# EYES ON THE SCREEN: EXPLORING THE WATCHING-EYES EFFECT ON ALTRUISTIC BEHAVIOUR IN ONLINE AND FIELD ENVIRONMENTS

by

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**ABSTRACT**

The overall aim of this thesis was to explore the watching-eyes effect on altruistic behaviour. Study one tested whether the presence of eye cues impacted prosocial behaviour in an online environment. Study two built on this by collecting eye-tracking data to determine whether the participants paid attention to the image of eyes whilst completing prosocial tasks. Study three further expanded the range of eye stimuli used in three areas: Gender, Emotion and Salience. No conclusive evidence was found in these studies that the presence of watching-eye images affected online prosocial behaviour, but there was some evidence that the presence of eyes may deter people from anti-social behaviour. The eye-tracking data from study two revealed that increased attentiveness to eyes did not result in an increased prosocial behaviour. However, there were issues with low statistical power identified in each study which limits the ability to make any definitive conclusions about the watching-eyes effect in an online context. In study four A, the first known watching-eyes online field experiment was conducted to test whether the presence of eyes on a 'real-world' website increased charitable donations. However, despite over 33,000 web clicks, not a single donation was received. The final study provided a critical reflection on a reportedly successful watching-eyes field experiment by Keep Britain Tidy (2014). Although the social experiment was highly effective in reducing anti-social behaviour, it was not possible to ascertain that it was due to the presence of eye images. Although the watching-eyes effect may not be the panacea for impacting human behaviour that perhaps early studies suggested, the indicative findings from this thesis and the results from more recent watching eyes studies suggests that in the right context, watching eyes could provide a simple and cost-effective way of having a small (but meaningful) impact on behaviour change.

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## Chapter One – Introduction and Literature Review

Altruism is an evolutionary puzzle. Humans are unusual within the animal kingdom in that they are highly altruistic towards unrelated strangers (Fehr & Fischbacher, 2003). They donate time, money, and even sometimes risk their lives to help others, even when there may be no benefit to themselves (Becker & Eagly, 2004; Goldberg, 1995; Stern, 1995). Reciprocal altruism (where individuals exchange helping acts) (Trivers, 1971) has been the backbone for research on the evolution of altruistic behaviour towards non-kin, but recent research has also begun to apply costly signalling theory (CST) to this problem (Barclay & Willer, 2007). CST posits that certain traits evolve because they convey honest information about the underlying qualities of an individual (Zahavi, 1977; Zahavi, 1995). Research has shown that altruism can signal many positive qualities about a person such as their wealth, generosity, commitment, and cooperative intent, therefore those who show off prosocial behaviour when being watched may be at an advantage (Roberts, 1998). As highly social creatures, humans have relied on group living to survive and thrive (van Vugt & Kameda, 2013), so these qualities, or indeed any behaviour or trait that benefits the group, would help establish a positive reputation, leading to long term benefits for the individual in attracting potential mates or allies (Roberts, 1998; van Vugt & Iredale, 2013).

The evolutionary legacy hypothesis (Burnham & Hare, 2007) posits that humans have evolved to be such highly social creatures that they possess evolved mental mechanisms that cause them to react to cues of being watched as if their reputations were at stake (Saunders, Taylor, & Atkinson, 2016). Research has shown that prosocial behaviours such as generosity, morality, and green behaviours are substantially enhanced even under false cues of observation (e.g. in the presence of an image of eyes), and this is known as the 'watching-eyes effect' (e.g., Bateson et al., 2015; Haley & Fessler, 2005; Sparks & Barclay, 2015).

In their seminal study, Haley and Fessler (2005) demonstrated that the mere display of stylised eyespots (i.e., eye-like images) on a computer screen significantly increased giving in a dictator game. Since this study was published, there has been a plethora of laboratory and applied research that has found evidence of the watching-eyes effect on a wide range of prosocial behaviours, from increasing people's generosity (Fathi, Bateson, & Nettle, 2014) to deterring littering (Bateson et al., 2015).

