Investigating the Impact of Mood on Children's Self-Control

Sophie Turnbull

Thesis submitted for the degree of Doctor of Philosophy (PhD)

Department of Psychology

The University of Sheffield

January 2018

Abstract

The aim of this research was to improve our understanding of how mood state affects children's self-control. Previous research examining emotion-cognition interactions in children through experimental approaches has been sporadic and has generated mixed findings. For example, only a few studies have investigated the effect of mood on executive function, with some finding positive effects (e.g. Pnevmatikos & Trikkaliotis, 2013), whilst others have not (e.g. Hawes et al., 2012). The interpretation of these data is further hampered by methodological limitations, including a lack of inclusion of measures of mood, and use of mood inductions with limited evidence of efficacy. In the current research, these issues were addressed by (i) conducting a series of studies to try to systematically replicate previous findings, and (ii) improving the methodology. The findings demonstrated a consistent lack of any effect of mood on children's self-control. Collectively, the results put forward a strong case for an absence of effect of mood state on children's self-control, and provide insights into techniques used to manipulate mood state in childhood.

Acknowledgements

I would like to thank Dan Carroll for being such a supportive supervisor. Thanks for never looking at your watch when meetings rolled over an hour, for being willing to wrap your brain around some puzzling data, and for always making me feel valued as a student. From you I have learnt that to be a “good” researcher means not only to be diligent and critical, but also generous and considerate.

Thank you to the schools, teachers, parents, and children of Sheffield who participated in the research for warmly accommodating me. And thanks to my excellent summer project student Anna Byrne who helped with data collection, and to Ellen Ridley and Ben Higgins for your assistance with coding.

Thank you to all of the members of the Sheffield Babylab. It’s been an honour to be part of such a friendly and helpful research group. Thanks to Elena Hoicka, for her advice on statistics and for stepping in when I needed. Thank you to my office buddies, Birsu, Andrea, Emma, and Ed for our many discussions about research and beyond, and thank you to Michelle for your encouragement.

I also wish to thank those in my life who supported and inspired me through my journey in education. Thanks especially to Sue Wilson and Tom Morton for encouraging me in my application to university.

I want to thank my family and friends for their ongoing support. Thanks Mum and Dad for fostering my curiosity, and for encouraging me to do my best, but always making me feel loved no matter what I achieve. Lastly, a huge thank you to Ross. Thanks for travelling together with me on this journey, for being there to advise me at every turn, to reassure me at every stumble, and most importantly to make me smile at every step.

Contents

List of Figures

List of Tables

Chapter 1: Introduction to the effects of mood on children's self-control

Children experience a variety of emotional states in daily life, including boredom, sadness, and frustration; joy, excitement, and pride. These emotions have the power to change children's way of thinking about and perceiving themselves and the world around them. For example, being upset after a fight in the playground might make it difficult to concentrate in class, or feeling pride from good work could encourage a child to help others with their work.

Of the many psychological capacities that are impacted by children's emotions, self-control is especially likely to be affected. Self-control is a broad construct that encompasses a number of distinct capacities and traits. Underlying self-control capacities is the ability to inhibit impulses in order to achieve goals. Examples of self-control in real life include turning off the television to concentrate on homework, or resisting lashing out at a peer to maintain a friendship. As such, self-control requires prioritising goals and inhibiting desires, processes that emotional states are liable to inform or bias.

Investigating the impact of emotional states on children's cognition generally, and self-control capacities specifically, is important for three key reasons. Firstly, understanding and regulating the consequences of our emotions is a vital part of emotional development in childhood. Thus by investigating scientifically what the consequences of emotional states are, children might be better assisted to manage their emotions. Secondly, if emotional states contribute variance to cognitive tasks, this can inform us about what existing measures of cognition are tapping into. Thirdly, childhood self-control specifically is related to later health, academic performance, and emotional coping (Blair & Razza, 2007; Bull & Lee, 2014; Mischel, Shoda, & Peake, 1988; Razza & Raymond, 2013; Schlam, Wilson, Shoda, Mischel, & Ayduk, 2013). Therefore understanding how situational factors affect self-control capacity could lead to interventions to promote children's application of self-control, and in turn improved academic performance, health and emotional skills.

This thesis focuses on the effect of incidental emotion, rather than integral emotion, on self-control. There is a key distinction in emotion-cognition interactions between emotions that arise from within a current activity (integral emotions) and those that arise from other sources (incidental emotions) (Schmeichel & Inzlicht, 2013). When emotions are described as integral, this suggests that the emotions arise from, and are about, the same task that is being used to evaluate cognition. An example of the effects of integral emotion comes from research on children's regret. When children experience regret in response to choosing a prize envelope from two options, they are more likely to choose the alternative option given another chance (O'Connor, McCormack, Beck, & Feeney, 2015). This demonstrates how an emotion that arises from and is directed at a task (i.e. regret from choosing envelopes) impacts subsequent performance on the same task, and thus the effect of integral emotions on cognitive performance. Unlike integral emotions, incidental emotions arise from processes *unrelated* to the current activity. Incidental emotions include those that endured from a previous, unrelated activity, or emotions arising from a concurrently ongoing activity. For example, incidental emotions from watching a funny or sad video carry over to affect children's subsequent attention to detail (Schnall, Jaswal, & Rowe, 2008). Incidental emotions are more likely than integral emotions to affect cognition in maladaptive ways, because incidental emotions are by nature not directly connected with, and therefore not informative about, the task at hand.

The impact of incidental moods on children's self-control is understudied. To establish causal relations between incidental mood states and children's cognition, studies that experimentally manipulate mood need to be conducted. Relatively few studies have examined how induced mood impacts children's self-control. The few studies that do exist have not demonstrated systematic effects. Thus more research is needed to examine how children's incidental moods affect their self-control.

This chapter begins by defining mood, and provides a context for the research in this thesis by setting out the ways in which relations between mood and cognition can be studied. It then focuses specifically on mood induction, the methodology used in this thesis, and evaluates the literature pertaining to the efficacy of the two mood induction techniques used in the present research. Next, the literature investigating the effect of mood induction on two key aspects of self-control is examined: those are executive functions and intertemporal choice. Finally, methodological limitations are identified to determine where further research is most needed.

1.1 Emotion-cognition interaction

1.1.1 Definitions: emotion, mood and valence

Emotion is an umbrella term applied to both temporary feeling states (mood states, emotion states) and longer-term traits (for example depressive symptoms, or emotion-related personality traits such as optimism). All of these emotional phenomena contain a subjective sense of feeling good or bad, accompanied by physiological and behavioural patterns that are specific to the particular type of emotion experienced.

Mood states can be distinguished from emotional states in two key ways (Beedie, Terry, & Lane, 2005). Firstly, a mood state is a diffuse emotional experience that is not targeted at a specific event or object: mood states engender a general good or bad feeling, without a specific attribution of where the feeling comes from. This is different from emotional states, such as anger or pride, which carry specific appraisals: for instance, anger arises from a situation appraised as unjust, while pride results from an appraisal of successful performance. Secondly, moods also tend to operate on a longer timescale than emotions, lasting from minutes to hours, whereas emotions last from seconds to minutes.

The valence of a mood state describes the positivity or negativity, or pleasantness or unpleasantness, of that mood (Remington, Fabrigar, & Visser, 2000; Russell, 1980, 2003). Valence is considered as a continuous, bipolar construct, with positively and negatively valenced moods deviating from a neutral midpoint (Crawford & Henry, 2004). Emotions can also be categorised by valence – for example joy, pride and excitement are generally considered to be positively valenced, and anger, sadness and boredom are considered negatively valenced.

Mood valence is independent of the level of arousal engendered by a mood state. Arousal is defined as the level of activation or stimulation experienced from a mood or emotion (Feldman & Russell, 1999). It is associated with increased physiological activation and a feeling of alertness. Arousal is orthogonal to valence, as both positive and negative emotions can be high or low in arousal. For example, the negative emotion of boredom is low in arousal, whereas the negative emotion of anger is high in arousal.

Likewise, the positive emotion of contentment is low in arousal, while excitement is high in arousal.

The present research focuses on the impact of mood valence (hereon often referred to as *mood*) on self-control. The valence of our mood is the most fundamental aspect of our emotional experience, and therefore provides an obvious starting point for exploring the emerging field of emotion-cognition interactions in children.

1.1.2 Types of interactions between emotion and cognition

The focus of the present research – namely, the impact of mood states on self-control – is situated within a much broader realm of research examining the interactions between emotion and cognition. The research presented in this thesis uses experimental manipulations of mood, which is an effective way to test causal relations between emotion and cognition in children. However, it is informative to consider this approach in the context of the wider research field.

The interactions between emotion and cognition have been studied by researchers in six key ways. First, researchers have correlated survey measures of emotional traits or dispositions with cognitive capacities, for example to examine the relationship between trait anxiety and poor test performance (Bertrams, Englert, Dickhäuser, & Baumeister, 2013). Second, they have manipulated rewards for performance, to determine the impact of motivational context on cognitive performance (Pessoa, 2009). Third, emotional stimuli (e.g. happy and sad faces) have been included in cognitive tasks to determine the impact of emotion processing on cognitive performance (Bluell & Montgomery, 2014; Rebeck & Lohaus, 1986). Fourth, variability in naturally existing mood states has been measured and correlated with cognitive capacities, by following the same individuals at many time points (von Stumm, 2016). Fifth, researchers have measured emotional responses to tasks and related these to task performance, for example correlating test anxiety with working memory performance (Ng & Lee, 2010) or correlating regret with subsequent decision-making (O'Connor et al., 2015). Finally, researchers have manipulated mood to examine the effects of mood states incidental to the current task on cognitive performance, such as examining the effect of listening to happy or sad music on perception of slopes (Riener, Stefanucci, Proffitt, & Clore, 2011). The present research takes this final approach, using mood inductions to examine the effect of incidental mood state on cognitive ability.

Experimental manipulations of mood states are the most effective way to test causal relations between mood and self-control. Manipulating mood states experimentally has the advantage that the direction of causation can be established. One possible disadvantage is a loss of ecological validity, in that mood states induced in a laboratory experiment may not be similar to those experienced in real life, in terms of intensity or durability. However, previous research implies that laboratory manipulations can be sufficiently intense and durable to impact subsequent behaviour (e.g. Brenner, 2000; Isen, 2001; Martin, 1990; Rottenberg, Ray, & Gross, 2007).

A sparse literature has examined the effects of mood on children's cognition using experimental manipulations of mood states. This field is characterised by individual, relatively isolated studies more so than by systematic programs of research, and the small number of studies span a wide range of cognitive abilities. For instance, studies have examined the effect of mood induction on measures of children's long-term memory (Davis & Levine, 2013); learning (Masters, Barden, & Ford, 1979); divergent thinking (Greene & Noice, 1988); attention (Schnall et al., 2008); maths (Scrimin, Mason, & Moscardino, 2014); literacy (Tornare et al., 2017); spatial ability (Rader & Hughes, 2005); and general intelligence (Blau & Klein, 2010). Most of these studies have found that positive mood improves cognitive performance, or that negative mood impairs performance (although there are some exceptions, e.g. Schnall et al., 2008). However, there have been few attempts to replicate findings, or to examine the mechanisms of these effects. As such, the field would benefit from more focussed and systematic investigation of the effect of mood on specific areas of children's cognition.

1.1.3 Developmental changes in emotion-cognition interaction

Only cursory attention has been paid to how the effects of mood on cognition might change with development. The field has generally treated developmental populations in a homogenous way. One exception is a theoretical account which proposed that the impact of mood on cognition is likely to decrease over development, due to improvements in emotion regulation (Bugental, Lin, & Susskind, 1995). The authors argued that while younger children are dependent on others (e.g. caregivers) to regulate their emotions, with increasing age children develop strategies to independently regulate their emotions which reduce the intensity of emotional states and thus reduce their impact on cognition. However, the dearth of mood induction studies using developmentally sensitive designs means that there is very limited data to inform discussions about developmental changes in mood-cognition interactions. Thus, this thesis follows the convention of the field by treating children as a singular population, with the hope that this can provide a foundation for more detailed developmental investigation in future.

While there are numerous proposed methodologies for inducing mood in children, relatively few have sufficient empirical evidence relating to their efficacy. In the following section, the efficacy of mood inductions used in this thesis is considered.

1.2 Efficacy of mood inductions with children

Mood induction techniques for use with children are still relatively underdeveloped. With adults, a range of mood inductions have been demonstrated as effective, such as presenting emotionally valenced videos (Gross & Levenson, 1995; Rottenberg et al., 2007), or giving gifts (Isen, 2001). While a variety of mood induction techniques have also been implemented to try to induce mood in children, the efficacy of these techniques has often not been evaluated. Instead, efficacy is often assumed without being directly tested.

Thus it is essential to delineate some criteria for evaluating the efficacy of mood inductions with children, and to evaluate the extent to which existing interventions meet these criteria. In the following sections, criteria for evaluating mood inductions are first outlined. Techniques for measuring mood are then reviewed, as mood measures are essential to evaluate the efficacy of mood inductions in previous work and in the experiments presented in this thesis. This is followed by a discussion of the efficacy of the two mood induction techniques used in this thesis: specifically, video mood inductions and self-generated imagery mood inductions.

There are three key criteria that a mood induction should meet in order to be effective (as summarised in Table 1). The mood induction technique should induce sufficiently (1) divergent and (2) durable mood states, and ideally (3) have an impact on mood specifically, rather than on other psychological states. These three criteria are explained in more detail below. Of course, these criteria need to be met within the usual constraints for conducting experiments, including the need for procedures to be ethical and practical within laboratory circumstances. This is especially significant in the case of mood inductions, because manipulating moods – particularly inducing negative moods – creates potential for emotional harm to participants. This necessarily restricts the intensity of mood states that can be induced.

Table 1. Summary of criteria used to evaluate the efficacy of mood induction techniques.

Criterion	Description
1. Divergence	Induction technique produces sufficient differences in mood valence between conditions to be detectable through measures of mood valence.
2. Durability	Effects of inductions on mood are durable enough to last beyond the mood induction procedure.
3. Specificity	Induction conditions have similar demands and do not produce divergent changes in arousal, motivation or other confounding psychological states.

Effective mood inductions should meet three criteria: the first criterion, divergence, means that mood inductions should produce sufficiently divergent moods to be detectable on a measure of mood state. This is important because moods induced need to be sufficiently intense to potentially affect subsequent behaviour, and this impact should be measurable to demonstrate that the mood induction worked. The second criterion, durability, means that the effects of mood inductions on mood should last for a sufficient duration to have a subsequent effect on cognitive tasks. If the mood state generated does not last beyond the mood induction itself, it becomes impossible to investigate the effect of mood on subsequent behaviour. One way to indirectly infer that the mood state endured is if the mood induction had an impact on subsequent behaviour. The third criterion, specificity, means that as far as possible, the psychological impact of these conditions should be differences in mood valence specifically, rather than differences in other psychological states, such as motivation or arousal. This is important because determining the mechanisms of effects of mood induction on cognition depends on specifying the psychological process(es) influenced by the manipulation. To meet this criterion, conditions should ideally have comparable incidental demands (e.g. they should have the same degree of social interaction, or similar requirements for memory processes), in order to reduce confounds that might drive differences between conditions. Conditions should also be similar in terms of effects on children's arousal and motivation, because these factors are known to affect cognitive performance (e.g. Kanfer & Ackerman, 1989; Yerkes & Dodson, 1908), and could account for differences between mood induction conditions, rather than mood valence.

A variety of methodologies have been implemented to try to induce mood in children, but to date only a few of these techniques have sufficient evidence of efficacy. Examples of mood induction stimuli used with children are giving false feedback or rigging performance to simulate experiences of success or failure (e.g. Gobbo & Raccanello, 2007); exclusion or inclusion in a social game (e.g. Hawes et al., 2012); stories with emotional content (e.g. Fartoukh, Chanquoy, & Piolat, 2014); music with emotional connotations (Schnall et al., 2008); giving children gifts or rewards (e.g. Levine, Burgess, & Laney, 2008); and showing children affectively valenced images (e.g. Poirel, Cassotti, Beaucousin, Pineau, & Houdé, 2012). However, the techniques with the most evidence relating to their efficacy are videos with emotional content (e.g. von Leupoldt et al., 2007) and self-generated imagery (e.g. Tornare et al., 2016).

An incipient research base has emerged which offers preliminary evidence to indicate that video and self-generated imagery are effective mood induction techniques. Because measurement of mood state is an essential aspect of evaluating mood inductions, the next section provides an overview of techniques used for measuring children's mood, including techniques which will be used in the experimental work in this thesis.

1.2.1 Techniques for measuring children's mood

To assess whether mood inductions are effective, a reliable measure of children's mood valence needs to be used. Measures used with children include self-report, behavioural, and physiological indices. Measures can be used to gauge either absolute mood, that is mood at a specific point in time (such as immediately after the mood induction), or change in mood, that is mood at two time points relative to each other (such as the difference between mood before and after the mood induction).

Self-report scales with schematic faces are the standard tool for measuring children's mood state. Most studies have used single scale items with schematic faces as response anchors (see Figure 1). Children are typically given these scales after a mood induction, and asked to evaluate their current mood (e.g. "Which one shows me how you feel right now?", Shimoni, Asbe, Eyal, & Berger, 2015) or their response to the mood induction stimulus (e.g. "How did the story make you feel?", Rader & Hughes, 2005). One example of such scales is the Self-Assessment Manikin or SAM (Hodes, Cook, & Lang, 1985; see Figure 1A and B). The SAM includes 9-point scales for valence and arousal, and both scales comprise drawings of a character with expressions, ranging from very unhappy to very happy (for valence), and from asleep to wide awake (for arousal). Other studies have used similar drawings of faces with five anchors (e.g. Schnall et al., 2008, see Figure 1C).

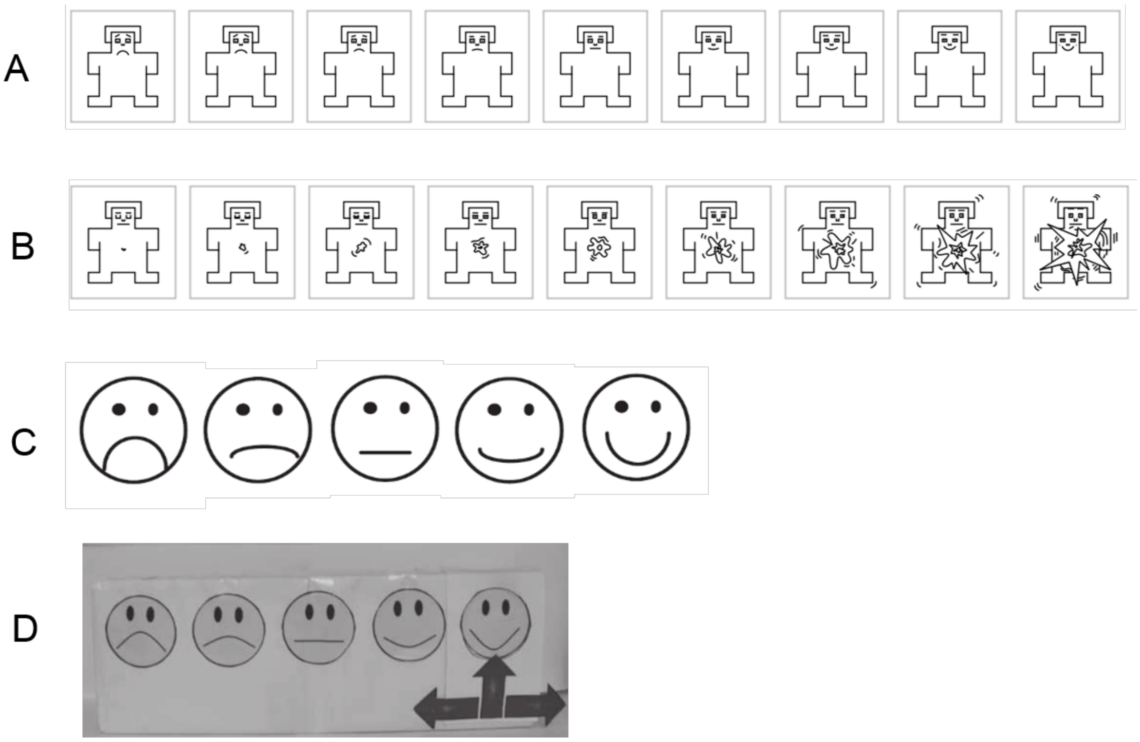

Figure 1. Mood self-report scales used in mood induction studies with children. (a) Self-Assessment Manikin (SAM) valence scale (from Hodes et al. 1985). (b) SAM arousal scale (from Hodes et al., 1985). (c) Faces scale (from Schnall et al., 2008). (d) Mood change scale (from Weisberg & Beck, 2012).

Self-report face scales provide an age-appropriate means to assess children's subjective mood state. They are likely less reliant on verbal abilities than standard Likert-type scales used with adults – such as the Positive And Negative Affect Schedule (Watson, Clark, & Tellegen, 1988). Such verbal Likert-type scales would require children to understand nuanced adjectives (e.g. hostile, inspired) and verbal response anchors. By depicting emotions visually, pictorial scales tap into children's understanding of positive and negative facial expressions, which emerges by the preschool years (LoBue, Baker, & Thrasher, 2017). Studies have used single-item self-report face scales to measure mood effectively with children as young as 5 years old (e.g. Gobbo & Raccanello, 2007; Levine et al., 2008; Williams, O'Driscoll, & Moore, 2014). Self-report face scales therefore appear to be an appropriate tool for measuring children's mood state.

The approach to measuring mood used in existing research – specifically using a single, absolute measure of mood following mood induction – could be improved by implementing a measure of *change* in mood instead. Previous research which used a single, absolute measure could not control for baseline mood, and had no measure of the change in children's mood from before to after the mood induction. As an alternative, there are two possible approaches to measure changes in children's mood state. One way to measure change in mood is to have children self-report their mood on an absolute scale before mood induction, and on the same scale after mood induction. The difference between the two ratings would give an index of mood changes. Another way to measure change in mood is to use a scale that requires children to judge their current mood *relative* to the past. Researchers investigating children's experience of regret have employed a child-appropriate scale that measures whether children's mood has improved, worsened, or stayed the same following an emotional event (e.g. Weisberg & Beck, 2012). This scale (shown in Figure 1D) works as follows: prior to the emotion induction, children rated their baseline mood using a five-point face scale. Following the emotion induction, children rated their current mood by pointing to one of three arrows representing feeling worse (left arrow), the same (central arrow), or better (right arrow) than before. Thus there are two feasible approaches to measure changes in mood.

There are two possible limitations to self-report measures of children's mood which have been given little attention by researchers in the field of mood induction. Firstly, the sensitivity of self-report is limited due to a general response bias towards positive mood ratings (e.g. Levine et al., 2008; Rader & Hughes, 2005). This could be due to either a genuine tendency for mood to be positive in the absence of strong emotional events (known as positive mood offset, Diener & Diener, 1996), or due to a difficulty in accurately reporting nuances in positive mood (Chambers & Johnston, 2002). Either way, this means that self-report scales can sometimes have limited sensitivity in measuring mood. Secondly, there is a risk that self-report measures are affected by demand characteristics arising from the mood induction. This is especially likely if the question used to elicit self-reports refers to the mood induced within the mood induction (e.g. "How did you feel when you were thinking about things that make you happy?").

The limitations of self-report can be overcome by implementing additional measures of mood, namely observations of facial expressions (e.g. Blau & Klein, 2010; Levine et al., 2008; Ridgeway & Waters, 1987) and physiological measures (e.g. Blau & Klein, 2010; Ridgeway & Waters, 1987). Facial expressions of fundamental emotions (e.g. happiness, fear, or sadness) are thought to be consistent and detectable by observers (Ekman, 1970; Izard, 1994). Rates of expression of these emotions can be reliably identified by coders during mood inductions, including when recalling emotional events (Masters et al., 1979; Ridgeway & Waters, 1987) or watching videos (Blau & Klein, 2010). One potential limitation of using facial expressions as an index of mood is that expressions tend to be a consequence of specific emotions (e.g. sadness or fear) rather than mood valence more broadly. Expressions might also be less frequent in non-social mood inductions, where the communicative function of emotions is removed (Jakobs, Manstead, & Fischer, 1999). Physiological measures include measures of heart rate and heart rate variability. Heart rate and heart rate variability are reliable indices of emotional arousal (Appelhans & Luecken, 2006), but it has yet to be conclusively shown whether physiological measures can distinguish positive from negative mood valence (Appelhans & Luecken, 2006; Tiller, McCraty, & Atkinson, 1996). Due to the limited scope of observation to detect moods in non-social situations, and the apparent inability of physiological measures to assess mood valence independent of arousal, these measures are arguably best used in concert with self-report face scales.

In summary, self-report face scales and observations of facial expressions are the best available tools for measuring mood valence in children. Self-report face scales and physiological measures can also be used to assess arousal. In the next section, the efficacy of mood inductions assessed using self-report, observation, and physiological measures is summarised. The mood inductions are evaluated with reference to the three criteria: (i) divergence, (ii) durability, and (iii) specificity.

1.2.2 Video mood inductions

Videos with emotional contents have been presented in a handful of studies with children to manipulate mood (Blau & Klein, 2010; Rice, Levine, & Pizarro, 2007; Schnall et al., 2008; von Leupoldt et al., 2007; Williams et al., 2014). Videos are believed to arouse emotions through exposure to emotional content (e.g. positive content such as singing and dancing, or negative content such as the death of an animal) and/or empathic experiences of emotions for characters within the narrative.

The existing evidence suggests that video mood inductions are effective with children. However, because the literature is relatively sparse, it is difficult to draw firm conclusions. To demonstrate the efficacy of video mood inductions, evidence is discussed in relation to the divergence, durability, and specificity criteria.

1.2.2.1 Divergence

Presenting emotion videos appears to produce sufficiently divergent moods in children according to self-report measures. In a study with 6- to 12-year-old children, von Leupoldt et al. (2007) showed three 3-minute clips from animated children’s films to groups of children who subsequently rated their mood using a self-report face scale. The videos were of a lion mourning the death of its father (negative condition); natural scenes from the opening credits of a film (neutral condition); and a song about living a care-free life (positive condition). There was a significant difference in valence ratings between all mood conditions, such that valence ratings were highest in the positive condition, and lowest in the negative condition, with the neutral condition in between. In another study, the same videos were shown to 6- to 7-year-olds (Schnall et al, 2008). This found the same pattern of mood ratings, with the positive condition higher in valence than the neutral condition, and the neutral higher than the negative condition. These data suggest that videos can induce divergent mood states in children according to self-report.

Video mood inductions can also be effective with younger children. One study found that videos can arouse mood states in 4- to 5-year-old children using observational measures of emotion (Blau & Klein, 2010). Two 6-minute clips were used for a positive mood induction, including a happy song and a comedy cartoon, and two 6-minute clips for a negative mood induction, comprising a mourning scene and a character being trapped in a cage. The children's facial expressions were observed whilst they watched the videos. These expressions were coded for basic emotions (joy, fear, anger, sadness, disgust). Higher levels of joyful facial expressions were identified during the positive than the negative video, and higher rates of negative facial expressions during the negative than the positive video. This suggests that videos can effectively induce differences in emotional state in younger children.

There appear to be age-related trends in the effectiveness of video mood inductions. This is suggested by a study which examined the effect of watching sad videos on children's ratings of their own mood, and that of the character within the narrative (Williams et al., 2014). In the negative condition, children viewed a video about a child who lost her pet, while in the neutral condition children viewed a video about a yard sale. For 5- to 6-year-olds, they found a difference in mood between negative and neutral conditions, suggesting that the induction was effective. There was no difference for 3- to 4-year-olds in the sample. The data showed that the younger children could understand that the character in the video felt sad, but did not feel sad themselves. This suggests that videos may not be effective with children under 4, especially when the impact of the video relies on vicarious experience of emotions through empathy with a character.

The effectiveness of video mood interventions also seems to be determined in part by the emotion regulation strategies that children use. A study with 5- to 11-year-olds induced negative mood with a video in which a child's father was injured, and neutral mood with scenes from a daily routine such as a child getting ready for bed (Rice et al., 2007). Children in the negative condition rated their mood as sadder on a self-report face scale than those in the neutral condition. Children in the negative condition also performed more poorly in a memory test. This suggests that the mood inductions were effective and produced a subsequent change in children's behaviour. Importantly, children in another condition who were instructed to employ emotion regulation after watching the sad video (actively disengaging from their feelings, by remembering that the events are not real) subsequently performed better in the memory test than children who watched the sad video and were given no emotion regulation instructions. This suggests that down-regulating negative emotions (that is reducing the negative emotions' intensity by suppressing or reappraising them) removes the effect of mood induction on subsequent behaviour. Therefore, the effectiveness of videos in inducing emotional reactions may depend on the emotion regulation processes that children use. If children employ emotion regulation strategies, the efficacy of the mood induction will be reduced. Indeed, children do employ emotion regulation strategies such as expressive suppression and cognitive reappraisal to varying degrees in childhood (e.g. Gullone & Taffe, 2012). Thus video mood inductions are effective at inducing divergent neutral and negative mood states, but their efficacy is likely to be reduced if certain emotion regulation strategies are employed.

1.2.2.2 Durability

The evidence base for the durability of video mood inductions is quite limited, but the existing evidence indicates that moods induced via video are durable. Differences have been found between induction conditions in subsequent cognitive tasks, suggesting that the effects of the mood induction lasted beyond the induction itself. Video mood inductions have been shown to have an effect on a range of cognitive measures, including measures of children's attention to detail (Schnall et al., 2008), semantic memory (Rice et al., 2007), and recognition memory for faces (Blau & Klein, 2010). The effect on mood may only last a few minutes, as mood was found to return to baseline following the cognitive task in one study which collected mood ratings later in the experiment (Rice et al., 2007). This suggests that there may be a limited window within which mood may affect subsequent cognition. Overall, effects of video mood inductions seem to be sufficiently durable to influence subsequent behaviour.

1.2.2.3 Specificity

To demonstrate specificity, video mood induction conditions should both have similar incidental demands, and should also not produce changes in arousal, motivation or other confounding psychological states. Conditions with similar incidental demands can be created using videos, although this has not been done consistently in past studies. When using video mood induction, conditions are comparable in terms of the experiences implicated in watching a video, such as processing audio-visual stimuli and following a narrative. The key difference between conditions should be only in the emotional content – for instance, studies have included a happy song for the positive condition and a mourning scene for the negative condition (von Leupoldt et al., 2007; Schnall et al., 2008). In some previous studies, conditions also arguably differed in other ways. For example, the neutral condition video used by von Leupoldt et al (2007) and Schnall et al (2008) was a titles reel from a film, which was likely less engaging than the other conditions, requiring less focussed attention. Thus while it is possible to create video mood induction conditions with comparable incidental demands, this has not always been applied in previous studies.

There is some suggestion that the effects of video mood inductions are not specific to valence, but also affect arousal. One study measured the effect of video mood induction on arousal, as well as valence (von Leupoldt et al., 2007). Arousal was measured using the arousal self-report scale from the Self-Assessment Manikin (see Figure 1B). With this scale, children were asked to rate their arousal from "not aroused/relaxed", to "highly aroused/excited". They found that children felt more aroused when watching the negative than the positive video, and more aroused when watching the positive than the neutral video. This suggests that effects on mood valence were confounded by effects on arousal. This is preliminary evidence that video mood inductions can affect arousal, and thus may not manipulate mood valence alone. No studies have measured the effect of mood on task motivation, which may also be a confound. Therefore, the possibility that effects of video mood inductions on subsequent cognition are due to arousal or task motivation cannot be ruled out.

1.2.2.4 Summary of the efficacy of video mood inductions

In summary, there is a small amount of good evidence that videos effectively generate divergent mood states. Whether sufficiently divergent mood states are induced may depend on children's age; on their ability to empathise with characters; and on their tendency to regulate their emotions. Limited existing evidence implies that the effects of video mood inductions are sufficiently durable, although the duration is probably restricted to a few minutes. Videos can be selected to have similar incidental demands across conditions. However, the single existing study that measured arousal following a video mood induction suggested that emotional videos might affect arousal as well as mood valence. Overall, the existing data suggests that videos are an effective mood induction for use with children, although they may have additional effects on arousal. In the next section, the efficacy of self-generated imagery mood inductions is discussed.

1.2.3 Self-generated imagery mood inductions

Self-generated imagery is the most common form of mood induction used with children (Brenner, 2000). Paradigms using self-generated imagery require participants to recall specific instances of experiencing an emotion, or objects or events that tend to make them feel that emotion. For example, children might be asked: "Imagine something that makes you really happy". Participants share the memory verbally with the experimenter before reflecting on it internally. It is hypothesized that imagining such experiences will invoke the related emotion. Several studies have demonstrated that children tend to generate appropriate examples in response to such prompts (e.g. Horn & Arbuckle, 1988; Masters et al., 1979; Ridgeway & Waters, 1987). For instance, Ridgeway and Waters (1987) found that 5-year-olds could generate sensible examples of exciting events (having a birthday party, getting lots of toys), calm events (reading a book, watching TV), and sad events (having no friends to play with, or being told off by a parent).

Despite self-generated imagery being the most commonly used mood induction technique with children, there remains a relative dearth of evidence for its efficacy. This is because few studies using this technique have included a measure of mood, and instead have made an assumption that the mood induction was effective without testing this. The few studies that have measured children's mood suggest that self-generated imagery is usually effective at inducing mood in children.

1.2.3.1 Divergence

According to the small amount of existing data, self-generated imagery mood inductions appear to be effective in terms of producing divergent mood states between conditions. Two studies conducted with 10- to 11-year-olds suggested that a positive self-generated imagery induction affected self-reported mood (Tornare et al., 2016). Children in the positive condition were asked to vividly recall a positive school experience, while those in the neutral condition had no intervention. Both conditions rated their level of joy on a 5-point verbal Likert scale. Across two studies, children in the positive condition rated their mood as more joyful than those in the neutral condition. This suggests that positive self-generated imagery can boost mood relative to no activity. A further study suggested that self-generated imagery affected self-reported mood in 6- to 12-year-olds (Stegge, Terwogt, & Koops, 1994). Children either recalled happy or sad personal experiences, or imagined their classroom (as a neutral condition). A mood rating was taken at the end of the experiment, which consisted of asking children whether imagining the event had made them feel happy, sad or "just normal", and subsequently asking them to rate how sad or happy they were on a scale from 1 to

5. Children reported feeling happier in the positive condition than the neutral condition, and sadder in the negative than the neutral condition. This suggests that self-generated imagery can induce positive, neutral and negative moods. Studies have also demonstrated impacts of positive and negative self-generated imagery inductions on mood in children as young as 5 years old when mood is measured with a simplified self-report mood measure (Levine et al., 2008). Therefore, there is some convincing evidence that self-generated imagery affects mood according to children's self-report.

