, or after, $\chi^2 (2, N = 145) = 3.72, p = .156$. Within each group, there was no difference for participants who read the factual statement after they learned the outcome compared to before, $Z = 0.73, p = .468$, participants who read the counterfactual statement agreed marginally more strongly that the action was good after they learned the outcome, $Z = 2.53, p = .011, r = .22$, and there was no difference for participants who read the semi-factual statement, $Z = 0.34, p = .731$, as Figure 3.4 shows.
Figure 3.4. Mean agreement that the action is morally good in Experiment 6. Error bars are the standard error of the mean.

**Elevation and Helping.** The 6 elevation items again showed very high reliability, Cronbach’s $\alpha = .87$, and were averaged to give each participant a composite elevation score. A one-way ANOVA comparing the three conditions on elevation scores showed that participants in the factual ($M = 4.66, SD = 0.19$), counterfactual ($M = 4.89, SD = 0.14$) and semi-factual ($M = 4.49, SD = 0.19$) conditions were elevated by the story to a similar extent, $F(2, 142) = 1.55, p = .216$. Approximately 20% ($n = 29$) of participants opted to donate half of their pay to future research, and there was no difference between the factual (14%), counterfactual (24.2%) and semi-factual (19.4%) conditions, $\chi^2(2, N = 145) = 1.73, p = .421$.

**Summary.** The results show that learning that the outcome was bad had no effect on judgments that the action should be taken relative to before the outcome was known in the factual condition. Counterfactuals amplified
and semi-factuals diminished participants’ judgments that the action should have been taken even when the action failed to bring about the intended good outcome. And the same as Experiment 5, the counterfactual statement that things would have been worse had Ann not acted amplified judgments that the action was morally good, but there were no effects of the factual or semi-factual endings. Again, there were no effects of story ending on elevation or subsequent helping behaviour.

One potential limitation of Experiments 5 and 6 is that information about the counterfactual and semi-factual statements was presented with information about the outcome, and so it is difficult to disentangle the impact of outcome information and the impact of imagining an alternative outcome. For example, the counterfactual amplification effect on judgments cannot be separated from the amplification effect of learning there was a good outcome for judgments that the action should have been taken. The next experiments examine whether alternatives have the same effect when presented separately from the outcome.

Experiments 7A and 7B

The aim of the experiments was to examine whether the counterfactual amplification and semi-factual diminishment effects occur when information about the alternatives is presented separately from information about the outcome. The outcome was good in Experiment 7A as in Experiment 5 and bad in Experiment 7B as in Experiment 6, but this time participants learned about the outcome before making their first set of judgments, and then they
read either the factual, counterfactual or semi-factual statement before making their second set of judgments, in order to isolate the effects of those ending statements. The same counterfactual amplification and semi-factual diminishment effects on judgments that the action should have been taken were hypothesised, but no effect of reading the factual story ending was expected (since it merely restated the outcome information).

Method

Participants. There were 150 volunteers (76 women, 74 men) recruited from Prolific Academic in Experiment 7A, aged 18 to 69 years with an average of 34 years. A further 21 participants were eliminated prior to analysis for failing to correctly answer the same trap questions as before. Again participants were restricted to those who had not taken part in the previous experiments and only those who spoke English as a first language; there were 43 participants from the United States, 99 from the United Kingdom, 6 from Canada and 2 from Australia. They were randomly assigned to the factual \((n = 61)\), counterfactual \((n = 42)\) and semi-factual \((n = 47)\) conditions. In Experiment 7B, there were 151 volunteers (94 women, 56 men, 1 non-binary person), aged 18 to 72 years with an average of 37 years. Again there were 21 additional participants excluded prior to analysis for failing attention-checks. The participants were from the United States \((n = 20)\), the United Kingdom \((n = 129)\), Canada \((n = 1)\) and Australia \((n = 1)\). They were randomly assigned to the factual \((n = 42)\), counterfactual \((n = 58)\) and semi-factual \((n = 51)\) conditions.
Design, Materials and Procedure. The design was the same as the previous experiments, as were the materials except that participants were given the outcome before they made their first two judgments, then they were presented with the counterfactual, semi-factual or factual story ending and they made their second two judgments. In Experiment 7A the outcome was good and in Experiment 7B the outcome was bad. The procedure was the same as before.

Results and Discussion

Moral Judgments: Action Should Be Taken. Again, agreement ratings were reverse scored so that higher scores indicate stronger agreement, and the same approach to analysing judgments as before was taken with an adjusted alpha of .01 for each comparison. For Experiment 7A, in which the outcome was good, a Kruskal-Wallis H test on judgments that the action should have been taken before participants read the ending statement showed no difference between the groups, $\chi^2 (2, N = 150) = 5.09, p = .079$, but the same test on judgments after the ending statement was read was significant, $\chi^2 (2, N = 150) = 14.35, p = .001, \eta^2 = .10$. Mann-Whitney U tests, with a further adjusted alpha of .003, on post-story ending judgments showed that there was no difference between the factual group and the counterfactual, $U = 982, p = .036$, or semi-factual groups, $U = 1106, p = .039$, but participants in the counterfactual group agreed significantly more strongly that the action should have been taken compared to the semi-factual group, $U = 545.5, p < .001, r = .40$. Wilcoxon Signed-Rank tests showed that there was no difference before
and after reading the ending statement for participants in the factual group, $Z = 1.93, p = .054$, the counterfactual group agreed marginally more strongly after reading the ending statement, $Z = 2.48, p = .013, r = .27$, and there was no difference in the semi-factual group, $Z = 1.29, p = .196$, as Figure 3.5 shows.

**Figure 3.5.** Mean agreement that the action should be taken in Experiment 7A. Error bars are the standard error of the mean.

In Experiment 7B in which the outcome was bad, a Kruskal-Wallis H test on judgments before reading the ending statement showed that there were no differences between the groups, $\chi^2 < 1$, but there were differences after reading the story ending, $\chi^2 (2, N = 151) = 17.41, p < .001, \eta^2 = .12$. Mann-Whitney U post-hoc comparisons with an adjusted alpha of .003 showed that the factual group agreed significantly less that the action should have been taken compared to the counterfactual group, $U = 671.50, p < .001, r = .39$,
there was no difference between the factual and semi-factual groups, \( U = 1062, p = .944 \), and the counterfactual group agreed significantly more that the action should have been taken compared to the semi-factual group, \( U = 959, p = .001 \), \( r = .31 \). Wilcoxon Signed-Rank tests with an alpha of .01 showed that there was no effect on judgments that the action should have been taken after reading the factual, \( Z = 1.92, p = .055 \), or semi-factual, \( Z = 2.27, p = .023 \), story endings, but participants agreed more strongly after they had read the counterfactual story ending, \( Z = 4.78, p < .001 \), \( r = .44 \), as Figure 3.6 shows.

![Figure 3.6](image.jpg)

**Figure 3.6.** Mean agreement that the action should be taken in Experiment 7B. Error bars indicate the standard error of the mean.

Since no participant completed both experiments and all participants were sampled from the same source, the effect of outcome was directly compared between the two experiments. Three Mann Whitney U tests, with a
corrected alpha of .017 for the three comparisons, were conducted on post-story ending judgments that the action should have been taken to compare good outcome groups to bad outcome groups. There was no effect of outcome on counterfactual endings, $U = 1091.5, p = .348$, or semi-factual endings, $U = 1164.5, p = .807$, but participants who read the factual statement after learning about a good outcome agreed more strongly that the action should have been taken compared to participants who read the factual statement after learning about a bad outcome, $U = 906.5, p = .011, r = .25$. The effect of outcome on judgments after reading the factual statement is consistent with a hindsight effect for good moral actions.

**Moral Judgments: Action is Morally Good.** The same analysis on judgments that the action was morally good in Experiment 7A with good outcomes showed no differences between the groups before reading the story ending, $\chi^2 (2, N = 150) = 3.29, p = .193$, or after, $\chi^2 (2, N = 150) = 4.16, p = .125$. There were no differences between judgments that the action was good before and after reading the story ending for participants in the factual, $Z = 0.12, p = .904$, counterfactual, $Z = 1.00, p = .317$, or semi-factual groups, $Z = 0.00, p = .999$, as Figure 3.7 shows.
Figure 3.7. Mean agreement that the action is morally good in Experiment 7A. Error bars are the standard error of the mean.

On judgments that the action was morally good in Experiment 7B with bad outcomes, there was no difference between the groups before reading the story ending, $\chi^2 (2, N = 151) = 2.41, p = .299$, or after, $\chi^2 (2, N = 151) = 3.43, p = .180$. Within groups comparisons showed that there was no difference before and after reading the factual statement, $Z = 0.38, p = .705$, but the counterfactual group judged the action to be significantly more morally good, $Z = 3.33, p = .001, r = .31$, as did the semi-factual group, $Z = 2.67, p = .008, r = .26$, as Figure 3.8 shows. Note that the direction of the semi-factual difference is in the opposite direction of the predicted effect, but this is perhaps due to a slightly lower baseline score ($M = 5.75$) in the semi-factual group relative to the factual and counterfactual groups ($Ms = 6.38$ and 6.12,
respectively). Since all groups read similar materials up to this point, the most plausible explanation for this difference is random noise.

Figure 3.8. Mean agreement that the action is morally good in Experiment 7B. Error bars are the standard error of the mean.

The same analysis as before to compare the effect of outcome within each story ending group showed that there was no effect of outcome on agreement that the action was morally good after a counterfactual ending, $U = 1129, p = .373$, semi-factual ending, $U = 1140, p = .624$ or factual ending, $U = 1223.5, p = .649$.

**Elevation and Helping.** Reliability was again high for the elevation scale items in Experiments 7A and 7B, Cronbach’s $\alpha = .88$ and .89 respectively, and so the items were combined in a composite elevation score. One-way ANOVAs on average scores of elevation showed that there was no
effect of story ending in Experiment 7A, \( F < 1 \), or Experiment 7B, \( F (2, 148) = 1.79, p = .171 \), as Table 3.1 shows.

Table 3.1.

Mean Elevation Judgments in Experiments 7A and 7B.

<table>
<thead>
<tr>
<th></th>
<th>Good Outcome (E7A)</th>
<th>Bad Outcome (E7B)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Factual</strong></td>
<td>5.00 (1.20)</td>
<td>4.35 (1.29)</td>
</tr>
<tr>
<td><strong>Counterfactual</strong></td>
<td>5.09 (1.30)</td>
<td>4.73 (1.11)</td>
</tr>
<tr>
<td><strong>Semi-Factual</strong></td>
<td>4.87 (1.04)</td>
<td>4.77 (1.15)</td>
</tr>
</tbody>
</table>

*Note.* Standard deviations are reported in parentheses.

Again, the effect of outcome was compared across the experiments, but this time using t-tests since the data were of interval quality. Three comparisons with an adjusted alpha of .017 showed that there was no effect of outcome on elevation after the counterfactual ending, \( t (98) = 1.49, p = .139 \), or semi-factual ending, \( t (96) = 0.45, p = .651 \), but after reading the factual ending participants judged the story to be more elevating when the outcome was good compared to when the outcome was bad, \( t (101) = 2.64, p = .010, d = 0.52 \).

In Experiment 7A, 28.7\% (\( n = 43 \)) of participants opted to help, and there was no difference between the factual (32.8\%, \( n = 20 \)), counterfactual (35.7\%, \( n = 15 \)) or semi-factual (17\%, \( n = 8 \)) groups, \( \chi^2 (2, N = 150) = 4.64, p = .098 \). Similarly, when the outcome was bad in Experiment 7B, 26.5\% (\( n = 40 \)) of participants opted to help, and there was no difference between the
factual (31%, \( n = 13 \)), counterfactual (29.3%, \( n = 17 \)) or semi-factual (19.6%, \( n = 10 \)) groups, \( \chi^2 (2, N = 151) = 1.91, p = .385 \). There was no effect of outcome when compared across experiments for each story ending, \( \chi^2 s < 1 \).

**Summary.** The results show that, when the statement that suggested an alternative possibility was presented separately to information about the outcome, counterfactual statements about how things would have been worse, had the action not been taken, amplified judgments relative to semi-factual statements about how the outcome would have been the same even if the action had not been taken, both when the outcome was good and when the outcome was bad. The counterfactual statement amplified judgments that the action was morally good when the outcome was bad, but there was no effect when the outcome was good. There were again no effects of the ending statement on elevation or helping behaviour.

The results also show that participants agreed more strongly that the action should have been taken after reading a factual statement when the outcome was good compared to when it was bad, which provides further support for the hindsight effect on good moral actions. Moreover, participants judged the story to be more morally elevating when the action was good compared to bad in this case. This finding is the first indication of a hindsight effect for moral elevation judgments, but there was no effect on helping behaviour.
**General Discussion**

The experiments in this chapter examined whether outcome knowledge and imagined alternative outcomes influence judgments of good moral actions. The general idea was that, if judgments about good moral actions rely on some of the same cognitive processes as reasoning about moral violations, they should be similarly affected by outcome knowledge. This section discusses the results from the experiments and outlines their implications.

The results show an asymmetric hindsight effect for good moral actions. When good actions turn out well, people judge that they should have been taken more when they learn the outcome compared to before, as Experiment 5 showed, but when good actions do not turn out well, peoples’ judgments of whether they should have been attempted are unaffected by outcome knowledge, as Experiment 6 showed. The absence of any effect on judgments after participants learned that the outcome was bad suggests that people may still afford good actions some sort of credit for being attempted. The results are the first to demonstrate a hindsight effect for good moral actions, and complement research that shows a hindsight bias for moral risks and non-moral actions (e.g. Baron & Hershey, 1988; Fleischhut et al., 2017). Although it may be somewhat unfair for good actions to be judged more favourably when they turn out well, the results from the effects of the counterfactual and semi-factual statements suggest a possible explanation for this tendency. In the absence of an immediately perceptible causal connection between the action and its outcome, people may reassess the relationship between acting morally and achieving the desired outcome.
The results from the counterfactual conditions across the experiments in this chapter show that, when people imagine how things could have turned out worse, their judgments that a good action should have been taken are amplified, both when the actions turn out well, as Experiment 5 showed, and do not turn out well, as Experiment 6 showed. Experiments 7A and 7B showed that in general these findings hold even when the counterfactual alternative statement is presented separately from the information about the outcome. This counterfactual amplification effect on positive judgments about good moral actions mirrors research that shows that condemnation of bad moral actions is amplified by counterfactual alternatives (e.g. Parkinson & Byrne, 2017). The findings suggest that people re-assess the causal relationship between the good action and its outcome, because counterfactual thoughts facilitate causality judgments (e.g. McCloy & Byrne, 2002), and they judge that an action should be taken more when they imagine that it has prevented a worse outcome.

This causal link explanation is further supported by the results from the semi-factual conditions, which showed that, when people imagine that things could have turned out the same even in the absence of the good action, their judgments that the good action should have been taken are diminished, as Experiments 5 and 6 showed. Semi-factuals are well established to deny judgments of causality between antecedents and consequents (e.g. McCloy & Byrne, 2002), which suggests that imagining how things could have turned out the same in the absence of the good moral action diminishes how causal people judge the action to be in bringing about the good outcome. This
finding is the first to show that semi-factuals can diminish moral judgments, as previous research on bad moral actions has found no effect (e.g. Lench et al., 2015). Hence, the results are not consistent with the idea that an affect-based evaluation of the outcome being considered drives judgments because semi-factuals focus attention onto the same outcome as the one that really occurred.

The results from Experiments 7A and 7B suggest that this semi-factual diminishment effect may be weaker than the counterfactual amplification effect. Although the trend was towards a semi-factual diminishment effect in Experiments 7A and 7B, similar to the significant diminishment effects found in Experiments 5 and 6, there was no difference between pre- and post-story ending judgments in the semi-factual conditions when the semi-factual alternative was presented separately from the outcome information. Instead, the semi-factual diminishment effect was detected relative to the counterfactual amplification effect.

The hindsight, counterfactual amplification and semi-factual diminishment effects are evident on people’s judgments about whether a good action should be taken, and judgments about how people should behave are central to moral psychology. The same effects, however, were not evident on the other type of judgment examined in these experiments, that is about the moral goodness of the action. This finding points to a potential inconsistency in peoples’ moral judgments about how people should behave compared to their judgments about the quality of the behaviour. For example, although the action was judged to be as morally good before and after participants learned the outcome, they judged that it should have been taken more so after they
learned the outcome was good. This finding suggests that different types of cognitive processes may be recruited for different types of moral judgments; in making normative judgments, people rely on causal models that require effortful cognition, whereas judgments about moral quality may arise from simpler cognitive processes.

The results also showed no effect of thinking about alternatives on how elevating the stories were perceived to be, and hence no effect on subsequent helping behaviour. Presenting short vignettes is necessary to reduce content effects in the text to just those factors of interest, in this case that there was a good action that led to either a good or bad outcome. But one potential limitation of this approach is that these short vignettes may not be sufficiently detailed for participants to experience elevation. Although comparing good to bad outcomes in Experiments 7A and 7B demonstrated an effect of outcome on elevation after factual statements, a pilot study reported in Appendix 3.5 showed that shorter vignettes are less elevating than the more often used original form newspaper stories and videos. In order to adequately test whether hindsight affects judgments of elevation and subsequent helping behaviour, richer information about the good action may be required; the experiments in Chapter 4 aim to test this idea. Nonetheless, the effects of hindsight and imagined alternatives on judgments have important implications for moral psychology.

**Implications: Reasoning about Good Moral Actions.** The results support the finding from Chapter 2 that some of the same cognitive mechanisms that apply to reasoning about moral violations also apply to
reasoning about good moral actions. The results in this chapter show for the first time that specific cognitive processes that are known to impact judgments about moral violations, such as imagining alternatives, also affect reasoning about good moral actions (e.g. Parkinson & Byrne, 2017). Hence, the results suggest that research on the cognitive processes that underlie reasoning about the self-sacrificial acts of others may benefit from testing the sorts of processes already shown to impact reasoning about moral violations. Moreover, the results show that the same cognitive processes that impact reasoning about non-moral contents have similar effects on reasoning about moral matters.

Counterfactual alternatives to reality facilitate judgments about causal relationships when people reason about non-moral contents, and semi-factual alternatives inhibit causality judgments. The experiments in this chapter showed that imagining such alternatives has analogous effects on positive moral judgments. The results add to the literature demonstrating other effects common to non-moral reasoning and reasoning about moral matters (e.g. Petrinovich & O’Neill, 1996). Together, the findings are difficult to reconcile with the idea that moral reasoning depends on a specific moral faculty, or at least a moral faculty that relies on specific processes for moral reasoning that are distinct from the processes recruited for non-moral reasoning (e.g. Mikhail, 2007). Instead, the results support the idea that reasoning about moral contents relies on similar processes as non-moral reasoning (e.g. Bucciarelli et al., 2008; Sunstein, 2005).

**Conclusion.** The results show for the first time a hindsight effect for reasoning about good moral actions and that judgments of good actions are
influenced by causal representations of actions and outcomes. Chapter 4 pursues this novel hindsight effect for good moral actions by examining whether episodic memories about good actions can influence experiences of moral elevation and subsequent emulation, depending on whether those actions turned out well or not.
Chapter 4: Moral Hindsight about Morally Elevating Memories

The experiments in Chapter 3 showed that hindsight about good moral actions can influence whether people judge that good actions should be taken. Although examining such acts was inspired by the literature on moral elevation, the experiments were designed to examine reasoning. They showed that outcome knowledge affects reasoning about whether the action should have been taken, but in general it did not affect measures of elevation or subsequent helping behaviour. One possibility is that short vignettes may not be sufficiently detailed in order to elicit experiences of elevation necessary for people to emulate good actions, as suggested by the pilot study reported in Appendix 3.5.

The experiments in this chapter were designed to examine the effects of outcome on elevation and helping behaviour. Instead of reading short vignettes, participants were asked to recall a time when someone did something good for someone else, as episodic memory recall has been used successfully in previous research to elicit elevation and promote helping behaviour (e.g. Algoe & Haidt, 2009; Aquino et al., 2011; Siegel et al., 2014). One aim of the experiments reported in this chapter was to examine whether moral hindsight in peoples’ own episodic memories of elevating good actions affects their likelihood of emulating good actions. In other words, the aim was to examine whether thinking about morally elevating memories in which the helpful action succeeded affects peoples’ tendencies to help others differently from thinking about times when a helpful action did not succeed. Another aim was to
examine whether emulation is affected differently by different sorts of imagined alternatives to reality.

**Moral Elevation, Episodic Memories and Helping Behaviour**

When people are morally elevated by the inspirational moral good deeds of others, they are often motivated to act in morally good ways themselves (e.g. Algoe & Haidt, 2009). A substantial body of research shows that these motivations typically manifest themselves in prosocial behaviour (see Pohling & Diessner, 2016). Sometimes people act prosocially in the same domain in which they were elevated, for example people who experience elevation while volunteering are more likely to continue volunteering (Cox, 2010). But people may also emulate good deeds in domain-general ways, which suggests that elevation affects prosocial behaviour beyond observational learning or modelling effects. For example, after watching morally elevating film clips, people are more likely to help experimenters with unrelated tasks (e.g. Schnall & Roper, 2012; Schnall et al., 2010). Similarly, after recalling a memory of a morally elevating action, people are more likely to donate to charity (Siegel et al., 2014; see also Aquino et al., 2011; Freeman et al., 2009; Thomson & Siegel, 2013).

Using episodic memories may be a particularly fruitful way to induce elevation in order to examine effects on prosocial behaviour. People are more willing to interact with unfamiliar others after retrieving an episodic memory about previous helping behaviour compared to when they retrieve no such memory, and they are more likely to report a desire to help others after
simulating helping behaviour in the future compared to when they do not simulate good actions (Crisp, Husnu, Meleady, Stathi & Turner, 2010; Gaesser & Schacter, 2014; Husnu & Crisp, 2010). These studies, however, focus on memories and simulations of the participant’s own actions rather than another person helping someone else. The experiments in this chapter examine how episodic memories about the self-sacrificial acts of others might affect subsequent helping behaviour, in order to better match with the concept of moral elevation. Specifically, the experiments test whether the outcome of the good action needs to be successful in order to elevate people and inspire future moral behaviour.

**Good Actions, Imagined Alternatives and Future Behaviour**

Moral elevation is conceptualised as the response that people have when they witness or learn about acts of virtue or moral excellence (e.g. Algoe & Haidt, 2009; Pohling & Diessner, 2016), but the experiments in the previous chapter showed that the outcomes of those acts are important for peoples’ responses to them. People tend to judge that self-sacrificial actions should be committed, for example when a child, Jill, falls in front of an oncoming truck, people judge that a woman, Ann, should run into traffic to save her, and these judgments show an asymmetric moral hindsight effect, as the experiments in the previous chapter showed. People agree more strongly that the action should be taken when the attempt to help was successful, for example that Jill only sustains minor injuries, compared to before they know the outcome, as Experiment 5 showed, but there is no difference in judgments when the
outcome was bad, that Jill sustains life-threatening injuries, as Experiment 6 showed. The first aim of the experiments in this chapter was to examine whether the outcomes of good actions can have different effects on peoples’ judgments of moral elevation and tendencies to help others when elevated, which may have important implications for the conceptualisation of moral elevation.

Judgments of good actions are also affected by imagined alternatives, as the experiments in the previous chapter showed: when people hear a counterfactual about how things could have turned out worse, for example if Ann had not rushed to help, Jill’s injuries would have been worse, their judgments that the good action should have been taken are amplified compared to before they learned about the outcome, both when the outcome is good and when the outcome is bad (see also Lench et al., 2015; Parkinson & Byrne, 2017). Imagining how things could have been worse in this way may influence moral judgments by emphasising the causal relationship between the action and its outcome. In the experiments in Chapter 3, participants were presented with counterfactuals that described how things could have been worse had the action not been taken, but in the experiments in this chapter the aim was to examine the types of counterfactuals people generate themselves after recalling morally elevating episodic memories.

People create different sorts of counterfactuals and some sorts of counterfactuals may be more helpful for influencing future behaviour than others. After a bad outcome, people tend to imagine how things could have been better by adding something to reality that had not happened (e.g.
McMullen & Markman, 2000). Thoughts about how things could have been better in this way may help people to prepare for similar experiences in the future (e.g. Epstude & Roese, 2008; Markman et al., 1993; Roese &Epstude 2017). For example, after reading about negative everyday events, such as John spilling food on his shirt, people respond faster to intentions that would prevent this event from reoccurring in the future (e.g. “eat more carefully”) after reading a counterfactual statement (e.g. “it would have been possible to eat more carefully”) than a factual statement (e.g. “in the past John had eaten more carefully”; Smallman & Roese, 2009; see also Smallman & McCulloch 2012).

For morally elevating memories, if people remember a time when someone attempted to do something good for someone else and it did not turn out as intended, that is the outcome was bad, thinking about how it could have turned out better may provide people with a blueprint for how to be successful in their prosocial actions in the future. Alternatively, counterfactual thoughts may help people to prepare for the future by facilitating the causal links between actions and their outcomes (e.g. Morris & Moore, 2000; Markman et al., 2008). Imagining the counterfactual alternative to an unsuccessful good action may strengthen the causal connection between attempting the good action and no good occurring, which could diminish peoples’ propensity to attempt moral behaviour in the future.