However, in recent years, there has been a growing body of literature that has found little or no support for this effect (Northover, Pedersen, Cohen, & Andrews, 2016). In other cases, where significant results have been found, they have been conditional on a range of boundary conditions (Northover, Pedersen, Cohen, & Andrews, 2017). This chapter argues that these mixed results suggest that any impact of the watching-eyes effect on prosocial behaviour is, to some extent, dependent on the characteristics of the participant, environment, or the specific eye stimuli used. In principle, the watching-eyes effect requires nothing more than displaying an image of eyes on a poster or computer screen to evoke prosocial behaviour, and yet the answer may be more complicated than that. If researchers can pinpoint the environmental and stimuli characteristics which can evoke the watching-eyes effect, this could potentially provide an inexpensive and straightforward way of encouraging people to act in more prosocial ways in a wide range of contexts (Nettle, Nott, & Bateson, 2012).

Public philanthropy and donating to charity is one of the most common costly signals in humans (McAndrew, 2019). At the time of developing the research proposal for this thesis (circa 2016), the watching-eyes effect had previously been tested on charitable donations in both a laboratory (e.g., Keller & Pfattheicher, 2011) and field experiment context (e.g., Ekström, 2012), and on cooperation in a dictator game in an online environment (e.g., Raihani & Bshary, 2012). However, there had been no studies into the effects of watching eyes specifically towards online charitable donations. With the near-ubiquitous use of the internet in modern day-to-day life across the globe, this was deemed a novel and necessary area in this thesis in which to test the watching-eyes effect. If the watching-eyes effect could be evoked in this context, it could pave the way for a range of low-cost but potentially high-impact online interventions; from increasing charitable donations to deterring anti-social behaviour (Dear, Dutton, & Fox, 2019). Thus, the first main aim of this thesis was to explore altruism as a costly signal in an online environment. Specifically, whether the watching-eyes effect could be evoked in an online context to promote donations to charity.

## Thesis Structure

This thesis is made up of seven chapters: This introduction and literature review chapter, a general methodology chapter, four empirical chapters, and a general discussion and conclusion chapter. This thesis was designed via an iterative process, with each study planned and conducted based on the findings of the previous study to enable a robust but adaptable exploration into the watching-eyes effect.

This introduction and literature review chapter discusses the altruistic problem in more depth and provides an overview of the prominent theories addressing the problem. It goes on to discuss how costly signalling theory (CST) is the leading explanation for altruistic acts towards un-related strangers who cannot reciprocate. The chapter then provides an in-depth literature review of the ’watching-eyes effect’ and the potential impact on altruism.

The general methodology chapter presents an overview of the general methodological approaches used in this thesis, many of which were consistent across the empirical chapters. The chapter provides a summary of these under the headings of participant recruitment, materials, procedures, ethics and data processing and analysis.

In total, this thesis consists of five studies written up into four empirical chapters:

- Study One: Exploring how Watching Eyes Impact Prosocial Behaviour in an Online Environment
- Study Two: A Lab-based, Eye-tracking study on Participant Attention to Watching Eyes
- Study Three: Exploring Eye Gender, Emotion, and Salience
- Study Four A: A Field-based Exploration of the Watching-eyes Effect

- Study Four B: Keeping Britain Tidy; A Closer Look.

Each empirical chapter consists of a focused introduction and literature review for each study, an overview of the methodology taken, the results and a discussion of those results and how they fit in with the wider literature. The thesis concludes with a general discussion and conclusion chapter which draws together the findings from each empirical chapter and the implications of those findings.

## Literature Review

### The Altruistic Puzzle

Altruism is a highly social behaviour where an individual will promote the welfare of others even when at a cost or risk to themselves (Trivers, 1971). It has been the subject of much interest within behavioural sciences as, unusually within the animal kingdom, humans will promote the welfare of unrelated strangers even though there may not be any direct benefit to doing so (Fehr & Fischbacher, 2003).

As the saying by Donne (1624) goes, “No man is an island”. Humans are highly social creatures and the survival and evolution of humans has been founded on the basis of living in large cooperative social groups (Tooby & Cosmides, 1990). Ancestral human groups had much to gain from the capacity to cooperate in large groups. It made hunting more successful as it allowed ancestral humans to take on big game, it reduced the risk of famine through food sharing, it provided an advantage in warfare, and it allowed for cooperative child-rearing which would have raised the reproductive success of the group (Vlerick, 2020).

Silk and House (2016) maintain that human cooperation is facilitated at the individual level by prosocial emotions, such as compassion and guilt, and altruistic social preferences, including a concern for the welfare of others and a preference for equity. Whereas it is regulated at the group level through social norms that establish standards for how people should behave in particular situations and punitive sanctions against those that violate social norms.