Convergent evidence for the efficacy of imagery manipulations comes from studies using observer ratings of children's facial expressions. Some studies found overall differences in ratings of facial expressions between happy, sad, angry and neutral conditions (Carlson, Felleman, & Masters, 1983) and positive, negative, and neutral conditions (Masters et al., 1979). But specific statistical comparisons were not made, so it was unclear whether there were differences between all conditions or only some. A further study found evidence that emotional expressions differed between positive, neutral and negative self-generated imagery conditions (Ridgeway & Waters, 1987). Children were asked to recall positive, neutral, or negative events and their facial expressions during the induction procedure were rated in terms of eight emotions varying in arousal and valence. Children in the positive condition expressed the most positive emotion, followed by the neutral condition, which in turn expressed more positive emotion than the negative condition. Therefore, self-generated imagery can elicit positively or negatively valenced mood according to children's facial expressions.

However, the effect of self-generated imagery on mood appears to be either inconsistent, or highly context-dependent. A few studies have found no evidence of effects of self-generated imagery on mood. In one such study, 7- to 12-year-olds were asked to generate sad or happy memories, or to answer neutral questions about themselves (e.g. age, where they live) to induce mood (Barnett, King, & Howard, 1979). They rated how they felt during the induction period on a 5-point verbal Likert scale (from very happy to very sad) at the end of the experiment. There was a difference in mood ratings between children in the negative and neutral conditions. But, there was no difference between positive and neutral conditions. This could either be because the moods of children in the positive and neutral conditions did not differ enough, or because the mood scale was not sensitive to differences between neutral and positive conditions. Indeed, children in the neutral condition rated their mood positively. Together, the findings demonstrate that paradigms using self-generated imagery can induce sufficiently divergent moods, although there is some question over whether they do so consistently.

1.2.3.2 Durability

Self-generated imagery inductions can have a durable impact on mood. This is suggested by studies where inductions have effects on subsequent cognitive tasks. For instance, following different self-generated imagery inductions, children have shown differing performance in grammar tasks (Tornare et al., 2016), judgements of emotional expressions (Stegge et al., 1994), and memory (Levine et al., 2008). This suggests that the mood induced lasted for several minutes at least, in order to have an impact on subsequent behaviour. However, in some studies there has been an effect of self-generated imagery induction on measures of mood, but no effect of mood induction on subsequent cognition (Bryan & Bryan, 1991; Tornare et al., 2016). These data could be interpreted in one of two ways. They could suggest that the mood induction did not endure for long enough to affect subsequent behaviour. Alternatively, it could be that the induced mood *did* endure, but that there was genuinely no effect of mood on that aspect of cognition. Therefore, when considering all available evidence, self-generated imagery can produce durable effects on mood, but it is difficult to ascertain whether it does so consistently.

1.2.3.3 Specificity

There is very limited evidence about whether self-generated imagery induces specific effects on mood valence. A few studies have used self-generated imagery conditions which appear to have comparable incidental demands, but many have not. When neutral conditions have different demands, it increases the likelihood that confounding differences between conditions, other than in the emotional valence, could drive differences in subsequent cognition. Positive and negative conditions require children to recall and discuss emotional events, which likely involves similar processes in terms of autobiographical recall, mental imagery, and social interaction. Some neutral conditions have also included imagery, such as imagining a named neutral object – for example, their classroom, or a chair (Stegge et al., 1994; Kenrick, Baumann, & Cialdini, 1979); or recalling the names of classmates (Yates, Lippett, & Yates, 1981). This likely means they have similar incidental demands. Other studies have used neutral conditions with slightly different demands, such as no activity (e.g. Tornare et al., 2016); or performing a simple task such as counting (Shimoni et al., 2015) or completing an easy jigsaw (Fry, 1975). Thus, it seems possible to create positive, negative, and neutral conditions with comparable incidental demands, but existing studies have not implemented these consistently.

There is no evidence to evaluate whether self-generated imagery inductions influence mood valence specifically, without affecting confounding states such as arousal or motivation. It is conceivable that the tasks of imagining positive, negative and neutral events could result in different levels of arousal, but without any data it remains unclear. The likelihood that different self-generated imagery conditions lead to differences in other psychological states is increased because of the differences in incidental demands between neutral and other conditions. Thus this is a possible limitation of self-generated imagery that has yet to be explored.

1.2.3.4 Summary of the efficacy of self-generated imagery mood inductions

In summary, self-generated imagery can be an effective method for manipulating children's mood states. Self-generated imagery can effectively induce changes in mood state, although its impact is not always consistent. There is some evidence that self-generated imagery inductions can successfully induce a mood state lasting several minutes at least, although precisely how long these effects last is unclear. There is no evidence as to whether self-generated imagery affects factors other than mood valence, which leaves open the possibility of experimental confounds.

1.3 The impact of mood on children's self-control

As reviewed in the previous section, certain mood induction techniques can be used to examine the effect of mood on children's cognition. The area of cognition focused on in this thesis is self-control. Self-control is a broad construct that encompasses a number of distinct capacities and traits. Children might apply self-control when refraining from lashing out after a disagreement, when stopping to look before crossing the road, or when transitioning effectively between loud outdoor play time and quiet indoor work at nursery. In this thesis, the effects of mood on two key aspects of self-control are investigated: these are executive function (Chapter 2) and intertemporal choice (Chapters 3 and 4).

The literature examining the effects of mood induction on self-control in children is relatively sparse, and the results are somewhat conflicting: some studies have generated positive results, and others null results. Interpreting these data is made more difficult by the great variety of methodologies used. Studies have varied in their design in terms of both the mood induction procedure, and how self-control is measured. Furthermore, several of these studies are methodologically limited, in terms of: not including valid measures of mood; using mood inductions with limited evidence for their efficacy; or using measures of self-control with limited validity. These limitations reduce confidence in the conclusions of previous studies. In the following sections, evidence pertaining to the effect of mood states on self-control is reviewed. The effect of mood on executive function is reviewed first, followed by the effect of mood on intertemporal choice. For both of these areas, theoretical accounts are summarised initially, followed by a review of the empirical data. Theoretical accounts are summarised first because they provide insight into possible mechanisms which remain largely unexplored by the empirical data.

1.3.1 The impact of mood on children's executive functions

Executive functions are high-level cognitive processes involved in the control of cognition. They are required to execute behaviours in a goal-directed, rather than automatic, fashion. The most influential model of executive function (Miyake et al., 2000) posits the existence of three executive functions in adults: working memory (updating and manipulating information in memory), inhibitory control (inhibiting pre-potent actions or thoughts), and cognitive flexibility (switching flexibly between tasks with conflicting rules). These three executive functions correlate strongly with each other (Miyake et al., 2000). Latent variable analyses suggest that in childhood, variation in executive functions is best explained by one or two factors (e.g. Huizinga, Dolan, & van der Molen, 2006; Lee, Bull, & Ho, 2013; Wiebe, Espy, & Charak, 2008). Thus, while executive function is an umbrella term for a number of skills, they share a strong overlapping cognitive basis.

Children's executive function is measured using a range of tasks, most of which are designed to measure either working memory, inhibitory control, or cognitive flexibility individually. Working memory tasks involve manipulating or updating information in memory. For example, the backwards digit span task requires children to listen to a string of digits (e.g. "2 5 6") and report them in a backwards order (i.e. "6 5 2"). Inhibitory control tasks involve resisting an action that is pre-potent: pre-potency of an action is established either because an action is habitual in everyday life, or because the task is intentionally designed to induce a pre-potent response. For example, in go/no-go tasks, children press a button when they see frequent "go" stimuli (e.g. a range of animals), but have to resist pressing the button when they see the infrequent "no-go" stimulus (e.g. a dog). Cognitive flexibility is typically measured with switching tasks, in which children have to switch between following two conflicting rules. For example, in the Dimension Change Card Sort (DCCS) task (Zelazo, 2006), children sort pictures that vary in colour and shape (e.g. blue and red rabbits and boats) into piles, first sorting by colour, and then switching to sorting by shape.

Both theoretical accounts and empirical data have suggested that mood affects executive function. The existing theoretical accounts are largely drawn from the adult literature to interpret results of empirical studies with children on an *ad hoc* basis. The theories posit a broad range of mechanisms to explain why mood might impact executive function. However, only a very small number of empirical studies have directly examined the relationship between mood and executive functions in children. The following two sections review the theoretical accounts, followed by the empirical studies.

1.3.1.1 Theoretical accounts for the effect of mood on executive function

Theoretical accounts for the effect of mood on executive function in childhood are varied, but relatively superficial. They are largely frameworks put forward to interpret the results of individual mood induction studies with children. These frameworks are mostly drawn from the adult literature, and are underpinned by data drawn from studies using both different age groups and different paradigms to the present research. Nevertheless, it is informative to give a cursory overview of these theories, as they provide plausible hypotheses for how and why mood might impact executive function in childhood.

Theories posit diverse mechanisms to account for the effects of mood on executive function. For example, the strength model of self-control suggests that mood affects executive function due to a general and limited pool of cognitive resources (Baumeister & Vohs, 2007). This theory postulates that there is a general psychological resource required for self-control, and that performing acts of self-control (such as emotion regulation, or executive function) temporarily depletes this resource, resulting in poorer subsequent self-control. Down-regulating negative emotions is one of the processes thought to deplete self-control resources (e.g. Johns & Schmader, 2008). Therefore, this theory posits that negative emotional states result in attempts to down-regulate emotions, reducing resources for self-control and leading to poorer executive function (e.g. Hawes et al., 2012; Pnevmatikos & Trikkaliotis, 2013). Thus negative mood is expected to impair executive function performance.

An alternative mechanism thought to explain the impact of mood on executive function is processing style. The feelings-as-information account (Gasper & Clore, 2002) suggests that negative moods result in a different information processing style. Negative moods are thought to engage systematic, perceptual level processing of stimuli, due to the heightened need to attend to potential problems in the environment. Conversely, positive moods are thought to engage a heuristic style of processing. This has led some to suggest that negative mood will lead to poorer performance in tasks requiring top-down processing, which includes executive function tasks (Lapan & Boseovski, 2017).

The capacity theory (Seibert & Ellis, 1991) provides a slightly different prediction for the effect of mood on executive function, specifically that both positive and negative emotions impair executive function due to increased demands on attentional resources. This theory proposes that both positive and negative emotions generate emotion-related intrusive thoughts. Having such thoughts is said to consume attentional resources, and therefore to increase cognitive load. As a result, performance on executive function tasks is thought to be poorer in both positive and negative moods (e.g. Fartoukh et al., 2014).

Motivational accounts have also been proposed which suggest that mood is associated with task motivation. Positive moods are related to increased task motivation and engagement, while negative moods are related to decreased task motivation and engagement (Pekrun, 1992). Therefore, mood valence might affect executive function performance simply through its effects on general task motivation (Pnevmatikos & Trikkaliotis, 2013; Fartoukh et al., 2014).

In sum, while a range of mechanisms have been proposed for how mood might affect executive function in children, the predictions made by theoretical accounts significantly overlap. Specifically, most theories suggest that executive function is impaired by negative mood. Theories diverge in their predictions related to positive mood, in that some suggest that positive mood impairs executive function, while others suggest that positive mood improves executive function. However, these theories were developed based on data from paradigms used with adults, and so are relatively underdeveloped in relation to developmental populations. The following section provides a review of the empirical literature with children which tries to establish whether and how mood affects executive function in childhood.

1.3.1.2 Empirical findings on the effect of mood on executive function

The empirical literature examining the effect of mood induction on executive function in children is limited in scale. The majority of studies support theoretical suggestions that negative mood has a detrimental impact on executive function. However, a few studies have produced null results. Further, methodological limitations of the existing work – particularly in the specificity of the mood induction procedures used and the lack of inclusion of sufficient measures of mood – restrict the conclusions that can be drawn about whether mood affects executive function in children. Existing studies do not address the mechanisms of any effects.

There appears to be a detrimental effect of negative mood induction on executive function. Initial evidence for this is suggested by a study which aimed to examine the effect of guilt on inhibitory control in preschoolers (Lapan & Boseovski, 2017). In this study, children received either a negative mood induction (through being given a toy that was rigged to break, designed to induce guilt) or a neutral induction (playing with a wooden block). They then performed a standard measure of inhibitory control, the shape school task (Espy, 1997). In this task, children see a set of coloured shapes made into characters like children in a school. In the initial baseline phase, children label all of the characters by colour. Then in the inhibitory phase, children have to label *only* the characters wearing hats, and should *not* label characters without hats. There was a trend towards poorer performance on the shape school measure of inhibitory control in the negative compared to the control condition. This effect was only significant for 3- to 4.5-year-olds, and not for 4.5- to 6-year-olds (who in fact had ceiling-level performance on the cognitive task).

This study suggests that executive function is impaired by a guilt induction in preschool children, but it is unclear whether mood valence is the cause. Observations of children's behaviours suggested that children in the negative condition experienced guilt: purportedly guilt-related behaviours (such as avoiding eye contact, covering or touching the face, or squirming) were fairly frequent in the guilt condition. But, because behaviours in the neutral condition were not coded for comparison, it is unclear whether children's guilt behaviours were greater than in the neutral condition. Furthermore, while the negative mood induction led to somewhat poorer performance, it is not unambiguously clear what caused this decline in performance. The difference between conditions could be driven by negative mood, or by the negative social relations between experimenter and child. For example, being made to feel guilty by the experimenter might lead to reduced compliance to instructions or task motivation.

However, some further data also implies that negative mood might impair working memory. One study found that working memory was worse in a negative mood for 9- to 12-year-olds (Fartoukh et al., 2014). Children were assigned to one of three mood induction conditions in which they were collectively read a story that contained either happy, sad, or emotionally neutral content. Working memory was tested both before and after mood induction with a paper and pencil letter-number sequencing task taken from the Wechsler Intelligence Scale for Children. The letter-number sequencing task required children to listen to a string of characters between two and eight characters in length. Children had to write the characters down grouped by numbers in numerical order, followed by letters in alphabetical order (for example "1 G 6 A 3" should be written 136AG). Accuracy on this task was poorer after the mood induction for the negative condition, suggesting that negative mood can impair verbal working memory. There was no effect in the positive or control conditions. This suggests that negative mood impairs executive function. It is uncertain as to whether positive mood has no effect, or if the positive mood induction was ineffective, because there was no measure taken of children's mood. Another caveat is that the mood induction used is likely to have affected psychological states other than mood valence. Factors such as the social atmosphere, arousal, or task motivation might also have been affected by the group story manipulation. Therefore these data suggest that listening to a negative story impairs children's working memory, although whether mood valence was the key factor driving this effect remains unclear.

While these single studies provide some suggestion that negative mood induction impairs executive function, this was demonstrated more systematically by a series of three studies conducted by Pnevmatikos and Trikkaliotis (2013). They investigated the effect of mood on inhibitory control in 8-, 10-, and 12-year-olds, using a novel mood induction technique which involved manipulating children's sense of autonomy. The mood manipulation began with children being told that their teacher would allow some children to decide how many rounds of a game (the inhibitory control task) they would play. Children were then either allowed to choose (positive mood) or not to choose (negative mood). These conditions were compared to a neutral condition in which children were told that the teacher would decide for all children how many rounds they would play. In the first study, children then performed a go/no-go task as a measure of inhibitory control. The go/no-go task required participants to press a button when they saw one of 14 different animals (75% of trials), but to withhold the response when a dog was displayed (25% of trials). Those in the negative mood condition produced more errors on no-go trials, suggesting poorer inhibition in the negative mood condition. There was no difference between the positive and neutral conditions. This was despite children's self-report suggesting that the positive, neutral, and negative conditions diverged in mood valence as expected. This suggests that negative mood impairs inhibitory control, but that positive mood has no effect.

The effect of negative mood on executive function was replicated in a second study by Pnevmatikos and Trikkaliotis (2013). The same mood induction was used, but this time mood was manipulated within subjects, and the positive condition was omitted. Executive function was again measured with a go/no-go task. They found increased errors in the negative mood condition relative to the neutral mood condition.

In a third study, an anti-saccade task was used to measure inhibition. In this task, a cue is displayed rapidly on one side of a screen before a target arrow appears for a short duration on the opposite side. To perform the task and identify the direction of the target arrow, the participant has to inhibit the reflex to make a saccade to the cue, and instead orient attention to the target before it disappears. As with the go/no-go task, participants in the negative mood condition showed higher error rates. Taken together, the data from these three studies suggest that a negative mood induction involving restricting children's autonomy impairs inhibitory control.

However, the informative value of the three studies conducted by Pnevmatikos and Trikkaliotis (2013) is slightly limited because the mood induction used is particularly liable to affect other psychological states. The mood induction used was performed in a social group context, which presents the possibility that extraneous variables other than mood valence, such as the social environment, could account for the differences between conditions. Further, when a positive condition was included there was no effect of this condition on mood. This could be because the mood manipulation was not durable enough. Thus these data suggest that negative mood inductions involving restricting autonomy impair executive function, although again there is some doubt over whether mood valence is the key mediating factor.

Taken together, the studies reviewed so far suggest that negative mood impairs executive function, across different mood inductions and measures of executive function. Where positive mood conditions have been included, they have not affected performance compared to neutral conditions. However, this apparent lack of effect of positive mood could in some cases be because positive mood inductions fail to boost children's mood enough above baseline levels.

While there is a modest body of evidence to indicate that executive function performance can be impaired by negative mood, this is by no means unanimously the case. Contrary to the studies reviewed so far, some data found no evidence of an effect of mood on executive function. In Lapan and Boseovski's (2017) study reviewed above, they also included two measures of cognitive flexibility. Cognitive flexibility was measured both in another phase of the shape school task, and also using the DCCS (Zelazo, 2006). In the cognitive flexibility part of the shape school task, after having labelled characters by colour, children are required to switch to labelling them by shape.

The DCCS required children to sort cards with pictures that varied in colour and shape (e.g. blue and red rabbits and boats) into piles first by one dimension and then by the other. In this study, there was no difference in cognitive flexibility performance between negative and neutral conditions for either task. This was despite finding that those in the negative condition displayed expected guilt behaviours. Therefore, this study provides no evidence for an effect of negative mood on cognitive flexibility.

Indeed, there may not be a reliable effect of mood on working memory either. Another study found no evidence for an effect of negative mood on working memory in 8- to 12-year-olds (Hawes et al., 2012). To induce mood, children were led to believe they were playing a digital game of catch with other children, and were either excluded from the game (negative mood condition) or included in the game (neutral mood condition). Participants then completed written self-reports of their mood during the game, by rating how much they agreed with the statements “I felt happy”, “I felt sad” and “I felt angry”. Children then completed a backward digit span task as a measure of working memory performance. In the digit span task, children heard a series of digits and had to verbally repeat these in reverse order. There was no effect of condition on backward digit span. This was despite evidence that the mood induction was effective, because children in the negative condition reported feeling sadder, angrier, and less happy than those in the neutral condition. These findings provide no evidence for an effect of negative mood on working memory.

In summary, there is a limited evidence base which suggests that mood may affect executive function. The few studies investigating effects of induced mood state on executive function suggest that negative mood causes impaired executive function. However, this effect has not been replicated in all studies. The two studies that included positive mood conditions have found no effect of positive mood on executive function relative to neutral mood conditions, but it is difficult to determine whether this is because of a failure of mood induction or a genuine lack of effect. Thus, the evidence as to whether mood affects executive function is mixed, and so there is a clear need for future research to establish whether there is an effect or not.

There are two contrasting interpretations of the existing pattern of findings. The first is that there is a genuine effect of negative mood on executive function, and that the failures to replicate are due to the effect being mediated by additional factors (such as the specific facet of executive function measured), or the occurrence of false negative results. The alternative is that there is not a genuine effect of mood on executive function: effects identified in some studies could be highly specific to the paradigm used, or due to confounding factors such as effects on task motivation, or simply represent a collection of false positives arising from a publication bias. Distinguishing between these two interpretations is difficult due to some methodological limitations in existing studies.

Two key methodological issues with the literature prevent clear interpretation of the pattern of findings. Firstly, the mood inductions used are likely to impact psychological states other than mood valence (such as motivation, social compliance, or arousal). This makes it difficult to establish whether mood, or other factors, account for any effects of the mood induction on executive function. Furthermore, these mood inductions are novel, and therefore have a limited evidence base, increasing the uncertainty over whether the mood inductions used are consistently effective. Secondly, studies have not included baseline measures of mood or cognitive ability. This may be partially responsible for the mixed findings, as variation between groups in children's baseline mood could obscure or augment effects of induced mood. Thus, significant questions remain as to whether and how mood states might impact executive functioning in children, and improvements to methodology will enable research to get closer to answering these questions.

1.3.2 The impact of mood on children's intertemporal choice

The effect of mood on self-control has also been examined in terms of effects on intertemporal choices. Intertemporal choice is the process of making a decision between two outcomes which differ temporally. In children, this has typically been investigated in terms of a trade-off between time and the magnitude of an outcome – usually whether children prefer a smaller reward now or a larger reward after a delay (e.g. Prencipe & Zelazo, 2005) – but sometimes whether they prefer a smaller penalty now or a larger penalty after a delay (e.g. Mischel & Grusec, 1967). Making such intertemporal choices involves two processes: (1) making a choice between two temporally separated options, and (2) enacting this choice over time. For example, when a child has to decide whether to do their homework now, or later after playing outside, they firstly make an intertemporal choice (i.e. whether to do the homework now or later). In this thesis, this process is termed *making* an intertemporal choice, or making a choice for short. Secondly, if they chose to do the less immediately rewarding option (i.e. doing homework now), children then expend effort to continue their homework and not lapse into a more appealing activity. In this thesis, this process is termed *enacting* an intertemporal choice. Thus the making and enacting stages of intertemporal choice are separable, but both require children to resolve a conflict between choice options which differ in value and timing, and to manage the impulse to take an immediate reward.

Two key types of paradigm are used for measuring intertemporal choice in children. These are delay of gratification tasks, and delay choice tasks. Delay of gratification tasks require children to enact a choice between a small immediate and a larger delayed reward over time. The classic delay of gratification task is Walter Mischel's marshmallow task (Mischel, Ebbesen, & Zeiss, 1972). In this task, children are offered a desirable snack, such as a marshmallow, and instructed that they may either eat this one marshmallow now, or wait for an unspecified delay period and receive two marshmallows. Children are left alone with the single snack in view for up to 15 minutes. Despite initially choosing to opt for the larger reward, few preschool children will wait for the larger reward. Thus the task is named after the outcome considered to be more optimal, which is to enact a choice for the larger, delayed reward. Delay choice tasks, in contrast, require participants to state a preference between pairs of small immediate or larger future rewards. For example, children might be asked whether they would prefer one sticker now or three stickers later. The rewards can be real or hypothetical, and delay periods are often longer than in delay of gratification tasks (e.g. hours or days, instead of minutes).

In this thesis, the two tasks for measuring intertemporal choice are considered to map onto the two separable processes in intertemporal choice. By contrast, researchers in the mood induction literature have tended to assume that delay of gratification tasks and delay choice tasks measure precisely the same construct. An analysis of the requirements of the tasks suggests that this is unlikely to be the case. In delay of gratification tasks, children consistently make an initial choice to wait for the larger reward, but it appears that their inability to enact this choice over time leads them to take the smaller, immediate reward. Thus, delay of gratification tasks appear to measure *enacting* of an intertemporal choice. In such tasks, waiting for longer is assumed to be the optimal choice because delay periods are relatively short, and not waiting is thought to be due to failure of the ability to resist temptation. In delay choice tasks, children make a choice between two options, but do not have the opportunity to rescind this decision, meaning that they do not have to enact this choice. Thus, delay choice tasks seem to measure *making* a choice. This involves children making a judgement about how to value reward size and time relative to each other. Thus there is not an assumption that delaying is always optimal, but instead that adaptive behaviour is receptive to factors such as the size of the reward and the length of the delay. In addition to having different requirements, data suggest that performance on delay of gratification and delay choice tasks are either uncorrelated (Sin Mei Tsui & Atance, 2017) or only weakly positively correlated (Duckworth & Kern, 2011). Therefore, a distinction is made between these two tasks as measuring making versus enacting of intertemporal choices in the studies discussed.

There are a handful of theoretical accounts for how and why mood affects children's intertemporal choice, but there is a relative dearth of empirical research. Theories have been developed to provide bespoke accounts for the observations of single studies, rather than offering broad explanations of the phenomena involved. The empirical evidence itself is very scarce. Theoretical accounts and empirical evidence relating to the effect of mood on intertemporal choice are reviewed in the following sections.

1.3.2.1 Theoretical accounts for the effect of mood on intertemporal choice

Theoretical accounts for the effect of mood on intertemporal choice in childhood are underdeveloped. Some theories are mostly speculative, whilst others draw on accounts from the adult literature underpinned by quite different experimental paradigms. Thus existing accounts should be interpreted as sensible hypotheses for the mechanisms of effects of mood on intertemporal choice, rather than refined predictive frameworks.

The theory most commonly used to explain effects of mood on intertemporal choice is the self-therapeutic motive (e.g. Moore, Clyburn, & Underwood, 1976; Rosenhan, Underwood, & Moore, 1974; Schwarz & Pollack, 1977; Seeman & Schwarz, 1974). This theory postulates that negative emotions lead to a desire to self-soothe in order to assuage negative feelings. This suggestion is supported by evidence that children are less generous to others, and reward themselves more, when in a negative mood (e.g. Rosenhan et al., 1974; Barnett et al., 1979). Thus children are more likely to seek gratification when in a negative mood. Negative mood is said to increase the subjective value of an immediate reward relative to a delayed reward for children, because children anticipate that immediate rewards have greater potential to boost current mood, and that waiting for delayed rewards can promote frustration (Schwarz & Pollack, 1977). Therefore, according to the self-therapeutic motive, children in a negative mood are more likely to prefer immediate rewards to delayed rewards when making intertemporal choices.

An alternative theory which could explain the effect of mood on intertemporal choice is the strength model of self-control (Baumeister & Vohs, 2007). This theory proposes that the capacity for self-control is a general, limited psychological resource, and that performing acts of self-control temporarily depletes this resource, resulting in poorer subsequent self-control performance. Emotion regulation is thought to deplete the self-control resource (e.g. Johns & Schmader, 2008). Thus it is hypothesized that negative moods lead to emotion regulation, reducing resources for self-control, and leading to an impaired ability to enact intertemporal choices for delayed rewards.

A number of other plausible mechanisms for the effects of mood on intertemporal choice have been posited. Moore and colleagues (1976) proposed that mood affects expectations of obtaining a delayed reward, such that negative mood would lead to reduced expectation of obtaining the delayed reward (because the delayed reward has more uncertainty attached to it), making it less likely that children would make choices for delayed outcomes. These researchers also posited that negative mood might cause children to perceive the delay time as longer, and therefore be less likely to make choices for delayed outcomes because waiting longer is aversive. Others proposed that mood induction could affect children's desire for future contact with the experimenter, such that experiencing a negative mood might discourage children from wanting contact with the experimenter, causing them to avoid making delayed choices (Schwarz & Pollack, 1977).

In sum, theoretical accounts provide overlapping predictions for the impact of mood on intertemporal choice, though they suggest a variety of mechanisms by which mood might have its effect. All theories predict a greater tendency to choose smaller, immediate rewards in negative mood states and/or a greater tendency to choose larger, delayed rewards in positive mood states. However, the mechanisms proposed are as yet untested. Existing empirical studies will be reviewed below, although within these studies mechanisms have largely not been addressed.

1.3.2.2 Empirical findings on the effect of mood on intertemporal choice

A handful of studies have used mood inductions to investigate the effect of emotional states on intertemporal choice. Studies have used both delay of gratification tasks – requiring children to enact a choice to receive a larger reward over time – and delay choice tasks – requiring children to make a choice between two temporally separate rewards.

1.3.2.2.1 Mood and enacting intertemporal choices

The existing data tentatively suggest that positive mood leads children to enact a choice to wait for a delayed reward in delay of gratification paradigms. In the only published paper on this topic, researchers examined the effect of positive self-generated imagery on enacting an intertemporal choice in 4- to 8-year-olds across two studies (Yates et al., 1981). Enactment of intertemporal choice was measured using an adapted version of the marshmallow task. In this task, children were offered the choice to either receive fewer toy rewards immediately, or wait up to 15 minutes to receive more rewards. A series of lights were illuminated as a cue to how many rewards children would receive. In study 1, children were either assigned to a neutral condition, with no intervention, or one of two positive conditions. In the positive mood condition, children simply recalled happy memories; while in the positive distraction condition, children recalled happy memories *and* were instructed to continue thinking about these memories during the delay period. The positive distraction condition was included because it was predicted that maintenance of positive thoughts during the delay period was the key mechanism by which the mood induction would affect waiting times. The results showed that 8-year-olds in the positive mood and positive distraction conditions waited for longer than the neutral condition. This suggests that positive mood enables older children to enact their decision to wait for a delayed reward. On the other hand, 4-year-olds only waited for longer in the positive distraction condition. This implies that for younger children, there is an effect of positively valenced distraction, but not mood *per se*, on intertemporal choice.

A further study also suggested age-sensitive effects of positive mood on intertemporal choice (Yates et al. 1981, study 2). This study used a similar methodology, but with 5-, 6-and 8-year-olds. In this study, children in the neutral condition recalled the names of their classmates instead of having no intervention. They found that the 5-year-olds waited for longer only in the positive distraction condition, whereas both positive conditions had this effect for the 6-year-olds. There was no effect of condition for 8-year-olds, whose waiting times showed a ceiling effect. This replicates the finding from study 1, suggesting that positive mood leads to greater ability to enact a temporal choice for older children. Thus, there is preliminary evidence that positive mood might enable older children to better enact intertemporal choices for delayed rewards. However, this evidence comes from only one published paper.

The effect of self-generated imagery on enacting intertemporal choice may be due to it providing a positively valenced distraction. In Yates et al.'s (1981) second study, children were also asked to report their thoughts during the waiting period, and these were categorised as either on-task (e.g. thinking about the rewards) or off-task (e.g. thinking about playing with friends). They were categorised in this way because off-task thoughts are believed to distract attention from the desirable task rewards, reducing temptation to take the reward immediately and thus leading to longer waiting times (Mischel et al., 1972). Five-year-olds who reported off-task thoughts during the delay period waited longer than those who reported on-task thoughts or no thoughts, regardless of the mood induction. This is consistent with the interpretation that it is distraction – rather than mood – that helps younger children to enact their intertemporal choices. For 6-year-olds, more children in the positive mood condition than in the neutral condition reported off-task thoughts during the delay period. This suggests that the positive self-generated imagery induction generated distracting thoughts, which could in turn have been responsible for the increased waiting times in the positive condition for 6-year-olds. However, a direct mediation analysis was not conducted, so this is only a hypothesis. This implies that entertaining thoughts unrelated to the task increases waiting times, perhaps because of decreased attention to the tempting features of the task rewards (see also Mischel et al., 1972). Therefore, distracting thoughts are a plausible mechanism by which mood might affect the enactment of intertemporal choices. However, they can also be an entirely independent route through which self-generated imagery affects intertemporal choice, regardless of mood.

1.3.2.2.2 Mood and making intertemporal choices

There is more evidence available to suggest that mood affects the making of intertemporal choices, than enactment of intertemporal choices. Studies using delay choice tasks have found that children tend to make more choices for immediate rewards in negative moods. Seeman and Schwarz (1974) gave 9-year-old children a delay choice task following a mood induction involving success or failure experiences. Children drew a picture for a gallery, and were either told that their drawing was very good and would be included in the gallery (positive mood induction), or told that their drawing was not good enough to be included in the gallery (negative mood induction). In the delay choice task, the experimenter read out five pairs of rewards that the child could receive either immediately or the following week, such as “five balloons now, or seven balloons in a week”, and “a small notebook now, or a large notebook in a week”. The child selected which reward they would prefer from each pair, on the understanding that only one of their choices would actually be given to them. Children who received the failure manipulation rated their mood as more negative, and were more likely to choose the immediate rewards, than those who received the success manipulation. This suggests that negative mood increases the tendency to select smaller, immediate rewards rather than larger, delayed rewards, when compared to positive mood.

A similar effect was seen when mood was manipulated using self-generated imagery, instead of positive or negative feedback (Schwarz & Pollack, 1977). This study investigated the effect of positive and negative mood on making intertemporal choices with 7- to 11-year -olds. Children completed either a positive or negative mood induction which combined self-generated imagery with imagining three events specified by the experimenter. For example, in the positive mood induction, children were asked to imagine eating their favourite dessert, and in the negative mood induction, children were asked to imagine forgetting a poem in front of their class. Intertemporal choices were measured with a pen-and-paper delay choice task, in which children made choices between six pairs of items, such as "one penny now or two pennies in a month" and "one pencil now or two pencils in two weeks". The results showed that children in the negative mood induction were less likely to select delayed rewards than those in the positive mood induction. This suggests that children make different intertemporal choices when in negative and positive moods, although it does not identify whether this difference is driven by negative mood, positive mood, or both.

In fact, negative mood seems to increase immediate choices, rather than positive mood decreasing them. Moore, Clyburn and Underwood (1976) included positive, negative and neutral conditions in a study of intertemporal choice in 3- to 5-year-olds. Children recalled either happy memories (positive), or sad memories (negative), or counted to 10 (neutral). All children were then given a delay choice task with a single decision, to choose either one small pretzel now or a lollipop after lunch. (These options were pre-tested and it was found that the lollipop was consistently more desirable to children when both were offered at the same time.) Children in the negative condition were more likely to select the immediate reward, suggesting that negative mood shifts choices towards immediate rewards relative to neutral mood.

However, there appears to be no effect of positive mood on making intertemporal choices. In the study by Moore et al (1976), there was no difference between children in positive and neutral conditions in terms of whether they chose the delayed reward. Another study found that positive mood had no effect on intertemporal choice with 9-year-olds (Shimoni et al., 2015). Children were assigned to one of three kinds of mood induction: either a happy mood induction (recalling happy memories), a neutral mood induction (counting to 100), or a pride mood induction (recalling proud memories). In the delay choice task, children selected between a smaller immediate or larger delayed number of points across several rounds of a game. There was no significant difference between happy and neutral conditions in choices. This mirrors the finding from Moore et al. (1976) that positive mood has no effect on making intertemporal choices. However, because no measure of mood was included in either of these studies, we cannot rule out the possibility that the lack of effect of positive mood was due to the positive mood induction being ineffective.

One exception to the lack of effect of positive mood on intertemporal choice is with the emotion of pride. In Shimoni et al.'s (2015) study, children in the pride condition chose the delayed reward *less* often than children in happy and neutral conditions. Counter to theoretical accounts, this suggests that at least one positive emotion, specifically pride, leads to a greater preference for immediate rewards.