After a good outcome, people tend to imagine how things could have been worse by subtracting something that had happened from reality, similar to the counterfactuals presented to participants in the previous chapter (e.g. Roese
One possibility is that thinking about how things could have been worse following a good action may prevent people from forming intentions for how to act in the future. But the experiments in Chapter 3 suggest that imagining how things could have been worse after recalling a good action that led to a good outcome may strengthen the perceived causal link between good moral actions and good outcomes. If this effect replicates with information-rich episodic memories, it would suggest that people may be more elevated and more likely to emulate good actions after thinking counterfactually about them. Hence, it was expected that people would create different sorts of counterfactuals when they imagine how things could have been different for a morally elevating action that succeeded in helping someone compared to one that failed, and the aim was to examine effects on subsequent behaviour. The exact type of counterfactual that may be produced is difficult to predict. Morally elevating events are positive and they influence future behaviour (e.g. Algoe & Haidt, 2009). After good events people tend to imagine downward and subtractive counterfactuals, but upward and additive counterfactuals are more often associated with preparing for the future.

A third aim of the experiments was to compare counterfactual thoughts about the past to pre-factual thoughts about the future. After a bad outcome people sometimes create counterfactuals in which they change a controllable event: when Steven arrives home too late to save his wife from dying because he was delayed by several events, some within his control such as stopping for a beer at a bar, and some outside his control such as a traffic jam, people
imagine things could have been different if Steven had not stopped at the bar, that is, they change the event within his control (e.g., Girotto, Legrenzi & Rizzo, 1991). Generating counterfactuals in this way may help people to prepare for the future, but counterfactuals sometimes function instead to explain and excuse the past (e.g. Catellani & Covelli, 2013; Markman & Tetlock, 2000; McCrea 2008; Morris & Moore 2000; Tyser, McCrea & Knüpfen, 2012). For example, people tend to think about things that are outside their control when they try to explain poor past performance (Ferrante et al., 2013; Ferrante & Stragà, 2014; Hammell & Chan, 2016). When they fail to solve difficult puzzles, people create counterfactual excuses that focus on events outside their control, such as, “things would have been better for me if the allocated time were longer”. However, they create pre-factuals to prepare for future attempts by considering how they could control the outcome, such as, “things will be better for me next time if I concentrate more” (e.g., Ferrante et al., 2013). The same pattern is observed for good outcomes (Mercier et al., 2017). Hence, pre-factual thoughts about how things could be different in the future may be a more effective preparatory aid (e.g., Byrne & Egan 2004; Ferrante, Girotto, Stragà & Walsh, 2013). For the experiments in this chapter, differences were expected in the focus on controllable events in participants’ counterfactual and pre-factual thoughts about elevating actions that succeeded or failed, and it was hypothesised that thinking pre-factually may better facilitate future behaviour than thinking counterfactually.

In summary, the aims of the experiments in this chapter were to determine whether moral hindsight affects experiences of elevation and
subsequent helping behaviour, whether the types of alternatives people imagine about good moral actions are different for ones that succeed compared to ones that fail, and whether the effects of moral hindsight on elevation and helping might be influenced more strongly by pre-factual thoughts about the future compared to counterfactual thoughts about the past.

**Experiment 8**

The primary aim of Experiment 8 was to examine whether people attempt to help others more often after they have remembered a time that someone attempted to help someone else and the outcome was good compared to if the outcome was bad. Elevation may lead to emulation because of a focus on the morally good action, and if so remembering someone attempting to do something morally good for someone else should increase intentions to help and spontaneous helping behaviour, regardless of whether the remembered attempt was successful or not. Although the definition of elevation typically does not refer to the outcomes, it is defined as the experience people have when they witness or learn about morally good actions of others, psychological theories of morality suggest that outcomes may be important for how people perceive actions (e.g. Cushman, 2013). Hence, an alternative possibility is that elevation may lead to emulation because people construct a model that focuses on the causal link between the morally good action and a good outcome, and if this is the case remembering an attempt to help someone should increase intentions to help and helping behaviour, more often when the remembered attempt was successful than when it was not.
Future helping behaviour was measured in two ways. Firstly, participants were asked whether what they thought about might change their behaviour in the future. If they reported that it would, they were asked to provide some details about their future intentions. Secondly, after the participant said they were finished with the purportedly final task, the experimenter “accidentally” knocked a cup of pens that were placed on the edge of the desk between the participant and the experimenter and it was noted whether the participant began picking up the pens. This picking-up-pens task has been used successfully in previous research on helping behaviour (e.g. van Baaren, Holland, Kawakami & Van Knippenberg, 2004; see also Macrae & Johnson, 1998) but not yet applied to moral elevation. It was expected that participants would be more likely to help to pick up the pens when morally elevated than when not, because previous research has shown that participants are more likely to help researchers with tasks unrelated to the experiment when they are morally elevated (e.g. Schnall & Roper, 2012).

**Method**

**Participants.** The participants were 63 students (23 men, 39 women and one person who indicated their gender as “other”) from Trinity College Dublin whose ages ranged from 18 to 48 years, with an average of 23 years. One additional participant was excluded because they failed to recall a past memory. Sample size was determined using effect sizes from the helping behaviour literature, which are typically smaller than those in the counterfactual literature (e.g. Markman et al., 1993). Using proportions from
van Baaren et al. (2004; e.g. 73% vs. 48%), sample size was calculated to be approximately 32 participants per group. Participants were randomly assigned to the good outcome \((n = 32)\) or bad outcome \((n = 31)\) groups using the branching function of SurveyGizmo. Most participants \((n = 50)\) opted to receive €10 as reimbursement for participation, and some \((n = 13)\) opted to receive course credits.

**Design and Materials.** The design was between-participants with two groups: good outcome and bad outcome. The participants’ tasks were to recall a memory and create a counterfactual about it. They were given the following instructions (adapted from Algoe & Haidt, 2009):

Please think of an experience in which you saw someone, or heard about someone, demonstrating humanity’s higher or better nature. Please pick an example in which you were not the beneficiary, that is, someone tried to do something good, honourable or charitable for someone else.

Participants in the good outcome group were instructed:

But please think of a situation that resulted in a good outcome - a situation where the person’s actions were successful in helping another person. Please describe the experience briefly here.

Participants in the bad outcome group were instructed instead:

But please think of a situation that did not result in a good outcome - a situation where the person’s actions were not successful in helping another person. Please describe the experience briefly here:
Although good outcomes might have been easier to remember, the experiment could not proceed until the participant typed their recollection into a text box on-screen. Just one participant could not recall a good action that did not turn out well, and so the experiment ended at this stage for that participant; it might be useful for future experiments to include measures of difficulty of recall.

Participants then indicated how morally elevating their memory was, by answering three questions: “How much did you think or feel the following when thinking about your memory? (1) Inspired, (2) I want to be more like the person I thought of who did something good / honourable /charitable, and (3) People are really good. Since measures of the emotional, motivational and cognitive elements of elevation load onto the same single factor, it was decided that only the three highest-loading items were necessary for these experiments (and indeed they showed high reliability, as reported below). The three elevation questions were presented in a different random order to each participant and they recorded their responses on a scale from 1 “not at all” to 7 “a lot”. Participants then generated a counterfactual thought. They were instructed as follows (adapted from Markman et al., 1993):

Sometimes people think about how an event could turn out differently in the past ‘if only…’ Please think about the experience you wrote about and about how it could have turned out differently in the past. Please complete the following sentence: Things could have been different if...
They typed their recollection into a text box on-screen, and they were asked to self-code their counterfactual: “When you wrote about how things could have been different, did you write about… (1) …how things could have been: worse, better or the same, (2) …something that: happened and imagine it had not happened or did not happen and imagine it had happened, (3) …the person who did something good and imagine: something else within their control, something else outside their control or I did not think about the actor/agent.” (These self-coded responses were subsequently compared to independent rater’s codings as an additional check).

Participants were asked about their intentions for the future: “Considering what you have thought about, do you think this would change your own behaviour in the future? (Yes/No). Please briefly describe your intentions.” They were also asked, “Would you agree to be contacted regarding a short, online follow-up study? (Yes/No)”, and the results from this follow up are reported in Appendix 4.2.

When the participant said that they had reached the end of the study, the experimenter (who was blind to whether the participant had been assigned by the experimental software to the good or bad condition) stood up and “accidentally” knocked a cup of 12 pens that was situated on the edge of the desk between the experimenter and participant. The experimenter apologised, paused briefly, and then began to pick up the pens. It was noted whether the participant helped to pick up the pens (adapted from van Baaren et al., 2004; see Sheet 11 in Data Archive). At the end of the experiment, participants were asked whether they had any suspicions about what was being tested in the
study; just one participant queried whether the pens were knocked intentionally. All materials are presented in Appendix 4.1.

Procedure. Participants were tested individually in a small room. They completed the study via SurveyGizmo on a MacBook Pro (Retina, 13-inch, Mid 2014). The study took about 15-20 minutes; it was part of a larger set of studies that took about 50 minutes and involved reading unrelated vignettes and answering questions about the protagonists. The position of this experiment in the set was randomised between participants.

Results and Discussion

Elevation. The three elevation items showed high reliability, Cronbach’s $\alpha = .73$, and were averaged to give each participant a composite elevation score. Participants indicated that their memories were elevating, and participants in the good outcome group ($M = 5.23$, $SD = 1.19$) were more elevated than those in the bad outcome group ($M = 4.55$, $SD = 1.27$), $t(61) = 2.20$, $p = .032$, $d = 0.56$, consistent with a moral hindsight effect on experienced elevation.

Counterfactuals. Participants created different counterfactuals in the good outcome and bad outcome groups, as Figure 4.1 shows. Participants created counterfactuals in the good outcome group that imagined how things

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2 The counterfactuals were coded by 3 independent raters and inter-rater reliability was almost perfect for coding whether participants imagined a better or worse outcome, mean $\kappa = 0.89$; substantial for whether they added or subtracted from reality, mean $\kappa = 0.80$, and moderate for whether they focused on something controllable, mean $\kappa = 0.50$ (Landis & Koch, 1977). Disagreement was resolved by discussion (and disagreement about controllability was based on inaccurate initial specification that counterfactuals were to be coded as controllable in relation to the agent’s behaviour, rather than in relation to any person’s behaviour). The coding by the three independent raters also agreed with the participants self-coding, for coding as better or worse outcomes, $\kappa = .90$ and for controllability, $\kappa = .57$, but agreement was low for adding or subtracting, $\kappa = .11$, perhaps because of the phrasing used for participants to code their own.
could have been worse (78%) rather than better, binomial $p = .002$, whereas those in the bad outcome group tended to think about how things could have been better (90%) rather than worse, $p < .001$; the difference between the groups was significant, $\chi^2 (1, N = 61) = 28.15, p < .001, V = .68$. Participants in the good outcome group tended to delete an aspect of reality that had happened (69%), $p = .050$, whereas those in the bad outcome group tended to add something that had not happened to reality (84%), $p = .001$; the difference between the groups was significant, $\chi^2 (1, N = 61) = 16.36, p < .001, V = .52$.

Participants in both groups tended to focus equally on something controllable for the agent as something uncontrollable, $ps > .215$, but participants who created counterfactuals about good outcomes tended to focus on something within the agent’s control (62.5%) more than participants who thought about bad outcomes (38%); the difference between the groups was marginally significant, $\chi^2 (1, N = 61) = 3.67, p = .055, V = .25$. 
**Figure 4.1.** Types of counterfactuals created in Experiment 8. Error bars are the standard error of the proportion.

**Intentions and Helping.** Most participants (67%) self-reported an intention to act differently in the future. Participants in the good outcome group tended to indicate an intention to change (57%) as often as not, \( p = .597 \), whereas those in the bad outcome group tended to indicate an intention to change (77%) more than not, \( p = .003 \); but the difference between the groups was not significant, \( \chi^2 (1, N = 63) = 3.18, p = .075 \) (for further details about the intentions generated, see Appendix 4.2).
Figure 4.2. Proportion of participants who helped to pick up the pens in Experiment 8. Error bars are the standard error of the proportion.

Participants in the good outcome group tended to help to pick up the pens, whereas those in the bad outcome group showed no such tendency. Most participants spontaneously helped to pick up the pens (62%), and participants in the good outcome group (72%) tended to help rather than not, $p = .020$, whereas those in the bad outcome group (52%) tended to help as often as not, $p = .999$, as Figure 4.2 shows, although the difference between the groups was not significant, $\chi^2 (1, N = 63) = 2.74, p = .098$.

Summary. The results show that elevation experiences were affected not only by witnessing someone who acted in a morally virtuous way, but also whether their actions led to a beneficial outcome. The results support the moral hindsight effect for good actions established in Chapter 3.
When people thought counterfactually about good actions that led to good outcomes, they imagined how things could have been worse by deleting something from reality, and when they thought counterfactually about good actions that did not lead to good outcomes, they imagined how things could have been better by adding something to reality. Participants focused more so on controllable events when they thought counterfactually about good outcomes than bad outcomes.

Participants who thought counterfactually about bad outcomes were more likely than not to report an intention to act prosocially in the future, but were as likely to not spontaneously help as to help. Participants who thought counterfactually about good outcomes were as likely to report prosocial intentions as not, but were more likely to spontaneously help than not. The next experiment examines whether imagining alternatives to episodic memories in the future has different effects on intentions and helping behaviour.

**Experiment 9**

The aim of the experiment was again to examine whether people engage in activities to help others more often after they have remembered a successful attempt to help someone compared to a failed attempt, but in this experiment participants created a pre-factual thought about how things could turn out differently in the future. Pre-factual thoughts may help prepare for the future even more than counterfactuals, and so differences between participants who recalled a memory about a good action that succeeded compared to one that failed should be more pronounced (Byrne 2016; De Brigard & Giovanello,
Method

Participants. The participants were 67 students (18 men, 49 women) from Trinity College Dublin, whose ages ranged from 18 to 40 years with an average of 22 years. One additional participant was excluded because they failed to recall a past memory. Some participants \(n = 32\) opted to receive €10 as reimbursement for participation, and others \(n = 35\) opted to receive course credits. Sample size was determined the same way as the previous experiment. Participants were randomly assigned to the good outcome \(n = 35\) or bad outcome \(n = 33\) groups using the branching function of SurveyGizmo.

Materials, Design & Procedure. The materials, design and procedure were the same as the previous experiment, except participants were instructed to create pre-factuals as follows:

Sometimes people think about how an event could turn out differently in the future ‘if only…’ Please think about the experience you wrote about and about how it could turn out differently in the future. Please complete the following sentence.  
*Things could be different in the future if...*

Results and Discussion

Elevation. The same as before, reliability was high across the three elevation items, Cronbach’s \(\alpha = .70\), and so participants were compared again on their composite elevation scores. Participants indicated that their memories
were elevating, and those in the good outcome group \((M = 5.66, SD = 0.72)\) were more elevated than those in the bad outcome group \((M = 4.19, SD = 1.08)\), \(t (53.09) = 6.50, p < .001, d = 1.60\), replicating the previous experiment with a stronger effect. (Welch’s t-test for unequal variances was used as the data failed Levene’s test of homogeneity of variance, \(F = 7.11, p = .010\).)

**Pre-factuals.** Participants created different pre-factuals in the good and bad outcome groups\(^3\). Participants created pre-factuals in the good outcome group that imagined how things could have been worse (60%) as often as better, binomial \(p = .311\), whereas those in the bad outcome group imagined only how things could have been better; the difference between the groups was significant, \(\chi^2 (1, N = 66) = 27.28, p < .001, V = .64\), consistent with the previous experiment. Participants in the good outcome group tended to delete an aspect of reality that had happened (49%) as often as add something, \(p = .999\), whereas those in the bad outcome group tended to add something to reality (87%), \(p < .001\); the difference between the groups was significant, \(\chi^2 (1, N = 66) = 9.64, p = .002, V = .38\), again consistent with the previous experiment. Participants in the good outcome group tended to focus on things within the agent’s control (43%) as often as things outside the agent’s control, \(p = .500\), whereas those in the bad outcome group tended to think less often about things within the agent’s control (26%), \(p = .011\), but there was no difference between the groups, \(\chi^2 (1, N = 66) = 2.11, p = .147\). This result is different

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\(^3\)Inter-rater reliability was again almost perfect for coding whether participants imagined a better or worse outcome, mean \(\kappa = 0.87\), whether they added or subtracted from reality, mean \(\kappa = 0.70\), and moderate for controllability, mean \(\kappa = 0.37\), and disagreements were resolved by discussion. The coding by the three independent raters also agreed with the participants self-coding, for coding as better or worse outcomes, \(\kappa = 1.00\), and for controllability, \(\kappa = .49\), but agreement was again low for adding or subtracting \(\kappa = .01\).
from the previous experiment, and points to differences in focus on controllability between counterfactuals and pre-factuals (e.g., Ferrante et al., 2013; Mercier et al., 2017).

![Bar chart](image)

**Figure 4.3.** Types of pre-factuals generated in Experiment 9. Error bars are the standard error of the proportion.

**Intentions and Helping.** Participants in the good outcome group tended to report an intention to change (80%) more than not, \( p = .001 \), whereas those in the bad outcome group tended to report an intention to change (53%) as often as not, \( p = .860 \); and the difference between the groups was significant, \( \chi^2 (1, N = 67) = 5.48, p = .019, V = .29 \). This pattern is the opposite of the previous experiment. (And for more details, see Appendix 4.2.)

Participants in the good outcome group helped more than those in the bad outcome group. Most participants spontaneously helped to pick up the
pens (58.5%; note that one participant’s score on this measure was excluded because she indicated that she suspected it was part of the experiment).

Participants in the good outcome group (82%) tended to help rather than not, $p < .001$, whereas those in the bad outcome group (34%) tended to help as often as not, $p = .110$; and this time, the difference between the two groups was significant, $\chi^2 (1, N = 65) = 15.06, p < .001, V = .48$, as Figure 4.4 shows.

![Figure 4.4](image)

*Figure 4.4.* Proportion of participants who helped to pick up the pens in Experiment 9. Error bars are the standard error of the proportion.

**Summary.** The results show again that elevation experiences were affected not only by witnessing someone who acted in a morally virtuous way, but also whether their actions led to a beneficial outcome. The results provide further support for the moral hindsight effect for good actions established in Chapter 3 and replicates Experiment 8.
When participants thought pre-factually about good actions that led to good outcomes, they imagined how things could have been worse by deleting something from reality, and when they thought pre-factually about good actions that did not lead to good outcomes, they imagined how things could have been better by adding something to reality; this pattern is similar to when participants thought counterfactually in Experiment 8. However, in Experiment 8 participants who thought counterfactually about good outcomes focused more so on controllable behaviours than those who thought counterfactually about bad outcomes, but this pattern was not replicated in this experiment as both groups focused on controllable events to similar degrees.

Unlike the previous experiment, participants who thought pre-factually about good outcomes were more likely than not to report an intention to act prosocially in the future and participants who thought pre-factually about bad outcomes were as likely to report prosocial intentions as not. However, the same pattern in helping behaviour as Experiment 8 was observed with a stronger effect: participants who thought about good outcomes were more likely to help than participants who thought about bad outcomes. This stronger effect may have resulted from the boosted preparatory function of pre-factuals compared to counterfactuals.

Some of the results from Experiments 8 and 9 are consistent, for example that participants reported more elevation and were more likely to help after recalling actions that had good outcomes compared to bad ones. But one issue with comparing the results from these two experiments is that there was a larger difference in elevation scores between the groups in Experiment 9.
compared to Experiment 8, as indicated by the larger effect size. The larger effect of outcome on helping behaviour in this experiment may be due to the larger difference in elevation, rather than some sort of increased preparatory function of pre-factuals. The next experiment aimed to replicate the previous two experiments and provide a direct comparison of counterfactual and pre-factual thinking. Moreover, all participants in Experiments 8 and 9 imagined some sort of alternative to reality and it cannot be inferred whether this is any different to reflecting on the facts as they happened. The next experiment included two additional groups who were not instructed to imagine how things could have gone differently, and were instead instructed to reflect on the facts about the good outcome or bad outcome (note that these groups are analogous to the participants who read the factual statements in the previous chapter).

**Experiment 10**

The aim of the experiment was to compare counterfactual and pre-factual thinking directly, and also to compare the effects of thinking about alternatives to a control group instructed to reflect on the facts. Participants in this experiment recalled a time when someone did something good for someone else and the outcome was either good or bad, and they then thought about how things could have been different in the past, or in the future, or they reflected on the facts as they happened.

In this experiment, helping behaviour was measured by whether the participant opted to donate some portion of their payment to charity and, if so, the amount they donated. Elevation has previously been shown to promote
charity donations (e.g. Aquino et al., 2011) and this type of helping measure may be a particularly strong test of the effect of hindsight on emulation, because it is a financially costly behaviour and is completely anonymous. It could be argued that the helping behaviour in the previous experiments was inconsequential to the participants and may have been susceptible to social desirability effects (although note that both the good outcome and bad outcome groups should have been susceptible to the same effects). It was expected that participants who recalled good actions that turned out well would be more likely to donate compared to participants who recalled ones that did not turn out well, and that, because pre-factual thinking (in Experiment 9) seemed to have a stronger effect on the picking-up pens task than counterfactual thinking had (in Experiment 8), there would be an interaction effect between outcome and thought instruction.

Method

Participants. The participants were 299 volunteers (107 men, 190 women and 2 who reported their gender as “other”) who were tested on the online crowdsourcing platform Prolific Academic. Their ages ranged from 18 to 73 years, with an average age of 36 years. A further 49 were excluded prior to analysis for failing an attention check measure (to choose option 1 on a scale from 1 to 7 if they were paying attention), and this exclusion criterion was decided a priori. An additional participant was excluded because they could not think of a memory to report, and 1 more withdrew their data from participation immediately after taking part. Because one of the aims of this
experiment was to compare not only participants who imagined different sorts of outcomes but also participants who thought counterfactually compared to pre-factually about the *same* sort of outcome, the effect was expected to be smaller than before and so sample size per group was set to 50. Recruitment was restricted to countries with English as a first language, and the participants were from the United Kingdom (n = 248), the United States (n = 45), Canada (n = 4) and Ireland (n = 2). Participants who were instructed to think about good outcomes had a .33 probability of being assigned to the factual (n = 41), counterfactual (n = 55) and pre-factual (n = 55) conditions; and participants who thought about bad outcomes had a .33 probability of being assigned to the factual (n = 46), counterfactual (n = 55) and pre-factual (n = 47) conditions. They were paid 50p (GBP) for taking part.

**Design and Materials.** The design was a 2 (outcome: good vs. bad) x 3 (instruction: counterfactual vs. pre-factual vs. factual) between-participants design. Participants were given the same memory prompts as before and asked to either recall a time when someone did something good for someone else and it turned out well or did not turn out well. They then completed the same elevation scale and were prompted with the same counterfactual instructions as Experiment 8, or the same pre-factual instructions as Experiment 9, or to reflect on the facts:

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Sometimes, people think back on events exactly as they happened. Please think about the experience you wrote about and think about the facts of the event.
Please write here any further thoughts you have about the event as it happened:
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On the next screen, they were again asked if what they thought about
might change their own behaviour in the future and they responded yes or no.
If they responded yes, the subsequent screen asked them to briefly describe
their intentions. Donating some proportion of their payment was used as a
measure of helping behaviour. After taking part in studies, Prolific Academic
offers participants the opportunity to donate some of their payment to Cancer
Research UK and/or Save the Children; unfortunately it was not possible to
access these data. Instead a measure was included within the experiment:
participants were thanked for taking part and informed that Prolific Academic
allows participants to allocate some amount of their payment to one of their
chosen charities, Cancer Research UK and Save the Children, and they were
given the opportunity to allocate any amount of their payment, from 0p to 50p
in 5p increments, to either of the charities and they could choose which charity
they would like their donation to go to. At the end of the study, they were
informed that this question was a measure included only in the study and if they
wished to donate they would be offered the opportunity when they returned to
the Prolific Academic website.

**Procedure.** Participants accessed the experiment through a
SurveyGizmo link on Prolific Academic. Each task was presented on a
separate screen and participants were restricted from going back to the previous
screen throughout. The experiment lasted an average of 6 minutes.

**Results and Discussion**
**Elevation.** Reliability was very high for the three elevation items, Cronbach’s $\alpha = .80$, consistent with the previous experiments. A 2 (good, bad) x 3 (counterfactual, pre-factual, factual) ANOVA on averaged elevation scores showed a main effect of outcome, $F(1, 293) = 65.05, p < .001, \eta^2_p = .18$, such that participants who thought about good outcomes reported higher judgments of elevation than those who thought about bad outcomes, and there was no main effect of instruction or interaction between the two factors, $Fs < 1$, as Figure 4.5 shows.\(^4\)

![Figure 4.5. Mean elevation ratings in Experiment 10. Error bars are the standard error of the mean.](image)

\(^4\)Note that, although the data failed Levene’s test for homogeneity of variance, $F(5, 293) = 5.02, p < .001$, Mann Whitney U tests comparing good and bad outcomes in each group showed that participants in each thought-instruction group were more elevated after good outcomes than bad ones: counterfactual $U = 629, p < .001, r = .51$; pre-factual $U = 765, p < .001, r = .35$; factual $U = 489.5, p < .001, r = .42$.\]
**Imagined Alternatives.** Participants imagined different types of counterfactuals depending on whether the outcome was good or bad, as Figure 4.6 shows. Whether participants imagined a better or worse alternative depended on the outcome of their memory rather than whether they thought counterfactually or pre-factually: when the outcome was good, they imagined how things could have been worse (92.4%) rather than better, binomial $p < .001$, and when the outcome was bad they imagined how things could have been better (99%) rather than worse, $p < .001$, and those in the good outcome conditions imagined better alternatives more so than those in the bad outcome conditions, $\chi^2 (1, N = 202) = 168.45, p < .001, V = .91$. A similar pattern was found when the structure of the alternatives was analysed: when the outcome was good, participants tended to subtract (80%) something from reality rather than add, $p < .001$, and when the outcome was bad, participants tended to add (81.1%) something to reality rather than subtract, $p < .001$, and the difference between the groups was significant, $\chi^2 (1, N = 202) = 107.90, p < .001, V = .73$. These patterns are consistent with Experiments 8 and 9. (Additional analyses are reported in Appendix 4.2.)
Figure 4.6. Types of counterfactuals generated in Experiment 10. Error bars are the standard error of the proportion.