Displays of altruism have been shown to increase when observed by other group members. For example, in a public goods game study, Hardy and van Vugt (2006) found that donations to group funds significantly increased when donations were made public rather than private, suggesting there are reputational fitness benefits for showing off altruism. As discussed in more detail in the next section, these reputational benefits may be twofold: One, it reduces the chance of violating social norms and being punished by the group. Two, it heightens chances of future reciprocal help from allies, and access to resources and mates. From an evolutionary perspective, it pays for someone to behave altruistically when being observed. Those ancestors that increased altruism when watched by others would be at a fitness advantage over those who did not.

There is a plethora of research on the factors that elicit altruistic behaviour and the focus of the next section is to provide a brief overview of the prominent proximate (the causal mechanisms underlying a behaviour) and ultimate (the fitness consequences of a behaviour) explanations. It should be noted that understanding proximate and ultimate explanations to a behaviour does not require a choice between the two, on the contrary, they are complementary to each other (Scott-Phillips et al., 2011). To properly understand behaviour, it is important to understand both types of instigators of altruism, as this strengthens the validity of psychological theories of altruism, and can aid in applied settings (Tybur & Griskevicius, 2013).

***Proximate Explanations of Altruism***

For any behaviour to evolve, there must be a proximate mechanism that motivates an organism to carry out the behaviour (Norman, 2020). Proximate explanations for why many people behave altruistically (without expecting anything in return) include feelings of a warm glow, empathic concern, an innate sense of fairness, a tendency to follow prosocial norms, a fear of punishment and strong reciprocity.

The theory of ‘warm glow’ (Andreoni, 1990) posits that people are motivated to behave altruistically because it simply makes them feel good about themselves. Altruistic acts often follow from genuine concern for others and is inherently rewarding, engaging in altruistic behaviour stimulates the feel-good hormones of the brain: Dopamine, oxytocin, and serotonin (Vlerick, 2020). This indicates that this physiological response has evolved within human populations as a mechanism for altruistic acts.

A frequently mentioned possible source of altruistic motivation is empathic concern (Batson, Lishner, & Stocks, 2014). Empathy is the ability to understand and share the feelings of another individual and Batson et al's (1991) empathy-altruism hypothesis posits that feelings of empathy for another person produce an altruistic motivation to increase that person’s welfare. Therefore, an individual who experiences empathy for someone in need may be motivated to relieve the need of said individual, and if they are, their motivation is altruistic if it is evoked by empathy and not motivated by the anticipated self-benefits of helping (Norman, 2020).

There is good evidence that humans have an innate sense of fairness which is another possible proximate mechanism for altruism (Vlerick, 2020). An innate sense of fairness is present in very young children and across all cultures. Research by Warneken, Lohse, Melis, and Tomasello (2011) found that young children tend to share goods equally after having collaborated equally to obtain them – even if they could keep them for themselves. The children understood and defended the entitlement of others and gave less to free riders than collaborators.

People are inclined to follow social norms, separate from any innate sense of fairness or empathy which does not necessarily lead to them feel good about themselves (Vlerick, 2020). Human cooperation is regulated by social norms that establish standards for how people should behave in particular situations and strengthened by punitive sanctions against those that violate social norms, so their compliance may be due to a fear of incurring reputation damage or being punished (Oda & Ichihashi, 2016). They may also follow norms because they accord values to these norms or because they want to fulfil the legitimate expectations of others. Participants often follow the social norms that govern the social interaction in their societies (Vlerick, 2020).

Research has shown that altruistic motivations may be driven by a fear of punishment (Fehr & Gächter, 2002). Early evolutionary models by Boyd and Richerson (1992) showed that cooperation in groups can evolve when individuals punish noncooperators. Subsequent research which has utilised repeated rounds of public goods game has found over time punishment can increase levels of cooperation, with players contributing more money following punishment (Fehr & Gächter, 2002). In essence, the benefits of avoiding punishment by the group outweighs the costs of engaging in altruistic behaviour (Kurzban, Burton-Chellew, & West, 2015). Altruistic punishment is revisited in the next section on ultimate explanations.