Overall, it appears that mood affects intertemporal choices, although it is difficult to draw firm conclusions from the small number of studies conducted. In terms of enacting intertemporal choice, one published paper suggests that positive thoughts lead to a greater ability to enact the choice for a larger, delayed reward. In terms of making an intertemporal choice, a few studies suggest that negative mood leads to choices in favour of smaller, immediate rewards rather than larger, delayed rewards, and that positive mood tends to have no effect. Thus there is some evidence that mood affects intertemporal choices, and perhaps in different ways depending on which of the two processes in intertemporal choice are measured.

Three issues with the existing literature hamper current understanding of the effect of mood on children's delay of gratification. Firstly, there is a particular dearth of research into the effect of mood on the enacting of intertemporal choices. Replication of the one published paper addressing this question is needed to establish whether the findings are robust. Secondly, that existing studies have not included a mood manipulation check, which makes it difficult to determine (i) whether null results are due to manipulation failure *or* to a genuine lack of effects, and (ii) whether significant results are likely due to mood valence rather than other factors (e.g. social compliance). Thirdly, the validity of the delay choice tasks used have been limited by use of hypothetical rewards that are not present during the task. This reduces the ecological validity of the tasks, and their validity as measures of self-control, because having rewards present makes the task more challenging in terms of resisting temptation (Mischel et al. 1972) and increases the predictive validity of delay of gratification tasks (Shoda, Mischel, & Peake, 1990). Overall it remains uncertain as to whether there is a reliable effect of mood on intertemporal choices.

1.4 Summary and next questions

This literature review has highlighted that mood induction can be an effective way to investigate relationships between mood states and cognition in children. A number of methodologies have been developed to manipulate mood, which include presenting videos and self-generated imagery. Both of these techniques have proved to be effective in a small number of studies, although further work may be required to assess whether these mood inductions specifically target mood valence.

A sparse literature has used mood induction to examine the effects of mood on aspects of children's self-control. The results have been mixed. For executive function, a handful of studies has suggested that negative mood impairs performance, although two studies have generated null effects. The divergence in findings could be due to one of two things. There could be genuine differences in the effect of mood induction on self-control dependent on the specific emotion induced or the facet of executive function. Alternatively, it could be that some studies have generated spurious results or false negatives. For intertemporal choices, studies suggest that negative mood leads children to choose smaller, immediate rewards over larger delayed rewards, but that positive mood generally has no effect. Methodological limitations of these studies prevent strong conclusions from being drawn about whether there is an effect of mood induction on self-control, and, if there is an effect, whether mood valence is the driver of that effect. Null results are particularly challenging to interpret where measures of mood have not been included within studies. So, while studies have begun to make progress in exploring how mood affects children's self-control, further investigation is needed.

Due to the sparseness of the literature and conflicting findings, two key issues need to be clarified. Firstly, and most importantly, it remains unclear whether there is an effect of mood on self-control or not. The field is relatively understudied, and the existing studies have generated divergent findings. Because a variety of methods are used, it is hard to explain discrepancies in results when they arise. Furthermore, the results from some studies are difficult to interpret because of poor methodology, such as failure to implement mood manipulation checks. Thus there is a higher risk of existing findings being false positives, especially if there is a file-drawer effect leading to non-publication of null results. Therefore, studies with improved methodology are needed that attempt to replicate previous effects and establish whether there is a reliable effect of mood on children's self-control.

Secondly, assuming there is an effect of mood on self-control, the mechanisms of this effect are unexplored. To explain the mechanisms of effects, previous researchers have drawn on a range of theories established in the adult literature, or developed new theories *ad hoc*. However, effects identified in the empirical literature have been too inconsistent to begin to test these theories to establish why mood might affect children's self-control. To make the initial step towards determining mechanisms, it needs to be established that mood valence is the driver of effects of mood induction on self-control. Implementing mood manipulation checks and measures of other factors (including arousal and motivation) will begin to establish how mood inductions impact children's emotional and motivational state, the first link in the mechanistic chain. Only then can future research begin to probe the possible theories as to why mood valence impacts self-control.

The following chapters present studies investigating the effect of children's mood on their self-control, specifically executive function (Study 1) and intertemporal choice (Studies 2, 3 & 4), using mood induction techniques to experimentally manipulate mood. The studies begin to address the methodological limitations of previous studies by (1) implementing manipulation checks and using mood inductions that have proved to be the most efficacious in previous literature (Studies 1-4); (2) including measures of potential confounding variables (Study 1); (3) controlling for baseline mood (Study 1, 3 & 4); and using measures of self-control with high validity (Studies 1-4). The studies also improve on the lack of consistency in methodologies applied in previous research in three key ways. Firstly, the studies systematically examine the effect of mood on self-control, where in the past researchers have rarely conducted multiple experiments in this area. Secondly, a close replication of an existing study is performed (Study 2) to determine replicability. Thirdly, an identical measure of self-control is used across multiple studies (Studies 3 and 4). These improvements provide the optimal circumstances to try to answer the question of whether mood inductions affect self-control in children, and to determine whether these effects are driven by mood.

Chapter 2: Effects of mood on executive function

The aim of Chapter 2 is to investigate the effect of mood on executive function. As discussed in Chapter 1, there is only a handful of studies examining how mood state affects children's executive function, and these have produced conflicting findings. The first study presented here contributes much needed further data to this literature. This study also allows for greater confidence in interpreting results, due to a number of methodological improvements on past studies. These include both using a mood induction technique with demonstrated efficacy, specifically video mood induction; and improving experimental control, through controlling for baseline mood, cognitive performance, and variation in task motivation and arousal.

2.1 Introduction

Executive functions are high-level cognitive processes which are required to plan and execute behaviours in a goal-directed, rather than automatic, fashion. Executive functions are a key component of self-control because they allow individuals to maintain information in mind, inhibit impulses, and behave flexibly in order to override automated behaviours to achieve goals.

The most influential model of executive function (Miyake et al., 2000) posits the existence of three executive functions which correlate highly with each other: working memory (updating and manipulating information in memory), inhibitory control (inhibiting pre-potent actions or thoughts), and cognitive flexibility (switching flexibly between tasks with conflicting rules). There is some debate over the extent to which these three functions are unitary or dissociable (Friedman & Miyake, 2017) particularly during development (e.g. Lerner & Lonigan, 2014; Wiebe et al., 2011), a review of which is beyond the scope of this introduction. Nevertheless, researchers in the field of mood-cognition interactions have treated executive function as a unitary construct, and thus this convention is adopted in this chapter.

Theoretical accounts propose a number of distinct mechanisms by which mood states might affect executive function. For example, capacity theory (Seibert & Ellis, 1991) posits that both positive and negative emotional states generate emotion-related thoughts which consume attentional capacity, in turn reducing the attentional resources available for other tasks. Therefore, this theory predicts that emotional states of either positive or negative valence will lead to poorer performance on attention-demanding tasks compared to relatively neutral mood states. Another account, the strength model of self-control (Baumeister & Vohs, 2007), proposes that negative mood will impair executive function. This theory suggests that there is a general psychological resource required for self-control, and that performing acts of self-control (such as emotion regulation, or executive function) temporarily depletes this resource. Therefore, if experiencing negative emotions activates emotion regulation processes, this will reduce resources for self-control, and lead to poorer executive function (e.g. Pnevmatikos & Trikkaliotis, 2013; Hawes et al., 2012). Others have proposed a motivational account, such that mood is thought to affect task motivation – specifically, positive mood increasing motivation, and negative mood decreasing motivation – and task motivation in turn will affect executive function performance (e.g. Fartoukh et al., 2014).

Despite theoretical accounts providing an explanation for how mood might affect executive function, there is relatively little empirical evidence to support this. Three published articles have reported a detrimental effect of negative mood on executive function, whilst two articles have generated null results. This research is recapped below.

One study that found some indication that negative mood impairs inhibitory control was conducted by Lapan and Boseovski (2017). In this study, mood was induced by having children play with a toy, which was either a wooden block (neutral condition), or a toy dog that was rigged to break and make children feel guilty (negative condition). Children then completed the shape school task, an age-appropriate measure of inhibitory control. There was a trend towards poorer inhibitory control in the negative compared to the neutral condition. The difference between mood conditions was significant for a subsection of the sample only (specifically 3- to 4.5-year-olds), and not the eldest children (4.5- to 5-year-olds), apparently because they showed ceiling-level performance on the inhibitory control task.

There is also some evidence that negative mood impairs working memory (Fartoukh et al., 2014). In this study, mood was induced by reading 9- to 12-year-old children a happy, sad, or neutral story and verbal working memory was assessed both before and after this mood induction. Working memory was poorer after the mood induction for children in the negative condition, while there was no change in performance from before to after mood induction in the positive or neutral conditions. This suggests that negative mood impairs working memory. However, it is difficult to interpret the lack of effect of positive mood because there was no measure taken of children's mood. Thus it could be that positive mood has no effect, or that the positive mood induction was ineffective.

A further article reported that negative mood impaired children's inhibitory control across three studies (Pnevmatikos and Trikkaliotis, 2013). These studies used a novel mood induction technique with 8-, 10-, and 12-year-olds, which varied whether or not children could select how many rounds of a game they would play: children were either allowed (positive), not allowed (negative), or not offered the opportunity to choose (neutral). Inhibitory control was then measured, using a go/no-go task in study 1 and 2, and an anti-saccade task in study 3. The results showed that children in the negative mood condition performed poorer in both types of inhibitory control task. In study 1, there was no difference between the positive and neutral conditions. This was despite children's self-report suggesting that the positive, neutral, and negative conditions differed in mood valence as expected. The pattern of findings suggests that negative mood impairs inhibitory control, but positive mood does not affect it.

While there is a small body of evidence suggesting that executive function performance can be impaired by negative mood, this has not always been replicated. In two studies, there was no evidence found for an effect of negative mood on executive function. In Lapan and Boseovski's (2017) study reviewed above, two measures of cognitive flexibility were included, in addition to the measure of inhibitory control already discussed. There was no difference in cognitive flexibility performance between negative and neutral conditions for either task. Thus, within the same study effects of mood on executive function were inconsistent, depending on the measure of executive function used.

One further study found no evidence for an effect of mood on working memory in 8- to 12-year-olds (Hawes et al., 2012). To induce mood, children were either included in, or excluded from, a virtual game, and following this working memory was measured using a backward digit span task. There was no effect of condition on backward digit span. This was despite evidence that the mood induction was effective, because children in the negative condition reported feeling sadder, angrier, and less happy than those in the neutral condition. This suggests that negative mood does not consistently impair working memory.

In summary, a small existing research base suggests that negative mood may impair executive function in children, while positive mood has no effect. However, some studies have failed to replicate an effect of negative mood on executive function and it is not clear why there are such discrepancies. The lack of effect of positive mood inductions could be due to a failure of the mood inductions themselves. Thus, further research is needed to ascertain whether there is a robust effect of mood on executive function when mood inductions with demonstrable efficacy are used.

The previous literature is limited in two further areas which prevent confident interpretation of the results. Firstly, the studies use mood inductions which are particularly liable to affect psychological states other than mood valence, which makes it difficult to interpret whether mood valence is the mechanism underpinning the effects identified. Inducing mood through having a child break an experimenter's toy (Lapan & Boseovski, 2017); manipulating their autonomy in a classroom setting (Pnevmatikos & Trikkaliotis, 2013); or reading stories as a group (Fartoukh et al., 2014) all involve changing the social environment, including children's feelings towards the experimenter, and, in the case of the latter two inductions, feelings of other children participating at the same time. Thus factors other than mood valence (such as lack of compliance, reduced task motivation, or social influences) may drive the effects. Thus it is possible that mood valence is not the mechanism at play in the existing research. Secondly, the studies have not controlled for baseline cognitive performance or mood state. Baseline differences in children's mood or cognitive abilities could obscure or amplify effects of mood inductions, resulting in the mixture of positive findings and null findings in the existing literature.

There is, therefore, a demonstrated need for further research on this topic, and in particular research that uses a more rigorous methodology to establish whether there is a robust effect of mood induction on executive function and whether mood valence is the key mediator of this relationship. That is the goal of the first study in this thesis. This study uses a mood induction validated with children to increase the likelihood of effectively inducing mood. Executive function is measured before and after the mood induction, to control for baseline differences in executive function. Furthermore, self-report indices of mood valence, arousal, and task motivation are taken before and after the mood induction to verify that mood valence is effectively manipulated.

The mood induction used in the current study, specifically videos, has the most consistent evidence of efficacy in literature with children. Children shown short videos with positive content (such as singing and dancing) or negative content (such as a story in which one of the characters dies) subsequently demonstrate expected differences in mood valence (Blau & Klein, 2010; Rice et al., 2007; Schnall et al., 2008; von Leupoldt et al., 2007; Williams et al, 2014). The videos used in the present study were selected based on criteria recommended in previous research (Rottenberg et al., 2007) including being in colour, with sound, picture motion, and clear emotional content, a high level of intelligibility, and being of at least 3 minutes in length. The positive and negative videos were also selected to be familiar to children, because familiarity with films tends to intensify emotional responses (von Leupoldt et al, 2007; Gross & Levenson, 1995).

To measure executive function, the current study uses a measure of cognitive flexibility, specifically a task-switching paradigm with multiple switches, called the Switching Inhibition and Flexibility Task or SwIFT (Carroll & Cragg, 2012). This task was chosen because it is a measure of cognitive flexibility which also relies on both working memory and inhibitory control (Blakey, Visser, & Carroll, 2015), and so provides a general measure of executive function that is not specific to any one sub-component. Within the SwIFT task, participants see a target image and have to select the response option that matches the target in either colour or shape, depending on the matching rule for that trial. Trials are organised into pure blocks, in which the same matching rule is used across all trials, and mixed blocks, in which the matching rule differs across trials. In mixed blocks, trials either involve following the same rule as in the previous trial (non-switch trials) or the other rule as in the previous trial (switch trials). From these data, switch and mixing costs can be calculated which indicate the difference in processing cost between maintaining and switching rules. Specifically, switch costs are calculated as the difference in reaction time between switch and non-switch trials. They therefore reflect the processing cost needed in order to switch from one rule to another. Mixing costs are the difference in reaction time between non-switch trials and pure block trials. They therefore reflect the processing cost needed to maintain the possibility of switching rules.

Thus, the present study aimed to examine the effect of mood on children's executive function. Within the study, children were assigned to one of three conditions to induce mood – positive, neutral, or negative. Children first completed a baseline session of the SwIFT (to control for differences in children's executive function capacity). They then received one of the three mood inductions, and finally they completed the SwIFT again. Self-report measures of mood were taken at four time points to capture changes in mood during the experiment: mood valence was measured to determine whether the mood manipulation was effective, while arousal and task motivation were measured to control for potential effects of the mood induction on these factors.

2.2 Method

2.2.1 Overview

An overview of the procedure for the experiment is shown in Figure 2. Children were first given instructions for the mood rating scales and the executive function task. Then they completed the first of two sessions with the executive function task. Children were then assigned to one of three mood conditions (positive, neutral, or negative) and watched one video, commensurate with their mood condition. Then all children completed the second session with the executive function task. Self-report mood ratings were taken at four time points. These were after the rating scale instructions (baseline); immediately *before* the mood induction (pre-induction); immediately *after* the mood induction (post-induction); and after executive function task session 2 (end).

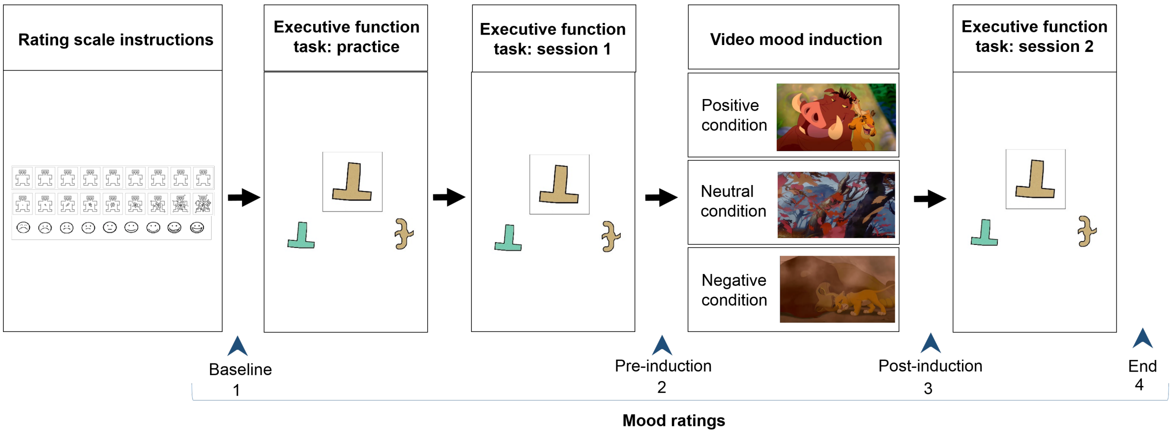

Figure 2. A schematic overview of the procedure of Study 1.

2.2.2 Participants

Participants were recruited from three primary schools in the north of England. The schools were in areas that represented a broad range of socio-economic backgrounds (1^{st}, 5^{th} and 8^{th} decile in the Index of Multiple Deprivation, Department For Communities and Local Government, 2015). Participants were 82 children (45 male) aged 7 to 8 years (M_{age} = 7 years 10 months, range = 7 years 1 month to 8 years 9 months). Two further participants were excluded, one due to computer error leading to missing task data from session two, and one due to not understanding task instructions.

2.2.3 Design

A 3 x 2 design was used, with a between-subjects factor of mood induction (positive, neutral or negative) and within-subjects factor of session of the executive function task (session 1 and session 2). Each child was randomly assigned to one of the three mood inductions (positive, N=27; neutral, N=27; negative, N=28). There was no difference between the three mood induction conditions in terms of gender composition (χ^2 (2, N = 82) = 0.03, p = .986). There was a significant difference in age (F (2, 79) = 3.22, p = .045), such that children in the positive condition (M = 7.68, SD = 0.37) were younger than children in the neutral condition (M = 7.94, SD = 0.41, t (52) = 1.97, p = .018) and marginally younger than those in the negative condition (*M* = 7.89, *SD* = 0.41, *t* (53) = 2.45, *p* = .054)[1]. All children completed two sessions of the executive function task, one before and one after the mood induction.

The key dependent variables were performance measures on the executive function task, specifically switch and mixing costs.[2] These were derived from reaction times on the three types of trials within the executive function task: pure, switch, and non-switch. Pure trials are trials in pure blocks, in which the matching rule is the same for every trial. Switch and non-switch trials are trials in mixed blocks, in which the matching rule can differ between trials. In switch trials, the sorting rule for the current trial is different to that of the previous trial. In non-switch trials, the sorting rule for the current trial is the same as the previous trial. Switch costs are calculated as the difference in reaction time between switch and non-switch trials. Mixing costs are the difference in reaction time between non-switch trials and pure block trials.

The other dependent variables were self-reported mood ratings. Mood valence and arousal ratings were taken at four time points (shown in Figure 2). Task motivation ratings were taken at three time points (pre-induction, post-induction, and end).

2.2.4 Mood induction

Three short clips taken from animated children's films were used to induce mood. The clips were chosen based on pilot data which demonstrated that some positive videos

---

[1] All analyses of task performance were conducted with age as a covariate. Simple ANOVA results are reported unless the age covariate affected the interpretation of the results.

[2] Two additional dependent variables were calculated but were not reported in the interest of succinctness because the results were the same as the primary dependent variables. One variable was an efficiency

[2] Two additional dependent variables were calculated but were not reported in the interest of succinctness because the results were the same as the primary dependent variables. One variable was an efficiency score, which gave an index of the amount of time taken to give a correct response. The other was a variability score, which gave an alternative index of attentional capacity based on the standard deviation in reaction times.

which were similar to those used in past research were effective, whilst others were not (for more details, see Appendix A). The video for the positive condition was a scene from *The Lion King* in which characters sing '*Hakuna Matata*', a song about living a worry-free life (duration 2'57"). The negative video, also taken from *The Lion King,* depicted the death of the main character's father (duration 3'03"). The neutral video contained natural scenes taken from *Bambi*, of woodland scenery with choir music, followed by a segment of autumn leaves blowing and falling from a tree (duration 1'59").

The emotional videos were familiar to children as hoped, because most children in the positive and negative conditions reported that they had seen their video clip before (positive: 93%; negative: 89%). Conversely, most children (93%) in the neutral condition reported that they had *not* seen their video clip before.

2.2.5 Mood and motivation measures

The self-report measures of mood and motivation used are shown in Figure 3. Children rated their mood state using two of the Self-Assessment Manikin (SAM) scales (Hodes et al., 1985). This self-report face scale depicts a series of drawings of a character with different mood states. The valence scale shows expressions ranging from very unhappy to very happy, and the arousal scale ranges from sleepy to very aroused. Nine-point paper versions of each scale were used, and children were asked to point to the picture that represented how they felt right now (how happy or unhappy; or how relaxed or wide awake). To measure motivation for the executive function task, children were asked "How much would you want to play the sorting game right now?". They rated their response using a similar visual 9-point scale with faces from happy to sad. Each scale was printed on a separate A4 sheet.

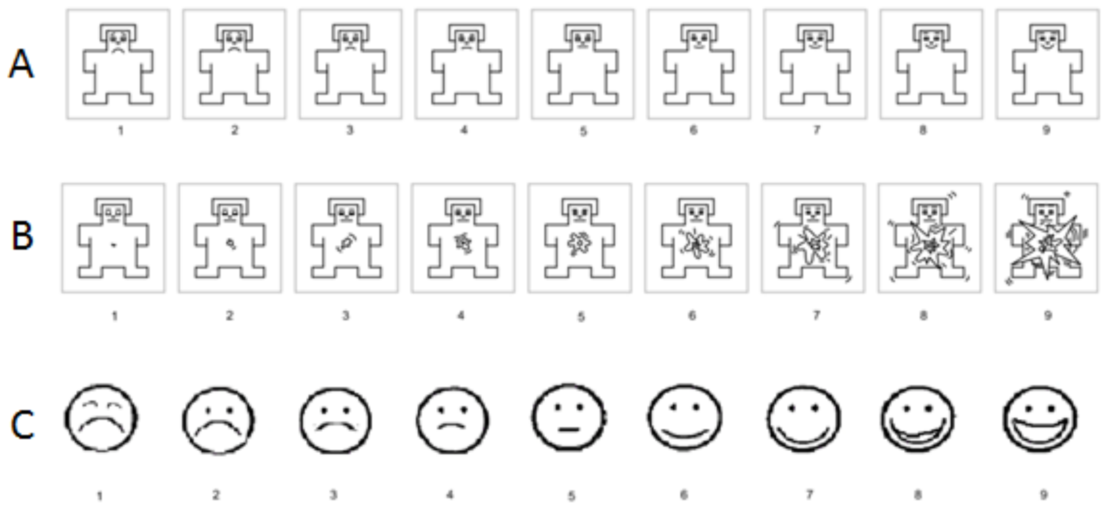

Figure 3. Emotion and motivation mood measures used in Study 1. A) Valence scale B) Arousal scale C) Motivation scale. Valence and arousal scales taken from the Self-Assessment Manikin (Hodes, Cook & Lang, 1985).

2.2.6 Executive function task

To measure executive function capacity, the Switching Inhibition and Flexibility Task was used (Carroll & Cragg, 2012). An example of a trial is shown in Figure 4. In this simple computerised matching game, a coloured shape appeared on screen (as a prompt stimulus), followed by two further coloured shapes (as response stimuli). One response stimulus matched the prompt in colour, and the other response stimulus matched the prompt in shape. On each trial, children were told to select the response stimulus that matched the prompt on one of the dimensions (either colour or shape) based on an auditory cue which stated the rule for that trial.

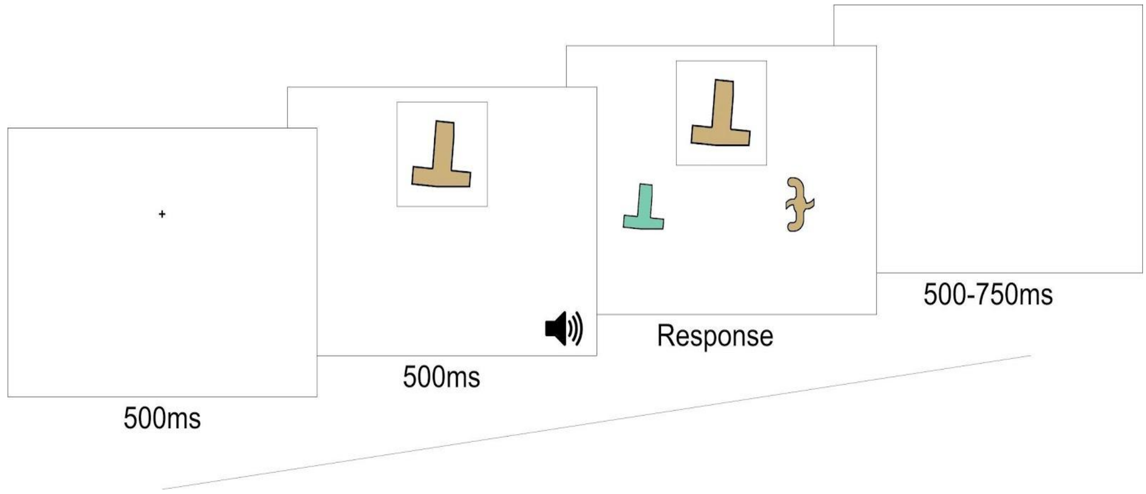

Figure 4. Schematic of a test trial on the SwIFT.

The task stimuli consisted of coloured shapes, examples of which can be seen in Figure 4. Stimuli were drawn from a pool of 81 containing all combinations of nine shapes and nine colour shades. Stimuli for each trial were pre-selected randomly, with the constraint that the colours of the two response stimuli were always clearly distinguishable from each other.

Each of the two SwIFT sessions consisted of three blocks: two pure blocks, in which the rule was the same throughout, and one mixed block, in which the rule varied unpredictably between trials. The two pure blocks, which had 12 trials each, consisted of one block of only colour trials, and one block of only shape trials. In the pure blocks, the order of trials was randomised across the 12 trials. Order of pure blocks (colour or shape first) was counterbalanced between mood conditions, with participants receiving the same order in both sessions. The mixed block was always last. In the mixed block, there were 24 switch trials and 24 non-switch trials. Two fixed trial orders were created, one for each of the two sessions. Trial orders were selected to ensure that switching was not predictable (e.g. switch trials were as likely following switch as non-switch trials).

The task was created and administered using E-Prime version 2.0 (Psychology Software Tools, Pittsburgh, PA) on a ThinkPad laptop. On each trial (see Figure 4), a fixation was shown for 500ms, followed by a prompt stimulus and auditory rule cue for 500ms. The auditory cue was a recording of a female adult voice stating the rule for that round (either colour or shape). Then, two response options appeared at the bottom of the screen. One of the response options matched the prompt in colour, the other in shape. This array remained onscreen until participants gave a response. Participants responded using *a* or *l* keys on a keyboard to select left or right hand response options respectively. Following a response, an inter-trial interval consisting of a plain white screen that randomly varied in duration between 500 and 750ms appeared before the next trial. Before each block, the type of block ("colour game", "shape game", or "mixed game") was displayed for 3 seconds. Each task session lasted approximately 6 minutes, depending on response times.

2.2.7 Procedure

Participants were tested in a quiet area in their school. Children first received instructions on using the mood rating scales; then completed session 1 of the SwIFT, followed by a mood induction, and then session 2 of the SwIFT. Measures of mood were taken before the pre-induction SwIFT session; prior to the mood induction; immediately after the mood induction; and after the post-induction SwIFT session.

The experimenter first explained how the mood and motivation rating scales worked. For the valence scale, the experimenter indicated the extreme and central anchors of the scale as showing someone who feels "very sad, upset, or angry" [1]; "very happy" [9]; or "neither happy nor unhappy" [5]. For the arousal scale, the anchors given were "very calm or relaxed" [1]; "very wide-awake or energetic" [9]; or "having a normal amount of energy" [5]. For the motivation scale, the anchors were "really doesn't want to play" [1]; "really wants to play" [9]; or "doesn't mind if they play or not" [5]. The experimenter gave two exemplars using intermediary points along the scale (for example "If I felt a little bit happy, I might point to this one [6]"). To test their understanding, the child was then asked which picture they would point to if they felt a certain way (for example "Show me which one you would point to if you felt quite upset"), and were corrected if their answer was incorrect. The same process was used to explain the arousal and motivation scales. Children were then asked to point to the picture that showed how they felt right now to provide baseline ratings of mood valence and arousal[3].

Children were then introduced to the SwIFT. Children completed three practice trials with feedback, and nine practice trials without feedback (three colour, three shape, and three mixed). They then completed SwIFT session 1. Following this, the mood induction was conducted. First, children completed pre-induction ratings of valence, arousal, and motivation. They were then told to watch the video carefully. They watched the appropriate video for their mood induction condition while wearing noise-cancelling headphones. Post-induction ratings of valence, arousal, and motivation were then taken: children were asked to indicate which picture showed how they felt after watching the video. Next, children completed SwIFT session 2. Children then gave final mood and motivation ratings.

---

[3] Note that motivation ratings were not taken at this stage because children had not yet tried the SwIFT task, so would not be able to evaluate their motivation accurately.

2.3 Results

2.3.1 Data preparation

For the SwIFT, data from the first trial of all mixed blocks was excluded because such trials are neither switch nor non-switch trials. Trials in which reaction times were below 200ms (N = 3) or above 5000ms (N = 135) were excluded. To obtain average reaction time data for each participant in pure blocks, median reaction times were taken for all trials with correct responses (89.3% retained in session 1; 94.3% retained in session 2). To obtain average reaction time data for each participant in mixed blocks, median reaction times were taken for trials with correct responses which also followed a correct response, as only these trials can be considered to be switch or non-switch trials. (The proportions of trials retained were as follows - switch trials: 67.3% in session 1, 77.3% in session 2; non-switch trials: 78.8% in session 1, 84.1% in session 2. There was no difference between mood induction conditions in the proportion of trials retained.)

Switch cost was calculated as the difference in reaction time between switch and non-switch trials. Mixing cost was calculated as the difference in reaction time between non-switch and pure block trials.

2.3.2 Effects of mood on executive function

To determine the effect of mood on executive function, the effect of mood induction and SwIFT session on switch costs and mixing costs was examined. Table 2 shows descriptive statistics for reaction times on each trial type, switch costs, and mixing costs. Switch costs and mixing costs were each submitted to a 3 x 2 ANOVA with a between-subjects factor of mood induction and a within-subjects factor of session. The key comparison of interest is the interaction between mood induction and session, which would indicate an effect of mood induction on executive function that is not driven by baseline differences in executive function between groups.

Table 2. Descriptive statistics for SwIFT performance as a function of mood induction and session in Study 1.

Mood induction		**Positive**		**Neutral**		**Negative**	
		Session 1	**Session 2**	**Session 1**	**Session 2**	**Session 1**	**Session 2**
Switch cost[a]		333 (333)	227 (248)	231 (243)	110 (333)	175 (206)	129 (310)
Mixing cost[a]		295 (289)	346 (367)	232 (287)	239 (322)	370 (428)	357 (406)
Reaction times	**Switch trials**	1676 (765)	1640 (595)	1516 (417)	1470 (413)	1640 (406)	1571 (453)
	Non-switch trials	1342 (552)	1412 (511)	1285 (362)	1359 (480)	1465 (473)	1442 (457)
	Pure trials	1047 (415)	1066 (307)	1052 (337)	1120 (283)	1094 (312)	1085 (351)

NB: All units are in milliseconds.
[a] Lower switch and mixing costs indicate better performance.

For switch costs, the key mood induction-by-session interaction was not significant (F (2, 79) = 0.29, p = .752, $\eta^2 p$ = .007). Therefore there was no evidence of an effect of mood induction on the change in SwIFT performance between session 1 and session 2. There was a main effect of session (F (1, 79) = 4.54, p = .036) consistent with a practice effect: the switch cost reduced between the pre-induction session (M = 246, SD = 265) and the post-induction session (M = 155, SD = 300). There was also a marginal main effect of mood induction (F (1, 79) = 3.08, p = .052) which remained a trend after controlling for age (p = .072). The means suggest a trend towards larger switch costs in the positive condition (M = 278, SD = 213) than the neutral condition (M = 171, SD = 210) or negative condition (M = 153, SD = 208), regardless of session. This suggests that there were baseline differences in executive function which were sustained following the mood induction. The results show that the mood inductions did not lead to any change in switch costs.

For mixing costs, the key mood induction by session interaction was not significant (F (2, 79) = 0.19, p = .832, η^2p = .005). There was also no main effect of session (p = .741) or mood induction (p = .269). This shows that the mood inductions did not lead to any change in mixing costs.

One possible explanation for the lack of overall effect of mood on executive function is that the mood manipulation may only have worked for a subgroup of children. Thus, by removing the data of children for whom the manipulation was not effective, a more focused test of the key experimental question can be achieved. To do this, analyses were conducted including only children whose self-reported mood change was consistent with their condition (i.e. higher valence in positive (N = 9), lower in negative (N = 18), and no change in neutral conditions (N = 12)). Two 3 x 2 ANOVAs were conducted with a between-subjects factor of condition and a within-subjects factor of session and dependent variables of switch and mixing costs. The key condition-by-session interactions were not significant (switch cost: p = .523; mixing cost: p = .811). There were also no main effects of mood induction or session (*p's* > .05). Therefore there was no effect of mood induction on switch costs or mixing costs, even when only children for whom the mood induction showed evidence of efficacy were included.[4]

2.3.3 Mood manipulation efficacy

To check whether the mood inductions were effective at manipulating mood, the effect of mood induction on mood valence ratings was examined at each time point. Differences between mood inductions in motivation and arousal were also compared, to determine whether the effects of the induction on mood valence were confounded. Mean valence and arousal ratings taken from children at four time points are shown in Figure 5, along with motivation ratings taken at three time points.

Mood rating data were analysed with nonparametric tests due to skew that could not be addressed with transformation.[5]

2.3.3.1 Mood manipulation check

To determine whether there was an effect of mood induction on mood valence, mood induction conditions were compared at each of the four time points during the experiment with four Kruskal-Wallis H tests. As expected, there was no difference between mood inductions at baseline (χ^2 (2, N = 82) = 1.24, p = .538 or pre-induction (χ^2 (2, N = 82) = 0.33, p = .850). At post-induction however, there was a significant difference between mood induction conditions (χ^2 (2, N = 82) = 13.07, p < .001). Mann-

---

[4] Two further types of analysis were conducted to determine (i) whether the magnitude of change in children's mood following the mood induction determined the effect on executive function; or (ii) whether there was an effect of mood on executive function in earlier trials due to induced mood fading over time. There was no relationship between the magnitude of the change in children's mood during the mood inductions and performance on the SwIFT. There was also no effect of mood on earlier trials (first half) on the SwIFT.