The pattern of whether the imagined alternative focused on something controllable for the good agent was more dependent on whether participants were instructed to think counterfactually or pre-factually compared to the direction and structure, as Figure 4.6 shows. Participants who thought counterfactually about good outcomes focused on something controllable (65%) rather than not, $p = .049$, but those who thought counterfactually about bad outcomes focused on something uncontrollable (67%) rather than controllable, $p = .020$; the difference between the groups who thought counterfactually was significant, $\chi^2 (1, N = 105) = 10.33, p = .001, V = .31$, similar to Experiment 8. Participants who thought pre-factually about good outcomes showed no preference for controllable (59%) or uncontrollable alternatives, $p = .220$, and the same was true for participants who thought pre-
factually about bad outcomes (47% controllable), $p = .761$; the difference between the groups who thought pre-factually was not significant, $\chi^2 (1, N = 97) = 1.56, p = .211$, the same as Experiment 9. The results show that counterfactual and pre-factual thinking differ primarily in their focus on controllable events.

**Intentions and Helping.** Participants in the good outcome groups (52.3%) reported intentions to change their own behaviour at a similar frequency to participants in the bad outcome groups (56.1%), $\chi^2 < 1$, and there were no differences between the factual (58.6%), counterfactual (52.7%), or pre-factual groups (52%), $\chi^2 < 1$. One difference that was noted when analysing the intentions in this experiment compared to the previous two, however, was that some participants reported intentions about being reluctant to engage in future helping behaviour, whereas in Experiments 8 and 9 all of the reported intentions described acting prosocially in the future. Analysing the intentions showed that there was an association between whether the intention was to act prosocially or not act prosocially in the future and the outcome participants thought about, $\chi^2 (1, N = 161) = 34.08, p < .001, V = .46$, as no participant who thought about a good outcome and reported an intention to act differently in the future was reluctant to act prosocially in the future but 35.4% of participants who thought about bad outcomes expressed not intending to act prosocially in the future. There was no association between having an intention that was good and whether participants thought factually (81.3%), counterfactually (86.2%) or pre-factually (78.2%), $\chi^2 (1, N = 161) = 1.26, p = .534$ (for further analysis of intentions, see Appendix 4.2).
Figure 4.7. Proportion of participants who were willing to donate some of their payment to charity in Experiment 10. Error bars are the standard error of the proportion.

In this experiment, helping behaviour was measured by whether the participant opted to donate some of their payment to charity. Although overall helping was somewhat lower than the previous experiments (27.4% of participants donated), perhaps because in this case helping was anonymous and required a financial cost, participants who thought about a good action having a good outcome were more likely to donate (33.1%) than participants who thought about a good action having a bad outcome (21.6%), $\chi^2 (1, N = 299) = 4.96, p = .026, V = .13$, as Figure 4.7 shows, and this is consistent with Experiments 8 and 9. There was no effect of having thought factually (20.7%), counterfactually (29.1%) or pre-factually (31.4%), $\chi^2 (2, N = 299) = 2.94, p = .231$. 
A 2 (good, bad) x 3 (factual, counterfactual, pre-factual) ANOVA on the amount donated by those who did showed that there was a main effect of outcome, $F(1, 76) = 6.97, p = .010$, $\eta_p^2 = .08$, with participants who thought about good outcomes donating significantly more overall than participants who thought about bad outcomes, and no effect of instruction or interaction between the two factors, $Fs < 1$, as Figure 4.8 shows. (Note that this analysis was on a reduced sample size of 82 participants.)

**Summary.** The results show that the effect of outcome on moral elevation replicated: participants were more elevated by good actions that led to good outcomes than ones that led to bad outcomes. The types of alternatives participants generated were also consistent with the previous experiments.
When the outcome was good, participants imagined worse alternatives by deleting something from reality both when they thought counterfactually and pre-factually. When the outcome was bad, participants imagined better alternatives by adding something to reality both when they thought counterfactually and pre-factually. However, they focused on something controllable when the outcome was good rather than bad when they thought counterfactually, whereas there was no difference between good and bad outcomes when they thought pre-factually. The results from the self-reported intentions were not consistent with either Experiment 8 or 9, and suggest that such self-report measures may be somewhat unreliable. However, the finding that good actions with good outcomes lead to greater emulation than ones with bad outcomes was replicated, both in terms of whether the participant was willing to donate to charity and how much they donated.

The experiment showed that thinking counterfactually and pre-factually about good actions that led to good outcomes had similar effects on subsequent helping behaviour, as participants in both cases were more likely to help compared to participants who thought about good actions that led to bad outcomes. The results suggest that the larger effect of hindsight on helping behaviour in Experiment 9 compared to Experiment 8 may have resulted from the larger difference in elevation following memory recall in Experiment 9. Moreover, the results show that imagining alternatives had no distinguishable effects from reflecting on the facts, possibly because in all cases participants were aware of the causal link the remembered action had with his outcome. These results are discussed in more detail in the next section.
General Discussion

The experiments in this chapter tested one of the novel effects from the previous chapter, that outcomes affect how people respond to good moral actions, but this time about information-rich events: participants’ episodic memories about moral actions. The general idea was that, if elevation depends on a good action having a good outcome, participants would be more elevated by good actions that led to good outcomes than bad outcomes, and that they would subsequently be more likely to act prosocially. This section discusses the results from the experiments reported in this chapter and their implications.

The results show for the first time a moral hindsight effect on moral elevation and tendencies to emulate good moral actions. The experiments show that people experience more moral elevation after recalling a successful attempt to help than a failed one. The three experiments also clearly demonstrate that people tend to help others more after recalling a successful attempt to help than one that failed, and this applies to inconsequential spontaneous helping in face-to-face situations such as helping someone to pick up dropped pens, as well as more costly financial helping in anonymous situations such as donating to charity online. The results support the idea that good actions are not always judged on their own merit; peoples’ positive evaluations also depend on the success of those good actions.

The results show that the effect of outcome knowledge on elevation has a stronger effect than potential differences between counterfactual and pre-factual thoughts. Whereas Experiment 9 suggested moral hindsight might have
more pronounced effects on subsequent helping after pre-factual thoughts compared to counterfactual thoughts in Experiment 8, Experiment 10 showed that there was no difference between counterfactual and pre-factual thoughts when directly compared. The larger effect on behaviour in Experiment 9 seems to instead be a result of the larger difference in elevation between the two groups relative to the difference between the groups in Experiment 8, as evidenced by the effect sizes.

Moreover, it is noteworthy that Experiment 10 showed that imagining alternatives has no effects on subsequent emulation compared to factual thoughts about good actions. Experiment 5 in the previous chapter showed that reflecting on the facts and thinking about how things could have been worse have similar amplifying effects on judgments about good actions that result in good outcomes, and the same was found on elevation and helping behaviour in Experiment 10. However, Experiment 6 in the previous chapter showed that factual statements about good actions that lead to bad outcomes have no effect on positive judgments and counterfactuals about how things could have been worse amplify them. The same pattern on elevation and helping was not observed for good actions that led to bad outcomes in Experiment 10: participants experienced similar levels of elevation after reflecting on the facts and when they thought about how things could have been different. One explanation for this difference is that, for bad outcomes in Experiment 6, the counterfactuals described a worse alternative to reality, whereas for bad outcomes in Experiment 10 participants tended to imagine better alternatives. Hence, the effects of counterfactuals on responses to good moral actions may
be sensitive to the type of counterfactual generated. Imagined worse alternatives to reality amplify positive responses to good actions that fail, but people may be more likely to think about how things could have turned out better.

**Implications.** The results contribute to the recurring idea that perceived causal links between actions and their outcomes have downstream effects on moral cognition, including peoples’ responses to good moral actions: when good actions are connected to good outcomes, people experience more elevation and they are more likely to emulate those acts. Chapter 3 showed that judgments about whether good actions should be taken are affected by whether the outcome was good or bad, and the results from this chapter show that emotional appraisals and behavioural consequences are similarly affected. Cognitive factors are typically not incorporated into research on moral elevation, but the results of this chapter show that the understanding of phenomena such as moral elevation benefits from considering the cognitive factors in addition to emotional and behavioural ones. For example, the current definition of moral elevation is a “response to witnessing [or learning about] acts of moral beauty” (Pohling & Diessner, 2016, p. 412; see also Algoe & Haidt, 2009), but the results from this chapter show that moral elevation is a response to acts of moral beauty *that lead to good outcomes*. Such a contribution would not have been possible without considering the cognitive processes that underlie responses to good moral acts.

The results also extend the idea discussed in Chapter 3 that demonstrating effects such as the hindsight bias on responses to moral events
favours the notion that similar processes influence reasoning about moral contents as non-moral contents, rather than the claim that there is a specific moral faculty (e.g. Bucciarelli et al., 2008; Mikhail, 2007). The findings in this chapter support this idea, and also highlight an interesting corollary. If it is accepted that people rely on domain-general reasoning processes when reasoning about moral contents, it follows that research on cognitive processes in moral reasoning should be able to contribute to the understanding of those processes as well as the understanding of human morality.

Indeed, the types of counterfactuals generated by participants in this chapter contribute to the current debate on the preparatory function of imagined alternatives. Recent research has suggested that pre-factuals serve a more preparatory function than counterfactuals, which may instead function to explain the past (e.g. Ferrante et al., 2013). In support of this idea, various experiments have shown that people tend to focus more so on controllable features of behaviour when thinking pre-factually compared to counterfactually (e.g. Ferrante et al., 2013; Mercier et al., 2017). Importantly, the research to date has examined the types of alternatives people imagine when thinking about their own behaviour, whereas participants in the experiments in this chapter thought about the behaviour of other people. The results showed that when people think counterfactually about moral actions of others that have bad outcomes, they tend to focus on uncontrollable features rather than controllable ones, but there is no such bias when people think pre-factually. When the outcomes were good, participants were more likely to focus on controllable features than uncontrollable when thinking counterfactually, and again there
was no such preference in participants who thought pre-factually. The results suggest that the increased preparatory function of pre-factuals relative to counterfactuals may be limited to when participants think about their own behaviour, a factor not currently considered by research in this area (e.g. Ferrante et al., 2013; Mercier et al., 2017). The results provide a novel and potentially fruitful contribution to understanding the preparatory function of imagined alternatives.

**Conclusion.** The results show that moral hindsight affects elevation following good moral actions and whether people emulate inspiring moral behaviour. In the next chapter, the results of this chapter and the preceding chapters are discussed in detail by considering further how the results relate to debates on whether moral reasoning relies on similar processes to non-moral reasoning and the nature of effortful cognitive processes. The implications of these contributions to everyday morality are also considered.
Chapter 5: Discussion

The aim of this thesis was to investigate cognitive processes in moral reasoning. Experiments 1 to 4 reported in Chapter 2 examined the effect of fatigued cognitive resources on moral judgments, Experiments 5 to 7 reported in Chapter 3 examined the effect of outcome knowledge and imagined alternative outcomes on judgments about good moral actions, and Experiments 8 to 10 reported in Chapter 4 examined the hindsight effect for good moral actions on moral elevation and helping behaviour. Together, the results support the idea that deliberative cognition plays a role in moral reasoning about both negative and positive moral events, and also that some of the same processes that affect reasoning about non-moral contents are recruited to reason about moral matters. The results suggest that effortful cognitive processes help to form mental representations of actions and their outcomes when people make judgments about whether moral actions are permissible or whether people should act in moral ways.

In this chapter, the key results from the reported experiments are summarised, how they relate to psychological theories of morality is outlined and some practical implications are suggested. The first section discusses the five experiments on cognitive fatigue in Chapter 2, the second section discusses the six experiments on morally good actions in Chapters 3 and 4, and the third section concludes this chapter by considering the general implications for understanding moral psychology and some thoughts for future research.
Effortful Cognition in Moral Reasoning

As outlined in Chapter 1, the role that effortful reasoning processes play in moral judgment formation is a key debate in moral psychology (e.g. Bucciarelli et al., 2008; Crockett, 2013; Cushman, 2013; Greene et al., 2001; Haidt, 2001; Mikhail, 2007; Tasso et al., 2017). In Chapter 2, the role of effortful cognition in moral reasoning was investigated by examining the impact of cognitive fatigue on moral judgments. Five experiments (the pilot study and Experiments 1 to 4) demonstrated a fatigue effect on moral judgments, which affected judgments (in Experiments 1 and 2) and how participants felt about their judgments (in Experiments 3 and 4).

In the five experiments participants firstly completed a cognitively demanding task that required extensive use of core executive functions, such as inhibiting prepotent responses, sustaining attention and manipulating information in working memory, and other participants completed less demanding versions of the same task. The moral judgments the participants made on a subsequent task were then compared. The rationale was that any difference in judgments between the groups would reflect the role that effortful cognitive processes play in typical judgment formation.

Indeed in the pilot experiment, participants who initially completed a difficult essay-writing task, in which they could not use words with the letters “a” or “n”, judged that causing harm to one person in order to save a larger group of people was less permissible compared to participants who completed the control essay-writing task, and they took longer to make those judgments. The results showed for the first time an effect of fatigue on moral judgments.
and corroborated suggestions that effortful cognitive processes are required in order to make the utilitarian judgment that harm is permissible if it is outweighed by the benefits (e.g. Cushman, 2013; Elqayam et al., 2017; Greene et al., 2001).

Experiment 1 elaborated on this effect of fatigue on moral judgments by prompting some participants to consider the action, for example “Killing the teenager in this case…”, and other participants were prompted to consider the outcome, for example “Doing this in order to save the crowd of spectators…”. The results showed that the effect of fatigue on moral judgments replicated when participants considered the action-focused questions but was eliminated when participants were prompted to consider the outcomes of those actions. These findings corroborated the prediction that effortful cognitive resources are required in order for people to make judgments about moral violations in light of their outcomes, that is, when they must consider the causal relation between the action and the outcome, supporting model-based accounts of morality (e.g. Crockett, 2013; Cushman, 2013).

Moreover, Experiment 2 showed that the same effect applies to situations that involve self-sacrificial acts in order to help others. After reading about morally noble deeds, such as running in front of a train in order to save another person, fatigued participants whose attention was drawn to the action judged that such self-sacrificial acts were less morally required compared to non-fatigued participants. Again this difference was overcome when participants were prompted to consider the good outcome this action could have. Thus Experiments 1 and 2 demonstrated a novel effect of fatigue on
moral judgments and suggested that cognitive resources are required in order to incorporate information about beneficial outcomes when judging different moral actions; in the absence of these resources, the actions become the focus of evaluation.

Experiments 3 and 4 highlighted some nuances of the fatigue effect on moral judgment. In Experiment 3 participants were tested during the break of an evening lecture or at the end of the lecture. The difference in fatigue in these two situations was lower compared to the preceding experiments (as evidenced by the manipulation check effect sizes). There was no observed effect of fatigue on judgments that it was permissible to harm one person in order to save others, but participants who were fatigued reported feeling worse about the decisions they made than non-fatigued participants. Similarly, when the moral dilemmas involved less extreme sacrifices as in Experiment 4, for example about conservation land rather than life-and-death situations, fatigue again affected how participants felt about their decisions but not the decisions they made.

These results suggest a nuanced role for effortful cognitive processes in moral decision-making. In cases where both the level of fatigue and the decision to be made were severe, effortful cognition was required in order to judge the action in light of its outcome. In cases where the level of fatigue or the decision to be made was less extreme, effortful cognition seemed to be required in order to regulate the emotional consequences of moral decisions. These novel results have important implications for theories of moral judgment.
Theoretical Explanations. The results from Chapter 2 are incompatible with the social intuitionist model of moral judgment, which claims that moral judgments arise from unconscious intuitions without input from deliberation. Intuitionist theorists might argue that people have differing intuitive responses to questions that focus on the action and questions that focus on the outcome, and these intuitions drive judgments (e.g. Haidt, 2001; Haidt & Bjorklund, 2008). Whereas an isolated main effect of question focus on judgments would support this view, an interaction between cognitive fatigue and question focus would not be expected. In other words, this model cannot account for the finding that fatigue affected judgments about actions but not judgments about outcomes. Instead, this interaction suggests that judgments about moral violations require effortful cognition in order to also consider their outcomes. The results support the large body of other research evidencing that deliberation impacts moral judgments (e.g. Bartels, 2008).

The results are compatible with a dual-process model of moral reasoning, but not wholly so with the prominent default-interventionist account, which claims that slower, deliberative processes favour utilitarian judgments by overturning immediate emotion-based judgments (e.g. Greene et al., 2001). Whereas it could be claimed that deliberative processes overturned judgments about actions but not outcomes in Chapter 2, such an argument would be problematic because the current default-interventionist model does not account for differences in reasoning about actions and outcomes. This explanation could only be provided post-hoc; the model would need to be updated in order to predict these results. Moreover, the required evidence for such a slower
process, that is reaction time data, does not argue for exclusively utilitarian
decisions requiring more deliberation time.

The default-interventionist model claims that a slower system leads to
utilitarian judgments for dilemmas that involve personal harm (e.g. Greene et
al., 2001, 2004, 2008; Suter & Hertwig, 2011). This assertion makes the
simple prediction that participants will take longer to make utilitarian
judgments about personal dilemmas that involve causing harm to another
person using direct force than impersonal dilemmas that involve causing
indirect harm. However, the results from the pilot study add to the growing
consensus that participants do not require more time to make utilitarian
judgments about personal dilemmas: the pilot experiment showed that fatigued
participants took longer to make judgments in general, but these participants
also judged harm to be less permissible — the opposite to the prediction made
by the dual process model. Moreover, the default-interventionist account
claims that judgments about emotion-laden personal dilemmas take longer than
judgments about impersonal dilemmas, but no difference between dilemma
types was observed (see also Baron & Gürçay, 2016; Manfrinati et al., 2013;
McGuire et al., 2009; Białek & De Neys, 2016).

In its current conceptualisation, the default-interventionist model
proposes that in all cases “utilitarian judgments depend preferentially on
controlled cognitive processes” (Greene, 2009, p. 582). Experiments 1 and 2 in
Chapter 2 showed that, even when cognitively fatigued, participants could
favour utilitarian decisions that harm is permissible if it is outweighed by the
benefits when the judgment question prompted a consideration of the outcome.
In other words, less cognitive effort is required for utilitarian judgments when a model that incorporates outcome information is easier to construct.

Hence, rather than controlled reasoning overturning an immediate intuition, the results are more compatible with a model-based dual-process account that points to the importance of effortful cognition in simulating mental representations of the dilemmas in question; the results from Chapter 2 suggest that cognitive resources were required in order for participants to spontaneously consider the outcomes of actions. The data therefore support the idea that utilitarian judgments result from complex mental models of the dilemmas (e.g. Bucciarelli et al., 2008; Crockett, 2013; Cushman, 2013; Elqayam et al., 2017; see also Sloman & Lagnado, 2015). In this way, the results are consistent with recent evidence suggesting that mental representations of actions and action-outcome links are important for moral reasoning (e.g. Hannikainen et al., 2017; Miller et al., 2014).

The results of Experiments 3 and 4, which showed that fatigue affected how participants felt about their judgments, suggest that deliberative reasoning has additional functions beyond representing moral actions and their outcomes. When the level of fatigue or the severity of the dilemma was less extreme, fatigued participants made the same decisions as non-fatigued participants but felt worse about them. This novel finding suggests that the participants may have constructed models that connected actions to their outcomes in these cases but reasoning processes may be further recruited to regulate the emotional consequences of moral decision-making. One possibility is that reasoning may
be used to justify moral decisions made, which may help alleviate negative affect (e.g. Cushman et al., 2006; Haidt, 2001).

More specifically, it could be the case that thinking about alternatives to the judgments they make has implications for how people feel about those choices. When asked to think about choosing the counterfactual alternative to the typical choice in personal and impersonal dilemmas (that is, the alternative to the deontological and utilitarian judgments, respectively), people judge that these alternatives would make them feel worse (Tasso et al., 2017). A possible next step could be that comparing the worse counterfactual alternative choice to the judgment they made makes people feel better about their decision; counterfactual thoughts about how things could have been worse are well established to make people feel better (Epstude & Roese, 2008). Generating such counterfactual alternatives and comparing them to reality requires effortful cognition, and this extra cognitive effort was perhaps not possible for fatigued participants in Experiments 3 and 4.

Although this explanation is speculative, it is compatible with the idea that cognitive and intuitive processes are distinct from emotional processes when people make moral judgments, and that higher level cognitive evaluations sometimes affect the emotional responses people have to moral conflict (Bucciarelli et al., 2008). This idea also relates to the more specific finding that adolescents and adults sometimes use counterfactuals to alleviate negative affect about situations that “could have been worse” (Payir & Guttentag, 2016). The findings from Chapter 2 thus suggest a possible extension to the model-based account in order to explain how cognitive processes might influence
emotional responses. For example, in the first stage of judgment formation intuitive processes could construct a model that is a simple representation of the action; alternatively, controlled processes could construct a complex model that links the action to its outcome. Depending on the process, either the deontological or utilitarian judgment is made. (Note that this account is wholly compatible with the model-based dual process accounts.) An additional stage that requires further effortful processing may then follow in order to regulate emotions. People may imagine the counterfactual alternative outcome to the judgment they have made and they judge that this alternative would have made them feel worse (consistent with Tasso et al., 2017). Finally, people may compare this counterfactual alternative to the judgment they made, and hence feel better about their judgment, but doing so requires additional higher-level cognitive processing (consistent with the domain-general view of moral reasoning; Bucciarelli et al., 2008). This idea would of course need to be tested further in order to distinguish it from alternative views of emotion and moral judgment, for example that emotion is a heuristic responses that occurs immediately and separately, but the current evidence nonetheless suggests that cognitive processes can affect emotional responses following moral judgments.

The results from Chapter 2 suggest possible extensions to current theoretical explanations of cognitive processes in moral reasoning to account for how effortful cognition might influence emotional processes, but future research would be needed in order to directly test this idea. The key aim of these experiments was to examine how effortful cognition influences the formation of moral judgments, and the results are consistent with the idea that
moral judgments are formed from mental representations of moral actions that arise from intuitive processes, or from mental simulations of moral actions and their outcomes that arise from effortful cognition. In the next section, some alternative explanations and potential limitations to the experiments in Chapter 2 are outlined.

**Alternative Explanations and Limitations.** An alternative explanation to the idea that fatigued participants failed to construct a model that incorporated information about the outcome is that fatigued participants were simply less motivated to try to think about the moral dilemmas. However, the fatigued participants tended to judge that reasoning about the moral dilemmas was more difficult than non-fatigued participants did. This metacognitive perception of difficulty suggests they at least attempted to think about the dilemmas. In addition, the finding that fatigued participants sometimes felt worse about their judgments is difficult to reconcile with the suggestion that they were simply not attending to the dilemmas — they would need to comprehend the severity of the sacrifice in order to feel bad about their judgments.

Relatedly, one potential limitation is that the results do not shed light on the mechanisms that underlie fatigue. People may be unable to allocate more deliberative processing power to tasks when cognitively fatigued, or they may simply be reluctant to expend any more cognitive effort following these tasks (Baumeister & Heatherton, 1996; Inzlicht & Schmeichel, 2012). Although differentiating between these two possibilities would shed light on whether the effects of fatigue on judgments may be overcome with increased motivation,
this was not the aim of the current research. Moreover, whether fatigue following cognitively demanding tasks has the same effects on judgments as fatigue that might follow from physically exhausting tasks has not been tested, and the results are therefore generalisable only to cognitive fatigue.

In addition to the mechanism that underlies fatigue, one further limitation of these findings arises from using methods from the ego depletion literature. A large-scale replication project estimated that the effect of depletion may be minimal and others have claimed that many of the experiments in the literature are severely underpowered (e.g. Carter & McCullough, 2014; Hagger et al., 2016). Another issue in this literature is that researchers have rarely included any sort of manipulation checks to establish whether participants feel fatigued following the purportedly demanding task (Inzlicht, 2016). To combat these issues, the cognitively demanding tasks used in Chapter 2 cohere with more strict definitions of what is required for a task to be sufficiently demanding, that the task requires the breaking of some habit and some feature of controlled cognition (Baumeister & Vohs, 2016). Moreover, the sample size for Experiments 1 and 2 were increased to allow for the potential effect of fatigue being quite small, and in fact this increase had no effect on results. Finally, self-reported measures of perceived effort in demanding tasks moderates whether downstream effects are reliably demonstrated (Dang, 2016), and such measures were used in the experiments in Chapter 2. This research will benefit further from physiological manipulation checks for fatigue, which are currently in development (e.g. Milyavskaya, Inzlicht, Johnson & Larson, 2017). Nonetheless, the experiments in Chapter 2
show that cognitively demanding tasks can have downstream effects on cognition in experiments that (1) use tasks that are effortful, (2) are sufficiently powered to detect predicted effects and (3) measure participants’ metacognitive judgments of the difficulty of the tasks in question. With these limitations in mind, the next section outlines some practical implications from these experiments.