Strong reciprocity refers to a predisposition to reward co-operators and punish noncooperators, even in the absence of current or future rewards (West et al., 2011). For example, Fehr and Gächter (2002) found in a public goods game, that when participants were advised of the contribution amounts of other group members and given the opportunity to ‘punish’ a group member, individuals would punish free riders. Following punishment, free riders subsequently increased the amounts they contributed to the public goods game. As the public goods game was not played with the same set of individuals each time, the punishers themselves would not benefit from either reputation or reciprocity, only the members of the future group would benefit from the punishment of free riders. However, in a series of four studies that investigated whether punishers gain social benefits from punishing, Barclay (2006) found that punishers of free riders were seen as more trustworthy, group-focused, and worthy of respect than non-punishers. This suggests that there may be some reputational benefits for strong reciprocity which could explain why the psychological mechanisms that modulate altruism and altruistic punishment evolved (Iredale, 2009)

***Ultimate Explanations of Altruism: The focus of this thesis***

The purpose of this thesis was to explore altruism from an ultimate perspective. Although the above proximate mechanisms offer possible explanations as to what may motivate a person to behave altruistically, they do not provide an answer to why humans evolved altruistic dispositions in the first place and what the adaptive fitness benefits may have been (Kurzban et al., 2015). The leading ultimate explanations of this group selective behaviour include kin selection (Hamilton, 1964), reciprocal altruism (Trivers, 1971), indirect reciprocity (Alexander, 1987), punishment (Fehr & Gächter, 2002), competitive altruism (Roberts, 1998) and costly signalling theory (Zahavi, & Zahavi, 1999). To quote Farrelly, Lazarus, and Roberts (2007):

"Kinship selection can explain rescuing drowning people if they are relatives, reciprocal altruism if they return the favour, indirect reciprocity if a third party returns the favour and signalling if the rescuer is judged more attractive" (p. 314).

**Kin Selection.** The basis of kin selection is that assisting a close relative increases one's inclusive fitness (the ability of an individual organism to pass on its genes to the next generation). Any altruistic actions which are costly to an individual are more likely to be directed toward relatives, as this will help to ensure the continuity of their genetics by helping those with whom they share their genes to survive (Hamilton, 1964; Maynard Smith, 1964). Hamilton (1964) demonstrated that an altruistic gene can spread through the population if it causes an individual to help a relative, whenever the cost to the individual is offset by the reproductive benefit gained by the receiver. However, this explanation does not account for altruistic behaviours towards unrelated strangers, such as; giving blood (Ferguson & Lawrence, 2016), donating to charity (Grace & Griffin, 2009), or risking death to rescue an unrelated individual (Stern, 1995).

**Reciprocal Altruism.** Reciprocal altruism occurs when individuals exchange helping acts (Trivers, 1971). The theory of reciprocal altruism has largely dominated the discussion of non-kin altruism (Roberts, 1998) and can help to explain the motivation behind altruistic acts toward unrelated strangers, as it maintains that altruistic behaviours are performed because they increase the likelihood of repayment in the future (Alexander, 1987). Direct reciprocity occurs where an individual (A) performs a generous act to help another (B) and B subsequently repays A (Bradley et al. 2018). If the benefit received is larger than the cost incurred, then individuals who engage in such behaviour will out-reproduce those who do not. For this behaviour to uphold several conditions must be met: Individuals must associate for long-enough periods of time to develop reciprocal interactions (e.g., members of the same community), the likelihood of social exchange should be predicted based on past associations, the roles of giver and receiver should reverse at least once, the short-term benefits to the recipient are greater than the costs to the donor and finally, givers should be able to recognise and expel cheaters from the system (Trivers, 1971).

A classic framework for studying reciprocal altruism is the prisoner's dilemma game (Killingback & Doebeli, 2002). In their widely cited analysis, Axelrod and Hamilton (1981) first explored reciprocal altruism via the prisoner's dilemma game; a theoretical scenario in which cooperation benefits both players but a 'cheat' can gain a higher pay-off. In real-life scenarios, if social exchanges occurred more than once, then individuals will remember those who have defected (cheated) in the past and adjust their behaviour accordingly. In *Evolution of cooperation*, Axelrod (1984) organised a tournament where computer programs repeatedly played the prisoner's dilemma game and a clear winner of 'tit for tat' (the computer programme) was evident. This is a simple strategy cooperated on the first move and then copied by its opponents on every subsequent move. This demonstrated that although it may be beneficial to cheat in a one-off exchange, if you will likely encounter the person more than once, mutual cooperation will serve both parties best. This scenario is analogous to many human behaviours where the collective benefits more from cooperation (e.g., sharing food).