[5] Mood rating data was missing from two participants for the fourth time point (end of experiment) due to experimenter error. Tests were conducted with pairwise deletion because full data were present for the key comparison between pre- and post-induction.

Whitney U tests showed a significant difference in valence rating at post-induction between negative and neutral conditions ($U = 207$, $Z = 2.96$, $p = .003$), and negative and positive conditions ($U = 194$, $Z = 3.19$, $p = .001$), but not between neutral and positive conditions ($U = 347$, $Z = 0.34$, $p = .738$). Children in the negative condition reported a lower mood valence than the other two conditions at post-induction. The valence rating in the negative condition at post-induction was still greater than the midway point of 5, as confirmed by a one-sample t-test (t (27) = 2.17, $p = .039$). There was no effect of mood induction at the end of the experiment (χ^2 (2, N = 82) = 2.90, $p = .234$), suggesting that mood returned to a similar level between conditions after the second SwIFT session. Overall, this suggests that children in the negative condition felt more negative than those in the other two conditions following the mood induction.

2.3.3.2 Effects of mood induction on arousal and motivation

The mood induction may have had confounding effects on children's arousal and motivation, which could obscure the effect of mood valence on executive function. To determine whether arousal was affected by mood induction, the three mood induction conditions were compared at each of the four self-report time points using four Kruskal-Wallis H tests. There were no significant differences between mood induction conditions at any time point (Baseline: χ^2 (2, N = 82) = 2.18, $p = .336$; Pre-induction: χ^2 (2, N = 82) = 1.41, $p = .495$; Post-induction: χ^2 (2, N = 82) = 1.69, $p = .429$; End: χ^2 (2, N = 82) = 0.92, $p = .631$). Therefore, there was no evidence that mood induction affected arousal.

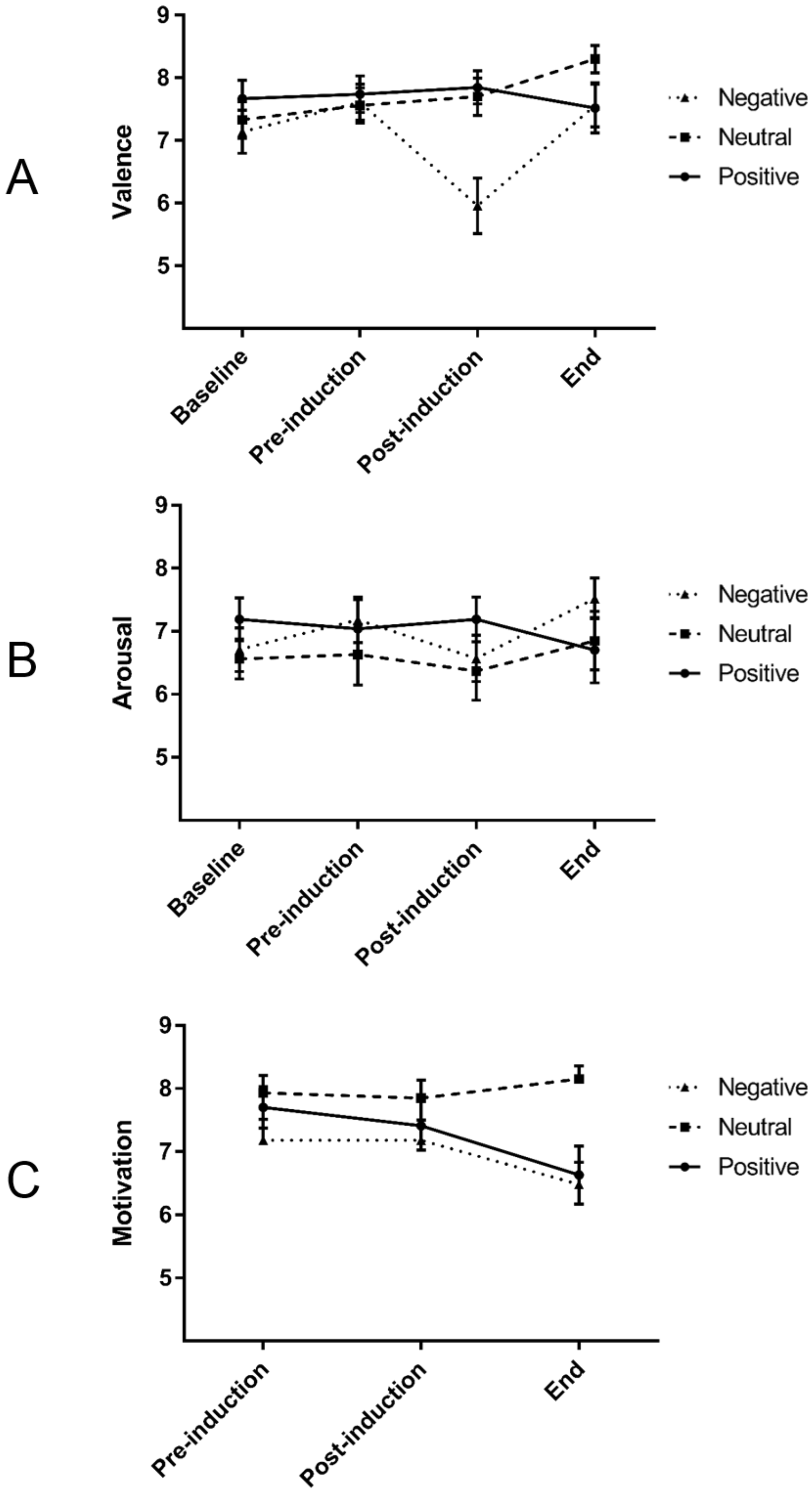

Figure 5. Mood and motivation ratings as a function of mood induction during the four phases of Study 1. (A) Valence (B) Arousal (C) Motivation.

To determine whether motivation was affected by mood induction, the three mood induction conditions were compared at the three self-report time points where motivation was measured (pre-induction, post-induction, and end) using three Kruskal-Wallis H tests. There was no difference in motivation between mood induction conditions at pre-induction (χ^2 (2, N = 82) = 3.31, p = .191) or post-induction (χ^2 (2, N = 82) = 2.16, p = .339). There was a significant difference between mood induction conditions at the end (χ^2 (2, N = 82) = 10.88, p = .004). Mann-Whitney U tests identified that at the end of the experiment, children in the neutral condition were more motivated than the positive condition (U = 235, Z = 2.34, p = .020) or the negative condition (U = 181, Z = 3.29, p = .001), but that the negative and positive conditions did not differ in motivation (U = 336, Z = 0.51, p = .61). Importantly, there was no evidence that the mood inductions had an effect on children's motivation at post-induction.

2.4 Discussion

The objective of this study was to determine whether there is an effect of mood on children's executive function. There was no evidence for an effect of mood on executive function, measured using the SwIFT. This was despite finding a significant difference in self-reported mood valence ratings between negative and positive mood conditions, indicating that the mood induction was effective overall. Even when children for whom the mood induction was ineffective were excluded, there was no effect of mood induction on executive function. Overall, the results suggest that mood does not impact executive function.

These results imply that children's executive function is unaffected by changes in mood. The findings conflict with studies which found that negative mood impaired children's executive function (Fartoukh et al, 2014; Lapan & Boseovski, 2017; Pnevmatikos &

Trikkaliotis, 2013), but are consistent with others which found no effect of negative mood induction on executive function (Hawes et al., 2012; Lapan & Boseovski, 2017). In the present study, a more rigorous methodology was established by controlling for baseline cognitive ability and mood, and ensuring that the mood induction affected mood valence specifically and not arousal or task motivation.

Thus, when other factors are adequately controlled for, there appears to be no effect of mood on executive function. It is possible that in previous studies, factors such as baseline differences in cognitive ability, arousal, or task motivation generated false positive results. Indeed, in the present data there were differences between mood conditions in executive function at baseline, which could have resulted in an apparent effect of mood induction had baseline cognitive ability not been controlled for.

Alternatively, the lack of effect of mood on executive function in some studies, including the present one, could be due to the effect of negative mood on executive function being mediated by other factors. While it is not clear from existing data what these mediating factors are, it is still conceivable that such factors could explain the mixed results concerning the effect of negative mood. One possibility is that individual differences, such as in propensity to ruminate, or emotion regulation capacity, might mediate the impact of mood. In which case, the present results suggest that the effect of negative mood on executive function is more context-specific than previously thought.

The findings presented here challenge theoretical accounts of the effect of mood on self-control as applied to children. There was no evidence that negative mood impaired executive function, which challenges both the capacity theory account (Seibert & Ellis, 1991) and the strength model of self-control account (Baumeister & Vohs, 2007). Both of these theories predict that negative moods impair executive function, the former due to generating distracting thoughts, and the latter due to depletion of resources for self-control. The present findings suggest that these theories, which were developed in the adult literature and drawn upon to explain empirical findings with children, may in fact not be generalizable to children. There are many possible reasons for why these theories may not apply to children – for instance it may be that mood states do not lead to distracting thoughts for children, or only do so under certain circumstances (such as in very intense mood states).

The data suggest that positive mood also did not affect executive function. However, there is some uncertainty about whether the positive mood induction was effective, which calls this null result into question. In the present study, children's self-reported mood valence did not differ between positive and neutral conditions at post-induction. This was surprising, given that the methodology used was similar to that in previous studies, which found differences in self-reported mood between positive and neutral conditions (von Leupoldt et al., 2007; Schnall et al., 2008). Furthermore, our data suggested that the videos used to induce mood were very familiar to children, which should augment their effect. Perhaps this could be due to the natural scenes in the neutral condition being enjoyable to children. This might be the case because the alternative activity at that time was being in their classroom, in comparison to which watching an emotionally neutral film might feel positive. Thus, the self-report data seem to suggest that the positive and neutral mood inductions did not induce sufficient differences in children's self-reported mood.

However, it is not possible to make a confident interpretation of the lack of difference in executive function performance between positive and neutral conditions. While it is plausible that the positive mood induction was not effective, the lack of difference between neutral and positive conditions could also be due to two issues with the self-report measure of mood. Firstly, because of a responses bias which meant that a large proportion of children rated their mood at the maximum at pre-induction, there was reduced scope for children in the positive condition to rate improvements in their mood. Secondly, using pre- and post-test mood ratings (rather than only post-test mood ratings as used in previous studies) might have changed the way children respond on the post-test scale. Specifically, they might have been anchored to their previous response, in the absence of a large change in mood. Thus, the lack of difference in mood ratings between positive and neutral conditions may be partially an artefact of how mood was measured. It is difficult to ascertain whether the positive mood induction was effective or not, and thus more research is needed to answer the question of whether positive mood affects executive function.

As well as having a bearing on the key question of whether mood affects executive function, the results suggest that established techniques for inducing and measuring mood with children may not be as efficacious as the previous literature implies. The data show that positive and neutral videos with similar content to those used in previous literature (e.g. von Leupoldt et al, 2007; Schnall et al., 2008) do not consistently produce divergent self-reported mood states in children. This could imply a lack of efficacy of the mood induction stimuli, or a lack of sensitivity of the self-report face scale, at least in the context of rating mood over multiple time-points.

Furthermore, the findings of this study provide an insight into the durability of mood inductions. Children's self-reported mood returned to baseline by the end of the experiment, suggesting that the impact of the negative mood induction did not last beyond completing a 6-minute cognitive task. This implies that the duration of induced mood is limited to a few minutes. This is important because it suggests that subsequent cognitive tasks need to be short in order for the induced mood to have an impact on performance.

Two key questions remain regarding the relationship between mood and self-control. Firstly, the question of whether *positive* mood boosts self-control remains slightly ambiguous. The data from the present chapter were ambiguous as to whether positive mood was induced successfully or not, because of possible limitations in the measure of mood used. As such, it remains unclear whether positive mood has an effect on self-control. To help address this issue, further research should aim to find an alternative positive mood induction which is more effective, and a measure of mood that is more sensitive to children's mood, particularly to changes in mood.

Secondly, the question of whether mood impacts other aspects of self-control is worthy of further investigation. Theoretical accounts suggest that the effects of mood and the mechanisms driving these effects differ depending on the way that self-control is measured. For example, mood is considered to affect the executive function facet of self-control via attentional capacity (Seibert & Ellis, 1991), but to affect intertemporal choice, another facet of self-control, through effects on emotion regulation behaviours (Rosenhan et al., 1974). Therefore further research exploring how mood affects other aspects of self-control, such as intertemporal choice, would help to identify under what circumstances mood affects self-control.

In conclusion, this chapter suggests that there is no effect of mood on executive function in children. The study showed that when a more methodologically rigorous design is employed - specifically, when arousal and task motivation are equivalent, and baseline executive function ability is taken into account - there is no effect of mood on executive function. The data also suggested that mood induction techniques and tools for measuring mood state in children may not be as consistently effective as implied by previous literature. Future research should attempt to identify effective mood induction techniques to investigate the effects of mood on other facets of self-control.

Chapter 3: Effects of mood on enacting intertemporal choices

In Chapter 2, no evidence was found for an effect of mood state on children's executive function. Children who watched videos with positive, negative, and emotionally neutral content did not show a difference in performance in an executive function task. This is despite children's self-report demonstrating that those in the negative mood induction experienced lower mood relative to both the neutral and positive mood inductions. This suggests that there is no effect of children's mood on executive function.

The data from Chapter 2 also somewhat contradict the prevailing view in the previous literature that children's mood can be reliably manipulated using videos, and that subsequent effects on a range of aspects of cognition should be found. Both pilot data (see Appendix A) and the data reported in Chapter 2 implied that manipulating *positive* mood using videos was not consistently effective. Thus there is reason to look for a study with a robust method for inducing positive mood and replicate this study as a first step to establishing a usable methodology for further research. Incidentally, being able to replicate an effect of mood on self-control would also provide evidence as to whether there is an effect of positive mood on children's self-control.

The aspect of self-control investigated in this chapter was intertemporal choice. Intertemporal choice refers to the process of selecting between outcomes at different points in time, for example between a lower-value reward immediately, versus a higher-value reward in the future. Intertemporal choice is the facet of cognition that has received the most research attention in the field of emotion-cognition interactions in children, and therefore is likely to produce the most reliable effects.

The specific study chosen for replication was Yates et al.'s (1981) study 2. This study was chosen to be replicated for two reasons. Firstly, this study used a mood induction procedure – specifically self-generated imagery – that has been commonly used with children. Self-generated imagery has been demonstrated to impact mood state in a number of previous studies (e.g. Levine et al. 2008; Stegge et al. 1994; Tornare et al., 2016), and is tailored to what the individual child finds emotionally positive or negative. Thus, it may be more effective than videos in inducing mood. Secondly, this study examined the effect of mood on another component of self-control, specifically intertemporal choices for rewards. Furthermore, Yates et al.'s (1981) result is the only effect of mood on children's intertemporal choice to be replicated across two studies using the same paradigm. Thus in a field where evidence is sparse, this study both used a mood induction that is the most likely to be effective, and identified an effect of mood on self-control that is the most likely to be replicable.

3.1 Introduction

Self-control consists of a number of distinct capacities that enable individuals to manage their goals. One facet of self-control is executive function, which was explored in Chapter 2. Another important instance of self-control is intertemporal choice. Intertemporal choice is a decision made between outcomes at different points in time. In children, this is usually assessed in a context where there is a trade-off between time and value, in particular choosing between a less valuable, immediate outcome and a more valuable outcome after a delay. As outlined in Chapter 1, intertemporal choice entails two processes: (1) making a decision, and (2) enacting this decision over time. Both of these processes require children to resolve a conflict between value and time, and overcome their impulse to take an immediate reward.

The two different processes in intertemporal choice are measured using two distinct methodologies. Delay of gratification tasks are used to measure enactment of choices, and delay choice tasks are used to measure making of choices. The prototypical example of a delay of gratification task is Walter Mischel's marshmallow task (Mischel et al., 1972), which observes the amount of time children are willing to wait to receive a greater quantity of marshmallows. Waiting for longer is assumed to be the optimal choice because delay periods are short. Therefore, this and other delay of gratification tasks measure the ability to enact an intertemporal choice where there is a clear optimal response, by resisting temptation over time. Delay choice tasks (e.g. Prencipe & Zelazo, 2005), in contrast, require children to simply make a choice between pairs of rewards, for example between one sticker now or three stickers tomorrow. Because there is no opportunity to renege on this choice, delay choice tasks measure making a choice independently of enacting this choice. Instead, it involves making a judgement about how to value reward size, and time, including present and future utility (e.g. Green, Myerson, & Ostaszewski, 1999). Thus, in delay choice tasks there is not an assumption that selecting the delayed reward is always optimal, but instead that adaptive behaviour is receptive to factors such as the size of the reward and the delay.

Because making a decision and enacting a decision in intertemporal choice differ in the cognitive processes implicated, the mechanisms by which mood could affect intertemporal choice are also likely to differ. Enacting a choice reflects a capacity to resist temptation, so any effect of mood on enacting a choice is more likely due to mood strengthening this ability. For example, as suggested by the strength model of self-control, negative mood is thought to deplete self-control resources (Baumeister & Vohs, 2007) which would limit the ability to resist temptation. For making a choice, effects of mood may instead be due to a shifting of the relative weightings children attach to reward size, time, and utility. For instance, negative mood may make children perceive the utility of the immediate reward as higher, as it might alleviate their negative mood state (Rosenhan et al., 1974).

While theories have suggested avenues by which mood might affect intertemporal choices, there is very little empirical evidence to determine whether such effects of mood exist or what the mechanisms are. Evidence for the effect of mood on the two processes in intertemporal choice will be reviewed below, firstly for enacting a choice and secondly for making a choice.

There is evidence to indicate a possible effect of mood on the enacting of intertemporal choices. One paper identified that positive mood leads children over 5 years old to more effectively enact a choice for a larger, delayed reward (Yates et al. 1981). The researchers examined the effect of positive mood in a delay of gratification paradigm with 4- to 8-year-olds across two studies. During the delay of gratification task, children waited whilst a series of lights were illuminated, with each one representing a further reward they would receive after the task. The first light illuminated immediately, the second after 3 minutes, and the third after 15 minutes. To induce mood, children completed a self-generated imagery mood induction. Children were assigned to one of two positive conditions, or a neutral condition. In study one, children in the positive mood condition recalled happy memories, which was compared to a no-intervention neutral condition. In addition, in a positive distraction condition, children were given the positive mood induction and instructed to continue thinking their positive thoughts during the delay period. The results showed that 8-year-olds in the positive mood and positive distraction conditions waited for longer than those in the neutral condition. This suggests that positive mood boosts older children's ability to enact their intertemporal choices. On the other hand, 4-year-olds only waited longer in the positive distraction condition. This implies that for younger children, positively valenced distraction affects intertemporal choice, but positive mood alone does not.

Demonstrating the consistency of this result, these key findings were replicated in a second study by Yates et al (1981). In this study, 5-, 6- and 8-year-olds were assigned to the same conditions as the first study (positive mood, positive distraction, or neutral), except that those in the neutral condition listed the names of their classmates. As in the first study, children received the mood induction before completing the delay of gratification task. The findings of study two showed that 6-year-olds waited longer in the positive mood and positive distraction conditions compared to the neutral condition. This is similar to the finding for 8-year-olds in study one, and offers further support for the view that positive mood enables older children to enact their intertemporal choices. There was no effect of mood observed among 8-year-olds, likely due to ceiling effects in waiting times. Five-year-olds waited longer only in the positive distraction condition, similar to the 4-year-olds in study one. This again suggests that for younger children, positively valenced distraction affects intertemporal choices, but positive mood alone does not.

Yates et al.'s (1981) second study also suggested that distraction could be a mechanism by which self-generated imagery affects enacting of intertemporal choices. In study two, children were asked to report the content of their thoughts during the delay period.

Children's thoughts were coded as either on-task (thoughts about the task, light cues, or rewards), off-task (thoughts about anything else), or neither (such as reporting no thoughts). The aim of this was to examine whether distraction from the task through thinking off-task thoughts was the mechanism by which the mood induction affected intertemporal choices. The results showed that more of the 6-year-olds in the positive condition had off-task thoughts than in the neutral condition, suggesting that the self-generated imagery mood induction indeed led to more distracting thoughts. This suggests that for older children, distraction could be a mechanism through which positive mood impacts enacting intertemporal choice. However, 5-year-olds who reported having off-task thoughts waited for longer, *regardless* of condition. Recall that 5-year-olds also only waited longer overall in the positive distraction condition. This suggests that for younger children, distraction affects intertemporal choice, but mood does not. Thus, distracting thoughts could be a mechanism through which mood affects intertemporal choice, although they might also be a separate factor influencing choice independent of mood.

The interpretation of Yates et al.'s (1981) studies is limited with regard to whether mood itself affected the enactment of intertemporal choice. It is not clear whether mood was actually induced, because no measure of mood was included. It is quite possible that the self-generated imagery mood induction generated distracting thoughts without affecting mood, and that this is what drove increased waiting times (Mischel et al., 1972).

Thus, for delay of gratification paradigms, evidence from one published paper suggests that enactment of intertemporal choices for delayed rewards is boosted by positive mood induction in children over 5 years. For younger children at least, positive mood induction may affect enactment of choices only to the extent that the mood induction generates distracting thoughts which may consume their attention during the waiting period.

The effect of mood on the *making* of intertemporal choices has also been investigated in only a handful of studies. Studies suggested that negative mood, compared to positive mood, causes children to make more choices for smaller, immediate rewards (Seeman & Schwarz, 1974; Schwarz & Pollack, 1977; Moore et al., 1976). For example, Moore et al (1976) investigated the effect of a self-generated imagery mood induction on a delay choice task in 3- to 5-year-olds. Children were assigned to one of three mood conditions: children either recalled happy memories (positive condition), or sad memories (negative condition), or they counted to 10 (neutral condition). Children were then given a delay choice task with a single decision, between receiving a small lollipop now or a larger candy bar after lunch. Children in the negative mood condition were more likely to select the immediate reward than children in the neutral condition or positive condition. This suggests that negative mood affects the weighting of children's intertemporal choices such that children value the smaller, immediate reward over the larger, delayed reward.

In terms of making intertemporal choices, the available evidence suggests that positive mood does not affect choices. Moore et al. (1976) found no difference between positive and neutral conditions in their delay choice task. One further study found no effect of positive mood on delay choices compared to a neutral condition (Shimoni et al., 2015). In that study, to induce mood, children either recalled happy memories (happy condition), or proud memories (pride condition), or counted to 100 (neutral condition). Children then selected between a smaller immediate or larger delayed number of points across several rounds of a game. There was no difference between happy and neutral conditions, suggesting that diffuse positive mood has no effect on intertemporal choice. Counter to most theoretical predictions, those in the pride condition made *fewer* delayed choices than the other two conditions, and it is not clear why this specific positive emotion affected choices. Thus it seems that overall there is no effect of positive mood on intertemporal choices, with the exception of the specific emotion of pride. This lack of effect could be because there is no effect of positive mood on preference, or it could be because the positive and neutral mood induction conditions did not differ sufficiently in mood. These studies did not include mood manipulation checks, thus it is not possible to evaluate whether the mood induction was indeed effective.

To summarise the literature reviewed, existing studies suggest some consensus that negative mood affects the *making* of intertemporal choices, by shifting choices towards smaller, immediate rather than larger, delayed rewards. For positive mood, there is some evidence that positive mood increases children's ability to *enact* their intertemporal choices for delayed rewards, at least for some age groups. This effect was replicated in two studies with a delay of gratification task, but contrasts with data from two studies with delay choice tasks which found no effect of positive mood on intertemporal choices. This discrepancy could be due to mediating factors (such as the type of task used) or it could suggest that the effect of positive mood on intertemporal choices is not robust. Thus, it is unclear whether positive mood has a robust effect of on intertemporal choice.

Therefore, there is a necessity for further research on this topic, specifically to verify whether positive mood impacts intertemporal choices. Furthermore, the data from Chapter 2 raised some concerns about the replicability of results in this field. In Chapter

2, attempts to manipulate positive mood using videos were ineffective. This suggests that implementing mood inductions with children may not be as straightforward as the published literature implies. Thus identifying a study with an effective method for inducing positive mood and replicating this study would provide a foundation for further research using mood induction to examine the effect of mood on children's self-control.

Therefore, the aim of Study 2 is to examine the effect of positive mood on intertemporal choice by replicating a previous study which uses a reliable mood induction. The study chosen to be replicated was Yates et al.'s (1981) second study. This study was chosen for two reasons. Firstly, the study used a mood induction technique which has demonstrated reliability, specifically self-generated imagery (Levine et al., 2008; Stegge et al., 1994; Tornare et al., 2016). Secondly, the effect of positive mood on the ability to enact intertemporal choices was replicated across two studies using the same paradigm, making it the most likely finding to be replicable in a field where there is a scarcity of evidence.

Thus in the present study, the same procedure was used as in Yates et al (1981). Children aged 4 to 6 years were assigned to either a positive or neutral self-generated imagery mood induction. In the positive mood induction, children recalled three things that made them happy. In the neutral mood induction, children recalled the names of their classmates. Because the interest was in the effects of mood alone on intertemporal choice, rather than of mood and distraction, the positive distraction condition was not included. Children then participated in a delay of gratification task in which they had the opportunity to wait for lights to illuminate to receive more rewards. Further, children's cognition during the delay period was measured by coding their thoughts as either off-task or on-task. There were only two minor differences in method between Yates et al.'s (1981) study and the present study which were unlikely to affect the results: these were the specific rewards used (confectionary in the present study, instead of small toys) and the type of testing space (a room in a school in the present study, instead of a cubicle).

Two adjustments were made to the study design to increase confidence in the results. The sample size was increased to augment statistical power. Yates et al. (1981) had 12 children per condition, where in the present study there were at least 21 per condition. To increase confidence in the efficacy of the mood induction, a measure of mood was included in the procedure. The mood measure was included at the end of the experiment so as not to interfere with the original design of Yates et al (1981), which did not include a mood manipulation check. Previous studies have included mood measures after the experimental task (Forgas & Locke, 2005; Gobbo & Raccanello, 2007; Masters & Santrock, 1976). Mood was measured in a way designed to address the issue with scale sensitivity that occurred in Chapter 2 (adapted from Stegge et al., 1994). Children were first asked whether they felt happy, sad, or okay during the mood induction, then (if appropriate) were also asked to rate how happy they felt on a 3-point scale. Because children were only asked to rate their mood after the induction, the risk of children being anchored to a previous response (as in Chapter 2) was eliminated. It was also thought that presenting only three options initially might reduce the intrinsic allure of selecting the happiest face and encourage children to report a neutral mood when appropriate. Further, by establishing that children were happy before introducing the three possible levels of happiness, it was expected that children might evaluate their mood in a more nuanced way.

The key prediction was that children in the positive condition would wait for longer in the delay of gratification task than children in the neutral condition. If the results of Yates et al. (1981) are reliable, it would also be expected that (i) the effect of mood on waiting time would be greater, or only existent, in older children; and (ii) off-task thoughts during the delay task would be related to greater waiting times.

3.2 Method

3.2.1 Participants

Participants were recruited from two primary schools in northern England. The schools were in areas of middle to high socioeconomic status (5^{th} and 7^{th} decile in the Index of Multiple Deprivation, Department For Communities and Local Government, 2015). Participants were 86 children (39 male) aged 4 to 6 years (M_{age} = 5 years 7 months, range = 4 years 7 months to 6 years 8 months), with children randomly assigned to either the positive mood induction condition, or the neutral mood induction condition. Four further children were excluded due to being unable to complete testing: one child was absent on the days of testing, two did not demonstrate task understanding, and one was unwilling to complete the task. A fifth child was excluded because data was missing on their date of birth, and so they could not be assigned to an age group.

3.2.2 Design

A 2 (mood induction) x 2 (age) between-subjects design was used, with children randomly allocated to a mood induction (positive, N = 42, or neutral, N = 44) and divided into older and younger age groups via median split (median 5 years 7 months). There was no difference between the two mood induction conditions in age (positive: M = 5.62, SD = 0.52; neutral: M = 5.62, SD = 0.59; t (84) = .03, p = .976) or in gender composition (χ^2 (1, N = 86) = .000, p = .984). There were two dependent variables related to performance on the delay of gratification task. The first dependent variable was average waiting time, that is, the average time waited in seconds before the child rang the bell to terminate the task. (If the full 15 minutes elapsed and the child did not ring the bell, their waiting time was recorded as 15 minutes.) The second dependent variable was the proportion of delayers, which reflected how many children waited for the full 15 minutes of the task.

3.2.3 Delay of gratification task

The procedure was based on the delay of gratification task used by Yates et al. (1981, study 2). In the task, children could wait to receive a larger number of confectionary rewards. Children were seated in front of a laptop in a quiet room in their school. During the test, three images of unilluminated light-bulbs were displayed on the screen. Each of the light-bulbs were then illuminated one at a time, the first after three seconds, the second after three minutes, and the third after 15 minutes. The lights remained on continuously for the rest of the task and the number of illuminated lights was a cue indicating the number of snacks the child would receive. The participant could end the waiting time if they rang a bell positioned next to them, at which point the amount of time the child had waited (up to a maximum of 15 minutes) was recorded via a stopwatch. Rewards consisted of a selection of miniature chocolate bars, small packets of sweets, and boxes of raisins.

3.2.4 Mood induction

Positive and neutral mood inductions were run as in Yates et al. (1981, study 2). In the positive mood induction, children were asked to recall three things that make them happy and reflect on which of these made them feel happiest and why, using the instructions from Yates et al. (1981) – see Appendix B. In the neutral mood induction, children were asked to recall the names of their classmates. Specifically, children were told: “I’d like you to tell me the names of all of the children in your class. Can you please list them for me?”. If they stopped, children were prompted to see whether they could recall any more names. Both procedures lasted between 60 and 90 seconds.

3.2.5 Mood measure

As a measure of children’s mood, children were asked at the end of the experiment to indicate verbally how they had felt during the mood induction. Children were asked: “Remember earlier when I asked you to [tell me about things you like doing and you told me about .../tell me the names of your classmates]? How did you feel? Did you feel happy, sad, or okay?”. The positive mood induction was referred to without using the word *happy* to avoid priming children to give a positive answer. The options of happy, sad, or okay were presented in counterbalanced order across mood inductions. If participants indicated that they were happy, they selected from one of three schematic faces with increasingly positive expressions. This created a scale from -1 (sad), through 0 (okay) to 3 (very happy).

3.2.6 Procedure

Because the study involved giving children food rewards, when obtaining written consent, parents answered questions regarding dietary requirements for their child. Children were tested in a quiet room in their school.

Children were first shown a box of the snack rewards and told that they could win some of these rewards in the following game. Children were then familiarised with the task rules. The experimenter explained the delay of gratification task using the same instructions as Yates et al. (1981) – see Appendix C. Because testing took place in a room with the experimenter present, rather than in a cubicle as in Yates et al. (1981), participants were instructed about two further rules: not to leave their chair or talk to the experimenter during the task. The majority of participants (79%) complied with the two rules throughout the experiment.[6] To check participants' understanding of the instructions, children were then asked how many rewards they would receive for each number of lights, and how they could end the game. If they were incorrect, they were reminded of the instructions.

The mood induction was then administered. Children in the positive mood condition discussed with the experimenter three things that make them happy. Children who received the neutral mood induction were asked to think of as many of their classmates' names as they could. If they stopped, children were prompted to think of more until they reported that they couldn't. In both mood inductions, the experimenter wrote down their answers.

The delay of gratification task was then completed. In the task, children waited for up to 15 minutes to receive between one and three confectionary rewards. During the task, the experimenter sat out of the child's line of sight, approximately 2 metres behind and to the side of the participant, ostensibly occupied in making notes. The food rewards remained out of view throughout the delay of gratification task. The task ended either when the child rang the bell, or when the maximum time of 15 minutes had elapsed.

---

[6] The proportion of waiting time spent breaching rules was not significantly correlated with total waiting time (Spearman's $r = .15$, $p = .159$), demonstrating that rule breaks did not affect children's waiting behaviour.

Children were then asked a follow-up question about their thoughts during the delay of gratification task. Specifically, children were asked: "What were you thinking about while you were waiting?" The mood manipulation check was then conducted, in which children were asked how they had felt during the mood induction phase. Children then selected from the snack box the number of rewards earned and returned to class.

3.2.7 Data coding

Responses to the question about children's thoughts during the delay period were coded by the experimenter into three mutually exclusive categories. Thoughts were coded as (i) *on-task* if children referred to the waiting process, rewards or lights; (ii) *off-task* if children referred to events outside of the task; (iii) or *neither* if children did not refer to thinking either off or on-task thoughts.

3.3 Results

3.3.1 The effects of mood and age on intertemporal choices

Descriptive statistics are shown in Table 3. Waiting times showed negative skew that could not be addressed with transformation. Parametric and non-parametric tests produced the same results, so the results of parametric tests are reported.

To test whether there was an effect of mood and age on waiting time, a 2 (mood induction) by 2 (age) ANOVA was conducted on total waiting time. There was no main effect of mood induction (F (1, 82) = 0.84, p = .363). The mood induction by age interaction was also not significant (F (1, 82) = 0.20, p = .657). Therefore the effect of mood on intertemporal choice for older children found by Yates et al (1981) was not replicated. There was no significant effect of age group (F (1, 82) = 3.34, p = .071), although the data show a trend towards longer waiting time in the older group.

Table 3. Average waiting times and proportions of delayers from the delay of gratification task by mood induction and age in Study 2.

Mood induction	Age	Waiting time[a]	Delayers
		Mean (SD)	%
Positive	Younger	519 (370)	38.1
	Older	677 (315)	61.9
	Combined	598 (349)	50.0
Neutral	Younger	614 (330)	45.5
	Older	710 (272)	54.6
	Combined	662 (303)	50.0

[a] Waiting time in seconds.

The effect of mood induction and age on proportion of delayers was also examined. There was no association between mood induction and proportion of delayers (χ^2 (1, N = 86) = 0.01, p = .914). There was also no association between age group and proportion of delayers (χ^2 (1, N = 86) = 2.28, p = .131). In case of an interaction between mood induction and age group, the two mood inductions were compared separately for each age group. There was no association between mood induction and proportion of delayers for either age group (younger children: χ^2 (1, N = 43) = 0.24, p = .625; older children: χ^2 (1, N = 43) = 0.24, p = .625).

To determine whether self-reported mood was related to intertemporal choice, a correlation was calculated between post-test mood rating and total waiting time. This was not significant (Spearman's r = -.18, p = .102).