**Practical Implications.** The results of Experiments 1 to 4 have important implications for everyday moral reasoning. The results show that completing cognitively demanding tasks can have subsequent effects on moral judgments. Moral reasoners may benefit from this insight, especially because naïve reasoners tend to perceive moral decisions as objectively true and therefore may not suspect such an effect (Goodwin & Darley, 2008). This perceived objectivity may stem from the fact that moral principles are typically useful for guiding behaviour; in most cases it is indeed beneficial for people to find causing harm to others to be morally forbidden. Instances that involve violating such a moral principle are serious, and the finding that people intuitively do not judge such actions to be permissible may be comforting. But there may be serious and important conditions in which the violation of a moral principle is required in order to achieve better overall outcomes, and the results from Chapter 2 show that in the absence of effortful cognitive resources it is necessary for the good outcomes to be made explicit for reasoners.

For example, modern technological advancements provide a potential avenue for these findings to be applied. Some experts estimate that artificial intelligence will be responsible for most human jobs within 25 to 45 years and,
given how time-frames about these issues are usually underestimated, it could happen much sooner (e.g. Gibney, 2016; Grace, Salvatier, Dafoe, Zhang & Evans, 2017). This exponential progress in the abilities of artificial intelligence is likely to result in a new moral problem: artificial intelligence may soon be responsible for many moral decisions, inevitably with some probability for bad outcomes. For example, whether self-driving cars should be programmed to protect the passengers at all costs or whether it is permissible to risk the passenger if a larger group of pedestrians are in danger of being hit is already an important question for programmers, ethicists and policy-makers (e.g. Bonnefon, Shariff & Rahwan, 2016). Because artificial intelligence is likely to be on the whole more efficient for human safety (Gao, Hensley & Zielke, 2014), how the public respond to moral decisions made by artificial intelligence, especially those with bad outcomes, is a key concern. The results in Chapter 2 suggest that people may not always consider the overall benefits when faced with judging the violation of a moral principle, and it may be helpful therefore to make links to good outcomes explicit for naïve reasoners. Although some moral psychologists have begun to research this issue (e.g. Bonnefon et al., 2016; see also Malle, Scheutz, Forlizzi & Voiklis, 2016), the immediacy and scope of this moral concern provides a potentially fruitful venture for applied moral psychology.

The experiments in Chapter 2 identified a novel effect that influences moral judgments and elucidated some of its contours. Whereas Experiments 1, 3 and 4 examined the role of controlled cognition in dilemmas that pit causing harm against a greater good outcome, as is typical for moral psychology,
Experiment 2 focused on situations that involve morally inspirational acts. The results showed for the first time that effortful cognition plays a similar role in reasoning about these situations as it does for reasoning about moral violations. Chapters 3 and 4 extended this novel finding by investigating some of the cognitive processes that underlie thinking about good moral actions.

**Reasoning about Good Moral Actions**

As outlined in Chapter 1, the role that cognitive processes play in peoples’ judgments about the self-sacrificial actions of others has received little attention in the literature on altruism and moral psychology more generally. In the experiments reported in Chapter 3, the effect of outcome knowledge was investigated by examining judgments of whether good actions should be taken before and after participants learned about the outcome. Two experiments (Experiments 5 and 6) demonstrated an asymmetric hindsight effect on judgments about good moral actions: compared to before learning the outcome participants judged that good actions should be taken more after they learned the outcome was good but not when they learned the outcome was bad. Moreover, the same experiments demonstrated that counterfactual worse outcomes amplify judgments that the good action should be taken and semi-factual statements that keep the outcome the same diminish those judgements; and Experiments 7A and 7B showed that these effects are evident when alternatives are presented separately from outcomes.

The main task for the experiments in Chapter 3 was to read a story about a woman, Ann, who risked her life in order to save a girl, Jill.
Participants read the story and either a factual statement about the outcome, a counterfactual statement that things would have been worse had Ann not acted, or a semi-factual statement that things would have been the same even if Ann had not acted. Participants made judgments about whether Ann should have committed the action and whether the action was morally good before and after learning the outcome. The experiments showed that people altered their judgments about whether the action should have been taken based on the type of outcome knowledge they received.

Experiment 5 showed that participants tended to judge that Ann should have acted before they learned the outcome. Participants then read the good outcome that Jill survived with only minor bumps and bruises and were asked again whether they thought the action should have been taken. Their judgments that the action should have been taken were amplified when they read a factual statement about the outcome and also when they read that things would have been worse had Ann not acted, but they judged that the action should have been taken less so after reading that even if Ann had not acted, the outcome would have been the same.

Experiment 6 tested whether the same effects are found when the outcome is bad. The design was the same as Experiment 5, but participants read that Jill sustained life-threatening injuries from the accident. In contrast to Experiment 5, the results showed that reading about the bad outcome in a factual statement had no effect on participants’ judgments. Reading a statement about imagined alternatives had the same effect as before: the counterfactual statement amplified judgments that Ann should have committed.
the action and the semi-factual statement diminished their judgments. In
general, Experiments 7A and 7B replicated the effects in Experiments 5 and 6
when participants learned about the outcome separately from the factual,
counterfactual and semi-factual endings. The experiments showed for the first
time that there is an asymmetric moral hindsight effect for good moral actions,
such that people judge that good actions should be taken more when they learn
the outcome is good compared to before they know the outcome, but there is no
effect when the outcome is bad.

In Chapter 4, whether good moral actions need to turn out well in order
to elevate people and motivate them to emulate prosocial behaviour was
investigated. Three experiments (Experiments 8, 9 and 10) showed that
participants were more elevated by and more likely to engage in helping
behaviour after recalling a good moral action that succeeded compared to one
that failed. The main task for Experiments 8 to 10 in Chapter 4 required
participants to recall a past memory in which someone tried to do something
good, honourable or charitable for someone else. How morally elevating
participants judged their memory was measured, and they were then asked to
think about how that memory could have been different, either in the past or in
the future. Following this, they were asked whether what they thought about
might change their own behaviour in the future and whether they engaged in a
spontaneous helping task at the end of the experiment was also measured.

Experiments 8 and 9 showed that good actions that led to good
outcomes were more elevating than ones that led to bad outcomes, extending
the hindsight effect for good moral actions from judgments about whether an
action should be taken, demonstrated in Chapter 3, to judgments of moral elevation. The results also showed that hindsight affected whether people engage in helping behaviour while morally elevated, further extending the hindsight effect to behaviour. Participants in Experiment 8 were required to generate counterfactuals and those in Experiment 9 were required to generate pre-factuals, and the effect of hindsight on behaviour seemed to be larger in Experiment 9 compared to Experiment 8. This increase in effect could have resulted from the increased preparatory function of pre-factuals or from the larger difference in elevation between the two groups in Experiment 9.

Experiment 10 sought to clarify these differences by directly comparing counterfactual and pre-factual thinking, and it also included a control group who were instructed to reflect only on the facts. The experiment also tested whether the hindsight effect on helping behaviour applied to a condition in which people may be less likely to help: anonymous, online charity donations. The results showed that the key findings from Experiments 8 and 9 replicated in this larger, online sample. Participants were more elevated by good actions that led to good outcomes than ones that led to bad ones. Participants who thought about good outcomes were more likely to donate to charity than participants who thought about bad outcomes, and of those who did donate, the ones who had thought about a good outcome donated more money. Whether participants thought counterfactually, pre-factually or reflected on the facts showed no effects on elevation or helping.

In addition, the three experiments showed consistent patterns in the types of imagined alternatives participants generated. When the outcome was
good, participants imagined worse alternatives by deleting something from reality and when the outcome was bad participants imagined better alternatives by adding something to reality, and this was true whether participants thought counterfactually or pre-factually. Participants focused on something controllable when they thought counterfactually about a good outcome and something uncontrollable when they thought counterfactually about a bad outcome, but there was no difference between good and bad outcomes when they thought pre-factually. This difference in focus on controllability between counterfactual and pre-factual thoughts corroborates recent suggestions that there may be differences between counterfactual and pre-factual thoughts. However, the pattern was not entirely consistent with previous research, which showed that pre-factuals tend to be focused on controllable events and counterfactuals on uncontrollable (e.g. Ferrante et al., 2013; Mercier et al., 2017). Hence, the results show that when people imagine the actions of others, there may be further nuances in their imagined alternatives.

The key results from Chapter 4 support the idea that linking actions to their outcomes has important implications for moral judgments. When people thought about good actions that led to good outcomes, they gave higher judgments of moral elevation and were more likely to engage in spontaneous prosocial behaviour. This effect occurred whether they imagined alternatives or reflected on the facts, which supports the finding from Chapter 3 that reflecting on how a good action turned out well amplifies positive judgments in the same way that thinking about how things could have turned out worse does. This finding further corroborates the suggestion that the effect of
counterfactuals on judgments does not simply result from a focus on a different type of outcome, but instead results from a global consideration of how actions are linked to outcomes.

When people thought about good actions that led to bad outcomes, they were less elevated and they were less likely to engage in spontaneous prosocial behaviour, and again this was true whether they imagined alternatives or reflected on the facts. In Experiment 6 in Chapter 3, thinking counterfactually about a bad outcome amplified positive judgments, but no effect was observed on elevation and helping in Chapter 4. An important difference between Experiment 6 and Experiments 8, 9 and 10 was the nature of the alternatives people thought about when the outcome was bad. In Experiment 6, participants were directed to imagine how the bad outcome, Jill sustaining life-threatening injuries, could have been even worse, whereas in Chapter 4 participants tended to spontaneously imagine how the bad outcome could have been better. Although better alternatives sometimes provide people with a blueprint for how to act in the future (e.g. Markman et al., 1993), the results suggest that the amplification effect counterfactuals have on causality judgments may have been a stronger influence (e.g. McCloy & Byrne, 2002). These imagined better alternatives may have increased judgments of how good actions can fail, which may have hindered helping behaviour. Indeed, some participants even resolved not to act morally good in the future when they thought about how a good action had turned out badly in the past. A practical implication from these results may be to encourage people to change the types of alternatives to reality
they spontaneously imagine when good actions do not turn out as intended. The next section relates these findings to psychological theories of morality.

**Theoretical Explanations.** As outlined in Chapter 1, most of the psychological theories of moral reasoning have attempted to explain peoples’ judgments about some violation of a moral principle; few accounts have incorporated reasoning about good moral actions. In this section, how these theories would need to be extended to account for the results of Chapters 3 and 4 is outlined, and it is argued that the results support novel predictions derived from the model-based accounts of moral reasoning rather than the alternative explanations.

For example, the social-intuitionist model proposes that unconscious intuitions drive judgments about moral actions, which are then justified if necessary by effortful reasoning (Haidt, 2001). This account provides no scope for how intuitions may be updated by strengthened links to a good outcome, or indeed an account of the mechanisms that might underlie the updating of intuitions in the absence of controlled cognitive processing.

The results are instead compatible with a dual-process account of moral reasoning. For example, the default-interventionist account might predict that emotional reactions to the good action drives judgments, which then may be overturned by effortful cognition that causally connects the action to its outcome. Similarly, the model-based accounts might predict that judgments of the good action stem from a simple model that represents just the action and that when a model is constructed to connect the action to its outcome, peoples’ judgments then result from deliberative cognitive processing.
Although these explanations seem considerably similar, it should be emphasised that both have been extrapolated from the key principles of these accounts and neither currently make these predictions explicitly. Moreover, the current conceptualisation of the default-interventionist account lacks specification that details the importance of the connection between actions and outcomes, whereas this mechanism is a key component of the model-based account. Updating the default-interventionist account in this way would in essence equate it with the model-based account, with only one essential difference: the default interventionist account claims that the initial system is emotional whereas the model based account claims that the initial system is intuitive. Although the aim of this thesis was to examine how the slower, deliberate system operates, future research to differentiate between these initial processes will be needed in order for these accounts to be updated to explain how people reason about good moral actions. The results in this thesis contribute to a first step in this process.

It is argued here that the intuitive versus deliberative distinction proposed by the model-based account holds more explanatory power. The results from Chapter 4 show that when people think about good actions that lead to good outcomes, they experience more elevation than when people think about ones that lead to bad outcomes. The results from Chapter 3 show that when people think about these same events, that is events in which good

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5 The idea that the initial system is intuitive has some support from research on Moral Foundations Theory, which proposes that moral intuitions develop with inputs from culture. Peoples’ intuitive reactions to moral issues are proposed to stem from one of six moral foundations, depending on the content of the issue and the culture in which they were raised. Note that the harm/help foundation, relevant to the all the scenarios present in this thesis, is argued to be the universal moral foundation (e.g., Graham, Haidt & Nosek, 2009).
actions lead to good outcomes, they judge that good actions should be taken, and the results suggest that this is because of the causal link people think about between the action and outcome. Together the results provide tentative support for the idea that an emotional system may operate independently from the dual-process systems for decision-making (Bucciarelli et al., 2008), because the experiments suggest that experienced elevation, a core component of which is an emotional response, was impacted by whether the good action had a causal relation with a good outcome or a bad outcome. In this way, the results from Chapters 3 and 4 support the extension to the model-based accounts in order to incorporate the effects of cognitive processing on emotions outlined in the previous section. This suggestion would benefit from direct testing in future research, for example by examining whether the experience of the emotional components of elevation precedes causal connections constructed by cognitive processes. Nonetheless, the results from Chapters 3 and 4 make important contributions to the understanding of the cognitive processes that underlie reasoning about the self-sacrificial acts of others.

**Alternative Explanations and Limitations.** The key results from the experiments in Chapters 3 and 4 were interpreted to reflect a hindsight effect for good moral actions, that is that people esteem good actions that turn out well more so than those that do not turn out well. The absence of any hindsight effect on good actions that did not turn out well was interpreted to reflect some sort of credit that people might afford good moral actions; they were unwilling to be any less positive in their judgments of good actions even though they failed to bring about a good outcome in the same way that they are sometimes
unwilling to be any more lenient in their judgments of bad moral actions that fail to bring about the intended bad outcome (e.g. Lench et al., 2015). An alternative explanation, however, could be that the pattern of results instead reflects some sort of confirmation bias (e.g. Nickerson, 1998; Wason, 1966). More specifically, participants esteemed the good action in their initial judgments and when the outcome turned out well they form a meta-cognitive judgment that the good outcome confirmed the validity of their initial judgment of the action; but when the outcome did not turn out well they do not form a meta-cognitive judgment that the bad outcome disconfirmed the validity of their initial judgment. When the initial positive evaluation is validated, it may support the participant’s thought that acting morally is “right thing to do” which may prompt them to act morally themselves. In the absence of this metacognitive validation, they may be less likely to emulate the observed behaviour.

However, some key results present challenges for this interpretation. Firstly, when the post-outcome judgments that the action should have been taken made by the factual groups in Experiment 7A (in which the outcome was good) and 7B (in which the outcome was bad) were directly compared, participants in the good outcome group judged that the action should have been taken more so than those in the bad outcome group. Importantly, participants in these groups did not make any judgments prior to learning the outcome, suggesting that participants were not engaging in an evaluation of a pre-outcome judgment and assessing the outcome as confirming or disconfirming it. Secondly, the hindsight effect was extended to judgments of moral elevation
and behaviour in Experiments 8, 9 and 10, in which participants recalled memories in which they were aware of the success of the action; again judgments did not require considering new information against a pre-outcome judgment, but predictions from the hindsight effect, that good actions that had good outcomes would be esteemed more and lead to more emulation than those that had bad outcomes, were supported.

The results also suggested that some of the effects demonstrated on judgments may be sensitive to the types of alternatives that people imagine. The central manipulation in the experiments in Chapter 3 relied on presenting people with an alternative, that if Ann had not acted things would have been worse (in the counterfactual conditions) or things would have turned out the same (in the semi-factual conditions). Presenting the alternatives in this way may have imparted more certainty in them than would have been the case had participants generated their own alternatives, which may limit the generalisability of the results. The nature of the alternatives people spontaneously generate are naturally more variable than ones they are presented with, but it should be noted that the results from Chapter 4, in which participants were free to generate their own alternatives, supported some key predictions from the findings in Chapter 3.

One aim of the experiments in Chapter 4 was to determine whether hindsight affects future behaviour, but the intentions people reported in the experiments in Chapter 4 have not been discussed in detail because of potential limitations that hinder strong inferences. The pattern of responses varied unsystematically across the experiments: in Experiment 8, participants who
thought about bad outcomes reported having intentions to act differently in the future more so than those who thought about good outcomes; in Experiment 9, participants who thought about good outcomes reported having intentions to act differently more so than those who thought about bad outcomes; and in Experiment 10, all groups reported intentions at chance level. Self-reported intentions can be unreliable predictors of future behaviour (e.g. Ajzen, Brown & Carvajal, 2004; Sheeran, 2002), and so the results from the behaviour measures are taken as stronger evidence of the behavioural consequences of the moral hindsight effect for good actions. With these potential limitations in mind, the next section suggests some practical implications.

**Practical Implications.** Responses to the morally good deeds of others have been identified as a key factor in promoting good moral behaviour — it is well established that when people witness or learn about the good moral actions of others they are motivated to commit good acts themselves and often do so (Pohling & Diessner, 2016). The results from Chapters 3 and 4 show that these acts must turn out well in order for people to become inspired and emulate those actions, and this knowledge may help others promote prosocial behaviour.

For example, marketing campaigns for charities are often based on psychological research, such as the identifiable victim effect that shows that people are more likely to donate to charity when they are shown one individual in need compared to a larger group of people (e.g. Jenni & Loewenstein, 1997). Moral elevation has been established as an effective method of inspiring good behaviour. For example, Experiment 10 supports other research that shows that
elevation can inspire people to donate money to charity (e.g. Aquino et al., 2011; Freeman et al., 2009). It may be useful for effective charities to be transparent about the good outcomes their work achieves in order to inspire future donations.

**General Implications for Moral Psychology**

The results in this thesis have provided two core contributions to the understanding of moral reasoning. The first is that deliberative reasoning can have a direct influence on moral judgments that people make. The results suggest that the role of effortful cognition is to construct more complex models of the situations being reasoned about, that is whether considerations of actions drive judgments or whether the action is connected to its outcome. Secondly, the results show that similar processes can apply to reasoning about moral violations and reasoning about good moral actions.

The results presented in this thesis, especially in Chapters 2 and 3, inform the debate on the role of effortful cognitive processes in moral reasoning. The novel effects demonstrated in this thesis, the effect of fatigue on moral judgments and the hindsight effect for morally good actions, complement other research that shows how effortful processes are associated with favouring outcomes (e.g. Conway & Gawronski, 2013; Elqayam et al., 2017). Hence, the key findings suggest that moral judgments do not always rely on automatic, unconscious processes, contra to intuitionist theories and the linguistic analogy (Haidt, 2001; Mikhail, 2007).
The results do not only provide evidence against the idea that judgments rely solely on automatic processes, but also suggest how deliberative reasoning affects judgments. Rather than supporting the view that immediate reactions are overturned by a slower, controlled process, the evidence points to the importance of mental representations of key features of moral events; throughout the empirical chapters it has been shown that people reason differently about actions when information about outcomes is incorporated. The results support the idea that peoples’ mental representations of key features of moral events are important for the judgments they make (Bucciarelli et al., 2008; Crockett, 2013; Cushman, 2013).

Chapters 3 and 4 extended Chapter 2, in that they dealt with some specifics of deliberative reasoning processes applied to good moral actions rather than moral violations. Moral psychology has largely been focused on the judgments people make about moral violations, especially those that could have beneficial outcomes (e.g. Cushman, 2013; Greene et al., 2001; Mikhail, 2007). Indeed, very few theories have incorporated reasoning about morally good actions into their formulation (but see Bucciarelli et al., 2008). The experiments presented in this thesis are among the first to investigate reasoning processes in the kinds of situations that evoke moral elevation, and showed that the connection a morally good action has to its outcome is important for judgments that the action ought to be taken and the experience of elevation, and to helping behaviour.

The results therefore require a theory of morality that incorporates not only reasoning processes about moral violations, but also how people reason
about good moral actions. The independent processes view of moral reasoning emphasises parallel and interacting roles of emotional and reasoning systems in conflict situations, involving both moral and immoral acts, which allows for the types of situations participants reasoned about in this thesis (Bucciarelli et al., 2008). The dual-process, model-based accounts of morality emphasise how people sometimes reason based on models that feature the action and other times they reason about more cognitively demanding models that connect the action to its outcome (Crockett, 2013; Cushman, 2013). The results presented in this thesis suggest that people may engage in effortful reasoning about moral dilemmas that make them feel bad or morally noble deeds that make them feel good, and it functions to allow them to create more complex representations of the situations being reasoned about. Hence, the results in this thesis suggest that the former account provides the basis for the role that cognitive processing plays in moral reasoning and the latter accounts specify how cognitive processes are applied to normative judgments about moral actions.

More broadly, the results support the domain general approach to moral psychology. Fatigued general cognitive resources (Experiments 1 to 4) and the effects of outcome knowledge and imagined alternatives (Experiments 5 to 10) altered the moral judgments people made about good and bad moral actions in ways that would not be predicted from the idea that there are domain-specific rules that apply exclusively to moral contents. The results therefore support the theoretical standpoint that people rely on similar mental processes when reasoning about moral contents as they do for reasoning about non-moral
Future Directions

The results in this thesis have contributed to a better understanding of how effortful cognition influences the formation of moral judgments, but they have also suggested a possible mechanism through which effortful cognition might interact with emotional processes. The role of emotions in moral judgments has been assumed to be precursory, and indeed a large body of research supports this idea (e.g. Valdesolo & DeSteno, 2006). But, considered with some recent evidence that suggests a role for cognitive processes influencing emotions, a novel prediction for how effortful cognition might regulate emotional consequences of moral decision-making is proposed: people may compare the counterfactual option to the judgment they made in order to make themselves feel better about their chosen option. Future research will be needed to test this prediction, but affirmative support would be consistent with the extended model-based account described previously and further strengthen the support for the independent processes account and domain general views of moral reasoning.

The judgments examined in this thesis have largely been influenced by normative ethics: the primary judgments of interest were the permissibility of moral violations and judgments about whether good actions should be taken. Of course, such an approach is of key importance for understanding human morality — people are exceptionally concerned with what others should and
should not do; religions, constitutions and human rights declarations are built on these types of considerations. But future research might also benefit from taking inspiration from virtue ethics, which is concerned with judgments of virtues or moral character.

Focus on judgments of moral character has grown in recent empirical moral research (Goodwin, Piazza & Rozin, 2014; Pizarro & Tannenbaum, 2011; Robinson, Page-Gould & Plaks, 2017). The results from this thesis show that effortful reasoning links actions to their outcomes and this link can have effects on judgments about whether the action should be taken. Future research might investigate whether people make different judgments about moral agents depending on their effectiveness, for example whether agents are praised more for attempting good actions when they are successful compared to when they fail. Moral character judgments are sometimes more important than competency-based judgments, but research on moral competence has assessed knowledge of right and wrong rather than the success of moral actions (e.g. Gini, Pozzoli & Hauser, 2011; Wojciszke, Bazinska & Jaworski, 1998). Since morally good actions in Chapters 3 and 4 were judged more favourably when the outcome was good, analogous differences in character judgments of morally good agents who succeed and who fail could be expected.

The experiments in this thesis also examined the role of cognitive processing in individual reasoning, that is, in how participants came to personal conclusions about the moral judgment question. Although understanding these “intrapersonal” reasoning processes is clearly important for moral psychology research, focus on “interpersonal” reasoning between individuals has received
little empirical focus (but see Mercier, Castelain, Hamid & Marin-Picado, 2017). An examination of interpersonal reasoning processes about moral matters, for example moral argumentation or negotiation, may provide valuable insight into the factors that people consider when reasoning intrapersonally. Such an approach may be of particular practical importance as heated debates on moral matters such as immigration, xenophobia and torture become more frequent. Although there is scope for much more research on moral psychology, the results from this thesis have contributed to how psychology understands some of the underlying cognitive processes.