A limitation of reciprocal altruism is that it does not explain why people help those who cannot reciprocate (such as providing handouts to panhandlers) (Griskevicius, Tybur, & van den Bergh, 2010). To some extent, indirect reciprocity (Alexander, 1987), punishment (Fehr & Gächter, 2002), competitive altruism (Roberts, 1998), and costly signalling theory (Zahavi, & Zahavi, 1999) can help explain this.

***Indirect Reciprocity.*** Indirect reciprocity occurs when individuals help those who help others (Alexander, 1987). Indirect reciprocity comes in two forms: upstream and downstream. In upstream reciprocity, an individual who has received help goes on to help others (A helps B, then B helps C), whereas downstream reciprocity occurs when a person who has helped others in the past has a higher likelihood of being helped by others in the future (A helps B, then C helps A) (Bradley et al, 2018). As long as the altruistic act is observable by others (either directly or via gossip), an altruistic act can help enhance an individual's reputation and increase the likelihood of reciprocity at a later date (Yoeli et al., 2013). If someone is observed refusing to help, this harms their reputation and the chances that others will help them (Barclay, 2011).

Indirect reciprocity has been supported theoretically and empirically (Nowak & Sigmund, 1998). An influential model in demonstrating how indirect reciprocity can operate via reputation information is Nowak and Sigmund's (1998) model of image scoring. This model proposed that individuals who help others acquire a cooperation reputation, or 'image score', whereas this score decreased when individuals declined to help. Observers then use this score when deciding whether to help that individual. However, indirect reciprocity does not readily explain why people help others when the gains from reputation are small or absent (Fehr & Gächter, 2002).

***Punishment.*** Altruistic punishment could offer a possible explanation to this (Fehr & Gächter, 2002). For Direct and Indirect reciprocity to work successfully, there is a requirement that cooperators are rewarded and defectors are sanctioned (Roberts, 2021). As an example, public goods are collectively beneficial, but free riders who cooperate less than fellow group members are better off than people who are more cooperative. However, imposing sanctions on these defectors provides an incentive for free riders to cooperate more (Barclay, 2006). Laboratory and field experiments have shown that people punish noncooperators at a cost to themselves even in one-shot interactions (Boyd & Richerson, 2003). The presence of punishment makes it costly to refuse to help, thus providing a selective pressure for helping (Fehr & Gächter, 2002). Whereas indirect reciprocity can elicit altruistic behaviour from just a few group members, punishment is more efficient in sustaining altruistic behaviour in larger groups as group members only need to punish the few rare non cooperators (as discussed in Barclay, 2011).

This is puzzling because natural selection should work against those who engage in costly punishment and in favour of those who free ride on the cooperative benefits generated by punishers (Fowler, 2005). Although individuals who punish non cooperators can gain collective benefits if punishment increases within-group cooperation, punishing non cooperators also comes at an individual cost to the punisher (Batistoni, Barclay & Raihani, 2022). However, research has shown that punishment third-party punishment could be supported by reputational benefits to punishers (Batistoni et al., 2022) with punishment serving as a costly signal of trustworthiness (Barclay, 2006; Jordan & Rand, 2019) and to some extent, cooperative intent (Raihani & Bshary, 2015).

***Competitive Altruism and Costly Signalling.*** Competitive altruism is the process in which individuals attempt to outcompete each other in terms of generosity due to competition for partners and alliances (Roberts, 1998). The theory suggests that as altruism is a signal of quality, when competing for partners, individuals should compete to display themselves as the most altruistic. As humans are highly social creatures, by demonstrating prosocial behaviours, individuals are enhancing their status and reputation, and gain future benefits in the form of future help or mating success (Hardy & van Vugt, 2006).

Research has shown that people are more generous in the presence of observers (Hardy & van Vugt, 2006), are more generous when they are competing over partnerships (Barclay & Willer, 2007), and engaging in competitive altruism makes people more desirable as partners (Sylwester & Roberts, 2010). The evolutionary root of this desire for relative status can be explained by Costly Signalling Theory (CST) which posits that natural selection has favoured people who engaged in costly activities (i.e., involving significant resources, energy, risk, or time) as a way to signal their ability to incur costs (Griskevicius et al., 2010). The associated status then benefits the signaller as it increases the likelihood that the altruist may be chosen as a coalition partner or potential mate (Hardy & van Vugt, 2006).