3.3.2 Effectiveness of the mood manipulation

One possible explanation for the lack of effect of the mood induction on children's waiting time could be that the mood induction was not effective. Items listed during the positive mood induction suggested that children did successfully recall positive experiences and objects. The most common responses involved social activities (38%), playing (36%), hobbies (11%), watching TV or playing computer games (10%), special occasions such as holidays (6%), and eating favourite foods (5%). However, there was no difference in subjective mood ratings between the neutral (*Median* = 2.50, *M* = 2.05, *SD* = 1.20) and positive mood inductions (*Median* = 2.50, *M* = 2.05, *SD* = 1.21; *U* (86) = 924, *Z* = .000, *p* = 1.00). This was the case for both age groups (younger: *p* = .770; older: *p* = .773). This suggests that there was no difference between mood induction conditions in retrospectively reported mood and therefore that the positive mood induction was not successful in inducing positive mood.

The effectiveness of the mood inductions can also be examined in terms of their impact on children's off-task thoughts. Yates et al. (1981) argued that off-task thoughts were the mechanism by which the mood induction affected children's intertemporal choice. Children's reported thoughts are shown in Figure 6. Yates et al. (1981) found that for older children, a greater proportion of those in the positive condition reported off-task thoughts than those in the neutral condition, so this was tested with the present data. To replicate Yates et al.'s (1981) analysis, mood inductions were compared in terms of the dichotomous outcome variable of whether children reported off-task thoughts or not (i.e. *on-task* and *neither* categories were combined). There was a trend towards more children overall reporting off-task thoughts in the positive mood induction (χ^2 (1, *N* = 87) = 3.46, *p* = .063). Splitting children by age group, the data suggested that there was an association between mood induction and off-task thoughts for older children (χ^2 (1, *N* = 43) = 4.04, *p* = .044) but not younger children (χ^2 (1, *N* = 43) = 0.19, *p* = .666). In the older group, more children reported off-task thoughts in the positive mood induction (52%) than the neutral mood induction (23%). This suggests that the positive mood induction led to more children having off-task thoughts only in the older age group, and thus that the mood induction was successful in generating distracting thoughts.

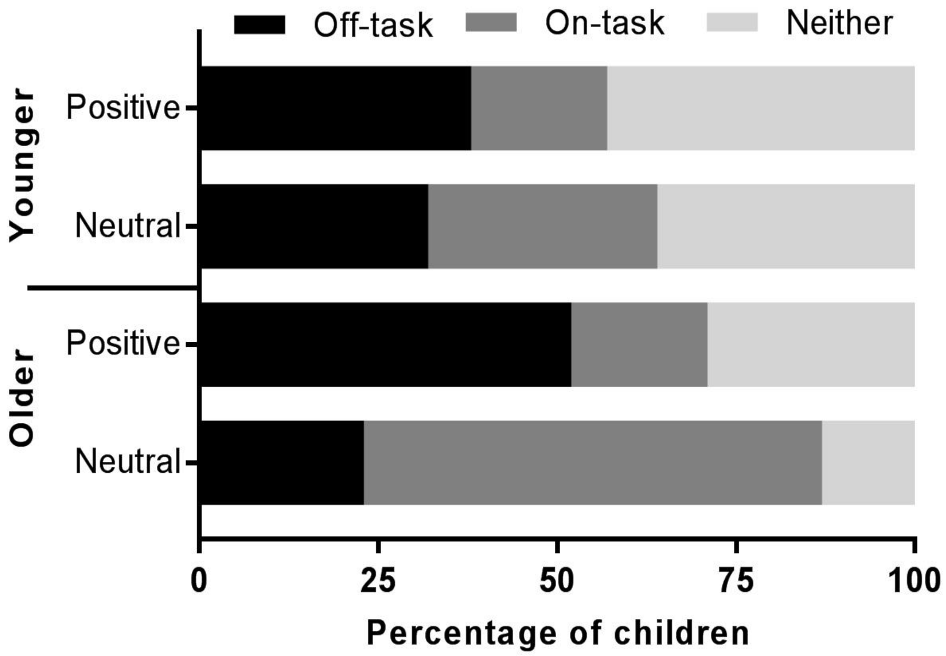

Figure 6. Proportion of participants reporting off-task and on-task thoughts during the delay of gratification task, by mood induction and age group in Study 2.

3.3.3 Relation of off-task thoughts to intertemporal choice

To determine whether children's off-task thoughts during the delay period affected their intertemporal choice, children who reported off-task thoughts were compared with those who did not, in line with Yates et al.'s (1981) analysis. A 2 (off-task thoughts) by 2 (age) ANOVA was conducted on waiting time. There was no significant effect of off-task thoughts (*F* (1, 82) = 0.86, *p* = .355): children who reported off-task thoughts (*M* =

676, *SD* = 302) did not wait longer than those who did not report off-task thoughts (*M* = 605, *SD* = 338). There was also no significant interaction between age and off-task thoughts (F (1, 82) = 1.07, p = .304).

The effect of off-task thoughts on proportion of delayers was also examined. There was no association between off-task thoughts and the proportion of delayers (χ^2 (1, N = 86) = 2.47, p = .116). In case of an interaction between off-task thoughts and age group, the children who reported and did not report off-task thoughts were compared separately for each age group. For younger children, there was no association between off-task thoughts and proportion of delayers (χ^2 (1, N = 43) = 0.22, p = .640). For older children, it appeared that more children who reported off-task thoughts were delayers (75%) than children who did not report off-task thoughts (48%) – although this association did not reach significance (χ^2 (1, N = 43) = 2.98, p = .084).

3.4 Discussion

This study aimed to establish an effective mood induction paradigm, whilst examining whether there was an effect of mood on children's enactment of intertemporal choice. To do so, a replication was conducted of Yates et al.'s (1981) study, which previously found that a positive mood induction, specifically self-generated imagery, lead to improved enactment of intertemporal choice in children over 5 years.

In the present study, there was no effect of positive self-generated imagery on intertemporal choice. Thus, the data failed to replicate Yates et al.'s (1981) findings. This failure to replicate is informative because it suggests that the effect of positive mood induction on intertemporal choice is not robust. The present study used the same experimental design as the original, but with a larger sample size. This makes it arguably more likely that the result presented here is reliable, as small sample sizes lead to overestimations of effect sizes for positive results (Button et al., 2013).

The absence of effect of positive mood induction on intertemporal choice in a delay of gratification task is consistent with some other studies that examined intertemporal choice using delay choice tasks. These studies found no effect of positive mood induction on making an intertemporal choice (Moore et al., 1976; Shimoni et al., 2015). The present data adds to this literature by suggesting that positive mood induction does not affect the *enactment* of intertemporal choice either.

The data also do not provide any evidence that off-task thoughts affect enactment of intertemporal choice. Children who reported off-task thoughts during the delay period waited the same amount of time as those who did not report off-task thoughts. This contrasts with the finding by Yates et al. (1981) that 5-year-olds who reported off-task thoughts waited longer. The present data do however support the finding by Yates et al. (1981) that the positive self-generated imagery mood induction leads to more off-task thoughts, as older children in the positive condition more frequently reported off-task thoughts than those in the neutral condition. This suggests there may be a confound in the self-generated imagery mood induction, because the conditions have different effects on children's level of distraction. However, this greater distraction did not appear to affect children's enactment of intertemporal choice.

One possible interpretation of the failure to replicate an effect of mood induction on intertemporal choice is that it could be driven by differences in the experimental design. However, this is unlikely as only two minor features of the method differed between the studies. Firstly, confectionary rewards were used instead of small toys. This could lead to a lack of difference between mood inductions, if the confectionary rewards were very undesirable (leading to floor effects in waiting), or so desirable that children showed ceiling effects in waiting. The present data do not support this interpretation. There was no evidence of floor or ceiling effects in waiting times, suggesting that children were optimally motivated by the rewards. Secondly, the testing space differed slightly between studies. In the original study, testing took place in a blank cubicle with no experimenter present, whereas in the current study the experimenter was present (though did not interact with the child), and testing took place in school rooms that typically contained some kind of wall display. This could lead to a lack of difference between mood inductions if children distracted themselves sufficiently using the testing space such that positive mood did not provide any additional benefit to waiting times. However, children in the present study reported lower rates of off-task thinking than in Yates et al. (1981), suggesting that the testing space in the present study did not prompt greater distraction. Therefore differences in the method are unlikely to account for the discrepancies between the two studies, suggesting that the failure to replicate represents a genuine lack of effect.

A limitation of the present findings is that it was somewhat ambiguous as to whether the self-generated mood induction successfully induced mood. The positive self-generated imagery mood induction had no effect on children's self-reported mood. However, the mood scale suffered limitations. Children's ratings showed a positive bias, and were limited by their capacity to retrospectively recall their mood. If the mood measure was insensitive to differences in children's mood, it is possible that children's mood did differ between conditions. It should be noted that there was no measure of mood in Yates et al.'s (1981) study, so there is not any direct evidence that the mood induction was effective in the original study. Thus while it is possible that the mood induction was effective in Yates et al. (1981), but was not in the present study, the evidence is unfortunately ambiguous on this point.

Because of the uncertainty about the effectiveness of the mood induction, the precise interpretation of the failure to replicate is unclear. It could either be that there was a lack of effect of positive self-generated imagery on mood, or a lack of effect of mood on intertemporal choice. Therefore the present findings do not unambiguously answer the key questions of interest: that is, (i) whether self-generated imagery is a reliable mood induction, and (ii) how *mood* affects intertemporal choice.

Regardless of the precise interpretation, the failure to replicate findings from the literature raises the question of how reliable other findings in this field might be. Yates et al.'s (1981) findings were demonstrated in two studies using the same paradigm, and yet were not replicated here. This highlights the possibility that other findings from mood induction studies with children may also not be reliable.

The key remaining question is whether or not there is an effect of mood on intertemporal choice. The present study attempted to induce positive mood using a self-generated imagery induction used in previous studies, but it was unclear whether or not this mood induction was effective. Furthermore, the present study did not examine the impact of *negative* mood on intertemporal choice. Thus more research is needed to establish whether mood, when reliably induced, affects intertemporal choice. To do so, a more reliable mood induction technique needs to be identified.

In particular, it remains unclear whether there is an effect of mood on the decision-making component of intertemporal choice. In the present study, a delay of gratification task was used and no effect of positive mood on intertemporal choice was found. Delay of gratification tasks measure the enactment of intertemporal choice, whereas the decision-making component can be measured by delay choice tasks. Thus an important remaining question is whether the effect of mood states on delay choice tasks demonstrated in previous literature (e.g. Schwarz & Pollack, 1976; Moore et al., 1976; Shimoni et al., 2015) are replicable.

In conclusion, these results suggest that there is no effect of positive self-generated imagery on enacting intertemporal choice. Because of doubts over the reliability of the mood induction, it is difficult to ascertain whether the lack of effect was due to a failure of the mood induction in the present study, or a lack of effect of mood on enacting intertemporal choice. However, the failure to replicate the findings of a published study suggests that some scepticism may be merited with regard to findings of previous mood induction studies with children.

Chapter 4: Effects of mood on making intertemporal choices

Chapter 3 showed that a self-generated imagery mood induction with 4- to 6-year-olds had no impact on children's enactment of intertemporal choice in a delay maintenance paradigm. This was contrary to the study that it was designed to replicate (Yates et al, 1981, study 2). This failure to replicate suggests that there is not a consistent effect of mood induction on children's intertemporal choice. It also highlights the possibility already raised from Chapter 2 that mood inductions used to examine the effects of mood on self-control are not as effective as the published literature suggests. Thus, there is a greater necessity to identify a more reliable method for inducing mood.

While Chapter 3 examined the effect of mood on the *enactment* of intertemporal choice, this can be considered as somewhat distinct from the *making* of intertemporal choices. Chapter 3 recapped a distinction between two components of intertemporal choice: those are (i) making a choice between two options, and (ii) enacting this choice over time. These two components of intertemporal choice are relatively independent, and indeed performance on tasks designed to capture these two processes (that is, delay of gratification tasks and delay choice tasks) tend to exhibit small or no correlations (Duckworth & Kern, 2011; Sin Mei Tsui & Atance, 2017). In Chapter 3, the effect of positive mood induction on the ability to enact an intertemporal choice was investigated, and demonstrated no effect. This would seem to suggest that intertemporal choice is not affected by changes in mood. However, the making of intertemporal choices might be differentially susceptible to effects of mood, because making a choice and enacting a choice likely involve different cognitive processes. Therefore, in this chapter, two studies aimed to examine whether mood affects the making of intertemporal choices.

In Study 3, children received a novel video mood induction designed to augment the impact on children's mood. For instance, one feature of the mood induction was that children were aware of the videos they might have watched in other conditions. Children's mood state was also measured by using a scale that was more sensitive to changes in mood than the absolute scales used in previous studies. Intertemporal choice was measured in a delay choice task in which children made a series of choices between two different positive outcomes: confectionery rewards that were either smaller but immediate, or larger but delayed.

A follow-up study, Study 4, had two aims: first, it aimed to increase confidence in the results from Study 3 by ruling out alternative interpretations. Second, it also extended its focus to investigate the effect of mood on intertemporal choices between *negative* outcomes. In typical intertemporal choice tasks, the choices are made between two rewards, or outcomes with positive value. In the context of negative outcomes, children's preferences differ (e.g. Grusec, 1968), suggesting that the impact of mood may also differ. In the novel delay choice task presented here, choices were made between two negative outcomes, specifically dull activities of different duration (for example, one round of clicking numbers now, or three rounds of clicking numbers tomorrow).

4.1 Introduction

When engaging in intertemporal choice, children do two things: firstly, they make a choice between two temporally distinct outcomes. Secondly, they enact this choice over time, resisting any potential temptations to go back on their initial choice. The first stage, making a choice, requires children to form a judgment about which outcome is more valuable to them, based on its size and the time delay. Making a reasoned choice is therefore essential in order for children to self-regulate in the context of intertemporal decision-making. Real life situations which involve intertemporal choice require children to make choices between both positive, rewarding outcomes, such as toys and favoured foods; and negative, costly outcomes, such as chores or disliked foods. In the present study, the effect of mood on the making of intertemporal choices is examined – both in terms of how mood affects choices between *positive* outcomes (e.g. one sweet now or three sweets later) and *negative* outcomes (e.g. one chore now or three chores later).

The making of intertemporal choices is liable to be affected by mood through three possible routes. Choosing between immediate and future rewards involves the weighing up of three key factors: (i) the length of delay interval, (ii) the relative value of the two rewards, and the (iii) level of uncertainty in receiving the rewards. Previous research relating to each of these factors suggests that negative mood will tend to lead to more choices for smaller, immediate rewards, compared to positive mood.

The first key factor affecting children's intertemporal choices is the length of the temporal interval before the larger reward will be received. Studies that have varied the interval for the delayed reward (for example "tomorrow", "in two weeks' time", or "in one month's time") found that children are less likely to choose the delayed reward with a longer interval (Green et al., 1999; Mischel & Grusec, 1967; Mischel, Grusec, & Masters, 1969; Schwarz, Schrager, & Lyons, 1983). This demonstrates that the length of the temporal delay is an important factor in determining children's intertemporal choice. Children's perception of the time interval could be altered by mood, as there is some evidence with adults that positive emotions cause time to be perceived as passing faster (Droit-Volet & Meck, 2007). Therefore, mood may affect children's perception of the delay interval. Specifically, positive mood may reduce the perceived duration of the delay interval, and in turn make it more likely that children will select larger, delayed rewards.

The second key factor affecting children's intertemporal choices is the difference in value between the immediate reward and the future reward. Researchers have manipulated this by holding the value of the immediate reward constant, whilst varying the value of the delayed reward (e.g. one packet of chewing gum now, or two, three, or four packets later). Existing data suggest that children's sensitivity to the value of rewards increases with development, as preschool children are not sensitive to this (Prencipe & Zelazo, 2005), whereas 8- to 9-year-olds (Grusec, 1968) and 12-year-olds (Green et al., 1994) are. The evaluation of rewards may be affected by mood because the perceived utility of the resource now versus in the future depends on current emotions. For example, current negative emotions may increase the immediate utility of the reward for boosting mood (Rosenhan et al., 1974). Thus, mood may affect children's weighting of the relative value of immediate and delayed rewards. Negative mood may boost the value of an immediate reward, thus reducing the likelihood that children would choose the delayed reward over the immediate reward.

The third key factor likely to affect children's intertemporal choices is the level of uncertainty in receiving future rewards. Future rewards may be regarded as more uncertain than immediate rewards, because over time there is a higher chance that events will occur to prevent the delivery of the reward (such as forgetfulness or lost contact). Preschool children wait for longer in a delay of gratification task when the experimenter is shown to be trustworthy (Michaelson & Munakata, 2016) suggesting that they take into account the likelihood of promises being fulfilled. If level of trust is affected by mood in children, as it has been shown to be in adults (Dunn & Schweitzer, 2005), then this might plausibly impact children's preference for delayed rewards – specifically, children in a negative mood might trust the experimenter less, and thus be less likely to select delayed rewards. So there is reason to predict an effect of mood on children's level of uncertainty in receiving future rewards. Negative mood might increase uncertainty about receiving the future reward, and therefore lead to fewer choices for delayed rewards.

In line with these theorised routes by which mood could affect the making of intertemporal choices, preliminary empirical evidence provides a suggestion that negative moods lead to more choices for immediate rewards. Two studies found evidence that negative mood inductions led to more choices for immediate rewards (compared to positive mood inductions) in children. In one study, 9-year-old children received either positive or negative feedback on a picture they had drawn as a mood induction, before performing a delay choice task (Seeman & Schwarz, 1974). Children drew a picture and received either positive or negative feedback, which induced positive or negative feelings respectively. In the delay choice task, participants selected between smaller immediate and larger delayed rewards (for example, one pack of chewing gum now, or two in a week). It was found that children in the negative feedback condition were less likely to select delayed rewards than those in the positive feedback condition. This finding could be explained by any of the three mechanisms outlined as to how mood affects children's intertemporal choice-making.

A similar finding was identified using a different mood induction (Schwarz & Pollack, 1977). In this study, a similar delay choice task was used, with choices between small rewards. It was found that 7- to 11-year-olds were less likely to select delayed rewards after recalling negative memories, compared to positive memories. This suggests that, compared to positive mood, negative mood shifts the weighting of reward value and temporal interval when making intertemporal choices. Specifically, negative mood causes children to favour smaller, immediate rewards over larger, delayed rewards, and this relationship holds across different types of mood induction.

Whilst negative mood may shift choices towards immediate rewards, positive mood appears to have little effect. In one study, 3- to 5-year-olds were assigned to one of three mood inductions: children recalled either sad memories (negative mood induction), or happy memories (positive mood induction), or they counted to 10 (neutral mood induction; Moore, et al., 1976). In the subsequent delay choice task, children made a single decision between a less valued immediate reward (a small pretzel) or more valued reward after lunch (a lollipop). As in the previously mentioned studies, children in the negative condition were more likely to select the immediate reward than children in the positive or neutral conditions. However, there was no difference in choices between children in positive and neutral conditions. This null difference between positive and neutral conditions has been found elsewhere in a study with 8-year-olds (Shimoni et al., 2015). In this study, children recalled a happy memory (happy condition), or a proud memory (pride condition), or counted to 100 (neutral condition).

In the delay choice task, children chose between receiving fewer points in a game now, or more points after a delay. There was no difference in choices made between happy and neutral conditions. Surprisingly, those in the pride condition made *fewer* delayed choices than the others, which runs counter to theoretical predictions. Overall, the data suggest that positive mood may have relatively little impact on intertemporal choice.

In summary, the few existing studies concur in suggesting that negative mood leads to a greater preference for smaller, immediate rewards relative to larger, delayed rewards. Positive mood does not appear have an impact on intertemporal choices relative to neutral conditions, perhaps with the exception of the positive emotion of pride.

But, while the handful of previous studies are suggestive of a consensus on how mood affects the making of intertemporal choices, there is reason to question this consensus. Given the lack of replicability of findings reported in Chapter 2 and Chapter 3, combined with the relative dearth of studies in this area, and the possible concerns raised previously over file-drawer effects in this field, these findings warrant further probing. Thus addressing the question of how mood affects the making of intertemporal choice would be beneficial.

Further, it is difficult to interpret the lack of effect of positive mood on intertemporal choice in delay choice tasks from previous studies. None of the existing studies included a measure of mood state, meaning it was not possible to determine whether conditions differed in the moods induced. Thus, the null effect of positive mood could be genuine, or could be a false negative result caused by failure of the mood manipulation. If the latter is the case, then it is uncertain whether positive moods influence children's intertemporal choice based on existing evidence. This is an important gap to be addressed.

A further limitation of most of the existing studies is that the delay choice tasks used may not have been adequate measures of self-control. Three of the four studies framed choices in an abstract way, where rewards were not present and without the potential to receive all choices (Schwarz & Pollack, 1977; Seeman & Schwarz, 1974; Shimoni et al., 2015). This is important because having rewards present makes intertemporal choice more challenging (Mischel et al., 1972) and increases the predictive validity of intertemporal choice tasks (Shoda et al. 1990), presumably because there is a higher level of temptation involved. Therefore previous studies did not use measures of intertemporal choice that tapped self-control. This issue is important to address in the present research.

All of the research discussed thus far has used positive outcomes (i.e. rewards) as the stimuli for making intertemporal choices. However, choices between negative outcomes are just as worthy of investigation. By inverting the valence of the outcome, the choice facing the child becomes one of avoiding negative events by placing them further ahead in time, something which happens in real life. For example, children might opt to eat all of their broccoli the next day instead of having to eat one piece of broccoli today. Thus using negative outcomes provides a novel and ecologically relevant way to investigate the construct of intertemporal choice.

Children show an opposite pattern of preference for negative compared to positive outcomes, supporting the idea that making choices with negative outcomes reflects a similar process of intertemporal choice. The larger the delayed negative outcome, the more likely children are to choose *immediate* negative outcome (Grusec, 1968). For instance, children are more likely to choose to memorise 10 lines of dull poetry now if the alternative is to memorise 40 lines in two weeks' time (rather than 20 lines in two weeks' time) (Grusec, 1968). This opposite pattern to positive outcomes is of course expected because for negative outcomes, a larger negative outcome is *bad*, while a long delay (i.e. avoidance of an aversive outcome) is *good* for the present self. By contrast, in the case of rewards, a larger reward is good, while a longer delay is bad for the present self.

The impact of mood on choices between negative outcomes may diverge from that of positive outcomes, because children do behave somewhat differently when making choices between these two outcome types. Firstly, children appear to be risk averse, being more willing to take an immediate punishment than wait for a delayed reward (Grusec, 1968). Secondly, children do not appear to take delay duration into account when making choices between negative outcomes (Mischel & Grusec, 1967; Mischel et al., 1969). This suggests that children perceive a smaller immediate negative outcome as undesirable, regardless of how soon they might incur a larger delayed negative outcome. Thirdly, children's intertemporal choices with negative and positive outcomes are not correlated with each other (Grusec, 1968; Mischel & Grusec, 1967), suggesting that somewhat separable mechanisms may determine these two types of choices. There is no existing research examining the effect of mood on intertemporal choice with negative outcomes, and so this represents an important gap in our knowledge of how mood affects intertemporal choices.

In sum, the previous literature demonstrates a need to determine whether there is a genuine effect of mood on children's intertemporal choice-making, and an opportunity to examine this in the context of negative outcomes. The two studies presented in this chapter address the question of whether there is an effect of mood on children's intertemporal choice, in terms of choices between both positive outcomes and negative outcomes. They improve on previous studies by (i) ruling out the possibility that positive mood inductions have been ineffective, and (ii) using a more ecologically valid measure of making intertemporal choices.

A video mood induction was used in both studies, because this was shown to be partially effective in Chapter 2. To augment the efficacy of the video mood induction, three changes were made to the procedure compared to Chapter 2. Firstly, a negative clip was selected that would be more likely to induce a durable change in mood. In Chapter 2, a sad video was used, which required children to have empathy for a character's grief. Because children are aware that the character is not real, their negative mood may be unlikely to persist beyond the duration of the video. In the present studies, the negative-mood videos were selected to be boring, because boredom is experienced personally, rather than vicariously. To induce boredom, the content of the video was a familiar and uninteresting task which would be aversive for children to watch for an extended period. Secondly, to create a clearer contrast between the mood induction and what the child might alternately be doing, children were made aware of the videos watched in alternative conditions. This provides a potential comparison for children to judge more precisely how positive or negative their experience is. Finally, the procedure was adjusted to reduce the possibility that effects of the mood induction might be confounded with feelings towards the experimenter. For instance, if children blamed the experimenter for making them watch a boring video, this might result in reduced compliance. Thus the videos were ostensibly randomly allocated by a computer, rather than the experimenter.

The present studies included a measure of mood to establish whether mood inductions changed children's mood state. A self-report scale which directly measures change in mood was adapted from Weisberg and Beck (2012). The scale consists of a series of faces with expressions ranging from negative to positive, as well as an arrow for indicating whether mood has improved, worsened, or stayed the same compared to previously. Using this scale, children rated their baseline mood prior to the mood induction, and how their mood changed following the mood induction. In addition, in Study 4 an observational measure of mood was included to provide convergent evidence for the efficacy of the mood induction. Children's positive and negative emotional expressions, and levels of disengagement, were coded.

The present research addressed concerns about lack of ecological validity of delay choice tasks used in previous mood inductions studies by using a delay choice task with real rewards that were physically presented. The delay choice task was similar to those used in research outside of the field of mood induction (Lee & Carlson, 2015; Prencipe & Zelazo, 2005b; Thompson, Barresi, & Moore, 1997). In the delay choice task with rewards, children were offered a choice between a single sweet now, and more sweets the following day, across 12 trials. The sweets were placed in front of children while they decided. The size of the delayed reward was also systematically varied (2, 3 or 4 sweets) to establish whether children's choices were sensitive to the size of the reward. The key outcome variable was the proportion of delayed choices – that is, trials in which the child selected the delayed reward. In Study 4, a novel adaptation of the delay choice task was used to measure children's choices between negative outcomes: the delay choice task with chores. In the delay choice task with chores, children were offered a choice between a single round of a boring activity now, and more rounds the following day, across 12 trials.

The following two studies examine whether there is an effect of mood on making intertemporal choices. In both studies, children first rated their baseline mood, and then completed a mood induction. In the mood induction, children watched either a positive, neutral or negative video. They subsequently rated the change in their mood. Children then completed a delay choice task to measure intertemporal choices.

4.2 Study 3

4.2.1 Method

4.2.1.1 Overview

Study 3 investigated the effect of mood on intertemporal choices between two different positive outcomes. In this study, children first rated their baseline mood, and then completed a mood induction. In the mood induction, children watched either a positive or negative video. (A neutral mood induction is not reported in this study because the data were combined from two data-sets which asked slightly different research questions, only one of which included a neutral mood induction.[7]) They subsequently rated the change in their mood. Children then completed a delay choice task for confectionery rewards to measure intertemporal choices.

4.2.1.2 Participants

Participants were recruited from five primary schools in the north and south of England. The schools were in areas that represented a broad range of socio-economic

[7] Specifically, the first data-set (*N*=73) was collected by an undergraduate research assistant and included only positive and negative conditions. This data showed a small but significant effect of mood on delay choices (in favour of positive mood), but there was a lack of counterbalancing of the specific sweet rewards given to children in the two mood conditions. Thus it was appropriate to collect a second data-set (*N*=77), and this included positive and negative conditions, and a no-intervention neutral group. The second data-set found no effect of mood condition, and when combined there was also no effect. It was judged that combining the two data-sets, and removing the neutral condition (*N*=26), was the most parsimonious way to represent the results.

backgrounds (1st, 5th, 9th, and 10th decile in the Index of Multiple Deprivation, Department for Communities and Local Government, 2015). Participants were 122 children (55 male) aged 7 to 11 years (M_{age} = 9 years 6 months, range = 7 years 9 months to 11 years 3 months). Two further participants were excluded, one due to absence on the dates of testing, and the other due to failure to follow task instructions.

4.2.1.3 Design

A mixed design was used with a between-subjects factor of mood induction (positive or negative) and a within-subjects factor of reward size (2, 3, or 4). Children were randomly allocated to a mood condition (negative, $N = 59$; or positive, $N = 63$). There were no differences between the two mood conditions in age (t (120) = 0.10, $p = .923$), or gender (χ^2 (1, $N = 122$) = 1.72, $p = .190$), nor in self-reported baseline mood (t (120) = 1.13, $p = .262$). All children completed the delay choice task, in which they made a series of choices between receiving one reward immediately, or more rewards after a delay. Reward size was manipulated within subjects, with one of the three reward sizes (2, 3 or 4) on each trial of the delay choice task. The key dependent variable was the proportion of delayed choices on the delay choice task.

4.2.1.4 Delay choice task

In the delay choice task, children opted to receive either one reward now or multiple rewards the following day[8]. Rewards were eight types of confectionery (e.g. chocolate buttons and gummy sweets). From these eight, four types were chosen for each child, depending on their dietary requirements. The size of the immediate reward was fixed (i.e. 1 individual sweet) whilst the size of the delayed reward was varied (2, 3, or 4

[8] Where testing was conducted on the final day of the school week ($N = 17$), rewards were offered for the first day of the next school week.

sweets later). There were 12 trials in total, created by crossing the four types of reward with the three reward sizes. Trials were presented in a fixed order. The experimenter presented sweets on two separate plates, one representing the immediate and one the delayed reward, while they asked the child to make a choice (e.g. "Would you like 1 now or 3 later?"). Following each choice, rewards were placed in the one of two paper bags. One bag was for immediate choices (placed next to the immediate plate) and one for delayed choices (place next to the delayed plate). The child's name was written on each bag.

4.2.1.5 Mood induction

Two video clips were used to induce mood. In the positive mood induction, children viewed a humorous scene from the animated film *Up*. The clip (lasting 2'52" in duration) showed a boy meeting and playing with a funny bird. In the negative mood induction, participants watched a live action clip of a woman completing a mundane task. The clip (lasting 3'33" in duration) showed a woman seated in front of a laptop computer and copying notes onto paper. The content of the videos for both mood inductions were described to children before the computer ostensibly selected which video they would watch. Videos were in fact randomly allocated before testing began.

4.2.1.6 Mood measure

To measure subjective mood, a self-report face scale was used. The scale was adapted from Weisberg and Beck (2012). The scale showed a series of nine faces ranging from very sad to very happy via a neutral point, and three perpendicular arrows were affixed below the scale (see Figure 7). Children rated their mood at baseline by selecting one of the nine faces that represented their current mood. Following mood induction, participants indicated whether they felt worse, the same, or better than they did previously by pointing to one of three arrows affixed below the scale (that is, the left arrow for worse, the upward arrow for the same, and the right arrow for better).

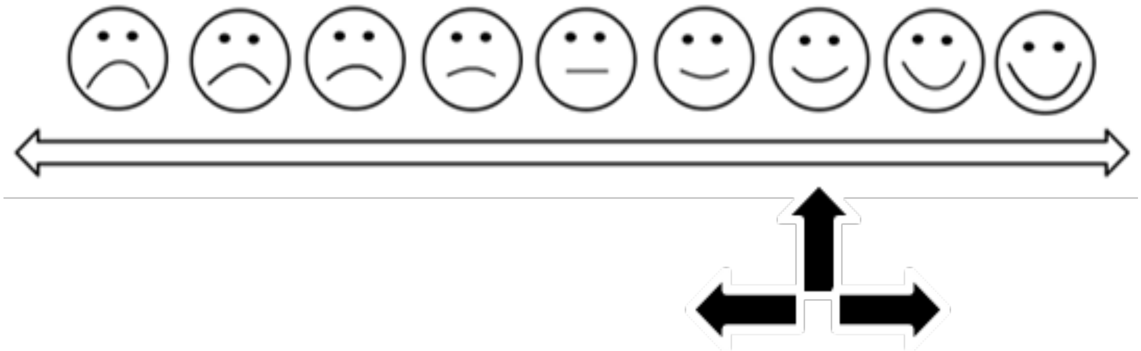

Figure 7. Self-report mood scale used in Studies 3 and 4.

4.2.1.7 Procedure

Children were tested individually in a quiet area of their school, by one of two female experimenters. Children first received instructions for the mood scale; then rated their baseline mood, before receiving the mood induction. After this, children rated their change in mood, followed by completing the delay choice task.

Instructions were first provided for how to use the mood rating scale. The experimenter provided an example of situations for using each of the three arrows in the context of playing football (worse, the same, or better). For example: "If the other team scored, I would feel worse, so I would pick this arrow [left arrow]". Children were then asked to demonstrate their understanding of the scale, by selecting the appropriate arrow for three further examples. Children were corrected if they responded inaccurately.

Then, children rated their current mood using the face scale. They were asked to "Point to the face that shows how you feel right now". They then completed the mood induction. Children were told that they would watch one of two videos and saw a screenshot of each video while the experimenter described the content. The positive video was described as "A clip from the animated film *Up*, of a boy playing with a funny bird"; the negative video was described as "A clip of a woman taking notes and doing her homework". They then saw a loading screen as the computer ostensibly allocated their video. They watched the video for their mood induction whilst wearing noise-cancelling headphones. After watching the video, children rated whether their mood had changed using the arrows on the rating scale.

Children then completed the delay choice task, making 12 choices between a small immediate reward, or a larger delayed reward. Following this task, children in the negative mood induction were allowed to watch the positive video if they wished in order to ensure that the negative mood did not endure. Children took the bag of immediate rewards straight away and received the bag of delayed rewards the following morning.

4.2.2 Results

4.2.2.1 Effect of mood on making intertemporal choices

To determine whether the mood induction had an effect on intertemporal choice, the effect of mood induction on the proportion of delayed choices was examined. The proportion of delayed choices by mood induction and reward size is shown in Table 4. The proportion of delayed choices showed negative skew, but because nonparametric and parametric tests produced equivalent results, results of parametric tests are reported.

The proportion of delayed choices was subjected to a 2 x 3 mixed ANOVA with mood induction as a between-subjects factor and reward size (2, 3 or 4 sweets) as a within-subjects factor. There was no difference in the proportion of delayed choices between positive and negative mood inductions (F (1, 120) = 1.66, p = .200, η^2 = .014). Nor was there a main effect of reward size (F (2, 240) = 0.46, p = .629), or a mood induction by reward size interaction (F (2, 240) = 0.80, p = .453). Because there was no effect of reward size, the data were pooled across reward sizes and a t-test used to compare overall performance in positive and negative mood inductions. There was no significant effect of mood induction on the overall proportion of delayed choices (t (120) = 1.24, p = .219).

Table 4. Proportion of delayed choices as a function of mood induction and reward size in Study 3.