**Conclusion**

This thesis aimed to advance understanding of cognitive processes in moral reasoning, and a number of unique contributions to the literature achieve this aim. The results showed a novel effect of fatigue on reasoning about moral violations, which affected reasoning about actions but was overcome when good outcomes were made salient. An asymmetric moral hindsight effect for good actions was shown for the first time, which was found to affect judgments about whether good actions should be taken as well as how elevated people are by good actions and whether they engage in helping behaviour. The results make some of the first steps to understanding cognitive processes in reasoning about self-sacrificial good moral actions, which show similar effects to reasoning about bad actions, for example a counterfactual amplification effect on judgments was demonstrated, and similar effects to reasoning about non-moral contents, for example a semi-factual diminishment effect was evidenced.
The findings provide support for the idea that domain-general cognitive processes help people to simulate mental representations of action-outcome links when making moral judgments.
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## Appendices

### Table of Contents

**Appendix 1.1** – Moral Elevation Review ................................................................. 248

**Appendix 1.2** – Detailed descriptions from Table A1 ................................. 254

**Appendix 2.1** – Experimental Materials for Chapter 2 .................................. 258

**Appendix 2.2** – Supplementary Analyses for Chapter 2 ............................ 271

**Appendix 2.3** – Information and Debrief Sheets for Chapter 2 ................. 275

**Appendix 2.4** – Ethical Approval for Experiments 1 to 4 ......................... 280

**Appendix 3.1** – Experimental Materials for Chapter 3 .............................. 281

**Appendix 3.2** – Supplementary Analyses for Chapter 3 ............................ 282

**Appendix 3.3** – Information and Debrief Sheets for Chapter 3 ................. 283

**Appendix 3.4** – Ethical Approval for Experiments 5 to 10 ......................... 284

**Appendix 3.5** – Moral Elevation Pilot Experiment ........................................ 285

**Appendix 4.1** – Experimental Materials for Chapter 4 .............................. 292

**Appendix 4.2** – Supplementary Analyses for Chapter 4 ............................ 295

**Appendix 4.3** – Information and Debrief Sheets for Chapter 4 ................. 298
Appendix 1.1 – Moral Elevation Review

The aim of this review was to examine the experimental materials used in moral elevation research. Searches were conducted using the combined PsychINFO and PsychArticles database, Science Direct, Web of Science and Google Scholar. As only studies that involved experimentally manipulating moral elevation were relevant, only one specific search term was used in each database: “moral elevation” in Title OR “moral elevation” in Abstract.

The search resulted in 35 studies, which were then analysed further in order to determine whether they involved an experimental manipulation of moral elevation. One study was removed as it did not relate to moral elevation in sufficient detail. Of the remaining 34 studies, 22 involved manipulating moral elevation in at least one experiment (the remaining 12 were related to moral elevation but did not involve experimental manipulation).

As Table A1 shows, the most common form of manipulation in the published literature was video, which was present in 15 (68%) of the studies, followed by text (n = 6; 27%) and recalling a past memory (n = 3; 14%). Writing events in a diary as they occurred, taking part in volunteering and listening to audio descriptions were used in one study each. (Note that summed percentages do not equal 100 as some studies used more than one type of manipulation.)

In order to test whether the manipulation induced an experience of moral elevation, the majority of studies included some sort of post-test measure (86%). However, how this was done varied across experiments, as Table A2 shows. The exact items used differed substantially but overall could be classified within four main categories: emotional responses (e.g. awe), cognitive appraisals (e.g. “There is still some good in the world”), physiological reactions (e.g. “warm chest”) and motivation to act morally good (e.g. “I want to be more like the person in the story”). Responses were usually recorded using Likert scales, most commonly from 1 (not at all elevated) to 7 (very much/extremely elevated). Whether responses were averaged onto the specific factor of elevation they reflected (e.g. emotion, cognitions, physiology and motivations) or onto a global measure of elevation also varied.

Table A1
Summary of Materials Used in Moral Elevation Research

<table>
<thead>
<tr>
<th>Study</th>
<th>Format</th>
<th>Description</th>
<th>Procedure</th>
</tr>
</thead>
<tbody>
<tr>
<td>Algoe and Haidt (2009)</td>
<td>Recall; Video; Diary</td>
<td>(V) Trevor, homeless shelter</td>
<td>(R) Classroom; (V) Laboratory; (D) Classroom</td>
</tr>
<tr>
<td>Aquino, McFerran, and Laven (2011)</td>
<td>Story; Recall; Video</td>
<td>(S) Amish Community; (V) World On Fire, Sarah McLachlan</td>
<td>(S; R) Online; (S; V) Laboratory</td>
</tr>
<tr>
<td>Authors</td>
<td>Type</td>
<td>Video/Recall/Story</td>
<td>Description</td>
</tr>
<tr>
<td>-------------------------------</td>
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<td>--------------------</td>
<td>-----------------------------------------------------------------------------</td>
</tr>
<tr>
<td>Bailey and Wojdynski</td>
<td>Video</td>
<td>(V) Firefighter, Rescue Me</td>
<td>(V) Laboratory</td>
</tr>
<tr>
<td>Cox (2010)</td>
<td>Action</td>
<td>(B) Volunteering trip</td>
<td>(A) Real-life</td>
</tr>
<tr>
<td>Diessner, Iyer, Smith,</td>
<td>Video</td>
<td>(V) Home-Run</td>
<td>(V) Online</td>
</tr>
<tr>
<td>and Haidt (2013)</td>
<td></td>
<td></td>
<td>(V) Laboratory</td>
</tr>
<tr>
<td>Englander, Haidt, and Morris</td>
<td>Video</td>
<td>(V) Marching band;</td>
<td>(V) Laboratory</td>
</tr>
<tr>
<td>(2012)</td>
<td></td>
<td>Basketball Game; Subway</td>
<td></td>
</tr>
<tr>
<td>Farsides, Pettman, and</td>
<td>Video</td>
<td>(V) Dead Poet’s Society; Medical Dramas</td>
<td>(V) Classroom</td>
</tr>
<tr>
<td>Tourle (2013)</td>
<td></td>
<td></td>
<td>(V) Laboratory</td>
</tr>
<tr>
<td>Freeman, Aquino, and McFerran</td>
<td>Video; Story</td>
<td>(V) Amy, 60 Minutes II; Joel, Public Eye; (S) Amish Community</td>
<td>(V) Online, Laboratory; (S) Laboratory</td>
</tr>
<tr>
<td>(2009)</td>
<td></td>
<td></td>
<td>(V) Online</td>
</tr>
<tr>
<td>Lai, Haidt, and Nosek (2014)</td>
<td>Video</td>
<td>(V) Mentor; Home-Run; Subway</td>
<td>(V) Online</td>
</tr>
<tr>
<td>Oliver et al. (2015)</td>
<td>Video</td>
<td>(V) Free hugs; Musicians</td>
<td>(V) Online</td>
</tr>
<tr>
<td>Piper, Saslow, and Saturn (2015)</td>
<td>Video</td>
<td>(V) Mentor; Home-Run</td>
<td>(V) Laboratory</td>
</tr>
<tr>
<td>Romani and Grappi (2014)</td>
<td>Story</td>
<td>(S) Company Practices</td>
<td>(S) Real-life</td>
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<tr>
<td>Sakai et al. (2016)</td>
<td>Video</td>
<td>(V) Subway</td>
<td>(V) Laboratory</td>
</tr>
<tr>
<td>Schnall, Roper, and Fessler</td>
<td>Video</td>
<td>(V) Mentor</td>
<td>(V) Laboratory</td>
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<tr>
<td>(2010)</td>
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<td></td>
<td>(V) Laboratory</td>
</tr>
<tr>
<td>Schnall and Roper (2012)</td>
<td>Video</td>
<td>(V) Mentor</td>
<td>(V) Laboratory</td>
</tr>
<tr>
<td>Siegel, Thomson, and Navarro</td>
<td>Recall</td>
<td></td>
<td>(R) Online</td>
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<tr>
<td>(2014)</td>
<td></td>
<td></td>
<td>(V) Laboratory</td>
</tr>
<tr>
<td>Silvers and Haidt (2008)</td>
<td>Video</td>
<td>(V) Mentor</td>
<td>(V) Laboratory</td>
</tr>
<tr>
<td>Strohminger, Lewis, and Meyer</td>
<td>Audio</td>
<td>(A) Chicken Soup for the Soul</td>
<td>(A) Laboratory</td>
</tr>
<tr>
<td>(2011)</td>
<td></td>
<td></td>
<td>(V) Laboratory</td>
</tr>
<tr>
<td>Thomson and Siegel (2013)</td>
<td>Story</td>
<td>(S) Waitress</td>
<td>(S) Online</td>
</tr>
<tr>
<td>Thomson, Nakamura, Siegel,</td>
<td>Story</td>
<td>(S) Waitress</td>
<td>(S) Online</td>
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<tr>
<td>and Csikszentmihalyi (2014)</td>
<td></td>
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<td>(S) Online</td>
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<tr>
<td>Van de Vyver and Abrams (2015)</td>
<td>Video</td>
<td>(V) Blind-man</td>
<td>(V) Online; Laboratory</td>
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<tr>
<td>Vianello, Galliani, and Haidt (2010)</td>
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<tr>
<td>Story (S) Massimo Castelli (S) Booklet</td>
<td></td>
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</tr>
</tbody>
</table>

*Note.* A = audio; B = behaviour manipulation; D = diary; R = recall; S = story; V = video.

Additional information relating to the descriptions is available in Appendix 1.2.
### Table A 2.

**Summary of Measures Used in Moral Elevation Research.**

<table>
<thead>
<tr>
<th>Study</th>
<th>Emotion</th>
<th>Cognition</th>
<th>Physical</th>
<th>Motivation</th>
<th>Scale</th>
</tr>
</thead>
<tbody>
<tr>
<td>Algoe &amp; Haidt (2009)</td>
<td>Admiration</td>
<td>Being like the other person</td>
<td>Lump in throat</td>
<td>E, P: 0 (not at all) to 6 (very much)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Respect</td>
<td>Getting to know the other person</td>
<td>High energy</td>
<td>M: -4 (much less) to 0 (no change) to +4 (much more)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Moved</td>
<td>Being a better person</td>
<td>Increased heart rate</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Inspired</td>
<td></td>
<td>Chills</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Awe</td>
<td></td>
<td>Light/bouncy</td>
<td></td>
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<tr>
<td></td>
<td>Gratitude</td>
<td></td>
<td>Laughter</td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>Love</td>
<td></td>
<td>Muscles relaxed</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Aquino et al.</td>
<td>Compassion</td>
<td>Making sure the other person is taken care of in the future</td>
<td>‘Rising’ or ‘open’ chest</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(2011)</td>
<td>Awe</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Admiration</td>
<td></td>
<td></td>
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<td></td>
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<tr>
<td>Bailey &amp; Wojdynski</td>
<td>Schnall et al. (2010)</td>
<td></td>
<td>I want to be more like the person/people in the story</td>
<td>E: 1 (not at all) to 5 (very much)</td>
<td></td>
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<tr>
<td></td>
<td>Schnall et al. (2010)</td>
<td></td>
<td>The person/people in the story have shown me how to be a better person</td>
<td>C: M: 1 (never) to 5 (always)</td>
<td></td>
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<tr>
<td></td>
<td>Schnall et al. (2010)</td>
<td></td>
<td>I am going to try to follow the story’s example</td>
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<tr>
<td></td>
<td>Schnall et al. (2010)</td>
<td></td>
<td>I need to do more to help other people</td>
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<tr>
<td></td>
<td>Schnall et al. (2010)</td>
<td></td>
<td>I can learn a lot from the person/people in the story</td>
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<tr>
<td></td>
<td>Schnall et al. (2010)</td>
<td></td>
<td>The person/people in the story are my new role model</td>
<td></td>
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<tr>
<td>Source</td>
<td>Emotions/Responses</td>
<td></td>
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<tr>
<td>Diessner et al. (2013)</td>
<td>Awe, Admiration, Gratitude, Love, Lump in throat, Be a better person</td>
<td></td>
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<tr>
<td>Freeman et al. (2009)</td>
<td>Compassion, Inspired, Admiration, People are really good, Widthness/‘bouncy’, Lightness/‘bouncy’, Increased heart rate, Blushing, Warmth in chest, Tears in eyes, Lump in throat, Chills/tingles, I wish I was a better person, I want to be more like the person/people in the story, The person/people in the story have shown me how to be a better person, The person/people in the story are my new role models</td>
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<tr>
<td>Lai et al. (2014)</td>
<td>How much did this video affect you emotionally?, To what extent did you feel morally uplifted while watching this video? 1 (not at all) to 5 (very much)</td>
<td></td>
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</tr>
<tr>
<td>Oliver et al. (2015)</td>
<td>Happiness/joy, Warmth, Inspiration, Admiration, Hope, Tenderness, Uplifted, Awe, Amused, Sympathy/compassion, Optimistic about humanity, Eyes watering/tearing up, Lump in throat, Chokked up, Warm/expansive feelings in chest, Heart beating faster, Change in breathing, Chills, goose bumps or tingling on skin, Laughter</td>
<td></td>
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<tr>
<td>Romani &amp; Grappi (2014)</td>
<td>Touched, Inspired, Moved, 1 (very weak) to 7 (very strong)</td>
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<td></td>
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<tr>
<td>Schnall et al. (2010)</td>
<td>Moved, Uplifted, Optimistic about humanity, Warm feeling in chest, Wanted to help others, Want to become a better person, 1 (didn’t feel at all) to 9 (felt very strongly)</td>
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<tr>
<td>Siegel et al.</td>
<td>Thomson and Siegel, Thomson and, Thomson and, Thomson and</td>
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<tr>
<td>Silvers &amp; Haidt (2008)</td>
<td>Pleasant</td>
<td>Interesting</td>
<td>1 (not at all) to 7 (extremely)</td>
<td></td>
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<tr>
<td></td>
<td>Emotionally affecting</td>
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<tr>
<td></td>
<td>Touched/inspired</td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>Amused</td>
<td></td>
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<tr>
<td></td>
<td>Happy/joyful</td>
<td></td>
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<td></td>
<td>Calm/content</td>
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<tr>
<td></td>
<td>Sad</td>
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<td></td>
<td>Angry</td>
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<td></td>
<td>Disgusted</td>
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<tr>
<td></td>
<td>Afraid</td>
<td></td>
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</tr>
<tr>
<td>Strohminger et al. (2011)</td>
<td>Did you have positive or negative feelings when listening to this clip?</td>
<td>How engaging did you find this clip?</td>
<td>Did you find your eyes tearing up when listening to this clip?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Thomson &amp; Siegel (2013)</td>
<td>All from Aquino et al. (2011)</td>
<td>All from Aquino et al. (2011)</td>
<td>Lump in throat</td>
<td>All from Aquino et al. (2011)</td>
<td>1 (not at all) to 7 (very much)</td>
</tr>
<tr>
<td>Van de Vyver &amp; Abrams (2015)</td>
<td>Inspired</td>
<td>Awe</td>
<td>Admiration</td>
<td>Uplifted</td>
<td>1 (not at all) to 9 (very much)</td>
</tr>
<tr>
<td>Vianello et al. (2010)</td>
<td>I felt more open towards others</td>
<td>Warmth in chest</td>
<td>Lump in throat</td>
<td>Do something good for other people</td>
<td></td>
</tr>
<tr>
<td></td>
<td>I feel like I’m a better person</td>
<td>Muscles relaxed</td>
<td>Behave as Massimo Castelli</td>
<td>Become a better person</td>
<td></td>
</tr>
</tbody>
</table>
Appendix 1.2 – Detailed descriptions from Table A1

Action
Volunteering
The students participated in a weeklong spring-break service trip to Nicaragua through an organization entitled Nicaraguan Orphan Fund.

Audio
Chicken Soup for the Soul
Inspirational stories were extracted from a volume of Chicken Soup for the Soul (Canfield, 2002). Audio clips based on these extracts were recorded by four student actors (two male, two female).

Stories
Amish Community
A newspaper story about an Amish community responded with extraordinary grace and forgiveness after Charles Roberts opened fire in an Amish schoolhouse, killing five girls and injuring another five before killing himself. The story described how hours after their children were killed, several Amish went to see Roberts’s widow to offer forgiveness and express sympathy and later offered financial assistance to her and her children.

Company Practices
A detailed description of the company and its responsible social actions. The positive narrative concerned a company operating in organized distribution that set up a support scheme for the community called the “time bank”. Although the name of the company was fictitious, the narrative was based on real corporate practices actually realized by existing companies at a certain time.

Waitress
Shelly and Lee went out on a date at a restaurant. Their food was delicious, they had a great conversation, and they enjoyed their experience. During the course of the meal, both Shelly and Lee overheard their waitress talking. She was discussing how she cannot afford the Christmas gifts she put on layaway for her two small children because [Good 1: two parties walked out on their check and she had to pay for it out of her tips/Bad 1: she had spent all of her tips buying expensive designer clothing for herself].

Unfortunately, her kids have been looking forward to these presents for months. As the meal comes to an end, the waitress brings the bill to the table. Shelly wonders how much Lee is going to tip the waitress because he is normally so giving. Tipping the waitress well might help her to afford the Christmas gifts for her children because [Good 2: two parties
walked out on their check and she had to pay for it out of her tips/Bad 2: she had spent all of her tips buying expensive designer clothing for herself].

As Shelly goes to the restroom, Lee tells her that he will take care of the bill while she is gone. When Shelly comes out of the restroom, Lee is waiting at the door for her. At the end of the night, Lee walks Shelly to her door. As he starts heading home, Shelly notices that Lee is not walking in the direction of the nearest train station as she expected. She calls to him, ‘Wait Lee, why are you headed that way?’ Lee replies:

I have to walk home because I cannot afford the train, and certainly not a taxi. I gave every thing I had to the waitress. Her kids did not do anything wrong, why should they be sad this Christmas? She promised she would spend every cent I gave her on her kids. As we were leaving I heard her talking to her kids on the phone. Good 3: I heard how happy she was that she could give them the gifts she promised./Bad 3: She said she knew the exact outfit she was going to buy herself. I realized, she had no intentions of buying her kids the gifts she promised.

Lee paused, ‘Obviously, I have no regrets. I did what I could to make two children a little happier.’

Videos
Amy Biehl, 60 Minutes II
The clip told the story about how Amy Biehl—a White, U.S. college student—was murdered by Black youths in South Africa while she was working on a Fulbright scholarship to help end apartheid. Rather than seeking vengeance, Amy’s parents responded to her death by establishing and funding the Amy Biehl Foundation to continue her work. Through the foundation, the Biehls have funded 15 programs and helped thousands of poor Black South Africans to better their lives, including two of the youths convicted of murdering Amy.

Basketball Game
A boy with autism participates in a basketball game.

Blind Man
A video describing the story of a man (Richard Moore) who was shot between the eyes with a rubber bullet, which permanently blinded him at the age of 10. It shows how he was able to forgive the perpetrator, take a positive attitude to life, and spend his life helping others.

Dead Poet’s Society
A 10-minute clip extracted from the film Dead Poets’ Society (Weir, 1989) showed an inspirational empathic teacher (Robin Williams) telling pupils they should “seize the day.” The emphasis throughout is that the teacher cares about each and every student and wants to inspire them to do what they think is valuable, in their own way; just as he is modelling.
Firefighter, Rescue Me
New York City firefighter, Tommy, struggles through a divorce and flashbacks of 9/11 where he lost his cousin at the Twin Towers. He is seen reflecting on his own heroic efforts with sadness.

Free Hugs
A man offering “free hugs” to passersby on a pedestrian street.

Home-Run
Video of Sara Tucholsky hitting a homerun, but damaging her knee at first base. In the Tucholsky video, two members of the opposing team then altruistically and spontaneously carried her around the bases so that she could score her first and final homerun of her college baseball career, even though that homerun helped contribute to their own team losing the game. https://www.youtube.com/watch?v=jocw-oD2pgo

Joel, Public Eye
The clip told the story about how, at the age of 22 months, Joel was badly burned and disfigured when a tractor trailer slammed into his family’s car. Joel lost his toes, fingers, and one hand in the accident; he was forced to endure 45 surgeries and “grow up in a world that did not welcome him.” Police suspected that the driver—Reginald Dort—was trying to hit a female acquaintance with his truck. Dort jumped bail after the accident and fled to Canada where he continued driving a truck before he was apprehended more than a decade later. The video showed what transpired in a courtroom during Dort’s sentencing when Joel and other members of the Sonnenberg family offered forgiveness.

Marching Band
A young man who was born blind and crippled learns to play the piano beautifully and is able to participate in a marching band.

Medical Dramas
A number of 30 second to 3 minute clips from medical dramas, e.g., ER, Patch Adams. Clips showed doctors who appeared highly empathic and altruistic toward the vulnerable and needy: They reacted emotionally to others’ subjective experiences, welcomed their own vicarious emotions, and both expressed and were guided in their actions by these “other-focused” emotions.

Mentor
A clip from The Oprah Winfrey Show in which a musician talks appreciatively about his former music teacher and mentor, who saved him from a life of gang activity; available at https://osf.io/fg5xb/

Musicians
A video depicting street musicians from various locations playing the same song simultaneously.

Subway
A news clip of a man who saved someone who had a seizure and fell onto the subway tracks; available at https://osf.io/fg5xb/

Trevor, homeless shelter
A young man named Trevor who, as a boy, had established a homeless shelter in Philadelphia.

World on Fire, Sarah McLachlan
The video describes how all but $15 of the $150,000 budget for the video was donated to various charitable causes around the world. The video then chronicles how the money benefited the impoverished communities that received the money versus how it could have been spent creating a regular music video. https://www.youtube.com/watch?v=FDmPcSWE0WU
Appendix 2.1 – Experimental Materials for Chapter 2

Fatiguing Task: Pilot Study and Experiment 4
In the space provided below, please write a short essay about a trip you have taken. It may be a trip to the shop, to Cork or to another country – wherever! Very important!

[Please do not use the letters a or n anywhere in your story. If you find yourself using words that have the letters a or n in them, please stop and try to find a different way to express yourself. Please write until the experimenter asks you to stop.]

Fatiguing Task: Experiments 1 and 2
Paragraph 1
There are two main ways in which a distribution can deviate from normal: (1) lack of symmetry (called skew) and (2) pointiness (called kurtosis). Skewed distributions are not symmetrical and instead the most frequent scores are clustered at one end of the scale. So, the typical pattern is a cluster of frequent scores at one end of the scale and the frequency of scores tailing off towards the other end of the scale. A skewed distribution can be either positively skewed (the frequent scores are clustered at the lower end and the tail points towards the higher or more positive scores) or negatively skewed (the frequent scores are clustered at the higher and the tail points towards the lower or more negative scores).

Paragraph 2
Distributions also vary in their kurtosis. Kurtosis refers to the degree to which scores cluster at the ends of the distribution (known as the tails) and how pointy a distribution is. A distribution with positive kurtosis has many scores in the tails (a so-called heavy-tailed distribution) and is pointy. This is known as a leptokurtic distribution. In contrast, a distribution with negative kurtosis is relatively thin in the tails (has light tails) and tends to be flatter than normal. This distribution is called platykurtic. In a normal distribution the values of skew and kurtosis are 0 (i.e. the tails of the distribution are as they should be). If a distribution has values of skew or kurtosis above or below 0 then this indicates a deviation from normal.

(Note that both paragraphs were presented in .png format to prevent participants from copying and pasting the text.)
Mood Scale: Pilot Study, Experiments 1, 2, 3 and 4

Brief Mood Introspection Inventory (Mayer & Gaschke, 1988):
Circle the response on the scale below that indicates how well each adjective or phrase describes your present mood.

<table>
<thead>
<tr>
<th>Definitely do not feel</th>
<th>Do not feel</th>
<th>Slightly feel</th>
<th>Definitely feel</th>
</tr>
</thead>
<tbody>
<tr>
<td>XX</td>
<td>X</td>
<td>V</td>
<td>VV</td>
</tr>
</tbody>
</table>

Lively | XX X V VV | Drowsy | XX X V VV |
Happy | XX X V VV | Grouchy | XX X V VV |
Sad | XX X V VV | Peppy | XX X V VV |
Tired | XX X V VV | Nervous | XX X V VV |
Caring | XX X V VV | Calm | XX X V VV |
Content | XX X V VV | Loving | XX X V VV |
Gloomy | XX X V VV | Fed Up | XX X V VV |
Jittery | XX X V VV | Active | XX X V VV |

Moral Dilemmas: Pilot Study, Experiments 1 and 3

The 8 scenarios used in pilot study are provided with the personal version first and then the impersonal version in italics. The 4 asterisked scenarios were also used in Experiment 1 and 3.

911*
You are the sole paramedic riding on a rescue helicopter, responding to a devastating flood. You have rescued several critically injured people and you have been treating them to keep them alive. Suddenly, the pilot notifies you that there has been an engine malfunction and the helicopter can no longer support the weight of you, the pilot, and all the injured people on board. If the helicopter crashes, it will kill everyone on board including you, the pilot, and the injured people that you have rescued. In order to avoid a crash that will kill everyone on the helicopter, you realize that you must lighten the load enough to keep the helicopter aloft. Because all of the equipment is bolted down, and you are needed by the injured people, the only way to do this is to throw one of the injured people off the helicopter. This will lighten the load enough to keep the helicopter in the air but will cause the death of the person thrown off. Killing this injured person in order to save yourself and everyone else on board is morally…/is to cut the cable that is raising up the last rescued person. This will lighten the load enough to
keep the helicopter in the air, but it will also kill the person who is dropped. Killing this injured person in order to save yourself and everyone else on board is morally...