As McAndrew (2019) maintains, altruistic signals are only useful to the recipient of those signals if the signal is communicating an honest and reliable signal. For example, a female who responds to a dishonest signal about the quality of a mate may end up with low-quality offspring and a non-supportive partner, which could negatively impact her long-term reproductive success. Consequently, there has been significant selective pressure to develop strategies for detecting honest signals of quality in others. In such a system, there must be a cost to the sender if a signal is discovered not to be honest (i.e., cheaters will be punished), and there will be a cost to receivers if dishonest signals are not detected.

For signals to be honest, the benefits of signalling must outweigh the cost for honest signallers, but not for dishonest signallers (Barclay et al. 2021). For example, a low-quality signaller who attempts to fake a high-quality signal will deplete whatever resources that they may have available, leaving the signaller in such a vulnerable position that the strategy will prove to be counterproductive. Conversely, a high-quality signaller who has many resources, can easily afford a high-quality signal; so the adaptive benefits will outweigh the costs. Hence, costly signalling theory proposes that individuals often engage in behaviours that are costly as a way of signalling honest information about themselves (McAndrews, 2019).

**What is Costly Signalling Theory?**

CST was first developed in the field of animal behaviour (Zahavi & Zahavi, 1999; Zahavi, 1975) and has subsequently garnered empirical support in anthropological and laboratory studies of human behaviour (van Vugt & Hardy, 2009). CST suggests that individuals often engage in potentially costly behaviours (e.g., behaviours that involve significant amounts of resources, energy, time or even risk to their own physical wellbeing) as a way of signalling to others useful information about themselves. According to CST, such signals are ultimately adaptive because they increase an individual's probability of attracting a mate or ally which increases their reproductive or survival fitness (Griskevicius, Tybur, & Sundie, 2007).

Costly signalling theory (CST) proposes that costly, behavioural, or morphological signals convey honest information about an individual's fitness quality because they are hard-to-fake. It pays for potential mates to pay attention to costly cues because they are more likely to be honest rather than false expressions of fitness quality. The high production cost of these signals makes them expensive to fake. A classic example is that of the peacock's tail. As a long, seemingly impractical appendage, the tail would have a high metabolic cost and may be detrimental to survival as the peacock would find it hard to escape from predators. However, despite the obvious handicap, the tail acts as a signal for an underlying genetic quality that is important to peahens and therefore increases the reproductive success of the peacock (Darwin, 1871; Petrie, 1994). This too may be applied to altruism, as a costly behaviour, it is seen to indicate an underlying quality which aids in attracting allies and in reproductive success (Iredale, van Vugt, Dunbar, & Miller, 2009).

### *Types of Costly Signalling*

Animals (including humans) may choose mates and allies based on signals of genetic and non-genetic quality thus individuals may employ high-cost signals to communicate their ability to supply ‘good genes’ and resources. Evidence for costly signalling has been found in many areas of human behaviour including philanthropy, conspicuous consumption, physical risk-taking behaviour, and religious activities.

**Philanthropy and Donations to Charity**. Public philanthropy and donations to charity are one of the most common costly signals of social status in humans. According to CST, public philanthropy is a costly signal that displays two important features about an individual; one that the person has abundant resources, and two, that the person is prosocial (McAndrew, 2019).

The evolutionary roots of philanthropy as a costly signal may be found in the tradition of meat sharing by prehistoric hunters. Successful hunters could demonstrate desirable physical qualities such as health, strength, and eye-hand coordination through activities such as mastery of weapons, hunting, and sharing meat. This in turn would in turn demonstrate cooperative, prosocial tendencies that would have been highly valued (McAndrew, 2019). Anthropological studies provide numerous examples of exaggerated displays of public altruism. For example, Smith and Bliege Bird (2000) described a form of costly signalling among the Meriam, a Melanesian society located on an island off the coast of Australia. The Meriam family members organise a party after a relative’s death, which includes giving food and gifts to all guests. Turtle meat is highly valued as it requires careful coordination of effort and great physical agility, strength, and diving abilities. The ability to supply many turtles for the funeral feast serves as an honest signal of the physical quality of the males in the family.

This is also corroborated by many laboratory studies which show that charitable donations are most likely to take place when the behaviours are easily observed and recognised by others (e.g., Iredale, Jenner, van Vugt, & Dempster, 2020). Van Vugt and Hardy (2009) have even shown that people will make wasteful contributions in public goods situations, even when they know full well that the contribution will not make a difference, as long as the contribution is publicly observed.