Reward size	Positive	Negative	All mood inductions
2	0.72 (0.29)	0.65 (0.32)	0.62 (0.31)
3	0.73 (0.31)	0.65 (0.29)	0.68 (0.29)
4	0.72 (0.28)	0.70 (0.34)	0.72 (0.30)
All reward sizes	0.72 (0.24)	0.67 (0.25)	0.67 (0.23)

In case the lack of effect of mood on intertemporal choice was driven by the mood induction being ineffective for some children, the data were analysed including only participants whose mood change ratings were in line with their mood induction (i.e. children in the positive mood induction who reported feeling better (N = 38), and children in the negative mood induction who reported feeling worse (N = 37)). A 2 (mood induction) by 3 (reward size) ANOVA revealed no effect of mood induction (F (1, 73) = 0.83, p = .364, η^2 = .011). There was also no effect of reward size (p = .788) and no interaction between mood induction and reward size (p = .324).

Further, in case the impact of the mood induction wore off over time, an additional 2 (mood induction) by 3 (reward size) ANOVA on proportion of delayed choices was conducted including only the first half of trials. This produced the same pattern of results, in that there was no main effect of mood induction (p = .364), nor a main effect of reward size (p = .788), or mood induction by reward size interaction (p = .324).

In order to test whether naturally existing variability in mood was related to intertemporal choice, the correlation between baseline mood rating and proportion of delayed choices was examined. There was no correlation between baseline mood and delayed choices (r = -.04, p = .636).

4.2.2.2 Mood manipulation check

One possible explanation for the lack of effect of mood on intertemporal choice could be that the mood inductions were not effective. To determine the effectiveness of the mood inductions, mood change ratings were compared across the two mood inductions using a chi-squared test (see Table 5). The test showed that mood induction and mood change rating were associated (χ^2 (2, N = 122) = 72.1, p < .001). Examination of residuals revealed that children in the positive mood induction were more likely to rate their mood as better than children in the negative mood induction. Likewise children in the negative mood induction were more likely to rate their mood as worse than children in the positive mood induction. Therefore, the mood inductions appear to have induced divergent changes in mood. Thus a lack of mood manipulation efficacy is unlikely to account for the lack of effect of mood on intertemporal choice.

4.2.2.3 Task validity check

The lack of effect of reward size on task performance was somewhat surprising, and means that it is possible that the delay choice paradigm did not function adequately. If children were making reasoned choices in the task, as opposed to responding randomly, we would expect that their choices would be sensitive to the value of the reward, such that they would make more delayed choices as the size of the reward increased. On the other hand, it might be the case that children in this sample made reasoned choices but simply did not take reward size into account in a consistent way.

Table 5. Self-report ratings of mood change as a function of mood induction in Study 3.

	Mood induction	
Rating	**Positive**	**Negative**
Worse (%)	1.6	62.7
Same (%)	28.1	37.3
Better (%)	60.3	0.0

One way to distinguish these two possibilities is to examine the effect of factors that might be expected to correlate with delay choices on performance. Such factors include age (e.g. Mischel & Meltzner, 1962) and socioeconomic status (e.g. Watts, Duncan & Quan, 2018). To explore this, a multiple linear regression was performed to predict the proportion of delayed choices as a function of age and socioeconomic status. Socioeconomic status was measured as the decile of children's school from the Index of Multiple Deprivation, Department for Communities and Local Government (2015). The overall regression model was significant ($F(2,119)=13.62$, $p<.001$) with an R^2 of .19. Age was not a significant predictor ($\beta=.083$, $p=.344$), but socio-economic status was ($\beta=.450$, $p<.001$). For every decile increase in socioeconomic status, the proportion of delayed choices increased by 3%.

These findings are in line with previous research suggesting that children from lower socioeconomic backgrounds make fewer delayed choices in intertemporal choice tasks (Watts et al., 2018), and indeed the effect of age on intertemporal choice is not consistent (Mischel & Underwood, 1974); Toner, Lewis & Gribble, 1979). Overall, this analysis confirms that the delay choice task was sensitive to factors that might affect intertemporal choice. This suggests that whilst children's choices were not sensitive to reward size in this task, their responses were not random, and thus that the paradigm used was functioning adequately.

4.2.3 Discussion

Study 3 aimed to examine whether there was an effect of mood on the making of intertemporal choices in children. The results showed that children's intertemporal choices do not differ according to being in a negative compared to a positive mood. Thus the data imply that there is not an effect of mood on making intertemporal choices.

The results contrast with previous observations that negative mood decreased children's tendency to choose delayed rewards relative to positive or neutral mood (Moore et al, 1976; Seeman & Schwarz, 1974; Schwarz & Pollack, 1977). Unlike these previous studies, the present study measured intertemporal choice using a delay choice task with real rewards, which were present during decision-making. This increases the validity of the task at measuring self-control (Mischel et al. 1972; Shoda et al. 1990). The new data therefore suggest that when a measure of intertemporal choice is used that requires greater self-control, there is no effect of mood on choice.

The contrast between present and past findings raises the question of to what extent the present findings are trustworthy. The lack of difference in intertemporal choice between positive and negative mood inductions is unlikely to be due to sample size. Previous literature found either large effect sizes (Moore et al, 1976; Seeman & Schwarz, 1974) or medium effect sizes (Schwarz & Pollack, 1977). With the present sample size there was 99% power to detect a large effect size and 78% power to detect a medium effect size. Thus, assuming that the true effect size is accurately estimated by previous studies, Study 3 was sufficiently powered, increasing confidence in its findings. The lack of effect of mood on intertemporal choices is also unlikely to be due to a failure of the mood induction. According to children's self-report, children in the negative mood induction felt worse, and children in the positive mood induction felt better, after the mood inductions. This suggests that the mood inductions were effective at lowering and boosting children's moods respectively.

Thus, the data from Study 3 suggest that there is no effect of mood on making intertemporal choices. However, a further test would be beneficial to increase confidence in this view. The results of Study 3 conflict with those of three previous studies which found that negative mood led to more choices for smaller, immediate rewards (e.g. Moore et al, 1976). Given the tendency for research in this field not to replicate demonstrated in Chapters 2 and 3, a replication of the present study would provide more compelling evidence for a lack of effect.

One alternative interpretation of the lack of effect of mood on making intertemporal choices could be that the mood induction was not as effective as the self-report data suggested. The self-report data indicated that the mood induction was effective, and self-report has the benefit of tapping into children's subjective mood. Indeed, self-report is relied upon as the only measure of mood in the previous literature. But self-report has limitations, in that it may be swayed by priming of positive or negative content from mood induction stimuli, and potentially by children's desire to conform to the experimenter's expectations. Thus self-report measures may tend to overestimate the efficacy of mood induction. One way to overcome this would be to include a convergent measure of mood that is not affected by demand characteristics, such as observations of facial expressions.

It is also possible that the moods induced by the mood induction were not sufficiently durable to affect children's intertemporal choices. The video mood inductions used contained similar content and were of a similar duration to those used in previous work (e.g. von Leupoldt et al., 2007; Schnall et al., 2008), and there was no evidence that the mood induction affected children's intertemporal choice in earlier trials any more than later trials. However, it remains possible that the mood induction was simply not sufficiently durable to affect children's subsequent behaviour. If it were possible to prolong the impact of the mood induction, then this would increase confidence in the finding that mood does not affect intertemporal choice. One way to do so might be to increase the duration of the mood induction stimulus.

To provide a replication of Study 3 and rule out the two possible alternative interpretations of the results, a further study was conducted. Study 4 used a similar method to Study 3, but included an additional measure of mood via observations of facial expressions, and attempted to increase the durability of the video mood induction. In addition, it presents a novel measure of intertemporal choice to examine choices between negative outcomes.

4.3 Study 4

Study 3 suggested that there is not an effect of mood on making intertemporal choices, and that earlier findings to the contrary (Moore et al, 1976; Seeman & Schwarz, 1974; Schwarz & Pollack, 1977) may have been anomalous. To increase confidence in this interpretation, a further empirical test of this same question would be beneficial. Thus, Study 4 provides a replication of Study 3. Study 4 also extends Study 3 by ruling out alternative explanations for the lack of effect of mood on making intertemporal choices: an observational measure of mood was included, and the video mood induction was adapted to try to make it more durable.

Study 4 attempted to rule out alternative explanations for the findings from Study 3, specifically that the mood induction was not sufficiently durable. To try to rule out this possibility, Study 4 increased children's exposure to the emotional stimuli by increasing the duration of the videos. The duration of videos is thought to be an important factor in their efficacy at inducing mood, with longer videos hypothesized to induce mood states more reliably (Rottenberg et al., 2007). In this study, the length of videos was increased to 7 minutes. It was also predicted that the impact of a negative video with boring content will be enhanced if the same video clip is repeated, because repetitiveness is a key feature of boring stimuli (Daschmann, Goetz, & Stupnisky, 2011).

Study 4 also improves upon Study 3 by including a convergent measure of mood, namely observations of children's facial expressions. While the common approach of measuring mood via self-report can be a reliable measure of subjective mood, confidence in the findings would be further enhanced by using a convergent methodology. Observations of facial expressions are an alternative way to index children's mood. Observing children's emotional expressions during mood induction has the advantage of measuring emotion independently of self-report biases and demand characteristics. Positive and negative emotions are associated with stereotypic facial expressions, such as smiles and frowns (Ekman, Freisen, & Ancoli, 1980). In addition, orienting away from the emotional stimulus can indicate dislike, or disengagement from the stimulus, which is a key feature of boredom (Eastwood, Frischen, Fenske, & Smilek, 2012). Facial expressions tend to follow from specific emotions (e.g. sadness, or fear), so to measure mood *valence,* coding schemes need to be sufficiently open to be sensitive to a range of negative and positive mood states. Thus expression ratings combined with self-report can provide convergent evidence on the efficacy of mood induction.

An interesting new question is whether mood affects intertemporal choices between negative outcomes. This is an important question for at least two reasons. Firstly, because the previous literature suggests that children reason differently about negative than positive outcomes (e.g. Grusec, 1968). Therefore the effect of mood on choosing between potential negative outcomes may be different to choosing between potential positive outcomes. Secondly, including negative outcomes may overcome a possible methodological limitation of intertemporal choice tasks with positive outcomes. Specifically, when positive outcomes are used, the negative mood induced in the negative mood condition might be overridden by children's positive emotional response in anticipation of the rewards. By examining the effect of mood induction on negative outcomes, this issue can be circumvented.

To attempt to investigate the question of how mood affects intertemporal choices between negative outcomes, a novel delay choice task with negative outcomes was created. In the delay choice task with chores, the negative outcomes were activities which were based on tasks used to induce boredom in adults (Markey, Chin, VanEpps, & Loewenstein, 2014) and designed to involve the key features of boring activities: they were easy, repetitive, and had no purpose (Daschmann et al., 2011). Thus children chose between completing one round of a chore now, or more rounds the following day.

Thus Study 4 aims primarily to increase confidence in the lack of effect of mood on intertemporal choices between positive outcomes. A secondary aim is to investigate whether mood affects intertemporal choices between negative outcomes. Children were assigned to one of three video mood induction conditions (positive, neutral or negative). After completing the mood induction, children completed one of two delay choice tasks in each session: a delay choice task with positive outcomes (i.e. sweets) in one session, and a delay choice task with negative outcomes (i.e. chores) in another session. The size of the delayed outcome was again varied systematically (2, 3 or 4) and the proportion of delayed choices was the outcome of interest. The efficacy of the mood induction was measured both by self-report, and by observations of facial expressions of mood observed during the mood induction.

4.3.1 Method

4.3.1.1 Overview

Children participated in two experimental sessions. In each session, children first self-reported their baseline mood. They were then assigned to one of three video mood inductions (positive, negative, or neutral) which was the same for both sessions, and watched video clips intended to induce the appropriate mood. Children then rated whether their mood changed. Children's facial expressions were recorded during the mood induction and rated to obtain a second measure of mood state. Children subsequently completed one of two delay choice tasks. In the first session, children completed the delay choice task with rewards, in which they chose between confectionary rewards (as in Study 3). In the second session, children completed the delay choice task with chores, in which they chose between uninteresting activities.

4.3.1.2 Participants

Participants were recruited from two primary schools in the north of England. The schools were in areas of low to middle socio-economic status (1st and 5th decile in the Index of Multiple Deprivation, Department for Communities and Local Government, 2015). Participants were 50 children (29 male) aged 7 to 8 years (M_{age} = 8 years 3 months, range = 7 years 8 months to 8 years 8 months)[9].

4.3.1.3 Design

A mixed, 3 (mood) x 2 (reward size) x 2 (outcome value) design was used. Mood was manipulated between-subjects, with children randomly allocated to one of three mood

---

[9] Note that the sample size in this study was smaller due to time limitations in recruitment. The number of children per condition is still comparable with previous literature in this field.

conditions (positive, *N* = 17; neutral, *N* = 17; or negative, *N* = 16). Reward size (2, 3 or 4 sweets) was manipulated within subjects and as such varied between trials. Outcome value (positive or negative) was manipulated within subjects, with children completing two versions of the delay choice task: the delay choice task with rewards, and the delay choice task with chores. These tasks were administered across two sessions to reduce the likelihood of emotional contagion from the previous session. Sessions were conducted at least 3 hours apart, and on average more than one day apart (*M* = 38.3 hours, *SD* = 16.9), therefore reducing the likelihood of any transfer of emotion from one session to the next. The two sessions were administered in a fixed order, with the delay choice task with rewards in the first session, and the delay choice task with chores in the second session. The delay choice task with rewards was presented first because this was the key measure of intertemporal choice, and thus possible order effects on this could be removed.

There were no differences between the three mood induction conditions in age (F (2, 49) = 0.98, p = .384). There was a trend towards a difference in gender composition (χ^2 (1, N = 50) = 5.70, p = .058), such that there were more males in the negative mood induction, and more females in the neutral mood induction.[10] There was no difference in baseline mood between mood inductions for either of the two sessions conducted (session 1: p = .609; session 2: p = .411). The key dependent variable was the proportion of delayed choices.

[10] Analyses of intertemporal choice were conducted with gender as a covariate. Simple ANOVA results are reported because the gender covariate did not affect the interpretation of any of the results.

4.3.1.4 Delay choice tasks

Two delay choice tasks were used. The delay choice task with rewards was identical to that in Study 3, in which children opted to receive either one confectionary reward now or multiple rewards the following day[11] over 12 trials.

The delay choice task with chores was devised to measure children's intertemporal choices in the context of negative outcomes. Children decided whether they would prefer to do one round of a dull computerised activity (or "chore") now, or do multiple rounds of the same activity the following day. There were four chores: Boxes, Circles, Grids and Dials (see Figure 8). To try to ensure that children understood that these tasks were unappealing, all children tried each of the four chores at the start of the experiment. The tasks were designed based on qualities known to elicit boredom, including being easy, repetitive, and not serving a purpose (Daschmann et al., 2011). Tasks were programmed so that children could not pursue faster performance as a form of positive reinforcement: inter-trial intervals in all trials varied as a function of children's reaction time, so that if children responded more quickly, they would simply wait longer during the inter-trial interval. Each task lasted approximately 20 seconds. The tasks were presented to children simply as activities, without any indication as to whether they would find them interesting or not.

[11] Where testing was conducted on the final day of the school week (session 1 $N = 2$; session 2, $N = 12$), delayed outcomes were offered for the first day of the next school week.

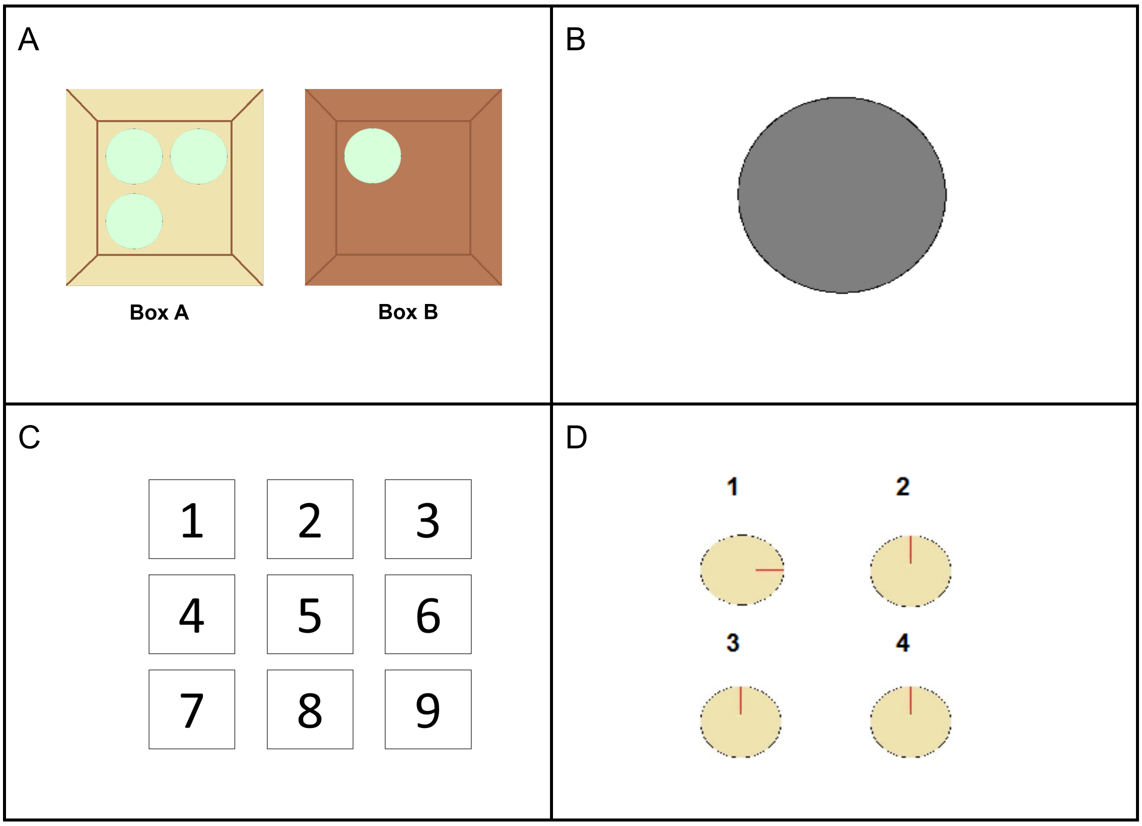

Figure 8. Images of activities in the delay choice task with chores. A. Boxes activity. B. Circles activity. C. Grids activity. D. Dials activity.

In the Boxes activity, participants had to move four balls from box A to box B, and then move all four balls back from box B to box A. In each trial, participants pressed the spacebar twice, once to remove the ball from one box, and once to place it in the other box. There were eight trials, with an inter-trial interval consisting of a fixation cross between each.

In the Circles activity, participants pressed the spacebar when a target (a large grey circle) appeared on screen, but did nothing when a distractor (a large white circle) appeared. In each round there were seven distractors and two targets. Distractors were on screen for 1 second, while targets remained onscreen until the spacebar was pressed.

In the Grids activity, participants had to click on the numerals from 1 to 9 in order. Children were presented with a grid of the numerals from 1 to 9, and in the first trial were required to click the number 1, then in the second trial the number 2, and so on. Once the correct number was clicked, that number was removed from the grid, and the next trial began. Thus there were nine trials, and there was an inter-trial interval with a fixation cross between each trial.

In the Dials activity, participants had to turn four dials so that the notch faced to the right, and then turn them back to the top. They had to do so in order of the number of each dial. In each trial, participants clicked the correct dial to turn it. There were eight trials with an inter-trial interval with a fixation cross between each trial.

In the delay choice task with chores, children were offered a choice between one round of a boring task now, and more rounds (2, 3 or 4) later. There were 12 trials in total. Trials were presented in a fixed order. Ten images of each task were printed onto 3cm square cards. On each trial, the experimenter placed the appropriate number of images of the tasks on two separate envelopes, one representing the immediate outcome and one the delayed outcome, whilst asking children to make a choice (e.g. "Would you like 1 now or 3 later?"). Following each choice, the images of the tasks were placed inside one of the two envelopes, depending on the child's choice.

4.3.1.5 Mood induction

Video clips were used to induce mood. The video clips for positive and negative mood inductions were similar in content to those used in Study 3, with amusing clips from animated films in the positive mood induction and boring clips in the negative mood induction. Two sets of video clips were created for each mood induction, so that different sets could be shown in each of the two sessions. The session in which the sets of clips were presented was counterbalanced between participants.

Videos in the positive mood induction consisted of happy clips taken from animated children's films, lasting 7 minutes for each set. Videos in the negative mood induction were clips taken from Youtube of monotonous activities, lasting 7 minutes for each set. The same clip was repeated twice to enhance the monotony of the experience. A plain black screen was inserted as a transition between each separate clip. Videos in the neutral mood induction were shorter clips taken from Youtube of an English language educational video, lasting approximately 1 minute. This was to control for the process of watching a video, whilst not introducing any content likely to significantly impact children's mood. Further details of the video clips used are included in Appendix D.

The content of the videos for both mood inductions were described to children before the computer ostensibly selected which video they would watch. Videos were in fact randomly allocated before testing began.

4.3.1.6 Mood measures

Mood was measured using self-report and observation. The self-report mood scale was identical to that used in Study 3, with a face scale for baseline mood, and arrows to report change in mood.

For the second measure of children's mood, children's emotional expressions were coded from video recordings. Children were video recorded during the mood induction. A webcam, which was embedded in the screen used to present videos, recorded the child's shoulders and face while they watched the video.

Video recordings of each child were coded by trained, independent coders who were blind to hypotheses and children's assigned mood induction. The coding criteria reflected facial actions identified in the Facial Action Coding Scheme (Ekman & Friesen, 1978) and behavioural actions associated with positive and negative emotions. However, an emphasis was placed on overall judgment, rather than the strict coding structure of the FACS, as has been done in previous research (Levine et al., 2008; Ridgeway & Waters, 1987; Watson-Jones, Whitehouse, & Legare, 2015). This was because the coding scheme needed to detect mood valence, rather than specific emotional states. Thus coders were to note every expression of positive emotion (which could include smiling or laughter), and every expression of negative emotion (which included a lack of smile, combined with frown, lowered brow, eyelid droop, or yawn).

Each instance of an expression of positive or negative emotion was noted for each child, as well as the duration of that expression. Examples of positive expressions were smiling and laughter. Examples of negative expressions were frowning or lowering of eyebrows without smiling. As a behavioural measure of disengagement from the mood induction, each instance of a look away from the screen was coded, as well as the duration of the look away.

The outcome variables reflected the percentage duration of positive and negative emotional expressions, and disengagement. The duration of positive and negative expressions were obtained by summing the number of seconds of positive or negative expressions, and calculating this as a percentage of the duration of the video recording. The duration of disengagement was similarly obtained by summing the number of seconds spent looking away, and calculating this as a percentage of the duration of the video recording.

Video data was obtained for 75% of sessions. (The remaining 25% of data was missing due to equipment malfunction which caused children's full faces not to be visible.) Both coders rated 20% of the recordings, stratified across mood inductions. Inter-rater reliability was assessed with intraclass correlations (Cicchetti, 1994). Inter-rater reliability was good to excellent for positive expressions (duration: 0.79), negative expressions (duration: 0.86), and disengagement (0.86) in positive and negative mood inductions. Therefore the remainder of the data was single-coded. Acceptable inter-rater reliability was not obtained for the neutral mood induction data, probably because the period of observation (~1 minute) was too short to obtain reliable coding.[12] Therefore ratings for children in the neutral mood induction were not retained.

4.3.1.7 Procedure

Children were tested individually in a quiet area of their school. Children were tested across two sessions. Session 1 was identical procedurally to Study 3. Children completed the mood induction – which involved rating their mood before and after watching a video (positive or negative) – and subsequently completed the delay choice task with rewards. After the delay choice test, participants were immediately given any immediate rewards they had chosen, and received any delayed rewards they had chosen the following day[13].

In session 2, children first completed two practice rounds of each of the four chores, to give them an impression of what the tasks involved (in a fixed order: Boxes, Circles,

---

[12] In the neutral condition, emotions were expressed at extremely low levels (0 to 2 expressions total), producing very low variability, and resulting in small discrepancies in coding (e.g. a difference for one expression) being amplified.
[13] Approximately half of children (52%) did not receive their delayed sweet rewards before completing session 2. This could conceivably affect trust in the experimenter. However, there was no effect of having received rewards on the proportion of delayed choices in the delay choice task with chores (t (48) = 1.52, p = .136).

Grids, Dials). Children were then reminded how to use the mood rating scales, and rated their baseline mood. They then completed the mood induction, by watching the set of videos they had not seen during the first session. After watching the videos, children rated their mood. Children then completed the delay choice task with chores, in which they made 12 choices between a small immediate negative outcome, or a larger delayed negative outcome.

After making their choices in the delay choice task with chores, children were asked to rate on a 9-point scale how much they liked the stimuli from the experiment. These data were collected as a manipulation check to determine whether or not children disliked the chores, as would be expected if the tasks were perceived as a negative outcome. Finally, children completed the chores they had chosen to complete immediately, rather than after a delay.

4.3.2 Results

4.3.2.1 Effect of mood on intertemporal choices between positive outcomes

The proportion of delayed choices by mood induction and reward size is shown in Figure 9. To determine whether the mood induction had an effect on intertemporal choice in the delay choice task with rewards, proportion of delayed choices was subjected to a 3 x 3 ANOVA with mood induction (positive, negative, neutral) as a between-subjects factor and reward size (2, 3, or 4) as a within-subjects factor. There was no main effect of mood induction (F (2, 47) = 2.27, p = .114, η^2 = .09). There was also no mood induction by reward size interaction (p = .497). There was a main effect of reward size (F (2, 94) = 3.82, p = .025). This was such that children delayed more often when the delayed reward was 4 sweets (M = 0.66, SD = 0.35), compared to 2 sweets (M = 0.56, SD = 0.36; t (49) = 2.30, p = .026) or 3 sweets (M = 0.58, SD = 0.34;

t (49) = 2.18, *p* = .034). The difference between 2 and 3 sweets was not significant (*p* = .569). Therefore there was no evidence for an effect of mood induction on intertemporal choice.

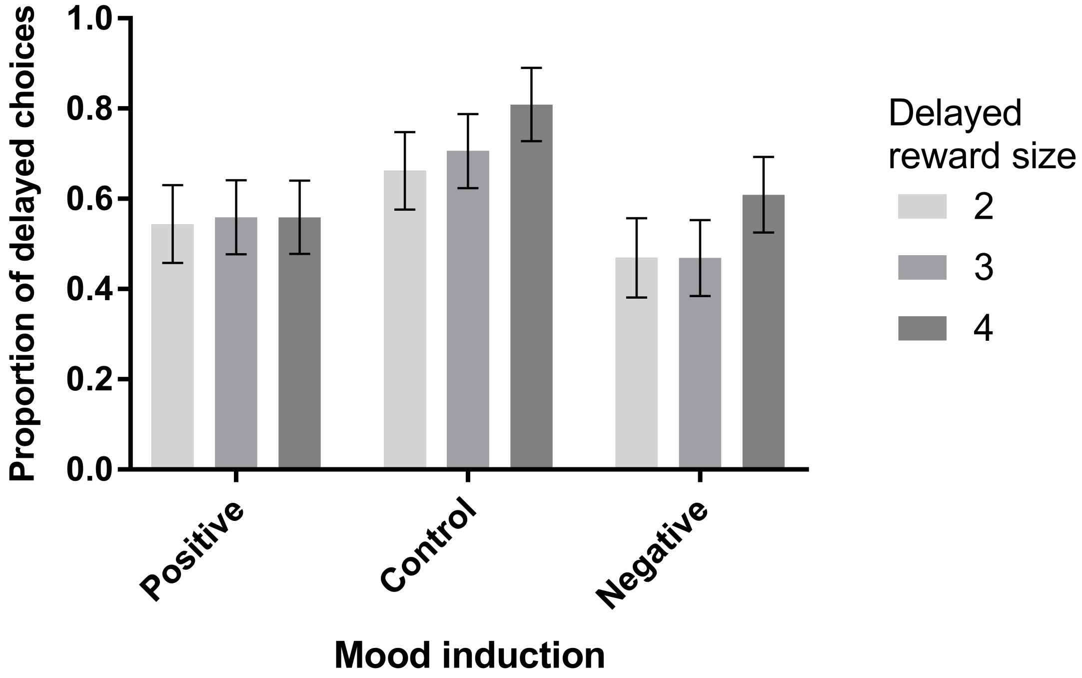

Figure 9. Proportion of delayed choices as a function of mood induction and reward size.

In case the lack of effect of mood on intertemporal choice was driven by the mood induction being ineffective for some children, the effect of mood on proportion of delayed choices was analysed including only participants whose mood change ratings were in line with their mood induction (i.e. children in the positive mood induction who reported feeling better (*N* = 12), children in the neutral mood induction who reported feeling the same (*N* = 10), and children in the negative mood induction who reported feeling worse, (*N* = 4)). The 3 (mood induction) by 3 (reward size) ANOVA on proportion of delayed choices showed the same pattern of results, including the lack of main effect of mood induction (*p* = .099) or mood induction by reward size interaction (*p* = .843).

Further, in case the impact of the mood induction wore off over time, an additional 3 (mood induction) by 3 (reward size) ANOVA on proportion of delayed choices was conducted including only the first half of trials. This produced the same pattern of results, in that there was no main effect of mood induction (*p* = .218), nor a mood induction by reward size interaction (*p* = .487). In this analysis, the main effect of reward size was not significant (*p* = .165).

In order to test whether naturally existing variability in mood was related to intertemporal choice, the relationship between baseline mood rating and proportion of delayed choices was examined. There was no correlation between baseline mood and delayed choices (*r* = .03, *p* = .883).

4.3.2.2 Effect of mood on intertemporal choices between negative outcomes

4.3.2.2.1 Validity of the delay choice task with chores

To determine whether the chores were indeed interpreted as negative outcomes, two analyses were conducted comparing enjoyment and performance in the delay choice tasks with positive outcomes and negative outcomes. Taken together, these analyses suggest that, counter to expectation, children perceived the chores as mildly enjoyable, and when making choices treated them similarly to rewards. Therefore they cannot be treated as negative outcomes.

Firstly, children's ratings of liking for the chores were analysed and compared to the delay choice task with rewards. If children experienced the chores as a negative outcome rather than as a reward, then they should (i) dislike the chores, and (ii) prefer the delay choice task with rewards to the delay choice task with chores. Children's enjoyment rating of the chores averaged across the four tasks had a median of 6.75 out of 9 ($M = 6.58$, $SD = 1.61$). This was above the neutral point of the scale (t (49) = 6.92, $p < .001$). Enjoyment of the chores was significantly lower than enjoyment of the delay choice task with rewards (*Median* = 9.00, $M = 8.56$, $SD = 0.86$; $Z = 5.75$, $p < .001$). This suggests that the delay choice task with chores was regarded as less enjoyable than the delay choice task with rewards, but still as being slightly positive. Children's positive evaluation of the activities could be because the chores, though uninteresting, are still preferable to the alternative for the child at that moment (i.e. sitting in a classroom).

Secondly, if children interpret the chores as a negative outcome, the relationship between proportion of delayed choices on reward and chore versions of the task should be negative. This is because better self-control is indexed by opposite behaviour in the two versions of the task. Better self-control would lead to more delayed choices in the delay choice task with rewards, but fewer delayed choices in the delay choice task with chores, creating a negative correlation. In fact there was a significant *positive* correlation between delaying on the delay choice tasks with rewards and chores ($r = .29$, $p = .042$). Thus children who delayed more tended to do so in *both* the rewards and chores versions of the task. This is consistent with the idea that children treated the chores similarly to rewards, and not as negative outcomes.

4.3.2.2.2 Effect of mood on choices in the delay choice task with chores

Based on the analyses presented above, it cannot be assumed that the delay choice task with chores successfully measured children's choices between negative outcomes. The chores were in fact experienced as mildly positive by children. Thus, while the data from the delay choice task with chores cannot provide evidence for the *a priori* question of whether mood affects preference for negative outcomes, it could provide support for the lack of effect of mood on intertemporal choices between positive outcomes. The effect of mood induction on choices was analysed using a 3 x 3 ANOVA. There was no main effect of mood induction (F (1, 47) = 0.35, p =.704) nor a mood induction by reward size interaction (F (4, 94) = 0.45, p = .775). These data provide tentative support for the lack of effect of mood on intertemporal choices between positive outcomes.

4.3.2.3 Mood induction efficacy

One possible explanation for the lack of effect of mood induction on intertemporal choice is that the mood manipulations were not effective. To determine the effectiveness of the mood inductions, self-reported mood data were analysed for each session separately. Mood change ratings were compared across mood inductions using two chi-squared tests, one for each session (see Table 6 for percentages of children rating their mood as worse, the same, or better).

Table 6. Self-report ratings of mood change as a function of mood induction and session in Study 4.

Mood induction	Positive		Neutral		Negative	
	Session 1	**Session 2**	**Session 1**	**Session 2**	**Session 1**	**Session 2**
Worse (%)	5.9	0.0	29.4	35.3	25.0	31.3
Same (%)	23.5	23.5	58.8	52.9	31.3	37.5
Better (%)	70.6	76.5	11.8	11.8	43.8	31.3

For both sessions, there was a significant association between mood induction and mood rating (session 1: χ^2 (2, N = 50) = 12.7, p = .013; session 2: χ^2 (2, N = 50) = 17.0, p = .002). Examination of residuals for both sessions revealed that children in the positive mood induction were more likely to rate their mood as better, rather than the same or worse. Children in the neutral mood induction were more likely to rate their mood as the same, rather than better. However, children in the negative mood induction were equally likely to rate their mood as worse, the same, or better. This suggests that the mood induction was effective for positive and neutral conditions, but not the negative condition.

As a second measure of mood, observations of facial expressions were analysed. The descriptive statistics are shown in Table 7. The duration of positive expressions, negative expressions, and disengagement were compared between positive and negative mood inductions, for each session separately. Observational data showed positive skew that could not be addressed with transformation. Thus these data were analysed using nonparametric tests (Mann Whitney U tests).

For positive expressions, there was no effect of mood induction in either session 1 (U = 38.0, Z = 1.01, p = .310) or session 2 (U = 102, Z = 0.71, p = .476). For negative expressions, there was also no effect of mood induction in either session 1 (U = 43.0, Z = 0.66, p = .512) or session 2 (U = 120, Z = 0.00, p = 1.00). For disengagement, there was an effect of mood induction for both session 1 (U = 13.0, Z = 2.82, p = .005) and session 2 (U = 27.00, Z = 3.68, p < .001). There was more disengagement in the negative mood inductions (session 1: *Median* = 15.62; session 2: *Median* = 20.83) than the positive mood inductions (session 1: *Median* = 0.72; session 2: *Median* = 2.38). Thus the data suggest that the duration of positive and negative facial expressions did not differ between positive and negative mood inductions, but that there was more disengagement in the negative mood induction.