Vaccine*
A viral epidemic has spread across the globe killing thousands of people. You are a medical researcher and have developed two substances in your laboratory. You know that one of them is a vaccine, but you don’t know which because both of the vials have been mislabelled as vaccine. You also know that the other substance is deadly. Once you figure out which substance is the vaccine you can create more to save thousands of lives. You have two lab assistants who work with you, and the only way to identify the vaccine with certainty is to inject the two substances into these people against their wishes. One person will live, the other will die, and you will be able to start saving lives with your vaccine. Killing one of these people with a deadly injection in order to identify a vaccine that will save thousands of lives is morally…/You could allow another doctor to inject two unsuspecting patients, who need the vaccine, with these substances without warning them or the doctor that one of the substances is deadly. One person will live, the other will die, and you will be able to start saving lives with your vaccine. Letting one of these people die from a deadly injection in order to identify a vaccine that will save thousands of lives is morally...

Cinder block*
You are the explosives expert for a company that has been hired to demolish a skyscraper. You are examining the last of the explosive charges when you notice a teenager below who is about to accidentally detonate one of the charges out of sequence. This explosion will result in the building’s uncontrolled collapse onto you, the teenager, and the crowd of spectators. The teenager is several floors below you and cannot hear you because of the loud demolition noise. You realize that the only way to stop the teenager from detonating the charge is to drop a heavy cinder block on his head. This will crush his skull and kill him almost instantly but will prevent the out-of-sequence explosion. Killing the teenager in order to save yourself and the crowd of spectators is morally…/is to charge is to flip a switch that reactivates the building’s electricity. Because he is touching an open circuit, this will electrocute him but will prevent the explosion. Killing the teenager in order to save yourself and the crowd of spectators is morally...

Crying Baby*
Enemy soldiers have taken over your village. They have orders to kill all remaining civilians over the age of two. You and some of your townspeople have sought refuge in two rooms of the cellar of a large house. Outside you hear the voices of soldiers who have come to search the house for valuables. Your baby, who is with you in the room, begins to cry loudly. You put your hand over his mouth to block the sound. If you remove your hand from his mouth his crying will summon the attention of the soldiers who will spare your baby’s life, but will kill
you and the others hiding in both rooms. To save yourself and the others you must keep your hand on his mouth and smother your baby to death. Smothering your baby in order to save yourself and the other townspeople is morally:

/Your baby begins to cry loudly in the other room. His crying will summon the attention of the soldiers who will spare your baby’s life, but will kill you and the others hiding in both rooms. If you turn on the noisy furnace to block the sound, the other room will become uncomfortably hot for adults and children, but deadly for infants. To save yourself and the others you must activate the furnace, which will kill your baby. Overheating your baby in order to save yourself and the other townspeople is morally...

Ebola
You are a Peace Corps health-worker who is volunteering in a rural African village. A prominent man from a nearby village has contracted an Ebola virus that is extremely contagious, incurable, and almost always fatal within a week. Miraculously, this man has survived for a month, and so he must be a rare carrier who is immune to the virus’ deadly effects. However, this man wrongly believes that your health centre can cure his disease. You see him approaching and you know that if he enters the village he will spread the virus to hundreds of innocent people who, unlike him, will die. There is a loaded gun in the health centre. You realize that the only way you can prevent him from entering the village and spreading the virus to you and the rest of the village is to shoot and kill him as he approaches. Killing the man in order to save yourself and the rest of the village is morally.../is to warn the village council that he is coming. This will result in him being shot and killed as he approaches. Warning the council in order to save yourself and the rest of the village is morally...

Waterfront
You are part of a shipyard dock team that attaches crane cables to huge transport containers that are to be unloaded from ships onto the shore. You and the others attach these cables and then ride on top of the containers, wearing safety harnesses, to make sure that the containers are unloaded properly. While you are riding on top of one container that is just being lifted out of the cargo bay, you see the red warning light that indicates that the crane cable is about to fail. You realize that if the cable fails and the container falls onto the deck of the ship, many of the crewmembers below will be crushed to death. You see that the cable is in danger because two other crewmembers are fighting on top of the container, causing it to sway dangerously. You could run over to the fight and push them apart, but one of the crewmembers has taken off his safety harness, and will certainly fall to his death. However, if you do not do this, the continued swaying will cause the cable to fail and the container to fall onto the deck, killing several people below. Pushing the un-harnessed crewmember off the container to his death to save the people below is morally.../You could hit the emergency release button, thereby dropping the container back into the cargo bay while you and the others remain securely attached to the
crane by your safety harness. However, there is one person below you who is in the cargo bay and will be killed by the falling container because he has nowhere to go.
If you do not do this though, the container will fall onto the deck of the ship and kill several other crewmembers. Activating the emergency release, thereby killing the worker below, in order to save several other workers is morally...

Fumes
You are an orderly in a hospital during the night shift. You notice that a chemical has just been spilled in a room containing six patients. This chemical is highly toxic and if left on the floor will rapidly evaporate creating a poisonous gas. This will inevitably kill all the patients in the room. This chemical cannot simply be mopped up, nor can it be soaked up with towels or sheets.
However, because you have worked around this chemical before, you know that this chemical absorbs into human skin very rapidly. You could pull one of the patients out of bed onto the spill so that the chemical will completely soak into this person’s skin instead of evaporating into the air in the room. This will quickly kill the one patient but save the other five patients from the gas. Putting this person onto the spilled chemical in order to save the other five patients is morally... /You can flip a switch controlling the ventilation system that will draw the poisonous fumes out of the room in which the spill has occurred, saving the five patients. However, the exhaust duct leads to a room upstairs containing one other patient in critical condition. This patient will certainly die from exposure to the poisonous fumes before you can get to her, and there isn’t anyone else around to help. Flipping the switch to divert the air flow, killing one patient but saving five is morally...

Mineshaft
You are a worker in a mine. The only way to exit the mine is to ride up in rock-buckets that can hold up to three people at a time. It is the end of the workday and miners from lower levels are riding up past you. As you are waiting for a bucket to ride in, you notice that the cable supporting the rock buckets is about to snap. If the cable snaps, all of the miners in the buckets will fall to their deaths.
The only way to prevent this is to use your axe to hit the last bucket causing it to flip over and dump its contents, lightening the load enough to save the miners above. There is one miner in this bucket who will be killed as a result. Flipping over the last miner’s bucket, killing him, to save the other miners is morally... /is to hit the emergency bucket release switch which will automatically detach the last bucket from the cable, lightening the load just enough to save the miners above. There is one miner in this bucket who will be killed as a result. Detaching the last miner’s bucket, killing him, to save the other miners is morally...

Filler Scenarios
Sculpture
You are visiting the sculpture garden of a wealthy art collector. The garden is built into the side of a hill and has multiple levels that overlook one another. As you are standing on one of the upper levels looking down at one of the lower levels, you see that an old gardener is about to be seriously injured by a runaway lawnmower. The old gardener is almost completely deaf and does not see the lawnmower coming. The only way to save the workman’s life is to push one of the art collector’s prized sculptures down into the path of the lawnmower. This will stop the lawnmower before it hurts the old gardener, but it will destroy the lawnmower and the statue as well. Destroying the sculpture and the lawnmower in order to prevent the old gardener’s injuries is morally…

Lost Wallet
You are walking down the street when you come across a wallet lying on the ground. You look around, but the street is deserted. After opening the wallet, you find that it contains several hundred dollars in cash as well as the owner’s driver’s license. From the pictures and other items in the wallet it’s very clear that the wallet’s owner is elderly and just cashed their social security check. You, on the other hand, though you are not rich, you are financially comfortable. You consider sending the wallet back to the owner without the cash, keeping the cash for yourself. Keeping the money you found in the wallet in order to have more money for yourself is morally…

Collateral Damage
You are a young lawyer who has been working for a prestigious law firm in a large city. For years you have sacrificed and put work before anything else, even your personal life. Now, however, you have been invited to the partners’ board meeting. The only reason that a non-partner is ever invited to a meeting like this is to be promoted to partner. This would be a huge step up for you allowing you to take some time for yourself and not work quite so much. As you are driving to work, however, traffic is very slow and you are coming close to running late as you approach a traffic light that you hate because it takes forever to change. It is turning yellow, and if you are caught, you will be late for your own promotion and that would look extremely bad. You speed up to get through the light when you see an old man crossing in front of you, blocking the way. If you stop to let him cross, you will miss most of the meeting and lose your promotion, but if you keep going you will run over the old man and probably kill him. Running over the old man and kill him in order to get to your meeting on time is morally…

Van
You are driving along when you notice a person slowly crossing the road ahead at the crosswalk. There is a yield to pedestrian sign, so he has the right of way. You also notice that
the driver of the mini-van in the lane next to you is distracted. She is turning to face her children in the back because they are being a nuisance. The driver is therefore unaware of the pedestrian just ahead. The mini-van is traveling too fast to stop before it reaches the crosswalk. The pedestrian is talking on a cell phone and does not see that the oncoming mini-van is not coming to a stop. You realize that you could run this mini-van off the road into a telephone pole. This would save the pedestrian's life, but would also certainly kill the family inside the mini-van.

Elevating Stories: Experiment 2


It was every subway rider’s nightmare, times two. Who has ridden along New York’s 656 miles of subway lines and not wondered: “What if I fell to the tracks as a train came in? What would I do?” And who has not thought: “What if someone else fell? Would I jump to the rescue?” Wesley Autrey, a 50-year-old construction worker and Navy veteran, faced both those questions in a flashing instant yesterday, and got his answers almost as quickly. Mr. Autrey was waiting for the downtown local at 137th Street and Broadway in Manhattan around 12:45 p.m. He was taking his two daughters, Syshe, 4, and Shuqui, 6, home before work. Nearby, a man collapsed, his body convulsing. Mr. Autrey and two women rushed to help, he said. The man, Cameron Hollopeter, 20, managed to get up, but then stumbled to the platform edge and fell to the tracks, between the two rails. The headlights of the No. 1 train appeared. “I had to make a split decision,” Mr. Autrey said. So he made one, and leapt. Mr. Autrey lay on Mr. Hollopeter, his heart pounding, pressing him down in a space roughly a foot deep, to a lever on the opposite side of the tracks. The train’s brakes screeched, but it could not stop in time. Five cars rolled overhead before the train stopped, the cars passing inches from his head, smudging his blue knit cap with grease. Mr. Autrey pulled the lever, diverting the train to the next track, just in time for the train to avoid hitting Mr. Hollopeter, and himself. Mr. Autrey heard onlookers’ screams. “We’re O.K. down here,” he yelled, “but I’ve got two daughters up there. Let them know their father’s O.K.” He heard cries of wonder, and applause. Power was cut, and workers got them out. Mr. Hollopeter, a student at the New York Film Academy, was taken to St. Luke’s-Roosevelt Hospital Center. He had only bumps and bruises, said his grandfather, Jeff Friedman. The police said it appeared that Mr. Hollopeter had suffered a seizure. Mr. Autrey refused medical help, because, he said, nothing was wrong. He did visit Mr. Hollopeter in the hospital before heading to his night shift. “I don’t feel like I did something spectacular; I just saw someone who needed help,” Mr. Autrey said. “I did what I felt was right.”

Baseball (adapted from “Unbelievable” Act Of Sportsmanship”, May 1, 2008 – CBS News)

With two runners on base and a strike against her, Sara Tucholsky of Western Oregon University uncorked her best swing and did something she had never done, in high school or
college. She hit her first home run, which cleared the center field fence. But it looked like the shortest of dreams-come-true when she missed first base, started back to tag it, and collapsed with a knee injury. She crawled back to first but could do no more. The first base coach said she would be called out if her teammates tried to help her. Or, the umpire said, a pinch-runner could be called in, and the homer would count as just a single. Then, members of the Central Washington University softball team stunned their home crowd in Ellensburg by carrying Tucholsky around the bases so the three-run homer would count – an act that contributed to the team’s elimination from the playoffs. Central Washington first baseman Mallory Holtman, the all-time home run leader in the Great Northwest Athletic Conference, asked the umpire if she and her teammates could help Tucholsky/could get Tucholsky a wheelchair. The umpire said there was no rule against it. So Holtman and shortstop Liz Wallace put their arms under her legs, and she put her arms over their shoulders. The three headed around the base paths, stopping to let Tucholsky touch each base with her good leg/ The two lifted Tucholsky into the wheelchair, which allowed her to make her way to each base. “It was the right thing to do,” Holtman told Early Show co-anchor Julie Chen Thursday. “She’d hit it over the fence. She deserved the home run.” “It’s kind of a big blur at the moment,” Tucholsky said to Chen. “I didn’t really realize what was going on – I’ve had people tell me – until I actually had time to reflect on it.” “The only thing I remember is that Mallory asked me which leg was the one that hurt,” Tucholsky said. “I told her it was my right leg and she said, ‘OK, we’re going to drop you down gently and you need to touch it with your left leg/going to lift you into the chair now and you need to wheel around the bases,’ and I said ‘OK, thank you very much.’” “We started laughing when we touch second base,” Holtman said. “I said, ‘I wonder what this must look like to other people.’” “We didn’t know that she was a senior or that this was her first home run,” Wallace said Wednesday. “That makes the story more touching than it was. We just wanted to help her.” Holtman is a business major from White Salmon, Wash., who hopes to study sports management in graduate school. She said she and Wallace weren’t thinking about the playoff spot and didn’t consider the gesture something others wouldn’t do. As for Tucholsky, the 5-foot-2 right fielder was focused on her pain. “I really didn’t say too much. I was trying to breathe,” she told The Associated Press in a telephone interview Wednesday. “I didn’t realize what was going on until I had time to sit down and let the pain relax a little bit,” she said. “Then I realized the extent of what I actually did.” “I hope I would do the same for her in the same situation,” Tucholsky said. As the trio reached home plate, Tucholsky said, the entire Western Oregon team was in tears. Central Washington coach Gary Frederick, 70, a 14-year coaching veteran, called the act of sportsmanship “unbelievable.” For Western Oregon coach Pam Knox, the gesture resolved the dilemma Tucholsky’s injury presented. “She was going to kill me if we sub and take (the home run) away. But at same time I was concerned for her. I didn’t know what to do,” Knox recalled. “It’s a great story,” Knox observed to Chen, “something I’ll never forget – the game’s about character and integrity and sportsmanship, and it’s not always about winning and losing.” Tucholsky’s injury is a possible torn anterior...
cruciate ligament that will sideline her for the season, and she plans to graduate in the spring with a degree in business. Her home run sent Western Oregon to a 4-2 victory, ending Central Washington’s chances of winning the conference and advancing to the playoffs. “In the end, it is not about winning and losing so much,” Holtman said. “It was about this girl. She hit it over the fence and was in pain, and she deserved a home run.”

Moral Dilemmas: Experiment 4
The 4 moral scenarios are presented first and then the 4 non-moral ones.

Moral
(1) The loss of endangered plant and animal species.
A logging company has the rights to 1,000 square miles of old-growth forest, home to many species of endangered plants and animals. The company is willing to trade this land for 100 square miles of similar land, part of a national park and home to some (different) species of endangered plants and animals. You can give this smaller area to the company and make the larger area into a national park. The logging company will destroy all trees (and inhabitants) in whichever area it owns.

Q1. Would you trade the smaller section of land for the larger section? (Y/N)
Q2. What is the largest area of land you would trade the logging company for the 1,000 square miles of forest it currently owns?
Q3. How bad would you feel about making such a trade?

(2) Racial discrimination.
A prosecutor who works for you has just begun to work on a case involving discrimination against blacks in applications for mortgages. 10 instances of discrimination are involved. A similar case comes to your attention, and you have no other prosecutor to assign. The new case involves 100 instances of discrimination. The case not assigned to your prosecutor will not be considered.

Q1. Would you trade the case with the fewer discrimination instances for the case with more? (Y/N)
Q2. What is the largest number of discrimination instances you would trade for the case involving 100 instances of discrimination?
Q3. How bad would you feel about making such a trade?

(3) Preventing access to life-saving medical treatment.
Funds for cancer treatments for poor people are limited. Right now, the program you run covers an expensive treatment cures 100 people each year. If you stop covering this treatment completely, you can use the same money for another, less expensive, treatment that can cure 1000 people of an equally bad kind of cancer.
Q1. Would you stop the funding for the treatment that cures 100 people each year in order to fund the treatment for 1000? (Y/N)

Q2. What is the largest number of people the current treatment cures at which you would cease funding in favour of the 1000-people treatment?

Q3. How bad would you feel about making such a trade?

(4) Denying relief to the global extreme poor.
A convoy of food trucks is on its way to a refugee camp during a famine in Africa. (Airplanes cannot be used.) You find that a second camp has even more refugees. If you tell the convoy to go to the second camp instead of the first, you will save 1000 people from death, but 100 people in the first camp will die as a result.

Q1. Would you send the food convoy to the second camp with 1000 people instead of the first camp with 100 people? (Y/N)

Q2. What is the largest number of people in the first camp at which you would direct the food convoy to the second camp of 1000 people?

Q3. How bad would you feel about making such a trade?

Non-Moral
(1) Causing noise-disturbance in residential areas.
A construction company has the rights to a large, disused warehouse, close to residential neighbourhoods with a population of 1000 people. The company is willing to trade this warehouse for a similar warehouse, close to residential neighbourhoods of 100 people. You can give this second warehouse to the company and retain the warehouse close to the 1000 people. The construction company will demolish whichever warehouse it owns, and create a lot of noise disturbance in the process (however there will be no other negative consequences for nearby neighbours).

Q1. Would you trade the construction company the warehouse near 100 people for the warehouse near 1,000 people? (Y/N)

Q2. What is the largest number of people living near second warehouse at which you would make the trade for the warehouse near the 1000-person neighbourhood?

Q3. How bad would you feel about making such a trade?

(2) Leaving roads in disrepair.
A contractor who works for you has just begun to work on a case involving filling potholes on a stretch of road. 10 potholes need to be filled. A similar stretch of road comes to your attention, and you have no other contractor to assign. The new road involves 100 potholes that need to be filled, at the same cost. The road not assigned to your prosecutor will not be repaired.

Q1. Would you trade the road with the fewer potholes for the road with more? (Y/N)
Q2. What is the largest number of potholes in the road you would trade for the road with 100 potholes that need to be filled?

Q3. How bad would you feel about making such a trade?

(3) Not maintaining sporting grounds.
Funds for sports facility maintenance are limited. Right now, the program you run covers the expensive maintenance of 100 grounds each year. If you stop covering this program completely, you can use the same money for another, less expensive, program that can maintain 1000 grounds for similar sports.

Q1. Would you stop the funding for the program that maintains 100 grounds each year in order to fund the program for 1000? (Y/N)

Q2. What is the largest number of grounds the current program maintains at which you would cease funding in favour of the 1000-ground program?

Q3. How bad would you feel about making such a trade?

(4) Not providing extra farming equipment for farmers.
A convoy of new farming equipment is on its way to a farming village in Ireland. You find that a second village has even more farms. If you tell the convoy to go to the second camp instead of the first, you will provide 1000 farmers with equipment, but 100 farmers in the first village will do without the new equipment as a result.

Q1. Would you send the farming equipment to the second village with 1000 farmers instead of the first village with 100 farmers? (Y/N)

Q2. What is the largest number of people in the first village at which you would direct the equipment to the village of 1000 farmers?

Q3. How bad would you feel about making such a trade?

Thinking Style Scale: Pilot Study and Experiment 4
Rational-Experiential Inventory (Pacini & Epstein, 1999)
Please use the following scale to answer these questions.

1 —— 2 —— 3 —— 4 —— 5
Completely false —— Completely true

1. I have a logical mind.
2. I prefer complex problems to simple problems.
3. I believe in trusting my hunches.
4. I am not a very analytical thinker.
5. I trust my initial feelings about people.
6. I try to avoid situations that require thinking in depth about something.
7. I like to rely on my intuitive impressions.
8. I don’t reason well under pressure.
9. I don’t like situations in which I have to rely on intuition.
10. Thinking hard and for a long time about something gives me little satisfaction.
11. Intuition can be a very useful way to solve problems.
12. I would not want to depend on anyone who described himself or herself as intuitive.
13. I am much better at figuring things out logically than most people.
14. I usually have clear, explainable reasons for my decisions.
15. I don’t think it is a good idea to rely on one’s intuition for important decisions.
16. Thinking is not my idea of an enjoyable activity.
17. I have no problem thinking things through carefully.
18. When it comes to trusting people, I can usually rely on my gut feelings.
19. I can usually feel when a person is right or wring, even if I can’t explain how I know.
20. Learning new ways to think would be very appealing to me.
21. I hardly ever go wrong when I listen to my deepest gut feelings to find an answer.
22. I think it is foolish to make important decisions based on feelings.
23. I tend to use my heart as a guide for my actions.
24. I often go by my instincts when deciding on a course of action.
25. I’m not that good at figuring out complicated problems.
26. I enjoy intellectual challenges.
27. Reasoning things out carefully is not one of my strong points.
28. I enjoy thinking in abstract terms.
29. I generally don’t depend on my feelings to help me make decisions.
30. Using logic usually works well for me in figuring out problems in my life.
31. I think there are times when one should rely on one’s intuition.
32. I don’t like to have to do a lot of thinking.
33. Knowing the answer without having to understand the reasoning behind it is good enough for me.
34. Using my gut feelings usually works well for me in figuring out problems in my life.
35. I don’t have a very good sense of intuition.
36. If I were to rely on my gut feelings, I would often make mistakes.
37. I suspect my hunches are inaccurate as often as they are accurate.
38. My snap judgements are probably not as good as most people’s.
39. I am not very good at solving problems that require careful logical analysis.
40. I enjoy solving problems that require hard thinking.

**Difficulty Checks: Pilot Study, Experiments 1, 2 and 4**

Please rate the difficulty level of each of the tasks you have completed:

Writing Task:
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**Manipulation Checks: Experiment 3**

(1) Thinking back on today, how many times did you make a decision that involved exerting willpower?

1. None at all today
2. A great many decisions today

(2) How much of the previous lecture did you pay attention to?

1. None at all
2. The entire lecture

(3) How difficult did you find it to pay attention to the previous lecture?

1. Not at all difficult
2. Extremely difficult

(4) How mentally tired do you feel right now?

1. Not at all mentally tired
2. Extremely mentally tired
Appendix 2.2 – Supplementary Analyses for Chapter 2

Pilot Experiment

Manipulation Checks. Responses to the Brief Mood Introspection Scale were scored based on the Pleasant-Unpleasant Scale, giving each participant a maximum score of 64 (indicating extremely pleasant mood) and a minimum score of 16 (indicating extremely unpleasant mood; Mayer & Gashcke, 1988). Although there was no difference in how difficult fatigued (Mdn = 1, IQR = 1-2) and non-fatigued (Mdn = 1, IQR = 1-2) participants found completing the mood scale, U = 195, p = .874, fatigued participants reported lower levels of positive mood (M = 45.15, SD = 6.87) compared to non-fatigued participants (M = 50.40, SD = 6.09), t (38) = -2.56, p = .015, r = .38. It should be noted, however, that both groups were still within the positive range (greater than 32) of the scale and mood was not related to responses to personal, r (40) = .03, p = .872, or impersonal, r (40) = -.07, p = .674, dilemmas. Given that both groups were still within the “pleasant” range of the scale and that mood was not found to be related to judgments on either dilemma type, it was concluded that the difference was not sufficient to impact subsequent moral judgments (cf. Strohminger et al., 2011; Valdesolo & DeSteno, 2006).

Statements to both subscales of the Rationality versus Experientialism Inventory were averaged, resulting in a minimum score of 1 and a maximum score of 5 on each for each participant (Pacini & Epstein, 1999). Higher scores indicated a higher reliance on that style of thinking (e.g., a score of 5 for rationality indicated a maximum reliance on rational thinking). A 2 (fatigue, no fatigue) x 2 (rationality, experientialism) ANOVA with repeated measures on the second factor showed that there was no effect of fatigue on answers, F < 1, but in general participants were more likely to rely on rational thinking than intuitive, F (1, 38) = 12.01, p = .001, η² = .24. Crucially, there was no interaction between the two factors, F < 1, as fatigued and non-fatigued participants reported similar levels of rationality (M = 3.79, SD = 0.45; M = 3.67, SD = 0.54, respectively) and intuitive thinking (M = 3.24, SD = 0.67; M = 3.31, SD = 0.60, respectively). Therefore, any differences in judgments cannot be attributed to trait differences in thinking styles in both groups.

Experiment 1

Manipulation Checks. There was no difference in pleasant mood between fatigued (M = 42.51, SD = 8.20) and non-fatigued (M = 41.87, SD = 8.06) participants, t (194) = 0.19, p = .848. There was also no difference in how difficult fatigued (Mdn = 1, IQR = 1-3) and non-fatigued participants (Mdn = 1, IQR = 1-3) found completing this scale, U = 4662, p = .737.

Experiment 2

Manipulation Checks. There was no difference in pleasant mood between fatigued (M = 41.57, SD = 9.27) and non-fatigued (M = 43.07, SD = 7.82) participants, t (185) = 1.20, p
There was also no difference in how difficult fatigued \((Mdn = 2, IQR = 1-3)\) and non-fatigued participants \((Mdn = 2, IQR = 1-3)\) found completing this scale, \(U = 4162.5, p = .554\).

Responses to the elevation scale were averaged, and a 2 (fatigue, no fatigue) x 2 (personal, impersonal) ANOVA on elevation scores showed that there was no effect of fatigue or dilemma, nor was there an interaction, all \(F\)s < 1; fatigue did not affect how elevating the participants judged the stories, as fatigued participants judged personal \((M = 5.51, SD = 1.38)\) and impersonal stories \((M = 5.52, SD = 1.30)\) the same as non-fatigued fatigued participants did \((M = 5.70, SD = 1.12; M = 5.64, SD = 1.21, \text{ respectively})\).