**Conspicuous Consumption.** One of the most readily observable forms of costly signalling in western societies (i.e., capitalist societies) is conspicuous consumption (Veblen, 1899); wasteful spending on luxury goods that are not essential for survival (as discussed in McAndrew, 2019). In its various guises, conspicuous consumption with its wasteful advertising and accumulation of symbolic capital (the resources available to an individual on the basis of status) signals wealth (Bliege Bird & Smith, 2005). Someone who participates in this type of behaviour purchases goods for status and to enhance reputation rather than for utility purposes (van Vugt & Hardy, 2010). Examples of conspicuous consumption can include flashy signs of wealth such as driving a sports car (van Vugt, Griskevicius, & Schultz, 2014) and wearing luxury brands (Nelissen & Meijers, 2011), or even purchasing pro-environmental products (Griskevicius et al., 2010). People who buy sports cars (such as Ferraris or Aston Martins), signal to their peers that they have enough money to spend on luxury goods, and this raises their prestige (van Vugt et al., 2014). Financial wealth enhances status as it is a desirable characteristic that implies both the control of valuable resources and the skills to acquire them (Nelissen & Meijers, 2011). In a series of seven studies, Nelissen and Meijers (2011) demonstrated that people treat a person who displays luxury brands more favourably than a person who does not. Those who wore luxury brands were perceived to be more competent, were offered higher wages for a job, achieved better outcomes in social dilemma and dictator games, and were more successful when soliciting charitable donations from others. However, research has shown that status via conspicuous consumption can also be obtained with non-luxury items, such as pro-environmental products, which often cost more and are of a lower quality than their conventional counterparts. In a series of three experiments, Griskevicius et al. (2010) found that activating a motive for status increased people's desire for green products when shopping in public (but not private) and when green products cost more (but not less) than non-green products. Griskevicius et al. (2010) posit that the purchase of green products may enable a person to signal that they are both willing and able to buy a product that benefits others at a cost to their personal use, which would in turn enhance their social status.

**Risk-taking Behaviour.** Brave individuals who are willing to unconditionally risk their own life or physical wellbeing for an unrelated stranger would be expected to be rare in both evolutionary terms (sacrificing self for unrelated others is not the ideal way of ensuring that genes are passed on) and in terms of lifetime survival chances (the more risk taken, the increased likelihood of death and decreased likelihood of successfully passing on genes). However, bravery and risk-taking behaviours are far from uncommon in humans (Kelly & Dunbar, 2001).

There are two striking facts about physical risk-taking: 1) it is done more by males than by females, and 2) among males, it is done more by young adults (about 18 to 21 years old) than by any other age group (Farthing, 2005). CST suggests that some cases of apparently frivolous risk-taking by males may serve the function of signalling the male's health, vigour and overall genetic quality to potential mates (Moore, Wigby & English, 2013).

It has been postulated (e.g., McAndrew, 2019) that in early human societies, competitive success in early adulthood established a man's social status in his community for the rest of his life (as it would be unlikely they could simply leave and join another group) so what happened during the teen years mattered a great deal. For this reason, high-risk competition between young males provided an opportunity for 'showing off' the abilities needed to acquire resources, exhibit strength, and meet any challenges to one's status. Fessler, Leonid, Holbrook, Gervais and Snyder (2014) found in multiple studies that physically risk-prone men are envisioned to be larger, stronger, and more violent than risk-averse men. As McAndrew (2019) maintains, this suggest that heroic, or even recklessly daredevil behaviour was rewarded with status and respect. The widespread promotion of sport in modern times could have developed as a constructive alternative to this high-risk competition. In a legally sanctioned gladiatorial arena, young men can exhibit the same skills – throwing, clubbing, running, wrestling, tackling, hand-eye coordination – that would have made them successful fighters or hunters in the ancestral environment. Participating in team sports enables athletes to exhibit other qualities such as cooperativeness, loyalty, and planning ability – all of which are hard to fake. Thus, risky male behaviour may not just be about advertising genetic quality, but it may also advertise how one might behave as an adversary or an ally (Fessler et al., 2014).

Heroic individuals are praised for successfully taking a physical risk for others (e.g., by hunting or saving lives) and research has shown that heroic physical risk-takers (usually male) are rated more attractive (usually by females) than non-risk-takers (Farthing, 2005). According to mate choice theory, the ability of female mammals (including humans) to reproduce is constrained mainly by the availability of resources such as food and shelter, and it would therefore increase a woman's fitness (by enhancing the survival chances of herself and her offspring) if she chose a mate who could provide her with these resources (Kelly & Dunbar, 2001). Therefore, audiences respond to such acts because they signal information about the signaller's abilities, qualities, or resources that the audience has inferred to be useful (Smith & Bliege Bird, 2000).