Table 7. Observational ratings of positive and negative mood expressions and disengagement by mood induction and session in Study 4.

Mood induction	**Positive**		**Negative**	
	Session 1	**Session 2**	**Session 1**	**Session 2**
Positive expressions	4.76 (4.48)	2.69 (3.19)	3.29 (3.72)	1.67 (2.08)
Negative expressions	0.89 (0.75)	0.80 (0.83)	0.87 (1.08)	0.78 (0.92)
Disengagement	3.73 (4.85)	4.57 (6.07)	15.55 (11.84)	25.72 (18.49)

4.3.2.4 Comparison of mood induction efficacy in Study 3 and Study 4

Given the variability in the effectiveness of mood inductions used in previous chapters, it is informative to examine whether the novel method of increasing the duration of videos affected the efficacy of mood induction. To compare the efficacy of shorter versus longer video mood inductions, mood change ratings from Study 3 and Study 4 were compared. Recall that shorter videos were used in Study 3, whereas longer videos were used in Study 4. Four chi-squared tests were computed to examine the association between video length and mood change ratings. For the positive videos, there was no association between study and mood change rating in either video set 1 ($\chi^2(2, N = 80) = 1.02$, $p = .600$) or video set 2 ($\chi^2(2, N = 80) = 4.70$, $p = .095$). Therefore, the longer positive mood inductions in Study 4 were similarly effective as the shorter positive video induction in Study 3. For the negative videos, there was an association between study and mood change rating for both video set 1 (χ^2 (2, N = 75) = 28.6, p < .001) and video set 2 (χ^2 (2, N = 75) = 21.9, p < .001). Examination of the residuals suggested that fewer children in the negative condition rated their mood as negative in the video sets for Study 3 than Study 4. In fact, more children rated their mood as positive in the video sets for Study 3 than Study 4. Contrary to expectation, this suggests that the longer negative mood inductions in Study 4 were *less* effective than the shorter induction in Study 3.

4.3.3 Discussion

Study 4 aimed to strengthen the evidence as to whether or not there was an effect of mood on making intertemporal choices. The results showed no effect of mood on children's intertemporal choices when choosing between positive outcomes. The results replicate the finding from Study 3 of a lack of effect of positive compared to negative mood on children's intertemporal choices with positive outcomes.

Thus this study increases confidence in the finding of a lack of effect of mood on making intertemporal choices. The study not only replicated Study 3, but also aimed to rule out an alternative interpretation of the findings. To rule out the interpretation that self-report measures of mood overestimated the efficacy of the mood induction, additional observational measures of mood were used. Taken together, the self-report and observations of disengagement provided some suggestion that the mood inductions were effective. Children in the positive mood induction self-reported feeling better, and children in the neutral mood induction self-reported feeling the same after completing the mood induction. Children in the negative mood induction showed higher levels of observed disengagement than children in the positive mood induction, suggesting that children were bored (Eastwood et al., 2012).

To reduce the likelihood that the effects of the mood induction were not durable enough to affect behaviour, a longer video mood induction was implemented. This change did not affect mood in the predicted way, therefore unfortunately this alternative explanation could not be ruled out. However, there is no reason to believe that the mood induction used in the present study is any less durable than similar methods used in other studies.

The data suggest that there is not a robust effect of mood on making intertemporal choices between rewards. Previous studies found an effect of negative mood which shifted intertemporal choices toward more immediate rewards (Moore et al, 1976; Seeman & Schwarz, 1974; Schwarz & Pollack, 1977) but no effect of positive mood (Moore et al., 1976; Shimoni et al., 2015). The present data suggest that neither negative nor positive mood affect the making of intertemporal choices. This was despite a difference in mood states between positive and neutral mood inductions. Therefore, the most parsimonious conclusion is that the effect of mood on making intertemporal choices is not robust. For instance, the effect may be highly sensitive to context or to individual differences in children's behaviour.

Study 4 also aimed to investigate how mood affects intertemporal choices between negative outcomes. However, unexpectedly, the pattern of children's responses suggested that the uninteresting activities designed to be perceived as negative outcomes were found to be mildly positive by children. If this task is reinterpreted as a measure of preference for rewards (albeit less desirable rewards than in the delay choice task with rewards), then the lack of effect of mood on choices in this task supports the view that mood has no effect of intertemporal choice between positive outcomes. However, the data cannot address the question of whether mood affects intertemporal choices between negative outcomes.

The data also provide insights into methodological issues in mood measurement and mood induction. Somewhat surprisingly, the duration of observed positive and negative expressions did not differ between mood inductions. There was also a generally low rate of expressions, particularly negative ones. There are two possible reasons why observations of positive and negative facial expressions were not a useful measure of mood in the present study. Firstly, it could be that children did not express their mood facially, perhaps because of the non-social nature of the mood induction, which reduced the need to express emotions outwardly. Secondly, it could be that children *did* express the induced mood states facially, but that either these expressions were not adequately coded, or the analyses were not adequately powered. This is possible because the mapping of *specific* emotional expressions on to general mood states may not be very clear, and the sample sizes for those comparisons (N=8 to 16 per condition) was relatively small. Overall, the data suggest that observations of facial expressions may have limited utility in measuring children's mood state in some contexts.

The present findings indicate that there is not a simple relationship between increased exposure to an emotional stimulus and the intensity of the mood induced. Surprisingly, increasing the duration of the mood induction videos did not augment the impact of the mood inductions. Increasing the length of positive videos had no effect on mood ratings. Furthermore, increasing the length of negative videos counterintuitively made children feel more positive. This finding is striking because intuition would suggest that increasing exposure to a boring stimulus (especially by repeating the same boring stimulus twice) might make children feel more *negative*. One speculative explanation could be a mere-exposure effect (Zajonc, 2001), in which familiarity with the video may have increased children's liking of it. Overall, these data suggest that increasing exposure to an emotional stimulus does not necessarily lead to a more effective mood induction. Thus, apparent "common-sense" assumptions about techniques to enhance mood inductions need to be tested in order to make optimal methodological decisions.

4.4 General discussion

The results of Study 3 and Study 4 converge to suggest that there is no effect of mood on making intertemporal choices in childhood. These studies were adequately powered, used mood inductions which demonstrably affected children's mood (according to self-report and levels of disengagement), and employed a measure of preferences with increased ecological validity. Thus they provide compelling evidence that there is no effect of mood on making intertemporal choices.

The lack of effect of mood on making intertemporal choices found in the two studies presented implies that effects of negative mood identified in other studies are not replicable. The three previous studies investigating this question found that negative mood led to more choices for immediate rewards in preschoolers (Moore et al. 1976), and in 7- to 11-year-olds (Schwarz & Pollack, 1977; Seeman & Schwarz, 1974). The discrepancy between studies suggests that there is not a robust effect of mood on intertemporal choice.

The discrepancy between present and past findings could possibly arise because of specific differences between paradigms. For instance, it could be that the effect of mood is specific to certain emotions. Previous studies induced an emotion like sadness (through autobiographical recall of sad memories or failure at a task), whilst in the present studies, the emotion induced was more like boredom. Because sadness is a more acute negative emotion than boredom, sadness might create a greater need for self-soothing, and thus a desire to receive the immediate reward, explaining why an effect was found in studies that induced sadness but not boredom. Alternatively, it could be that the effect of mood on preference has changed over time, such that mood had a bearing on children's preferences in the past but no longer does.

However, a more plausible explanation for the discrepancy is that there may be a file-drawer effect for findings in this area. If there are a number of unpublished negative or non-confirmatory findings, this would suggest that the positive results reported in the published literature were in fact spurious, and thus a failure to replicate reflects a genuine lack of effect of mood on preferences.

The lack of effect of mood on intertemporal choice implies that incidental emotional states do not have a large or consistent bearing on children's intertemporal choice. This could be because mood state information is not fed into the decision process to appraise the value of a present reward, as it is theorised to with adults (e.g. Aspinwall, 1998). It could also be that integral emotions (those that arise as a response to the task, such as joy from anticipating rewards) override children's prior mood state, and therefore that incidental mood state is not maintained for long enough to impact their preferences. Either way, lack of effect of mood on preferences implies that incidental emotions are unlikely to have a significant impact on children's expression of self-control in real world settings.

In conclusion, these results suggest that negative and positive mood states do not impact children's weighting of intertemporal choices. Combined with previous published studies, the data demonstrate that effects of mood on intertemporal choice identified in past research are not robust across different populations or experimental paradigms.

Chapter 5: General discussion

The objective of this research was to better understand the effect of mood on self-control in childhood. To achieve this, four experiments were run using mood inductions to manipulate mood state, and measuring the effects of these mood inductions on self-control tasks. Previous research in this area has been hampered by (i) a lack of replicability of some findings; (ii) use of poorly validated paradigms for inducing and measuring mood; and (iii) use of measures of cognitive constructs with low validity. The studies presented in this thesis aimed to address these issues by attempting to replicate findings from the literature using better controlled methodologies, for instance controlling for baseline differences in mood and using mood inductions that do not have extraneous effects on other psychological states. The findings suggest that when more carefully controlled methodologies are used, (i) mood inductions are not consistently effective, and (ii) when mood inductions *are* effective, effects of mood on self-control are not replicated. Overall the findings suggest that mood does not consistently affect self-control in children, and that the methodology of mood induction has serious limitations when used with children that need to be considered in future research.

This chapter begins with a summary of the findings of the four experiments presented in this thesis, and a discussion of the conclusions that can be drawn from each experiment. It then brings these conclusions together to draw broader implications for our understanding of both the effects of mood on self-control, and the methodology of mood induction in children. By drawing on consistent null findings across studies, clearer conclusions can be made about the current state of research on mood and children's self-control than would be possible from a single null effect. The chapter continues by presenting the limitations of the work, and suggestions for potentially fruitful avenues for future research in this area.

5.1 Overview of individual study findings and conclusions

5.1.1 Chapter 2: Effects of mood on executive function

The study in Chapter 2 aimed to determine the effect of mood on executive function. A number of different theoretical perspectives offer similar predictions about the effect of mood on executive function, specifically that negative mood should impair executive function. Several mechanisms were proposed, for example that down-regulating of negative emotions reduces the resources available for self-control (Baumeister & Vohs, 2007). The very limited number of existing studies generated mixed results, with most finding that negative mood impaired executive function (Pnevmatikos & Trikkaliotis, 2013; Fartoukh et al., 2014; Lapan & Boseovski, 2017), and others finding null effects of negative mood (Hawes et al, 2009; Lapan & Boseovski, 2017) and positive mood (Pnevmatikos & Trikkaliotis, 2013; Fartoukh et al., 2014). Interpretation of this mixed pattern of results was hampered by weaknesses in methodology, including the use of poorly validated mood inductions, and a lack of control for possible confounding factors. For instance, it was unclear whether the significant results reported were driven by mood valence, or by task motivation, or by baseline differences in cognitive performance between groups. The study presented in Chapter 2 attempted to use more rigorous methodology to address these problems, specifically by controlling for baseline mood, cognitive performance, emotional arousal, and task motivation.

In the experiment in Chapter 2, 7- to 8-year-olds performed an executive function task (the SwIFT) before and after a mood induction. Children were assigned to one of three mood induction conditions (positive, neutral, or negative) in which they watched a 3-minute video with happy, neutral, or sad content, respectively. During the experiment, children self-reported their mood valence, arousal, and motivation. As expected, children's self-reported mood was more negative in the negative condition than the positive or neutral conditions, though there was no difference between positive and neutral conditions. There was no effect of condition on any measure of executive function. This was the case even when only children for whom the mood induction was effective were included. Thus, while the mood inductions appeared to be effective overall, there was no evidence for an effect of mood state on children's executive function.

The findings from the study in Chapter 2 suggested that there is no effect of mood on children's executive function. This finding is consistent with some previous studies which found no effect of mood on executive function (e.g. Hawes et al., 2012). However, the null result is at odds with other empirical studies which found that negative mood impaired executive function (e.g. Pnevmatikos and Trikkaliotis, 2013). Because the experiment in Chapter 2 instantiated greater experimental control – by controlling for motivation, arousal, baseline mood valence, and baseline cognitive performance – we can be reasonably confident in this null finding. The failure to replicate an effect of negative mood on executive function suggests that the impact of mood on executive function is not consistent. This could be because previous findings were driven by the confounding factors controlled for in the study in Chapter 2 (e.g. arousal, motivation, or individual differences in ability). It could also be that the effect of mood on executive function is mediated by individual differences, such as propensity to ruminate, or emotion regulation capacity.

As well as clarifying the effect of mood on self-control, the results from Chapter 2 provide data on the efficacy of video mood inductions. Firstly, they suggested that emotional videos are not consistently effective at inducing changes in self-reported mood. The negative induction produced a negative change in mood ratings, as expected, but the positive induction did not produce a change in children's mood, and did not diverge from the neutral condition. This is contrary to existing literature (e.g. von Leupoldt et al., 2007; Schnall et al., 2008). The lack of impact of positive mood condition on children's self-reported mood could either be due to failure of the mood induction, or to the insensitivity of the mood measure, and the data were unable to distinguish between these possibilities. Secondly, the results suggested that the effects of mood inductions can be specific to mood valence, without affecting arousal or motivation. Arousal and motivation ratings did not differ between conditions post-induction, suggesting that mood valence was specifically affected by the mood inductions without differentially affecting emotional arousal or task motivation. This is important because if we want to examine the effect of mood valence alone on self-control, other confounding psychological states need to be relatively unaffected, or at least statistically controlled for. Thirdly, the data suggested that video mood inductions may be brief in their impact. Immediately after completing the SwIFT, mood ratings in the negative condition had returned to baseline, suggesting that the impact of the mood induction was no longer apparent after a 6-minute cognitive task.

5.1.2 Chapter 3: Effects of mood on enacting intertemporal choice

Chapter 3 presented an attempt to replicate a previous study investigating the effect of positive mood on enacting intertemporal choice in a delay of gratification task. The data from Chapter 2 had suggested that previous findings, including the efficacy of established mood induction procedures, may not replicate reliably. That is, when studies with the same research question but different techniques for inducing mood or measuring self-control are compared, they produce divergent results. Therefore a direct replication of a previous study was performed in Chapter 3 to ensure that any differences in results could not be attributable to differences in experimental setup. Chapter 3 examined intertemporal choice as a component of self-control. The study chosen for replication was Yates et al.'s (1981) study 2, which found that 6-year-olds, but not 5-year-olds, were better able to enact intertemporal choice in a positive mood condition compared to a neutral condition. This study was chosen for replication because it was the most consistent finding of an effect of mood on self-control using the same paradigm, having been reported in just two studies by Yates et al. (1981).

In the experiment in Chapter 3, 4- to- 6-year-olds first completed a mood induction, followed by a delay of gratification task. Children were assigned to either a positive condition, in which they recalled three things that made them happy, or a neutral condition, in which they recalled the names of their classmates. In the delay of gratification task, the amount of time children were willing to wait to obtain larger confectionary rewards was measured. Children also reported their thoughts during the delay period, and these were categorised as on-task, off-task or neither. The results showed no effect of mood induction on children's waiting time, and no interaction between age and mood induction. These results therefore failed to replicate the effect reported by Yates et al. (1981, study 2). The data suggest that a positive mood induction does *not* consistently affect children's enactment of intertemporal choice.

The implications that can be drawn from this failure to replicate were limited, because the results could be interpreted in one of two ways. Firstly, the failure to replicate could be due to a lack of veracity of the original effect of mood on intertemporal choice identified by Yates et al. (1981). It could be that the original effect was spurious, or because the effect is highly sensitive to small differences in the testing context or sample. Secondly, the failure to replicate could be because the mood induction was effective in the original study but not in the present study. Unfortunately, the evidence was ambiguous as to whether the mood induction was effective in terms of inducing mood states, in either the present study or Yates et al.'s (1981) study. There was no measure of mood in Yates et al.'s (1981) study, so there is not any direct evidence for or against its efficacy. In Chapter 3, the self-report measure of mood was limited, because it had to be administered at the end of the experiment and may have been insensitive to differences in mood. Thus it is possible that the mood induction was effective in Yates et al.'s (1981) study, but not in Chapter 3, although the evidence is ambiguous on this point.

Regardless of the precise interpretation, the failure to replicate Yates et al.'s (1981) finding calls into question the consistency of previous results. It emphasizes the importance of attempting replications of previous findings because apparent consensus based on a small number of studies may be potentially misleading. Furthermore, it suggests that replications within this field are necessary to enable both verification of the effectiveness of mood induction techniques, and the reliability of effects of mood on self-control.

5.1.3 Chapter 4: Effects of mood on making intertemporal choices

Following from the failure to replicate in Chapter 3, and the ambiguity over whether the mood induction used was effective, there was a need to identify a more effective mood induction to examine how mood affects intertemporal choice. Chapter 4 investigated the effect of mood valence on a different component of intertemporal choice, specifically *making* intertemporal choices.

A number of theoretical accounts suggested that negative mood leads people to make more choices for immediate rewards, for example because negative mood might induce a desire to boost mood through seeking immediate gratification (Rosenhan et al., 1974). The small number of empirical findings suggested that negative mood indeed increases the likelihood that people will choose immediate rewards, compared to positive or neutral moods (Moore et al. 1976; Schwarz & Pollack, 1977; Seeman & Schwarz, 1974), although this apparent consensus was questionable because of the failure to replicate in Chapter 3. Furthermore, some studies found no effect of positive mood compared to neutral mood on making intertemporal choices (Moore et al. 1976; Shimoni et al., 2016) – although it was possible that these null results were due to positive and neutral mood conditions failing to induce divergent mood states. The studies in Chapter 4 aimed to determine whether the effect of negative mood on making intertemporal choices was replicable, and to rule out the interpretation that the null effect of positive mood was due to failure of the mood induction.

In the first study in Chapter 4, 7- to 10-year-olds received a mood induction before performing a delay choice task. Children were assigned to one of two conditions in which they watched a video which was selected to be either amusing (positive mood condition), or boring (negative mood condition). In the delay choice task, children made a series of choices between smaller immediate rewards, and larger delayed rewards. Unlike in a number of previous studies, rewards were real and present during the decision-making period, increasing the realism of the choice. Children self-reported the change in their mood from before to after the mood induction. The results showed that there was no difference between positive and negative conditions in the proportion of delayed choices children made. This was despite the mood manipulation being successful according to children's self-report. This suggests that mood does not affect children's intertemporal choice.

The second study in Chapter 4 was conducted to replicate and extend the first study. It attempted to rule out an alternative interpretation of the lack of effect of mood on intertemporal choice – specifically that the null result was attributable to a failure of the mood induction. To rule out this possibility, longer videos and observations of facial expressions were used. In addition, it investigated intertemporal choices between negative outcomes by including an additional delay choice task with negative outcomes, instead of rewards. As in the first study in Chapter 4, there was no difference between mood conditions in the proportion of delayed choices. Overall these data suggest that there is no effect of mood on the intertemporal choices children make.

The data suggest that there is not an effect of negative or positive mood on making intertemporal choices. This challenges theoretical accounts which suggest that negative mood reduces delay preferences, for example because negative mood instigates a desire to boost mood through seeking immediate gratification (Rosenhan et al., 1974).

The data from Chapter 4 suggested that reporting changes in mood directly, instead of inferring them from repeated self-reports with the same scale, was effective. In Chapter 4, change in mood was measured directly by asking children whether they felt better, worse, or the same compared to prior to the mood induction. This contrasts with Chapter 2, in which change in mood was measured by asking children to rate their mood before and after the induction using the same Likert face scale. Children demonstrated understanding of the mood change scale, as they were able to follow the instructions and gave appropriate responses to the stimuli shown. Furthermore, this measure removed the main issue with sensitivity with Likert scales used in Chapter 2, because children were able to report improvements in mood even if they rated their mood at the maximum at baseline. To illustrate this point, funny, animated clips similar to those used in Chapter 2 led to increases in mood when measured using a change scale (Chapter 4) but not an absolute scale (Chapter 2).

Chapter 4 also has implications for the selection of content of video mood inductions. Firstly, it suggests that video mood inductions can be used to induce boredom. The mood rating data showed that negative mood can be induced through the use of boring content of short durations, and not just by sad content. This contributes a novel technique to the literature, which previously used sad contents to induce negative moods. Secondly, the data showed that increasing the duration of a stimulus does not necessarily intensify the mood induced. In Chapter 4, doubling the length of the negative video by repeating it made children have more *positive* reactions to it. Doubling the positive video by including more clips had no effect. This suggests that when designing methodologies to investigate children's mood, apparently plausible intuitions about children's likely emotional response to stimuli can be incorrect and need to be tested systematically. These observations are consistent with the idea that the effect of emotional stimuli on behaviour is not straightforward, nor necessarily easy to predict.

5.2 Contributions to our understanding of the effect of mood on self-control

The findings presented in this thesis contribute to our understanding of the effects of mood on self-control. Taken together, the data suggest that contrary to some previous findings there is not a consistent effect of changes in mood valence on children's self-control, in terms of executive function or intertemporal choice. The implications of these findings for our understanding of the existing empirical literature, theoretical accounts of the effects of mood on cognition, and for the real world impacts of mood on children's cognition will be considered here.

All four experiments challenge the general message from the empirical literature which suggests that mood reliably affects self-control in children. The published literature demonstrating an effect of mood on self-control remains relatively small, and some published studies identified a lack of any effect. However, it remains unclear as to why the effect of mood on self-control has been inconsistent across studies in the previous literature and the data reported in this thesis. There are several possible explanations for the discrepant findings. One possibility is that the efficacy of mood induction is highly variable. Only in studies where the mood induction works will effects of mood on self-control be identified, and thus the sporadic pattern of effects and null results could be explained in this way. A second possibility is that the effects of mood on children's self-control might be dependent on individual differences, such as in capacity to regulate emotions, or on whether children have goals to self-regulate (e.g. Perry, Perry, & English, 1985). A third possibility could be that effects of mood on self-control are highly specific to certain emotional states or stages in the process of self-control. For example, perhaps only an acute emotion like sadness affects delay of gratification; or perhaps negative mood only affects the valuation of rewards, but this does not transfer to behavioural choices. Existing data does not allow us to distinguish between these possible explanations.

A final possibility which should be considered is that the effects identified in the literature are false positives. The research presented here consistently suggests a lack of relationship between mood and self-control. This was true whether mood was induced or correlated from self-report measures. Because there is a publication bias against null results, it is plausible that the studies reported in the existing literature provide a false consensus about the effects of mood on children's self-control. Given the long time period over which research in this area has been conducted (since the early 1970s), there has been a notable dearth of published studies. One possible explanation for this could be that these few published results might reflect a few significant findings amongst more unpublished null results. Further, the small sample sizes used in some previous studies increase the risk that effect sizes are inflated. This could explain why the four, well-powered studies presented in this thesis – including one direct attempted replication – have failed to find any effect of mood on self-control. If the existing studies are false positives, this would suggest that there is not any effect of induced mood on children's self-control.

The lack of effect of mood on self-control reported here also has implications for theoretical frameworks that have been developed with adults, but applied to children. For instance, the strength model of self-control suggests that self-control is a limited resource, and regulating negative emotions taxes self-control (Baumeister & Vohs, 2007), and therefore that experiencing negative emotions should lead to poorer self-control. This theory was applied to explain the effect of mood on executive function by some researchers (e.g. Pnevmatikos and Trikkaliotis, 2013; Hawes et al., 2012). The data presented in this thesis offer no support for this theory, suggesting that it does not apply in childhood. This could be because children at this age do not regulate their negative emotions in the same way as adults might, or that emotion regulation, executive function and delay of gratification do not tap into a unified self-control resource at this developmental stage.

Another theory drawn from the adult literature that is challenged by the current findings is the capacity theory (Seibert & Ellis, 1991). This theory suggests that both positive and negative emotions generate emotion-related intrusive thoughts, which consume attentional resources, and prevent effective performance of attention-demanding tasks such as executive function tasks. Fartoukh et al. (2014) provided this theory as a possible explanation for the effect of mood on working memory. The present data offer no support for capacity theory, from which one might conclude that this theory does not hold for children. Capacity theory may not apply to children because intrusive thoughts are not consistently generated when experiencing mild emotional states, or because such thoughts are not given priority over other attention-demanding tasks. Thus it is important that frameworks to predict and explain children's behaviour are developed that are based on empirical research with children.

The data presented in this thesis also challenge explanations that have been developed *ad hoc* to explain results of experiments with children, particularly the self-therapeutic motive. This theory proposes that negative mood impairs delay of gratification because children want to regulate their mood, which leads them to seek immediate gratification (Rosenhan et al., 1974; Moore et al., 1976; Schwarz & Pollack, 1977; Seeman & Schwarz, 1974). The present data is not consistent with this theory. It may be that some of the assumptions underpinning this theory are premature or incorrect; for example, it is assumed that children believe that selecting immediate rewards has greater potential to boost current mood than selecting delayed rewards (Schwarz & Pollack, 1977). This may not always be the case, for example if children experience excitement in awaiting the greater rewards. The assumption that a goal to regulate negative mood is consistently activated in children may also be incorrect, as capacity to deploy emotion regulation strategies continues to develop during childhood (e.g. Gullone & Taffe, 2012).

To the extent that data here provide an analogue for real-life situations, they suggest that mood may simply not impact children's self-control in real life in a consistent way. For instance, if a child experienced an event which gave rise to strong emotions, such as celebrating their birthday or breaking their arm, their capacity to switch between tasks, or to resist the temptation to watch television instead of doing their homework, may in fact be relatively unaffected. It is possible that mood and self-control do not intersect in a reliable way at all in real life for children. It should be noted that laboratory-based mood inductions are likely limited in the extent to which they mimic real life emotional experiences, and so strong conclusions about such real life behaviour cannot be drawn.

Overall, the data presented in this thesis challenge the suggestion that there is a consistent effect of mood valence on children's self-control. No such effect was found across four studies. As such, general theoretical accounts for the phenomenon are likely to be of limited utility. There are a number of possible explanations for the lack of consistency of effects, but based on their small sample sizes, the lack of obvious explanations for the discrepancies in results, and the strong possibility of a file-drawer effect, it appears that previous results may have been false positives.

5.3 Contributions to our understanding of mood induction methodology

While investigating the effectiveness of mood induction techniques was not the primary aim of this thesis, the research has generated some new understanding about mood inductions in children. Firstly, it raises the question of whether children's mood can in fact be experimentally induced, an assumption that has not been questioned by previous researchers. Secondly, assuming that experimental mood induction is possible, the research provides suggestions to improve the reliability of mood inductions.

5.3.1 Can children's mood be experimentally manipulated?

The lack of ability to reliably induce mood in this thesis, and the lack of effect of mood inductions on subsequent behaviour, raise the possibility that children's mood may not be amenable to this kind of experimental research. The mood inductions used were not always effective according to children's self-report, and there could be several reasons for this. For instance, children's mood may be very unstable, due to being highly sensitive to small changes in the environment; or children's subjective awareness of their mood might only exist at the extremes. Regardless of the explanation, this implies that inducing mood, and verifying the success of mood inductions, might be challenging at best.

Furthermore, the possibility that mood inductions may not be effective with children at all should be considered. While the mood inductions used in this thesis meet the standards of the existing literature, there is an unstated assumption in the published literature that if a stimulus used as a mood induction produces changes in children's immediate self-reported mood, then it must be effective. In reality this may not be the case, for instance, because children's responses to self-report scales may reflect demand characteristics, or because children may regulate their mood in a way that means changes in mood are very short-lived. Furthermore, the ethical and practical limitations of experimental work limit the intensity of moods that can be induced. So if the effects of mood on cognition only exist for very powerful mood states (such as those arising through serious injury or bereavement), mood induction methodology may be ineffective to answer questions about the impact of mood on cognition. A conclusive resolution to the question of whether children's mood can be experimentally manipulated is beyond the scope of this thesis, but it is important to be aware of the tacit assumptions of the field when evaluating the evidence.

5.3.2 Suggestions for mood induction methodology

If we assume that experimental mood induction is possible with children, then the present research offers suggestions for how to make existing mood inductions more reliable. Firstly, the results provide suggestions for the selection of stimuli for mood induction. Secondly, the results indicate limits on the duration of laboratory mood inductions. Thirdly, the results suggest ways to measure changes in children's mood state more effectively.

The data suggest that children's responses to mood induction stimuli are not straightforward. The data show that videos *can* be an effective method for inducing mood, such that sad or uninteresting videos can induce a more negative mood, while happy or amusing videos can induce a more positive mood. But the effectiveness of specific stimuli is not reliable across studies. For example, scenes of nature selected to be emotionally neutral based on previous literature were interpreted as relatively positive in the study in Chapter 2. Furthermore, the impact of stimuli is difficult to predict beforehand. In the second study in Chapter 4, it was predicted that repeating identical boring content would induce a more negative mood, but this was not in fact effective. Thus stimuli need to be thoroughly piloted with the same population and testing conditions as the key experimental study, as it is not always possible to anticipate whether stimuli will induce mood effectively in different contexts.

The data also suggest that the impact of mood inductions on children's mood is short-lived. In the study conducted in Chapter 2, children's mood in the negative condition returned to baseline at the end of the experiment, suggesting that the effect of the mood induction lasted for the duration of the cognitive task (around 6 minutes) maximum. It is unclear at what rate children's mood dissipated during the cognitive task, and it is quite possible that children's mood dissipated immediately upon beginning the task.

Relatedly, the research presented here provides suggestions for how to more effectively measure changes in mood in children. In middle childhood, children are able to understand and use *relative* mood measures – measures which require them to compare their present mood to a previous mood. Such a measure of *change* in mood appears to be less liable to a general positivity bias or lack of sensitivity than measuring mood using an absolute scale, and is therefore likely to be more accurate at detecting changes in mood state.

Self-generated imagery inductions may have practical limitations as a mood induction, as suggested by the data reported in Chapter 3. It seems that as well as (or instead of) affecting mood valence, self-generated imagery generates distracting thoughts. This may be a separate mechanism by which such inductions affect children's behaviour, independent of mood. Therefore, if an induction that affects mood valence without generating distracting cognition is required, then self-generated imagery may be undesirable.

5.4 Limitations

The research presented suffers from some limitations, which reflect the challenges of the field more so than limitations of the specific studies reported in this thesis. Perhaps the most significant limitation is the apparent lack of robustness of paradigms used to induce mood. The techniques used in this thesis to induce mood are those with the most empirical support, and yet they were inconsistently effective. The existing inductions appear not to be reliable across populations of the same age group, or to be robust against subtle changes to the content or testing context. This has created problems in interpretation of results, particularly null findings. This could suggest that future research should develop and validate more effective mood inductions and measures of mood state for use with children. For instance, mood inductions that involve analogues of real life emotional events, such as winning or losing a game, could be explored further. Conversely, it may not be possible to develop such paradigms, if the factors affecting children's mood are simply too heterogeneous.

Another limitation of the field is that the reliability and validity of measures of mood for children remain unclear. Some self-report measures of mood had poor sensitivity. This impairs the ability to draw firm conclusions from some studies. For example, in Chapter 3 it was difficult to ascertain whether (a) the intended mood state was induced successfully, but the self-report measure used was inadequate, and there was genuinely no effect of mood on delay of gratification, or (b) the intended mood state was not induced, and therefore a strong conclusion cannot be drawn about the null effect of mood on delay of gratification. As such, ascertaining whether mood has been successfully induced is challenging.

5.5 Future research

Future research in this field may be hampered by challenges with measuring and manipulating mood in children. Previous research makes an assumption that examining the effect of induced mood state on self-control in children is tractable. However, the data in this thesis are somewhat contradictory to this. Firstly, there may not be any stimuli that can reliably induce the same mood state across a group of children. Secondly, there may not be a measure of mood state that accurately gauges children's emotional experience. Thus the possibility that mood valence is too subtle a construct to study experimentally with this age group should be considered. In which case, alternative methods for investigating the role of emotions in children's cognition might be more effective.

By exploring alternative methods to operationalise children's mood, and by focusing on specific aspects of children's cognition, the questions of whether and why mood affects cognition in children might be made more tractable. An alternative way to examine the impact of mood could be to use a quasi-experimental approach. This would involve making use of emotional states arising from real life events that are not in the control of the individual as an independent variable. Such an approach has been used effectively with adults, for instance capitalising on emotions from the outcome of team sports matches (e.g. Lagner et al., 2014). While reducing experimental control, this approach would be both more ecologically valid and would have the potential to invoke more impactful emotions than laboratory-based techniques.

Future research could also aim to break down self-control into its constituent stages to examine the interaction between emotions and these subcomponents. Self-control encompasses a process with separable stages – setting a goal, monitoring the goal, and adjusting behaviour accordingly (Carver & Scheier, 2012). Typically, measures of children's self-control conflate these stages, and in doing so these measures may fail to detect distinct effects of emotion at different stages. For instance, positive mood might encourage higher expectations for goal setting (Horn & Arbuckle, 1988), but not more accurate monitoring (Wegener, Petty, & Smith, 1995). Indeed, some evidence suggests that the impact of mood on self-indulgence in children is mediated by whether or not they have a goal to control themselves (Perry et al., 1985). Thus, examining the impact of mood on separate stages of self-control by developing new tasks would prevent conflation of these stages.

Finally, future research should seek to examine the effects of mood on children's cognition in other domains. A very small literature has examined the effect of mood induction on other facets of children's cognition, such as memory (Davis & Levine, 2013), attention (Schnall et al., 2008), and maths (Scrimin et al. 2014). Replication of these findings and exploration of the mechanisms for them would enable a greater understanding of the wider picture of how emotion and cognition interact in childhood.

5.6 Conclusion

In summary, the work presented in this thesis contributes to our understanding of the interactions between mood and cognition in childhood in two key ways. Firstly, it demonstrates that in childhood there is not a consistent effect of incidental mood states on self-control, across two domains (specifically executive function and intertemporal choice). The repeated failure to replicate findings from the previous literature of effects of mood on children's self-control suggests that the effects identified in past studies may be false positives, or at the very least that they may significantly exaggerate the size, consistency and generalizability of the effects of mood on self-control in childhood. It also suggests that theories for how and why mood affects self-control in children cannot account for present data, and need to be adapted. Secondly, the studies presented here provide impetus for reassessing the methods used to examine impacts of emotion on cognition in childhood. Suggestions for improvements to mood induction methodology include conducting thorough piloting of mood induction methods, measuring changes in mood directly, and using repeated cognitive testing to control for baseline variability in cognitive capacities. Due to the possibility that experimental manipulations of mood may not be feasible with children, alternative methods for examining the relation between emotion and self-control should also be explored further. These findings represent an important step forward in exploring the impact of mood on self-control in childhood.