**Experiment 4**

*Manipulation Checks.* Fatigued participants rated completing the mood scale as being more difficult to complete \((Mdn = 2, IQR = 1-2.75)\) compared to non-fatigued participants \((Mdn = 1, IQR = 1-2)\), \(U = 127, p = .032, r = .34\), but both groups rated it as not very difficult. Again the scale was scored according to the Pleasant-Unpleasant scale (Mayer & Gaschke, 1988), and both groups scored on the “pleasant” side of the scale with fatigued participants reporting an average of 46.95 \((SD = 5.76)\) and non-fatigued participants reporting an average of 47.55 \((SD = 5.93)\). There was no difference between the groups in this experiment, \(t(38) = -0.32, p = .747\).

Fatigued participants reported high scores \((M = 3.76, SD = 0.45)\) on the rationality subscale of the Rationality and Experientialism Inventory, as did non-fatigued participants \((M = 3.65, SD = 0.41)\). Scores on the intuitive subscale were also above the mid-point for both groups \((M = 3.51, SD = 0.63; M = 3.13, SD = 0.65, \text{ respectively})\). A 2 (fatigued, non-fatigued) x 2 (rationality, experientialism) ANOVA with repeated measures on the second factor on these scores showed that there was an effect of fatigue in this experiment, \(F(1, 38) = 4.67, p = .037, \eta_p^2 = .11\), such that fatigued participants gave higher ratings on both scales. The same effect of thinking style found in the pilot study was shown again, \(F(1, 38) = 8.84, p = .005, \eta_p^2 = .19\), such that the participants tended to favour rational thinking over intuitive thinking. Crucially, there was no interaction suggesting that the same patterns of preference was present in both groups, \(F(1, 38) = 1.17, p = .286\).

*Response Latencies.* Response latencies were recorded from the onset of the question that asked whether the sacrifice should be made until the participant chose a response. Similar to the pilot study, latencies were trimmed on a by-participant, by-condition basis. Short latencies were identified as those below 1000ms and long latencies were defined as those above the participant’s mean plus 1.96 times their standard deviation. Two data points were identified as short outliers \((1.25\% \text{ of data points})\) to the question about whether a trade-off should be made, and these were replaced with each participant’s mean. There were no other outliers identified. Responses to each of the moral dilemmas were averaged to give each participant
one score for protected values and one score for unprotected values. The data were not transformed in this experiment as they were normally distributed.

On average, participants took about 14 seconds to make a decision about whether or not the sacrifice should be made, as Figure A1 shows. A 2 (fatigue, no fatigue) x 2 (protected, non-protected) ANOVA on reaction times to this question showed no effect of fatigue, $F(1, 37) = 2.29, p = .138$, a marginal effect of status, $F(1, 37) = 3.96, p = .054, \eta^2_p = .10$, as participants took longer to make decisions about unprotected values, and the two factors did not interact, $F < 1$, as Figure A1 shows. When only the reaction times to endorse the sacrificial action were analysed, the pattern was the same. Fatigued participants took 13.02s ($SD = 6.60$) to endorse sacrificing a protected value, whereas it took non-fatigued participants 11.84s ($SD = 5.47$). For non-protected values, fatigued participants took 16.87s ($SD = 6.55$) compared to 13.30s ($SD = 6.31$) for non-fatigued participants. The results of the ANOVA on these times was the same as the previous analysis: there was no effect of fatigue, $F(1, 35) = 2.31, p = .138$, a marginal effect of status in the same direction, $F(1, 35) = 4.05, p = .052, \eta^2_p = .10$, and no interaction, $F < 1$.

![Figure A1](image)

**Figure A1.** Response latencies in milliseconds to respond to the sacrifice question in Experiment 4. Error bars are the standard error of the mean.

**Maximum Sacrifice Values.** Participants were also asked the maximum threshold at which they would make the trade (e.g. how large their plot of land would have to be in order to still make a trade with the logging company). The rational answer to this question is “$a-1$”, where $a$ is the amount of the value offered in the trade (e.g. if the logging company is planning to destroy 1000m$^2$ of land home to many species of endangered animals, the rational threshold
is to trade up to 999m$^2$ of land). Note that, for this analysis, all scores were extrapolated to a scale of 100 to 1000; 100 was the minimum possible value, as maximum sacrifice thresholds were requested only from participants who responded “yes” to the initial trade-off question and had therefore endorsed sacrificing 100 of the value to save 1000. In general, participants gave a maximum threshold of about 660 of the value (e.g., they would trade up to about 660m$^2$ of conservation land in order to save 1000m$^2$, but not more than that). A 2 (fatigue, no fatigue) x 2 (protected, unprotected) ANOVA on maximum thresholds showed no effect of fatigue, status, or interaction between the two factors, all $F$s < 1, as Figure A2 shows.

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<th>Status</th>
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<td>Unprotected</td>
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Figure A2. Maximum values participants would still endorse the sacrifice in Experiment 4.

Error bars are the standard error of the mean.
Appendix 2.3 – Information and Debrief Sheets for Chapter 2

Pilot Study: Information

This study is centred on moral reasoning. The study is being carried out by Shane Timmons (PhD Student in Psychology), with the supervision of Prof Ruth Byrne. The aim of the study is to assess how we make moral decisions.

Participation in this study will take about 30 minutes. You may withdraw at any time if you wish, without penalty, up until the submission of the report for this study. The study is anonymous and confidential. The data will be stored in accordance with the Freedom of Information and the Data Protection Acts. You are entitled to access data stored about you. In order to access this information, please retain the ID code at the top of this sheet. In this study the data will be scored as group scores and participants will not be identified individually. You may request access to a copy of the final report when it is available if you wish.

In the study, you will first be required to write a short essay about a trip you have taken recently. More detailed instructions will be provided prior to beginning the task. Following this, you will be required to complete a short mood questionnaire. Next, you will be presented with moral dilemmas to be read on a computer screen. These stories will appear in sections – to view the next section, please press the ‘Spacebar’ key. Please read each sentence carefully. At the end of each dilemma, you will be asked a question on the moral permissibility of the dilemma’s resolution. You can provide your answer by entering a numerical rating on the keyboard. There are no right or wrong answers. Please disregard legality in each situation and base your answers on your opinion of moral permissibility. You can take as much time as you wish. These instructions will be repeated before you attempt the task. This will be followed by a final questionnaire that will involve assigning values to a series of statements, depending on how well each statement describes you. You will then be asked to rate the difficulty of each of the completed tasks. The study should take between 20 and 40 minutes.

You are free to withdraw during any stage of the experiment without penalty, and may take a break between each task if required.

If you are a JF or SF student, you will be entitled to 2 research credits for your participation.

If you have any further questions about the study, or would like to access a summary of the findings, please do not hesitate to contact us at the following:

Please sign below to indicate that you have read and understand the information above and that you give your consent to participate. You may keep the information sheet if you wish. Please give this consent form to the experimenter.

Name (please print):
Signature:

Please also provide the following information:

Age: __  Gender: __
Pilot Study: Debrief

Please remove this sheet from the booklet. This sheet should be retained by you, with the rest of the booklet returned to the experimenter.

Thank you for agreeing to participate in this study. The general purpose of this research is to explore the cognitive processes that underlie moral judgments.

Thank you again for your participation. We do not anticipate that any aspect of the study will cause any distress to participants; however, if you feel especially concerned about any aspect of the study, please feel free to contact the Samaritans at 1850 60 90 90 (or email jo@samaritans.org) or Niteline at 1800 793 793.

Experiments 1 and 2: Information Sheet

Thank you for agreeing to participate in this study. It is a study of everyday thinking; it is not a test of intelligence.

Participation in this study will take approx. 10 minutes. You may withdraw at any time if you wish. The study is anonymous and confidential. The data will be scored as group scores and participants will not be identified individually. The data will be stored in accordance with the Freedom of Information Act (1997).

If you have any further questions about the study, or would like to access a summary of the findings, please do not hesitate to contact:

Shane Timmons, School of Psychology, Trinity College, Dublin 2, Ireland
Email: timmonss@tcd.ie

Ruth Byrne, School of Psychology, Trinity College, Dublin 2, Ireland
Email: rmbyrne@tcd.ie   Telephone: +353 1 896 4054

This research is funded by the John Templeton Foundation.

Experiments 1 and 2: Debrief

Thank you for participating in this study. The aim of this study is to examine whether cognitive fatigue influences responses to moral events. If you have concerns about the study, or would like to access a summary of the findings, please do not hesitate to contact the lead experimenter at the contact details below.

Shane Timmons, School of Psychology, Trinity College, Dublin 2, Ireland
Email: timmonss@tcd.ie

Ruth Byrne, School of Psychology, Trinity College, Dublin 2, Ireland
Email: rmbyrne@tcd.ie   Telephone: +353 1 896 4054

This research is funded by the John Templeton Foundation.
Experiment 3: Information

Aim: This study aims to assess the conditions in which certain types of moral decisions are made.

Procedure: If you agree to participate, you will first complete a brief questionnaire with general demographic and current state questions. After this, you will read through and make a judgment about 4 stories. This should take no longer than 10 minutes.

Compensation: In return for participating in this study, you can be entered into a draw for a €50 One-for-All voucher. If you would like to be entered into this draw, please fill in your email address in the space below.

Voluntary Participation: You are free to withdraw during any stage of the experiment without penalty.

Confidentiality: Responses to this study will be anonymous and confidential. The data will be scored as group scores and participants will not be readily identifiable individually. All data will be stored in accordance with the Freedom of Information and the Data Protection Acts; you are entitled to access data stored about you. You may request a copy of the finalised report.

Contact: If you have any further questions about the study, or would like to access a summary of the findings, please do not hesitate to contact us at the following:

(Note. A copy of this information sheet is available at the end of the booklet.)

Declaration: I have read and understand the information above and give my consent to participate in this study. I understand that my participation is completely voluntary and I may withdraw at any point without consequence.

Name (please print): ______________________
Signature: ________________________________
Date: _________________________________
Email (optional): ___________________________ (for entry into draw for €50 One-for-All voucher only)

Experiment 3: Debrief

Please remove this sheet from the booklet. This sheet should be retained by you, with the rest of the booklet returned to the experimenter.

Thank you for agreeing to participate in this study. The general purpose of this research is to explore the cognitive processes that underlie moral judgments.

In this study, we are examining whether everyday, cognitively depleting activities (such as exerting attention in an evening lecture) affects moral judgments. In this study, some participants completed the booklet before a 2-hour evening lecture and their responses will be compared to participants who completed the booklet after a 2-hour evening lecture.

Thank you again for your participation. We do not anticipate that any aspect of the study will cause any distress to participants; however, if you feel especially concerned about any aspect of
the study, please feel free to contact the Samaritans at 1850 60 90 90 (or email jo@samaritans.org) or Niteline at 1800 793 793.

Experiment 4: Information

This study is about the types of moral decisions people make. The study is being carried out by Shane Timmons (PhD Student in Psychology), under the supervision of Prof Ruth Byrne. The aim of the study is to assess how we make moral decisions.

Participation in this study will take about 40 minutes. You may withdraw at any time if you wish, without penalty, up until the submission of the report for this study. The study is anonymous and confidential. The data will be stored in accordance with the Freedom of Information and the Data Protection Acts; you are entitled to access data stored about you. In order to access this information, please retain the ID code at the top of this sheet. In this study, the data will be scored as group scores and participants will not be identified individually. You may request access to a copy of the final report when it is available if you wish.

In the study, you will asked to complete a series of tasks:

1. Writing Task — You will be requested to write a short essay about a trip you have taken recently.
2. Mood Scale — You will be presented with some adjectives and asked how well each one describes your current mood.
3. Values Task — You will be presented with a series of items and asked to imagine a time when it is acceptable to violate the statement. Following this, you will be presented with some hypothetical dilemmas to respond to.
4. Personality Inventory — You will be presented with a series of statements and asked to rate how well each of those statements describe you.
5. Difficulty Ratings — Finally, you will be asked to rate how difficult you found each of the above tasks.

More detailed instructions will be provided prior to beginning each task. There are no right or wrong answers; please answer honestly based on your opinion. You can take as much time as you wish. You are free to withdraw during any stage of the experiment without penalty, and may take a break between each task if required.

If you are a JF or SF student, you will be entitled to 2 research credits for your participation.

If you have any further questions about the study, or would like to access a summary of the findings, please do not hesitate to contact us at the following:

Experiment 4: Debrief
Please remove this sheet from the booklet. This sheet should be retained by you, with the rest of
the booklet returned to the experimenter.

Thank you for agreeing to participate in this study. The general purpose of this research is to
explore the cognitive processes that underlie moral judgments.

Thank you again for your participation. We do not anticipate that any aspect of the study will
cause any distress to participants; however, if you feel especially concerned about any aspect of
the study, please feel free to contact the Samaritans at 1850 60 90 90 (or email
jo@samaritans.org) or Niteline at 1800 793 793.
Appendix 2.4 – Ethical Approval for Experiments 1 to 4

F.A.O. Shane Timmons

School of Psychology Research Ethics Committee

04 November 2014

Dear Shane,

The School of Psychology Research Ethics Committee has reviewed your application entitled “Cognitive Processes and Moral Judgement”, and I am pleased to inform you that it was approved.

Please note that you will be required to submit a completed Project Annual Report Form on each anniversary of this approval, until such time as an End of Project Report Form is submitted upon completion of the research. Copies of both forms are available for download from the Ethics section of the School website.

Adverse events associated with the conduct of this research must be reported immediately to the Chair of the Ethics Committee.

Yours sincerely,

Richard Carson
Chair
School of Psychology Research Ethics Committee
Appendix 3.1 – Experimental Materials for Chapter 3

Story: Experiments 5, 6, 7A and 7B.

The four moral judgments are in italics and the three story endings are provided. The bad outcome ending is presented in brackets.

Ann is waiting for a bus with her son. Nearby a young child, Jill trips over an uneven pavement stone and stumbles into the road and falls down near the traffic lights at the busy intersection. Just then, a very large truck drives through the intersection. Ann rushes to help, she can see that the truck driver cannot see Jill struggling on the ground and will not be able to stop in time. She decides that the only way to help is to run into the road and hold Jill down so they both lie still as the truck passes over them. She knows that there is a terrible risk that the truck could crush them both.

Ann should run into the road to help Jill.
Ann’s action is morally good.

Ann ran into the road and held Jill down. When the truck drove on and passersby got to them, Jill had sustained only minor bumps and bruises from the truck. […]Jill had sustained serious life-threatening injuries from the truck.

Factual: The police examined the videos of the incident later and observed Ann rushing to help and Jill sustaining injuries from the truck.
Counterfactual: The police examined the videos of the incident later and observed that if Ann had not rushed to help, Jill’s injuries would have been worse.
Semi-factual: The police examined the videos of the incident later and observed that even if Ann had not rushed to help, Jill’s injuries would have been the same.

Knowing what I know now, I believe Ann should have run into the road to help Jill.
Knowing what I know now, I believe Ann’s action was morally good.

Moral Elevation and Helping Measures

(A) How much did you experience the following emotions or thoughts while reading the story?
Inspired
Uplifted
People are really good
The world is full of kindness and generosity
I need to do more to help other people
I want to be more like the person in the story
(B) I would like/I would not like to donate half of my 50p payment to future research

**Appendix 3.2 – Supplementary Analyses for Chapter 3**

**Tables of Medians and Interquartile Ranges**

Table A 3

*Median Agreement The Action Should Be Taken in Experiments 5, 6, 7A and 7B.*

<table>
<thead>
<tr>
<th>Experiment</th>
<th>Judgment</th>
<th>Factual</th>
<th>Counterfactual</th>
<th>Semi-Factual</th>
</tr>
</thead>
<tbody>
<tr>
<td>E5</td>
<td>Pre-Outcome</td>
<td>5 (4-6)</td>
<td>5 (4-6)</td>
<td>4.5 (4.5-6)</td>
</tr>
<tr>
<td></td>
<td>Post-Outcome</td>
<td>6 (4-7)</td>
<td>6 (5-7)</td>
<td>3.5 (1.75-6)</td>
</tr>
<tr>
<td>E6</td>
<td>Pre-Outcome</td>
<td>4 (3-6)</td>
<td>5 (3-6)</td>
<td>5 (3-6)</td>
</tr>
<tr>
<td></td>
<td>Post-Outcome</td>
<td>5 (4-7)</td>
<td>6 (5-7)</td>
<td>3.5 (1.25-5.75)</td>
</tr>
<tr>
<td>E7A</td>
<td>Pre-Outcome</td>
<td>5 (4-6.5)</td>
<td>6 (5-7)</td>
<td>5 (3-6)</td>
</tr>
<tr>
<td></td>
<td>Post-Outcome</td>
<td>6 (4-7)</td>
<td>7 (5-7)</td>
<td>5 (3-6)</td>
</tr>
<tr>
<td>E7B</td>
<td>Pre-Outcome</td>
<td>5 (4-6)</td>
<td>5 (3-6)</td>
<td>5 (3-6)</td>
</tr>
<tr>
<td></td>
<td>Post-Outcome</td>
<td>4 (3-6)</td>
<td>6 (5-7)</td>
<td>5 (2-6)</td>
</tr>
</tbody>
</table>

Note. Interquartile ranges are reported in parentheses.

Table A 4

*Median Agreement The Action is Morally Good in Experiments 5, 6, 7A and 7B.*

<table>
<thead>
<tr>
<th>Experiment</th>
<th>Judgment</th>
<th>Factual</th>
<th>Counterfactual</th>
<th>Semi-Factual</th>
</tr>
</thead>
<tbody>
<tr>
<td>E5</td>
<td>Pre-Outcome</td>
<td>7 (6-7)</td>
<td>5 (4-6)</td>
<td>6.5 (5-7)</td>
</tr>
<tr>
<td></td>
<td>Post-Outcome</td>
<td>7 (6-7)</td>
<td>7 (6-7)</td>
<td>7 (5-7)</td>
</tr>
<tr>
<td>E6</td>
<td>Pre-Outcome</td>
<td>7 (5-7)</td>
<td>7 (6-7)</td>
<td>7 (6-7)</td>
</tr>
<tr>
<td></td>
<td>Post-Outcome</td>
<td>7 (5-7)</td>
<td>7 (6-7)</td>
<td>7 (5-7)</td>
</tr>
<tr>
<td>E7A</td>
<td>Pre-Outcome</td>
<td>7 (6-7)</td>
<td>7 (7-7)</td>
<td>7 (6-7)</td>
</tr>
<tr>
<td></td>
<td>Post-Outcome</td>
<td>7 (6-7)</td>
<td>7 (7-7)</td>
<td>7 (6-7)</td>
</tr>
<tr>
<td>E7B</td>
<td>Pre-Outcome</td>
<td>7 (6-7)</td>
<td>7 (6-7)</td>
<td>6 (6-7)</td>
</tr>
<tr>
<td></td>
<td>Post-Outcome</td>
<td>7 (6-7)</td>
<td>7 (7-7)</td>
<td>7 (6-7)</td>
</tr>
</tbody>
</table>

Note. Interquartile ranges are reported in parentheses.
Appendix 3.3 – Information and Debrief Sheets for Chapter 3

Information
This study examines how people make judgments about moral situations. The study is being carried out by Shane Timmons, PhD student, supervised by Professor Ruth Byrne, from Trinity College Dublin, University of Dublin, Ireland.
Participation in this study will take about 5 minutes. You will be asked to read a short description of an event and to answer several questions about it.
Your participation is voluntary and you are free to withdraw during any stage of the experiment without penalty, up to the point when the data is pooled for analysis. Should you wish to withdraw, any data already collected will be destroyed. You will be paid a sum of 50p in recognition of your participation.
The study is anonymous and confidential. The data will be stored in accordance with the Freedom of Information and the Data Protection Acts of the Republic of Ireland. You are entitled to access data stored about you. In this study, the data will be scored as group scores and participants will not be identified individually. You may request access to a copy of the final report if you wish.
If you have any further questions about the study, or would like to access a summary of the findings, please do not hesitate to contact:
Shane Timmons, School of Psychology, Trinity College, Dublin 2, Ireland
Email: timmonss@tcd.ie
Ruth Byrne, School of Psychology, Trinity College, Dublin 2, Ireland
Email: rmbyrne@tcd.ie  Telephone: +353 1 896 4054
This research is funded by the John Templeton Foundation

Debrief
You have now reached the end of the survey. Thank you for taking part. The aim of this survey was to understand how people think about acts of moral goodness. Please note that the question about donating half of your payment to future research was simply a measure of the study; you will be paid the full 50p.
If you have any questions or concerns, please contact one of the researchers at:
Shane Timmons, School of Psychology, Trinity College, Dublin 2, Ireland
Email: timmonss@tcd.ie
Ruth Byrne, School of Psychology, Trinity College, Dublin 2, Ireland
Email: rmbyrne@tcd.ie  Telephone: +353 1 896 4054
This research is funded by the John Templeton Foundation.
Appendix 3.4 – Ethical Approval for Experiments 5 to 10

Dear Shane,

The School of Psychology Research Ethics Committee has reviewed your application entitled “Counterfactual thinking in moral elevation” and I am pleased to inform you that it was approved.

Please note that you will be required to submit a completed Project Annual Report Form on each anniversary of this approval, until such time as the research is complete and the thesis is submitted. The form is available for download from the Ethics section of the School website.

Adverse events associated with the conduct of this research must be reported immediately to the Chair of the Ethics Committee.

Yours sincerely,

Richard Carson
Chair
School of Psychology Research Ethics Committee
Appendix 3.5 – Moral Elevation Pilot Experiment

The aim of the study was to examine whether presentation format (video, news article, long vignette or short vignette) affects the level of elevation experienced for two commonly used stories.

**Method**

**Participants.** The participants were 168 volunteers who completed the pilot experiment online on /r/SampleSize, hosted through SurveyGizmo. Two participants were removed, as they indicated that they had not completed the survey honestly or to the best of their ability when responding to a quality control question at the end of the experiment. Of the remaining 166, 100 were women and 66 were men, aged 18 to 72 years with an average age of 33 years. Participants were randomly assigned to receive the stories in video (n = 46), news article (n = 28), long vignette (n = 42) or short vignette (n = 50) form.

**Design and Materials.** All participants were shown two stories (Home Run and Subway), both describing an act of moral excellence carried out by an agent to benefit another individual at some cost to the agent. The Home Run story described one player who hit a home run but was injured while running around the bases. The injured player was helped to finish the run by a member of the opposing team. The opposing team then lost the match. The Subway story depicted an event where one man jumped in front of an oncoming train to save another man’s life, by pressing him down beneath the train. The two men survived with minor injuries. Both of these stories are widely used in the moral elevation literature (e.g., Diessner et al., 2013; Englander et al., 2012; Lai et al., 2014). The principal manipulation was the format in which the events were presented: video, news article, short vignette or long vignette (see below). Participants were randomly assigned to one presentation format and saw both events in the same format. Events were presented in random order. Prior to each event, participants were told:

The following story describes a real-life experience. Please read it slowly and [watch it] carefully, and try to imagine yourself in the situation described. Try to experience the events as if they are really happening to you, and imagine how you would feel.

After each event, participants were instructed to “please take a moment to consider the events that have occurred and imagine the thoughts and feelings you might have about them”.

Following this, participants were presented with a series of items designed to measure the core aspects of moral elevation. The scale was adapted from previous research (e.g. Aquino et al., 2011; Diessner et al., 2013; Freeman et al., 2009; Thomson & Siegel, 2013; see Appendix 1.1). They were asked to what extent they experienced certain emotions (e.g. “awe”)
while reading the story, as well as to what extent they felt a physiological response to the story (e.g. “warmth in your chest”), on a scale from 1 “not at all” to 7 “extremely”. They were also asked how often they had positive thoughts about humanity (e.g. “there is still some good in the world”) while attending to the story, and how often they had a motivation to emulate the good act or agent in the story (e.g. “I want to be more like the person in the story”), on a scale from 1 (Never) to 7 (Always). There were some reverse-valenced filler items included, in order to ensure participants were attending to the items (e.g. “disgusted”, “people are very bad”). See Table A5 for all items. Items were presented in randomised order.