Appendix A

Details of two pilot studies of the effectiveness of video mood inductions

Pilot study 1

The aim of this pilot was to establish whether the videos selected for mood induction were effective at inducing mood. In this study, 49 participants aged 7- to 8-years-old were recruited from two primary schools in Sheffield. Children were assigned to receive one of three mood inductions (positive, neutral, or negative). Briefly, the procedure was that children completed one session of the SwIFT, then the mood induction, followed by completing another session of the SwIFT. Children rated their mood valence before and after the mood induction using a 9-point face scale, with anchors from very unhappy to very happy. Videos for mood induction were all taken from *The Lion King*. The negative video depicted the death of Mufasa, the main character, Simba's, father (duration 2'24"); the neutral video was a scene of Mufasa explaining the circle of life to his son (1'06"); and the positive video involved characters singing '*I Just Can't Wait To Be King'*, a positive song with exciting and funny scenes of animals (2'15").

The key finding was that there was no difference between positive and neutral conditions in mood valence after watching the videos (U = 114, Z = 0.23, p = .822). Nor did the mood rating of children in the positive condition increase after watching the video (Z = 0.33, p = .743). In the negative condition, children's mood rating worsened after watching the video (Z = 3.13, p = .002), and was significantly worse than the neutral (U = 21.5, Z = 3.82, p < .001) and positive (U = 33.5, Z = 3.73, p < .001) conditions. Thus the positive video used in this pilot was not effective at boosting children's mood.

Pilot study 2

The aim of this pilot was to select a more effective video to induce positive mood in future experiments. Ten participants aged 7- to 8-years-old were recruited from one primary school in Sheffield. In this pilot, participants completed one session of the SwIFT, and then viewed each of three positive clips (order was counterbalanced between participants). Children rated their mood valence before watching any videos and after each video using a 9-point face scale, with anchors from very unhappy to very happy. They were also asked which of the three videos made them feel happiest. The clips used were: (1) a scene from *The Lion King* in which characters sing '*Hakuna Matata*', a song about living a worry-free life (duration 2'57"); (2) a scene from *The Jungle Book* in which characters sing '*Bear Necessities'*, also a song about living a worry-free life (duration 1'55"); and (3) a scene from *Up* in which a boy meets and plays with a funny bird (2'52").

The key finding was that all three videos led to increases in children's mood ratings from baseline. Wilcoxon signed ranks tests showed that the median change in mood from baseline were significantly above zero for two of the videos ('*Hakuna Mutata': Median* = 1.00, p = .026; '*Bare Necessities': Median* = 1.00, p = .038) and marginally above for the third video (*Up*: *Median* = 1.00, p =.068). Children's reports of which video made them happiest were split between the three videos (N = 4 for '*Hakuna Mutata'*; N = 4 for *Up*; and N = 2 for '*Bare Necessities'*). These data suggested that all of the videos could effectively induce positive mood. '*Hakuna Mutata'* appeared to have both a significant effect on mood ratings and popularity amongst the children, so this video was selected for Study 1.

Appendix B

Instructions for positive self-generated imagery mood induction used in Study 2

Taken from Moore et al. (1976):

"I am trying to find out what makes children happy. I'd like you to tell me about things you like to do most of all."

"I want you to tell me three things that make you really happy." [Wait for child's response.]

"That's right _____ makes you happy and what else?" [Wait for child's response.]

"And ______ makes you happy, too. Tell me one more thing that makes you really happy." [Wait for child's response.]

"Now you have told me that ____ makes you happy and _____ makes you happy and _____ makes you happy. Which one makes you the happiest?" [Wait for child's response.]

"Why does that make you happy?" [Wait for child's response.]

"So now think about that. Think about _____ and how happy it makes you."

Appendix C

Instructions for delay of gratification task used in Study 2

Taken from Yates et al. (1981):

"You're going to see three light-bulbs that are all off, and one at a time they will turn on. When you see one light on the screen, that means you can have one prize. This isn't actually the game yet, I'm just showing you how the lights work before your proper go. Now when the second light turns on, that means you can have two prizes. That's better than one isn't it? And if you wait for a bit you'll see three lights and that means you can have three prizes. But if you cannot wait you can ring the bell. Here, give the bell a good ring. That's right! But if you ring the bell the game is finished and you get one prize for each light that is on."

Appendix D

Details of video stimuli used for mood induction in Study 4

Condition	Set 1				Set 2			
	Clip	**Description**	**Length**	**Source**	**Clip**	**Description**	**Length**	**Source**
Positive	Meet Kevin	A boy meets and plays with a funny bird.	2’52	*Up*, Disney Pixar (2010)	Operation Becky	Two fish ask sea lions for help. They recruit a funny bird to assist them.	3’48	*Finding Dory*, Disney Pixar (2016)
	Meet Dug	A boy meets a talking dog.	2’37	*Up*, Disney Pixar (2010)	Meet Heihei	A girl discovers a funny cockerel as a stowaway on her boat.	1’27	*Moana*, Disney (2017)
	Happy song	A character starts his day while a song about being happy plays.	1’27	*Despicable me 2*, Universal (2013)	Everything is Awesome	A Lego character starts his day while a song about everything being awesome plays.	1’30	*The Lego Movie*, Warner Bros (2014)
		Total with transitions	7’07			**Total with transitions**	7’07	
Negative	Home-work	A woman sits in front of a laptop computer writing notes on paper.	3’33	Youtube	Cards	A full pack of cards shuffled and dealt, face down.	3’33	Youtube
		Total with repeat	7’06			**Total with repeat**	7’06	
Neutral	Directio-ns	A man asks for directions to his friend’s house.	1’00	Youtube	Phone call	A woman talks about her daily routine over the phone.	1’00	Youtube

References

Appelhans, B. M., & Luecken, L. J. (2006). Heart rate variability as an index of regulated emotional responding. *Review of General Psychology*, *10*(3), 229–240. http://doi.org/10.1037/1089-2680.10.3.229

Aspinwall, L. (1998). Rethinking the role of positive affect in self-regulation. *Motivation and Emotion*, *22*(1), 1–32. https://doi.org/10.1023/A:1023080224401

Barnett, M. A., King, L. M., & Howard, J. A. (1979). Inducing affect about self or other: Effects on generosity in children. *Developmental Psychology*, *15*(2), 164–167. http://doi.org/10.1037/0012-1649.15.2.164

Baumeister, R. F., & Vohs, K. D. (2007). Self-regulation, ego depletion, and motivation. *Social and Personality Psychology Compass*, *1*(1), 115–128. http://doi.org/10.1111/j.1751-9004.2007.00001.x

Beedie, C., Terry, P., & Lane, A. (2005). Distinctions between emotion and mood. *Cognition and Emotion*, *19*(6), 847–878. http://doi.org/10.1080/02699930541000057

Bertrams, A., Englert, C., Dickhäuser, O., & Baumeister, R. F. (2013). Role of self-control strength in the relation between anxiety and cognitive performance. *Emotion*, *13*(4), 668–80. http://doi.org/10.1037/a0031921

Blair, C., & Razza, R. P. (2007). Relating effortful control, executive function, and false belief understanding to emerging math and literacy ability in kindergarten. *Child Development*, *78*(2), 647–63. http://doi.org/10.1111/j.1467-8624.2007.01019.x

Blakey, E., Visser, I., & Carroll, D. J. (2015). Different executive functions support different kinds of cognitive flexibility: Evidence from two-year-olds, three-year-olds and four-year-olds. *Child Development*, *87*(2), 513–526. http://doi.org/10.1111/cdev.12468

Blau, R., & Klein, P. S. (2010). Elicited emotions and cognitive functioning in preschool children. *Early Child Development and Care*, *180*(8), 1041–1052. http://doi.org/10.1080/03004430802674316

Bluell, A. M., & Montgomery, D. E. (2014). The influence of stimulus discriminability on young children's interference control in the stroop-like happy–sad task. *Journal of Cognition and Development*, *15*(2), 437–452. http://doi.org/10.1080/15248372.2013.767261

Brenner, E. (2000). Mood induction in children: Methodological issues and clinical implications. *Review of General Psychology*, *4*(3), 264–283. http://dx.doi.org/10.1037/1089-2680.4.3.264

Bryan, T., & Bryan, J. (1991). Positive mood and math performance. *Journal of Learning Disabilities*, *24*(8), 490–494. http://doi.org/10.1177/002221949102400808

Bugental, D. B., Lin, E. K., & Susskind, J. M. (1995). Influences of affect on cognitive processes at different ages: Why the change? In N. Eisenberg (Ed.), *Social Development* (pp. 159–184). Thousand Oaks: Sage.

Bull, R., & Lee, K. (2014). Executive functioning and mathematics achievement. *Child Development Perspectives*, *8*(1), 36–41. http://doi.org/10.1111/cdep.12059

Button, K. S., Ioannidis, J. P. A., Mokrysz, C., Nosek, B. A., Flint, J., Robinson, E. S.

J., & Munafò, M. R. (2013). Power failure: Why small sample size undermines the reliability of neuroscience. *Nature Reviews Neuroscience*, *14*(5), 365–376. http://doi.org/10.1038/nrn3475

Carlson, C. R., Felleman, E. S., & Masters, J. C. (1983). Influence of children's emotional states on the recognition of emotion in peers and social motives to change another's emotional state. *Motivation and Emotion*, *7*(1), 61–79. http://doi.org/10.1007/BF00992965

Carroll, D. J., & Cragg, L. (2012, April). *Precursors to cognitive flexibility: Rule-switching in two-year-olds*. Paper presented at the Development of Executive Functions Workshop, Utrecht, Netherlands.

Carver, C. S., & Scheier, M. F. (2012). Cybernetic control processes and the self-regulation of behavior. In *The Oxford Handbook of Human Motivation* (pp. 28–42). Oxford: Oxford University Press. http://doi.org/10.1093/oxfordhb/9780195399820.013.0003

Chambers, C. T., & Johnston, C. (2002). Developmental differences in children's use of rating scales. *Journal of Pediatric Psychology*, *27*(1), 27–36. http://doi.org/10.1093/jpepsy/27.1.27

Cicchetti, D. V. (1994). Guidelines, criteria, and rules of thumb for evaluating normed and standardized assessment instruments in psychology. *Psychological Assessment*, *6*(4), 284–290. https://doi.org/10.1037%2F1040-3590.6.4.284

Crawford, J. R., & Henry, J. D. (2004). The positive and negative affect schedule (PANAS): Construct validity, measurement properties and normative data in a large non-clinical sample. *The British Journal of Clinical Psychology*, *43*(3), 245–265. http://doi.org/10.1348/0144665031752934

Daschmann, E. C., Goetz, T., & Stupnisky, R. H. (2011). Testing the predictors of boredom at school: Development and validation of the precursors to boredom scales. *The British Journal of Educational Psychology*, *81*(3), 421–440. http://doi.org/10.1348/000709910X526038

Davis, E. L., & Levine, L. J. (2013). Emotion regulation strategies that promote learning: reappraisal enhances children's memory for educational information. *Child Development*, *84*(1), 361–374. http://doi.org/10.1111/j.1467-8624.2012.01836.x

Department for Communities and Local Government (2015). *Index of multiple deprivation* [Data file]. Available from https://www.gov.uk/government/statistics/english-indices-of-deprivation-2015

Diener, E., & Diener, C. (1996). Most people are happy. *Psychological Science*, *7*(3), 181–185. http://dx.doi.org/10.1111/j.1467-9280.1996.tb00354.x

Droit-Volet, S., & Meck, W. H. (2007). How emotions colour our perception of time. *Trends in Cognitive Sciences*, *11*(12), 504–513. http://doi.org/10.1016/j.tics.2007.09.008

Duckworth, A. L., & Kern, M. L. (2011). A meta-analysis of the convergent validity of self-control measures. *Journal of Research in Personality*, *45*(3), 259–268. http://doi.org/10.1016/j.jrp.2011.02.004

Dunn, J. R., & Schweitzer, M. E. (2005). Feeling and believing: The influence of emotion on trust. *Journal of Personality and Social Psychology*, *88*(5), 736–748. http://doi.org/10.1037/0022-3514.88.5.736

Eastwood, J. D., Frischen, A., Fenske, M. J., & Smilek, D. (2012). The unengaged mind: Defining boredom in terms of attention. *Perspectives on Psychological Science*, *7*(5), 482–495. http://doi.org/10.1177/1745691612456044

Ekman, P. (1970). Universal facial expressions of emotion. *California Mental Health Research Digest*, *8*(4), 151–158. Retrieved from https://www.paulekman.com/wp-content/uploads/2013/07/Universal-Facial-Expressions-of-Emotions1.pdf

Ekman, P., Freisen, W., & Ancoli, S. (1980). Facial signs of emotional experience. *Journal of Personality and Social Psychology*, *39*(6), 1125–1134. http://doi.org/http://dx.doi.org/10.1037/h0077722

Ekman, P., & Friesen, W. V. (1978). *The Facial Action Coding System (FACS): A technique for the measurement of facial action*. Palo Alto: Consulting Psychologists Press, Inc.

Espy, K. A. (1997). The shape school: Assessing executive function in preschool children. *Developmental Neuropsychology*, *13*(4), 495–499. http://doi.org/10.1080/87565649709540690

Fartoukh, M., Chanquoy, L., & Piolat, A. (2014). Mood induction in children: Effect of the affective valence of a text on phonological working memory. *Advances in Cognitive Psychology*, *10*(3), 113–118. http://doi.org/10.5709/acp-0162-z

Feldman, L., & Russell, J. A. (1999). The structure of current affect: Controversies and emerging consensus. *Current Directions in Psychological Science*, *8*(1), 10–14.

http://doi.org/10.1111/1467-8721.00003

Forgas, J., & Locke, J. (2005). Affective influences on causal inferences: The effects of mood on attributions for positive and negative interpersonal episodes. *Cognition and Emotion*, *19*(7), 1071–1081. http://doi.org/10.1080/02699930541000093

Friedman, N. P., & Miyake, A. (2017). Unity and diversity of executive functions: Individual differences as a window on cognitive structure. *Cortex*, *86*, 186–204. http://doi.org/10.1016/j.cortex.2016.04.023

Fry, P. S. (1975). Affect and resistance to temptation. *Developmental Psychology*, *11*(4), 466–472. http://doi.org/10.1037/h0076678

Gasper, K., & Clore, G. L. (2002). Attending to the big picture: Mood and global versus local processing of visual information. *Psychological Science*, *13*, 34–40. http://doi.org/10.1111/1467-9280.00406

Gobbo, C., & Raccanello, D. (2007). How children narrate happy and sad events: Does affective state count? *Applied Cognitive Psychology*, *21*(9), 1173–1190. http://doi.org/10.1002/acp.1324

Green, L., Myerson, J., & Ostaszewski, P. (1999). Discounting of delayed rewards across the life span: Age differences in individual discounting functions. *Behavioural Processes*, *46*(1), 89–96. http://doi.org/10.1016/S0376-6357(99)00021-2

Greene, T. R., & Noice, H. (1988). Influence of positive affect upon creative thinking and problem solving in children. *Psychological Reports*, *63*(3), 895–898. http://doi.org/10.2466/pr0.1988.63.3.895

Gross, J. J., & Levenson, R. (1995). Emotion elicitation using films. *Cognition and Emotion*, *9*(1), 87–108. http://doi.org/10.1080/02699939508408966

Grusec, J. (1968). Waiting for rewards and punishments: Effects of reinforcement value on choice. *Journal of Personality and Social Psychology*, *9*(1), 85–89. http://dx.doi.org/10.1037/h0025699

Gullone, E., & Taffe, J. (2012). The Emotion Regulation Questionnaire for Children and Adolescents (ERQ–CA): A psychometric evaluation. *Psychological Assessment*, *24*(2), 409–417. http://doi.org/10.1037/a0025777

Hawes, D. J., Zadro, L., Fink, E., Richardson, R., O'Moore, K., Griffiths, B., … Williams, K. D. (2012). The effects of peer ostracism on children's cognitive processes. *European Journal of Developmental Psychology*, *9*(5), 599–613. http://doi.org/10.1080/17405629.2011.638815

Hodes, R., Cook, E., & Lang, P. (1985). Individual differences in autonomic response: conditioned association or conditioned fear? *Psychophysiology*. http://doi.org/10.1111/j.1469-8986.1985.tb01649.x

Horn, H. L., & Arbuckle, B. (1988). Mood induction effects upon goal setting and performance in young children. *Motivation and Emotion*, *12*(2), 113–122. http://doi.org/10.1007/BF00992168

Huizinga, M., Dolan, C. V, & van der Molen, M. W. (2006). Age-related change in executive function: Developmental trends and a latent variable analysis. *Neuropsychologia*, *44*(11), 2017–36. http://doi.org/10.1016/j.neuropsychologia.2006.01.010

Isen, A. M. (2001). An influence of positive affect on decision making in complex situations: Theoretical issues with practical implications. *Journal of Consumer Psychology*. http://doi.org/10.1207/S15327663JCP1102_01

Izard, C. E. (1994). Innate and universal facial expressions: Evidence from developmental and cross-cultural research. *Psychological Bulletin*, *115*(2), 288–299. http://doi.org/10.1037/0033-2909.115.2.288

Jakobs, E., Manstead, A. S. R., & Fischer, A. H. (1999). Social motives and emotional feelings as determinants of facial displays: The case of smiling. *Personality and Social Psychology Bulletin*, *25*(4), 424–435. http://doi.org/10.1177/0146167299025004003

Johns, M., & Schmader, T. (2008). Stereotype threat and executive resource depletion: Examining the influence of emotion regulation. *Journal of Experimental Psychology. General*, *137*(4), 691–705. http://doi.org/10.1037/a0013834

Kanfer, R., & Ackerman, P. L. (1989). Motivation and cognitive abilities: An integrative/aptitude-treatment interaction approach to skill acquisition. *Journal of Applied Psychology*, *74*(4), 657–690. http://doi.org/10.1037/0021-9010.74.4.657

Kenrick, D. T., Baumann, D. J., & Cialdini, R. B. (1979). A step in the socialization of altruism as hedonism: Effects of negative mood on children's generosity under public and private conditions. *Journal of Personality and Social Psychology*, *37*(5), 747–755. http://doi.org/10.1037/0022-3514.37.5.747

Lagner, P., Kliegel, M., Phillips, L. H., Ihle, A., Hering, A., Ballhausen, N., & Schnitzspahn, K. M. (2014). Mood effects on memory and executive control in a real-life situation. *Cognition and Emotion*, *29*(6), 1107–1116. http://doi.org/10.1080/02699931.2014.962486

Lapan, C., & Boseovski, J. J. (2017). The effects of guilt on preschoolers' cognitive flexibility and inhibition. *Early Childhood Research Quarterly*, *41*, 95–102. http://doi.org/10.1016/j.ecresq.2017.06.004

Lee, K., Bull, R., & Ho, R. M. H. (2013). Developmental changes in executive functioning. *Child Development*, *84*(6), 1933–1953. http://doi.org/10.1111/cdev.12096

Lee, W. S. C., & Carlson, S. M. (2015). Knowing when to be "rational": Flexible economic decision making and executive function in preschool children. *Child Development*, *86*(5), 1434–1448. http://doi.org/10.1111/cdev.12401

Lerner, M. D., & Lonigan, C. J. (2014). Executive function among preschool children: Unitary versus distinct abilities. *Journal of Psychopathology and Behavioral Assessment*, *36*(4), 626–639. http://doi.org/10.1007/s10862-014-9424-3

Levine, L. J., Burgess, S. L., & Laney, C. (2008). Effects of discrete emotions on young children's suggestibility. *Developmental Psychology*, *44*(3), 681–694. http://doi.org/10.1037/0012-1649.44.3.681

LoBue, V., Baker, L., & Thrasher, C. (2017). Through the eyes of a child: preschoolers' identification of emotional expressions from the child affective facial expression (CAFE) set. *Cognition and Emotion*, 1–9. http://doi.org/10.1080/02699931.2017.1365046

Markey, A., Chin, A., VanEpps, E. M., & Loewenstein, G. (2014). Identifying a reliable boredom induction. *Perceptual & Motor Skills*, *119*(1), 237–253. http://doi.org/10.2466/27.PMS.119c18z6

Martin, M. (1990). On the induction of mood. *Clinical Psychology Review*, *10*(6), 669–697. http://doi.org/10.1016/0272-7358(90)90075-L

Masters, J. C., Barden, R., & Ford, M. (1979). Affective states, expressive behavior, and learning in children. *Journal of Personality and Social Psychology*, *37*(3), 380–390. http://doi.org/10.1037/0022-3514.37.3.380

Masters, J. C., & Santrock, J. W. (1976). Studies in the self-regulation of behavior: Effects of contingent cognitive and affective events. *Developmental Psychology*, *12*, 334–348. http://doi.org/10.1037/0012-1649.12.4.334

Michaelson, L. E., & Munakata, Y. (2016). Trust matters: Seeing how an adult treats another person influences preschoolers' willingness to delay gratification. *Developmental Science*, *19*(6), 1011–1019. http://doi.org/10.1111/desc.12388

Mischel, W., Ebbesen, E. B., & Zeiss, A. R. (1972). Cognitive and attentional mechanisms in delay of gratification. *Journal of Personality and Social Psychology*, *21*(2), 204–218. http://doi.org/10.1037/h0032198

Mischel, W., & Grusec, J. (1967). Waiting for rewards and punishments: Effects of time and probability on choice. *Journal of Personality and Social Psychology*, *5*(1), 24–31. http://doi.org/10.1037/h0025699

Mischel, W., Grusec, J., & Masters, J. C. (1969). Effects of expected delay time on the subjective value of rewards and punishments. *Journal of Personality and Social Psychology*, *11*(4), 363–373. http://doi.org/10.1037/h0027265

Mischel, W., & Metzner, R. (1962). Preference for delayed reward as a function of age, intelligence, and length of delay interval. *The Journal of Abnormal and Social Psychology*, 64(6), 425. http://dx.doi.org/10.1037/h0045046

Mischel, W., Shoda, Y., & Peake, P. K. (1988). The nature of adolescent competencies predicted by preschool delay of gratification. *Journal of Personality and Social Psychology*, *54*(4), 687–696. http://doi.org/10.1037/0022-3514.54.4.687

Mischel, W., & Underwood, B. (1974). Instrumental ideation in delay of gratification. *Child development*, *45*(4), 1083-1088. http://doi.org/ 10.2307/1128098

Miyake, A., Friedman, N. P., Emerson, M. J., Witzki, A. H., Howerter, A., & Wager, T. D. (2000). The unity and diversity of executive functions and their contributions to complex "frontal lobe" tasks: A latent variable analysis. *Cognitive Psychology*, *41*(1), 49–100. http://doi.org/10.1006/cogp.1999.0734

Moore, B. S., Clyburn, A., & Underwood, B. (1976). The role of affect in delay of gratification. *Child Development*, *47*(1), 273–276. http://doi.org/10.2307/1128312

Ng, E., & Lee, K. (2010). Children's task performance under stress and non-stress conditions: A test of the processing efficiency theory. *Cognition and Emotion*, *24*(7), 1229–1238. http://doi.org/10.1080/02699930903172328

O'Connor, E., McCormack, T., Beck, S. R., & Feeney, A. (2015). Regret and adaptive decision making in young children. *Journal of Experimental Child Psychology*, *135*, 86–92. http://doi.org/10.1016/j.jecp.2015.03.003

Pekrun, R. (1992). The impact of emotions on learning and achievement: Towards a theory of cognitive/motivational mediators. *Applied Psychology*, *41*(4), 359–376.

http://doi.org/10.1111/j.1464-0597.1992.tb00712.x

Perry, L. C., Perry, D. G., & English, D. (1985). Happiness: When does it lead to self-indulgence and when does it lead to self-denial? *Journal of Experimental Child Psychology*, *39*(2), 203–211. http://doi.org/10.1016/0022-0965(85)90037-2

Pessoa, L. (2009). How do emotion and motivation direct executive control? *Trends in Cognitive Sciences*, *13*(4), 160–166. http://doi.org/10.1016/j.tics.2009.01.006.How

Pnevmatikos, D., & Trikkaliotis, I. (2013). Intraindividual differences in executive functions during childhood: The role of emotions. *Journal of Experimental Child Psychology*, *115*(2), 245–61. http://doi.org/10.1016/j.jecp.2013.01.010

Poirel, N., Cassotti, M., Beaucousin, V., Pineau, A., & Houdé, O. (2012). Pleasant emotional induction broadens the visual world of young children. *Cognition and Emotion*, *26*(1), 186–91. http://doi.org/10.1080/02699931.2011.589430

Prencipe, A., & Zelazo, P. D. (2005). Development of affective decision making for self and other: Evidence for the integration of first- and third-person perspectives. *Psychological Science*, *16*(7), 501–505. http://doi.org/10.1111/j.0956-7976.2005.01564.x

Psychology Software Tools, Inc. [E-Prime 3.0]. (2012). Retrieved from http://www.pstnet.com

Rader, N., & Hughes, E. (2005). The influence of affective state on the performance of a block design task in 6- and 7-year-old children. *Cognition and Emotion*, *19*(1), 143–150. http://doi.org/10.1080/02699930441000049

Razza, R. A., & Raymond, K. (2013). Associations among maternal behavior, delay of gratification, and school readiness across the early childhood years. *Social Development*, *22*(1), 180–196. http://doi.org/10.1111/j.1467-9507.2012.00665.x

Rebeck, G., & Lohaus, A. (1986). Effect of emotional arousal on free recall of complex material. *Perceptual and Motor Skills*, *63*(2), 461–462. http://doi.org/10.2466/pms.1986.63.2.461

Remington, N. A., Fabrigar, L. R., & Visser, P. S. (2000). Reexamining the circumplex model of affect. *Journal of Personality and Social Psychology*, *79*(2), 286–300. http://doi.org/10.1037//0022-3514.79.2.286

Rice, J. A., Levine, L. J., & Pizarro, D. A. (2007). "Just stop thinking about it": Effects of emotional disengagement on children's memory for educational material. *Emotion*, 7(4), 812-823. http://dx.doi.org/10.1037/1528-3542.7.4.812

Ridgeway, D., & Waters, E. (1987). Induced mood and preschoolers' behavior: Isolating the effects of hedonic tone and degree of arousal. *Journal of Personality and Social Psychology*, *52*(3), 620–625. http://doi.org/http://dx.doi.org/10.1037/0022-3514.52.3.620

Riener, C. R., Stefanucci, J. K., Proffitt, D. R., & Clore, G. L. (2011). An effect of mood on the perception of geographical slant. *Cognition and Emotion*, *25*(1), 174–82. http://doi.org/10.1080/02699931003738026

Rosenhan, D. L., Underwood, B., & Moore, B. (1974). Affect moderates self-gratification and altruism. *Journal of Personality and Social Psychology*, *30*(4), 546–552. http://doi.org/10.1037/h0037038

Rottenberg, J., Ray, R. D., & Gross, J. J. (2007). Emotion elicitation using films. In J.

A. Coan & J. J. B. Allen (Eds.), *The Handbook of Emotion Elicitation and Assessment* (pp. 9–28). London: Oxford university press.

Russell, J. A. (1980). A circumplex model of affect. *Journal of Personality and Social Psychology*, *39*(6), 1161–1178. http://doi.org/10.1037/h0077714

Russell, J. A. (2003). Core affect and the psychological construction of emotion. *Psychological Review*, *110*(1), 145–172. http://doi.org/10.1037/0033-295X.110.1.145

Schlam, T. R., Wilson, N. L., Shoda, Y., Mischel, W., & Ayduk, O. (2013). Preschoolers' delay of gratification predicts their body mass 30 years later. *Journal of Pediatrics*, *162*(1), 90–93. http://doi.org/10.1016/j.jpeds.2012.06.049

Schmeichel, B. J., & Inzlicht, M. (2013). Incidental and integral effects of emotions on self-control. In M. D. Robinson, E. R. Watkins, & E. Harmon-Jones (Eds.), *Handbook of Cognition and Emotion* (pp. 272–290). New York: Guilford Press.

Schnall, S., Jaswal, V. K., & Rowe, C. (2008). A hidden cost of happiness in children. *Developmental Science*, *11*(5), F25–F30. http://doi.org/10.1111/j.1467-7687.2008.00709.x

Schwarz, J. C., & Pollack, P. R. (1977). Affect and delay of gratification. *Journal of Research in Personality*, *11*(2), 147–164. http://doi.org/10.1016/0092-6566(77)90013-7

Schwarz, J. C., Schrager, J. B., & Lyons, A. E. (1983). Delay of gratification by preschoolers: Evidence for the validity of the choice paradigm. *Child Development*, *54*(3), 620–625. http://doi.org/10.2307/1130048

Scrimin, S., Mason, L., & Moscardino, U. (2014). School-related stress and cognitive performance: A mood-induction study. *Contemporary Educational Psychology*, *39*(4), 359–368. http://doi.org/10.1016/j.cedpsych.2014.09.002

Seeman, G., & Schwarz, J. C. (1974). Affective state and preference for immediate versus delayed reward. *Journal of Research in Personality*, *7*(4), 384–394. http://doi.org/10.1016/0092-6566(74)90060-9

Seibert, P. S., & Ellis, H. C. (1991). Irrelevant thoughts, emotional mood states, and cognitive task performance. *Memory and Cognition*, *19*(5), 507–513. http://doi.org/10.3758/BF03199574

Shimoni, E., Asbe, M., Eyal, T., & Berger, A. (2015). Too proud to regulate: The differential effect of pride versus joy on children's ability to delay gratification. *Journal of Experimental Child Psychology*, *141*, 275–282. http://doi.org/10.1016/j.jecp.2015.07.017

Shoda, Y., Mischel, W., & Peake, P. K. (1990). Predicting adolescent cognitive and self-regulatory competencies from preschool delay of gratification: Identifying diagnostic conditions. *Developmental Psychology*, *26*(6), 978–986. http://doi.org/10.1037/0012-1649.26.6.978

Sin Mei Tsui, A., & Atance, C. M. (2017, April). *Is children's perception of their future selves associated with their future thinking, saving, and delay of gratification?* Poster presented at the Society for Research in Child Development Biennial Meeting, Austin, TX.

Stegge, H., Terwogt, M. M., & Koops, W. (1994). Postive and negative mood effects in children: The mediating influence of task characteristics. *The Journal of General Psychology*, *121*(4), 333–344. http://doi.org/10.1080/00221309.1994.9921207

Thompson, C., Barresi, J., & Moore, C. (1997). The development of future-oriented prudence and altruism in preschoolers. *Cognitive Development*, *12*(2), 199–212. http://doi.org/10.1016/S0885-2014(97)90013-7

Tiller, W. A., McCraty, R., & Atkinson, M. (1996). Cardiac coherence: A new non-invasive measure of autonomic nervous system order. *Alternative Therapies in Health and Medicine*, *2*(1), 52–65.

Toner, I. J., Lewis, B. C., & Gribble, C. M. (1979). Evaluative verbalization and delay maintenance behavior in children. *Journal of Experimental Child Psychology*, 28(2), 205-210. https://doi.org/10.1016/0022-0965(79)90084-5

Tornare, E., Cuisinier, F., Czajkowski, N. O., Pons, F., Tornare, E., Cuisinier, F., & Czajkowski, N. O. (2017). Impact of induced joy on literacy in children: does the nature of the task make a difference? *Cognition and Emotion*, *31*(3), 500–510. http://doi.org/10.1080/02699931.2015.1132682

von Leupoldt, A., Rohde, J., Beregova, A., Thorsden-Sorensen, I., zur Nieden, J., & Dahme, B. (2007). Films for eliciting emotional states in children. *Behavior Research Methods*, *39*(3), 606–609. http://doi.org/10.3758/BF03193032

von Stumm, S. (2016). Is day-to-day variability in cognitive function coupled with day-to-day variability in affect? *Intelligence*, *55*, 1–6. http://doi.org/10.1016/j.intell.2015.12.006

Watson, D., Clark, L. A., & Tellegen, A. (1988). Development and validation of brief measures of positive and negative affect: The PANAS scales. *Journal of Personality and Social Psychology*, *54*(6), 1063–1070. http://doi.org/10.1037/0022-3514.54.6.1063

Watson-Jones, R. E., Whitehouse, H., & Legare, C. H. (2016). In-group ostracism increases high-fidelity imitation in early childhood. *Psychological Science*, 27(1), 34-42. https://doi.org/10.1177/0956797615607205

Watts, T. W., Duncan, G. J., & Quan, H. (2018). Revisiting the Marshmallow Test: A Conceptual Replication Investigating Links Between Early Delay of Gratification and Later Outcomes. *Psychological science*. https://doi.org/10.1177/0956797618761661

Wegener, D. T., Petty, R. E., & Smith, S. M. (1995). Positive mood can increase or decrease message scrutiny: The hedonic contingency view of mood and message processing. *Journal of Personality and Social Psychology*, *69*(1), 5–15. http://doi.org/10.1037//0022-3514.69.1.5

Weisberg, D. P., & Beck, S. R. (2012). The development of children's regret and relief. *Cognition & Emotion*, *26*(5), 820–35. http://doi.org/10.1080/02699931.2011.621933

Wiebe, S. A., Espy, K. A., & Charak, D. (2008). Using confirmatory factor analysis to understand executive control in preschool children: I. Latent structure. *Developmental Psychology*, *44*(2), 575–587. http://doi.org/10.1037/0012-1649.44.2.575

Wiebe, S. A., Sheffield, T., Nelson, J. M., Clark, C. A. C., Chevalier, N., & Espy, K. A.

(2011). The structure of executive function in 3-year-olds. *Journal of Experimental Child Psychology*, *108*, 436–452. http://doi.org/10.1016/j.jecp.2010.08.008

Williams, A., O'Driscoll, K., & Moore, C. (2014). The influence of empathic concern on prosocial behavior in children. *Frontiers in Psychology*, *5*. http://doi.org/10.3389/fpsyg.2014.00425

Yates, G. C. R., Lippett, R. M. K., & Yates, S. M. (1981). The effects of age, positive affect induction, and instructions on children's delay of gratification. *Journal of Experimental Child Psychology*, *32*, 169–180. http://doi.org/10.1016/0022-0965(81)90101-6

Yerkes, R. M., & Dodson, J. D. (1908). The relation of strength of stimulus to rapidity of habit-formation. *Journal of Comparative Neurology and Psychology*, *18*(5), 459–482. http://doi.org/10.1002/cne.920180503

Zajonc, R. B. (2001). Mere exposure: A gateway to the subliminal. *Current Directions in Psychological Science*, *10*(6), 224–228. http://doi.org/10.1111/1467-8721.00154

Zelazo, P. D. (2006). The Dimensional Change Card Sort (DCCS): A method of assessing executive function in children. *Nature Protocols*, *1*(1), 297–301. http://doi.org/10.1038/nprot.2006.46