Table A 5

Moral Elevation Scale Items

<table>
<thead>
<tr>
<th>Item</th>
<th>Component</th>
</tr>
</thead>
<tbody>
<tr>
<td>Compassion</td>
<td>Emotional</td>
</tr>
<tr>
<td>Inspired</td>
<td>Emotional</td>
</tr>
<tr>
<td>Admiration</td>
<td>Emotional</td>
</tr>
<tr>
<td>Lump in Throat</td>
<td>Physiological</td>
</tr>
<tr>
<td>Warmth in Chest</td>
<td>Physiological</td>
</tr>
<tr>
<td>Tears in Eyes</td>
<td>Physiological</td>
</tr>
<tr>
<td>Disgusted</td>
<td>Filler</td>
</tr>
<tr>
<td>There is still some good in the world</td>
<td>Cognitive</td>
</tr>
<tr>
<td>People are really good</td>
<td>Cognitive</td>
</tr>
<tr>
<td>The world is full of kindness and generosity</td>
<td>Cognitive</td>
</tr>
<tr>
<td>The actions of most people are admirable</td>
<td>Cognitive</td>
</tr>
<tr>
<td>I want to be more like the person in the story</td>
<td>Motivational</td>
</tr>
<tr>
<td>There person in the story has shown me how to be a better person</td>
<td>Motivational</td>
</tr>
<tr>
<td>I need to do more to help other people</td>
<td>Motivational</td>
</tr>
<tr>
<td>I can learn a lot from the person in the story</td>
<td>Motivational</td>
</tr>
<tr>
<td>People are very bad</td>
<td>Filler</td>
</tr>
<tr>
<td>There are still a few good people out there</td>
<td>Cognitive</td>
</tr>
<tr>
<td>I want to be a better person</td>
<td>Motivational</td>
</tr>
<tr>
<td>I do not want to be like the person in the story</td>
<td>Filler</td>
</tr>
</tbody>
</table>

Procedure. Participants accessed the experiment via a link on /r/SampleSize, directing them to the host SurveyGizmo. They were given the general instructions that the task was a study of everyday thinking and not a test of intelligence, and that they would be presented with some short stories about some real-life events and to pay attention to these
stories, imagining how they would think and feel in such situations. They were asked to only take part if they were willing to consider the events seriously and take some time to consider their responses. The experiment took about 10 minutes to complete. At the end, participants were thanked and asked whether they had any further comments about the study.

Results and Discussion

The primary aim of this study was to determine whether presentation format influenced the extent to which participants felt morally elevated. First, it was necessary to determine whether the elevation scale adequately measured moral elevation. To reduce content effects, and given the high correlation between items across both stories (all $r > .56$, $p < .001$), responses were averaged to give one score per participant on each item (addressing each story separately did not affect the results described below).

Elevation Scale. Initial analysis of the 16 items on the elevation scale showed high internal consistency (Cronbach’s $\alpha = .97$). The data passed Bartlett’s test of sphericity ($p < .001$) and Kaiser-Meyer-Olkin measure of sample adequacy (.95), confirming suitability for factor analysis. Despite the theoretical construction of the elevation scale, Confirmatory Factor Analysis suggested that the four components (i.e., emotional, physiological, cognitive and motivational) were unnecessary. Instead, eigenvalue and scree plot examination following Exploratory Factor Analysis with Direct oblimin rotation (due to the anticipated correlation between factors measuring the single construct of moral elevation) suggested that two factors were sufficient (accounting for 79.99% of the variance). With a retention threshold set at .78, 13 items loaded onto the first factor with the remaining 3 loading heavily onto the second factor (see Table A6). Factor 1 consisted of those items constructed to assess emotional response to the stories, cognitive appraisals of humanity and motivation to emulate the agent; factor 1 is hereafter referred to as psychological response. Factor 2 consisted of the three items constructed to assess physiological response, and is hereafter referred to as such.

<table>
<thead>
<tr>
<th>Item</th>
<th>Factor 1</th>
<th>Factor 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Compassion</td>
<td>.8</td>
<td></td>
</tr>
<tr>
<td>Inspired</td>
<td>.88</td>
<td></td>
</tr>
<tr>
<td>Admiration</td>
<td>.85</td>
<td>.92</td>
</tr>
<tr>
<td>Lump in Throat</td>
<td></td>
<td>.92</td>
</tr>
<tr>
<td>Warmth in Chest</td>
<td></td>
<td>.78</td>
</tr>
<tr>
<td>Tears in Eyes</td>
<td></td>
<td>.92</td>
</tr>
</tbody>
</table>
There is still some good in the world .92
People are really good .89
The world is full of kindness and generosity .89
The actions of most people are admirable .87
I want to be more like the person in the story .9
There person in the story has shown me how to be a .92
better person
I need to do more to help other people .89
I can learn a lot from the person in the story .93
There are still a few good people out there .89
I want to be a better person .89

Note. The correlation between the factors was .61.

Presentation Format. Moral elevation within each presentation format was then considered based on the average of all the items as well as the average of item scores for the psychological and physiological factors separately (see Figure A3). Shapiro-Wilk tests of normality indicated that most of the data were non-normally distributed ($p < .05$), except for physiological elevation in the video and news article groups and total elevation for the news article group. As a result, all comparisons were conducted using non-parametric tests.

Figure A 3. Average psychological and physiological elevation ratings across presentation formats. Error bars are the standard error.
A Kruskal-Wallis H test, analysing the four presentation formats on overall elevation, indicated there were significant differences between the groups, $\chi^2(3, N = 166) = 13.33, p = .004, \eta^2 = .08$. Mann-Whitney U tests with a Bonferroni-corrected alpha of .008 for six comparisons showed that short vignettes were less elevating than the news articles, $U = 422, p = .004, r = .33$, and the videos, $U = 707, p = .001, r = .33$. No other comparisons were significant, all $ps > .084$. A Wilcoxon Signed-Rank Test on whether there was an overall difference between psychological and physiological items indicated that participants reported higher ratings on the psychological items than the physiological ones, $Z = 10.3, p < .001, r = .80$. The interaction between presentation format and elevation factor was assessed using a further two Kruskal-Wallis H tests. The analyses suggested no interaction, as the effect of presentation format across both factors was the same: $\chi^2_{\text{Psychological}}(3, N = 166) = 11.87, p = .008, \eta^2 = .07$; $\chi^2_{\text{Physiological}}(3, N = 166) = 15, p = .002, \eta^2 = .09$.

**Conclusion.** The results show that moral elevation can be decomposed into two different factors: psychological elevation and physiological elevation. The usefulness of assessing physiological elevation by self-report, however, is called into question when the responses are considered; participants gave responses at the mid-point of the scale for even the widely used video format.

The results also show that, although short vignettes may be useful for examining the factors that affect how participants reason about self-sacrificial acts, they may not be sufficiently detailed to elicit moral elevation. After reading the short vignettes, participants were significantly less elevated than after attending to the same events written as a newspaper article or watched as a news report. More detailed, realistic narratives than short vignettes may be required for participants to imagine the events to an extent that they feel morally elevated.

**Pilot Study: Materials**

*Home Run*

*Video*

https://www.youtube.com/watch?v=jocw-oD2pgo

*News Article*

The news article used was the same as the “personal” version of the Baseball news article used in Experiment 2 and is reported in Appendix 2.1.

*Short Vignette*

A University baseball team, the Reds, is taking part in a major game against another team, the Blues. A senior on the Reds, Mary, hits her first ever home run but as she is dashing past the first base she turns sharply, and collapses with a devastating knee injury. She cannot complete the run and the coach warns her team that no-one else can complete it for her.

The members of the opposite team, the Blues, rush to help; they know it is probably Mary’s last ever chance to score a home run. They decide that the only way to help is to carry her around
the bases, stopping to let her touch each base with her good leg. The Blues know that they will probably lose the game and compromise their chances at winning the league. The Blues eventually lost the game. The coaches examined the videos of the incident later and observed that, if the Blues had not rushed to help, Mary would not have been able to complete her homerun.

Long Vignette
Playing for her university in the semi-final of the baseball championships, Sara stood up to the plate and took her best swing. She did something she had never done before. The ball cleared the fence; she hit her first home run.

As she runs past the first base, she turns sharply, twisting her knee and collapsing with a devastating knee injury. She begins to crawl to the next base, but can go no further. She cannot complete the run.

The umpire warns that if any member of her team touches her to help her, she would be called ‘out’. Her coach wants to bring on a replacement runner, but this would void Sara’s first and only home run.

“Excuse me, would it be okay if we carried her around and she touched each base?” asks Mary, captain and lead home run hitter of the opposing team.

The umpire says there is no rule against it. Mary, along with another opposing team member, proceeds to carry Sara around the base paths, stopping at each base to touch it with Sara’s good leg. As they reach the home plate, supporters from both teams cheer.

Sara’s home run sent her university’s team to victory, ending Mary’s team’s chance at a place in the final.

“She’d hit it over the fence. She deserved the home run,” Mary said afterwards. “It was the right thing to do.”

Subway
Video
http://www.youtube.com/watch?v= N2zhu5RH34

News Article
The news article used was the same as the “personal” version of the Subway news article used in Experiment 2 and is reported in Appendix 2.1.

Short Vignette
Sam is waiting for a subway train with his two daughters. Nearby Jim collapses with convulsions, he manages to get up but then stumbles to the platform edge and falls onto the tracks between the two rails. Just then, the lights of the oncoming train come into view. Sam
rushes to help, he can see the train approaching rapidly and can see it will not be able to stop in
time. He decides that the only way to help is to jump onto the tracks and lie on top of Jim,
trying to press him down into the space between the two rails. He knows that there is a terrible
risk that the train could hit them both.
Sam jumped onto the tracks. When the power was cut and the workers got them out, both men
had sustained only bumps and bruises. The station managers examined the videos of the
incident later and observed that if Sam had not rushed to help, Jim would have been injured.

**Long Vignette**

Will and his two young daughters are standing on a subway platform in New York, waiting on
a train. Nearby, a young man collapses, his body convulsing. Will and two women rush to
help him. The man, Carl, manages to get up, but then stumbles to the platform edge and falls
backwards onto the tracks, between the two rails.
The train is fast approaching, too close to stop.
Will jumps onto the tracks, hoping that if he can lie on top of Carl and keep him from flailing,
the train will roll right over both of them. Will presses Carl down in the space beneath the
train.
The distance between the track and the bottom of the train is exactly 21 inches. Will and Carl’s
heads are 20.5 inches off the track. Both men survive the incident, with minor scrapes and
bruises.
“We’re okay down here,” Will yelled to onlookers, “but I’ve got two daughters up there. Let
them know their father’s okay.”
The crowd cheered and applauded.
“Will’s instinctive and unselfish act saved our son’s life,” Carl’s father said afterwards.
“I don’t feel like I did something spectacular; I just saw someone who needed help,” Will said.
“I did what I felt was right.”
Appendix 4.1 – Experimental Materials for Chapter 4

Episodic Memory Prompts: Experiment 8, 9 and 10

*Good Outcome*

Please think of an experience in which you saw someone, or heard about someone, demonstrating humanity’s higher or better nature. Please pick an example in which you were not the beneficiary, that is, someone tried to do something good, honourable or charitable for someone else. Please think of a situation that resulted in a good outcome — a situation where the person’s actions were successful in helping another person. Please describe the experience briefly here:

*Bad Outcome*

Please think of an experience in which you saw someone, or heard about someone, demonstrating humanity’s higher or better nature. Please pick an example in which you were not the beneficiary, that is, someone tried to do something good, honourable or charitable for someone else. But please think of a situation that did not result in a good outcome — a situation where the person’s actions were not successful in helping another person. Please describe the experience briefly here:

**Elevation Scale: Experiments 8, 9 and 10**

Please take a moment to consider the events that occurred and imagine the thoughts and feelings you have about them.

How much did you think or feel the following when thinking about your memory?

- Inspired
  - Not at all 1
  - 2
  - 3
  - 4
  - 5
  - 6
  - 7 A lot

- I want to be more like the person I thought of who did something good/honourable/charitable.
  - Not at all 1
  - 2
  - 3
  - 4
  - 5
  - 6
  - 7 A lot

- People are really good.
  - Not at all 1
  - 2
  - 3
  - 4
  - 5
  - 6
  - 7 A lot

**Counterfactual Thought Prompt: Experiments 8 and 10**

Sometimes, people think about how an event could have turned out differently in the past 'if only...' Please think about the experience you wrote about and about how it could have turned out differently.

Please complete the following sentence:
Things could have been different if...

Prefactual Thought Prompt: Experiments 9 and 10
Sometimes, people think about how an event could have turned out differently in the future ‘if only...’ Please think about the experience you wrote about and about how a similar event in the future could turn out differently.

Please complete the following sentence:

*Things could be different in the future if...*

Factual Thought Prompt: Experiment 10
Sometimes, people think back on event exactly as they happened. Please think about the experience you wrote about and think about the facts of the event.

Please write here any further thoughts you have about the event as it happened.

Counterfactual Self-Coding: Experiments 8 and 9
When you wrote about how things could have been different, did you write about...

...how things could have been:
\*    Worse
\*    Better
\*    The same

...something that:
\*    happened and imagine it had not happened
\*    did not happen and imagine it had happened

...the person who did something good and imagine:
\*    something else within their control
\*    something else outside their control
\*    I did not think about the actor/agent

Intention Question: Experiments 8, 9 and 10
Considering what you have thought about, do you think this would change your own behaviour in the future?

Please briefly describe your intentions:

Would you agree to be contacted regarding a short, online follow-up study?

Follow-Up Study: Experiments 8 and 9
When you took part in the survey, you indicated that thinking about the memory made you change your own intentions. This is what you wrote:

[Participant’s intention was presented here]
In the weeks since you took part in the study, have you acted on this intention?
  • Yes
  • No

[If Yes]
Please describe what you have done:

[If No]
Please provide any further information you would like to add:

**Charity Donation: Experiment 10 only**

Thank you for taking part.

Prolific Academic allows you to allocate some amount of your reward to one of their chosen charities, Cancer Research UK and Save the Children. If you would like to donate some portion of your reward to one of these charities, please indicate so on the scale below


Which charity would you like your donation to go?
  • Cancer Research UK
  • Save the Children
Appendix 4.2 – Supplementary Analyses for Chapter 4

**Experiment 8**

*Intentions.* The three independent raters classified participants intentions as “goal intentions” that describe a desired end state, or “implementation intentions” that detail the conditions under which a behaviour will be performed (Ajzen, Czasch & Flood, 2009; Gollwitzer, 1999; Gollwitzer & Brandstätter, 1997). Interrater reliability between three independent raters for implementation/goal intentions was low to moderate, mean $\kappa = .27$, but raters 1 and 2 had high agreement, $\kappa = .66$. Disagreements were resolved with discussion.

Participants in the good outcome group tended to generate goal intentions ($n = 15$, 83.3% goal intentions), binomial $p = .008$, whereas those in the bad outcome group tended to generate goal or implementation intentions equally ($n = 17$, 71% goal intentions), $p = .064$, the difference between the groups was not significant, $\chi^2 < 1$.

Intentions were also coded as domain-specific intentions if they were in the same domain as the memory, e.g. to provide food to homeless people, or domain general, e.g. to do more good deeds. Participants in both groups tended to generate specific and general intentions equally, $ps > .307$, and there was no difference between the good outcome ($n = 18$; 50% specific intentions) or bad outcome ($n = 24$; 63% specific intentions) groups, $\chi^2 < 1$.

The 42 participants who reported intentions to change their behaviour agreed to be contacted with a short online follow up study. They were emailed one month after the experiment, shown the intention they had written and asked whether they had acted on this intention and to provide details. Of the 33 participants (79%) who completed the follow up questions, 58% reported having acted on their intention, 25% reported thinking about their intention but not having the opportunity to act on it, 12.5% said they forgot about their intention and 3% did not give sufficient detail to interpret. Participants tended to have acted on their intention whether they had recalled a memory with a good outcome (71% acted) or a bad one (47% acted), $\chi^2 (N = 33, 1) = 1.91, p = .167$. There were no differences between intentions that were specific (53% acted) or general (64% acted), $\chi^2 < 1$, or implementation (44% acted) or goal (62.5% acted), $\chi^2 < 1$.

**Experiment 9**

*Intentions.* Interrater reliability for implementation/goal intentions was again low, mean $\kappa = .17$, but disagreements were resolved with discussion. Participants in the good outcome group tended to generate goal intentions ($n = 23$, 82% goal intentions), $p = .001$, whereas those in the bad outcome group tended to generate goal or implementation intentions equally ($n = 12$, 70% goal intentions), $p = .143$; the difference between the groups was not significant, $\chi^2 < 1$. Participants in both groups tended to generate specific or general intentions
equally, and there was no difference between the good outcome ($n = 16$, 57% specific, $p = .572$), as or bad outcome groups ($n = 12$, 71% specific, $p = .143$), $\chi^2 < 1$.

The 45 participants who reported intentions to change their behaviour agreed to be contacted with a short online follow up study. Of the 35 participants (78%) who completed the follow up questions, 15 (43%) reported having acted on their intention, 37.1% reported thinking about their intention but not having the opportunity, 11.4% forgot about their intention and 8.6% did not give sufficient detail to interpret. Participants tended to have acted on their intention whether they had recalled a memory with a good outcome or a bad one (43% in each case), $\chi^2 < 1$, and in both groups participants were as likely to act as not act on their intentions, $ps > .664$. There were no differences between intentions that were specific (38% acted) or general (54% acted), $\chi^2 < 1$, but implementation (11% acted) were less likely to be acted on than goal (56% acted), $\chi^2 (N = 34, 1) = 5.41, p = .020, V = .40$.

**Experiment 10**

*Supplementary imagined alternative analyses.*

The full breakdown of the binomial comparisons for imagined alternatives in Experiment 10 are reported in Tables A7 and A8.

Table A 7.

**Binomial Comparisons for Imagined Alternatives about Good Outcomes in Experiment 10.**

<table>
<thead>
<tr>
<th>Characteristic</th>
<th>Type</th>
<th>Counterfactual</th>
<th>Pre-factual</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$n$</td>
<td>%</td>
<td>$p$</td>
</tr>
<tr>
<td>Direction</td>
<td>Better</td>
<td>3</td>
<td>6</td>
</tr>
<tr>
<td></td>
<td>Worse</td>
<td>48</td>
<td>94</td>
</tr>
<tr>
<td>Structure</td>
<td>Add</td>
<td>6</td>
<td>12</td>
</tr>
<tr>
<td></td>
<td>Subtract</td>
<td>45</td>
<td>88</td>
</tr>
<tr>
<td>Controllable</td>
<td>Yes</td>
<td>33</td>
<td>65</td>
</tr>
<tr>
<td></td>
<td>No</td>
<td>18</td>
<td>35</td>
</tr>
</tbody>
</table>

Table A 8

**Binomial Comparisons for Imagined Alternatives about Bad Outcomes**

<table>
<thead>
<tr>
<th>Characteristic</th>
<th>Type</th>
<th>Counterfactual</th>
<th>Pre-factual</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$n$</td>
<td>%</td>
<td>$p$</td>
</tr>
<tr>
<td>Direction</td>
<td>Better</td>
<td>53</td>
<td>98</td>
</tr>
<tr>
<td></td>
<td>Worse</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>Structure</td>
<td>Add</td>
<td>47</td>
<td>87</td>
</tr>
</tbody>
</table>
Subtract 7 13 0 0
Controllable Yes 18 33 .02 20 .47 .761
No 36 67 23 .53

**Direction.** The frequency of better alternatives was the same for the counterfactual (53.8%) as pre-factual (46.2%) groups, $\chi^2 < 1$. Better alternatives were more frequent after bad outcomes than good outcomes in the counterfactual groups, $\chi^2 (N = 105, 1) = 89.71, p < .001, V = .92$, and the pre-factual groups, $\chi^2 (N = 97, 1) = 78.85, p < .001, V = .90$. There was no difference between counterfactual and pre-factual groups when the outcomes were good, $\chi^2 < 1$, or bad, $\chi^2 < 1$.

**Structure.** The frequency of additive alternatives was the same for counterfactual (47.7%) as pre-factual (52.3%) groups, $\chi^2 (N = 202, 1) = 1.77, p = .184$. Additive alternatives were more frequent after bad outcomes than good outcomes in the counterfactual groups, $\chi^2 (N = 105, 1) = 59.45, p < .001, V = .75$, and the pre-factual groups, $\chi^2 (N = 97, 1) = 51.94, p < .001, V = .73$. Participants in the pre-factual groups were more likely to add something to reality when the outcome was good, $\chi^2 (N = 105, 1) = 4.20, p = .040, V = .20$, and when the outcome was bad, $\chi^2 (N = 97, 1) = 6.01, p = .014, V = .25$, compared to the counterfactual groups.

**Controllability.** The frequency of controllable alternatives was the same for counterfactual (49.5%) as pre-factual (50.5%) groups, $\chi^2 < 1$. The overall effect of outcome on controllability was significant, $\chi^2 (N = 202, 1) = 10.42, p = .001, V = .23$, as controllable alternatives were imagined more frequently for good outcomes (61.9%) than bad (39.2%). There was no difference between counterfactual and pre-factual groups when the outcomes were good, $\chi^2 < 1$, or bad, $\chi^2 (N = 97, 1) = 1.75, p = .187$.

**Intentions**
Participants in the six groups who reported intentions to change (counterfactual-good $n = 26$, counterfactual-bad $n = 30$, pre-factual-good $n = 27$, pre-factual-bad $n = 26$, factual-good $n = 24$, factual-bad $n = 27$) were asked to provide details about their intentions. Participants tended to report goal intentions (goal: counterfactual-good 70%, $p = .052$, counterfactual-bad 77%, $p = .005$, pre-factual-good 74%, $p = .019$, pre-factual-bad 54%, $p = .845$, factual-good 79%, $p = .007$, factual-bad 81%, $p = .002$) and there was no difference between the groups, $\chi^2 < 1$. There was no difference in the frequency of specific and general intentions between the groups (specific: counterfactual-good 48%, $p = .99$, counterfactual-bad 70%, $p = .043$, pre-factual-good 63%, $p = .248$, pre-factual-bad 73%, $p = .029$, factual-good 54%, $p = .839$, factual-bad 52%, $p = .999$), $\chi^2 < 1$. 

Appendix 4.3 – Information and Debrief Sheets for Chapter 4

Information: Experiments 8 and 9
This study is centred on moral reasoning. The study is being carried out by Shane Timmons (PhD Student in Psychology), with the supervision of Prof Ruth Byrne. The aim of the study is to assess recalling moral memories.

Participation in this study will take about 15 minutes. You may withdraw at any time if you wish, without penalty, up until the submission of the report for this study. The study is anonymous and confidential. The data will be stored in accordance with the Freedom of Information and the Data Protection Acts. You are entitled to access data stored about you. In order to access this information, please retain the ID code at the top of this sheet. In this study the data will be scored as group scores and participants will not be identified individually. You may request access to a copy of the final report when it is available if you wish.

In the study, you will first be asked to recall a past memory, and then asked to answer some questions based on that memory.

You are free to withdraw during any stage of the experiment without penalty, and may take a break between each task if required.

If you are a JF or SF student, you will be entitled to research credits for your participation, or you may opt for a cash payment of €10 at the end of the testing sessions.

If you have any further questions about the study, or would like to access a summary of the findings, please do not hesitate to contact us at the following:

Consent Form
Please sign below to indicate that you have read and understand the information above and that you give your consent to participate. You may keep the information sheet if you wish. Please give this consent form to the experimenter.

Name (please print):
Signature:
Please also provide the following information:

Age: __ Gender: __

Debrief: Experiments 8 and 9
Thank you for participating in this study. The aim of this study is to examine how recalling past memories might impact future behaviour. If you have concerns about the study, or would like to access a summary of the findings, please do not hesitate to contact the lead experimenter at the contact details below.

Shane Timmons, School of Psychology, Trinity College, Dublin 2, Ireland
Email: timmonss@tcd.ie

Ruth Byrne, School of Psychology, Trinity College, Dublin 2, Ireland
Information: Experiment 10
This study examines how people think about past memories of moral situations. The study is being carried out by Shane Timmons, PhD student, supervised by Professor Ruth Byrne, from Trinity College Dublin, University of Dublin, Ireland.
Participation in this study will take about 5 minutes. You will be asked to recall a past memory and then asked some questions about it.
Your participation is voluntary and you are free to withdraw during any stage of the experiment without penalty, up to the point when the data is pooled for analysis. Should you wish to withdraw, any data already collected will be destroyed. You will be paid a sum of 50p in recognition of your participation.
The study is anonymous and confidential. The data will be stored in accordance with the Freedom of Information and the Data Protection Acts of the Republic of Ireland. You are entitled to access data stored about you. In this study, the data will be scored as group scores and participants will not be identified individually. You may request access to a copy of the final report if you wish.
If you have any further questions about the study, or would like to access a summary of the findings, please do not hesitate to contact:
Shane Timmons, School of Psychology, Trinity College, Dublin 2, Ireland
Email: timmonss@tcd.ie
Ruth Byrne, School of Psychology, Trinity College, Dublin 2, Ireland
Email: rmbyrne@tcd.ie
Telephone: +353 1 896 4054
This research is funded by the John Templeton Foundation.

Debrief: Experiment 10
You have now reached the end of the study.
The purpose of this study was to examine whether thinking about morally good memories in different ways affects people's intentions to act morally good themselves. In this study, you were either asked to think about how your memory could have turned out differently in the past, how a similar event could turn out differently in the future or to simply reflect on the facts.
We were also interested in measuring whether these affect prosocial behaviour following reading these types of stories, as previous research has found that reading morally elevating stories increases the likelihood people will engage in helping behaviour and increases donations to charity — in this study, the question about donating money to charity was simply a measure of prosocial behaviour and you will be paid the full reward, should you wish to donate money please do so on the Prolific Academic website.
If you have any questions about the study or would like to access a summary of the findings, please do not hesitate to contact the experimenters at the contact details below.

Shane Timmons, School of Psychology, Trinity College, Dublin 2, Ireland
Email: timmonss@tcd.ie

Ruth Byrne, School of Psychology, Trinity College, Dublin 2, Ireland
Email: rmbyrne@tcd.ie  Telephone: +353 1 896 4054

This research is funded by the John Templeton Foundation.