**Religion.** Religion can be thought of as a social mechanism for enforcing cooperation and facilitating costly behaviours within cultural groups (McAndrew, 2019). CST posits that the sometimes high costs of religion are repaid through the high levels of cooperation found in many religious communities (Shaver & Bulbulia, 2016) and experiments have demonstrated that induced religious thoughts reduce rates of cheating and increase altruistic behaviour among anonymous strangers (Norenzayan & Shariff, 2008).

As McAndrew (2019) discusses, all religions have rituals, taboos, and other requirements that can be very costly in terms of time, money, or effort. Fasting, tithing, frequent and lengthy prayer and/or religious services, and dietary requirements that are difficult to maintain require a good deal of commitment. Thus, religious commitment can be a hard-to-fake signal of commitment to the group's values and a signal that one is likely to be a reliable, cooperative group member (Sosis & Bressler, 2003). Religious behaviours and rituals, if more costly to cooperating group members than to freeloaders, may have reliably signalled the presence of devotion and, therefore, cooperative intention toward in-group members, in turn buffering religious groups against defection from freeloaders and reinforcing cooperative norms (Norenzayan & Shariff, 2008).

### ***Altruism as a Costly Signal***

To understand altruism as a costly signal, it is important to first define exactly what is meant by altruism. As stated earlier, the purpose of this thesis is to explore altruism from an ultimate perspective, the definition of altruism therefore focuses on adaptive fitness benefits rather than proximate reasoning. Altruism is defined in this thesis as a highly social behaviour where an individual will promote the welfare of others even when at a cost or risk to themselves (Trivers, 1971). Specifically, it refers to a type of 'pure altruism', a behaviour that is directed seemingly unconditionally (i.e., the altruist does not expect anything back in return) towards unrelated strangers (Andreoni, 1990). Altruistic behaviour is different from simple ‘prosocial behaviour’. As discussed by DeLamater, Collett, DeLamater, and Collett (2018), prosocial behaviour occurs when an individual acts in a manner that benefits another person or group of people, whereas altruism is a kind of prosocial motivation where individuals act to promote others’ welfare, even when at risk or cost to themselves. The key difference being that for a behaviour to be classed as altruistic, it needs to involve a risk or cost to the individual. Although prosocial behaviour may be altruistically motivated, this motivation is not a requirement for the behaviour to be considered prosocial.

For altruism to be seen as a costly signal, there are certain qualities it should have; the behaviour must be costly, it must be observable, it must be a reliable indicator of some underlying trait (e.g., resource potential, wealth, health or intelligence, and the behaviour must benefit the actor who displays it (Smith & Bliege Bird, 2000). There are multiple ways in which people can be altruistic including donating money to charity, contributing to a public good (e.g., any collective action where individuals have a strong incentive not to provide the good because the benefits are freely available to everyone once the altruist has incurred the cost), and volunteering their time to risking their lives to save others (e.g., firefighters). All of these types of behaviour are costly in terms of either resource (e.g., money), time or risk to one’s life. Laboratory studies have demonstrated that charitable donations and other acts of altruism are most likely to take place when the behaviours are easily observed and recognized by others (Bereczkei, Birkas, & Kerekes, 2010). Zahavi (1975) argues that costly behaviours can be regarded as reliable honest signals because only those who can afford the cost can afford to display it. Altruism could be a costly signal for humans as it can signal good physical health (e.g., via heroic acts), wealth (e.g., via public philanthropy) and high cooperation (e.g., by volunteering) (Moore et al., 2013). These costly displays of altruism tell observers about the individual’s underlying quality as a coalition partner, competitor, or mate via traits such as trustworthiness, physical strength, resources, and genetic quality. In summary, altruism can be seen as a costly signal as the altruistic acts are costly (in terms of resources, time or at risk to one's life) and they benefit the altruist indirectly by establishing a positive reputation and signalling quality to potential social allies and romantic mates.

As highly social creatures, humans rely on group living to survive and thrive (van Vugt & Kameda, 2013) so any behaviour or trait that benefits the group, would help establish a positive reputation, leading to long term benefits for the individual in attracting potential mates or allies (Roberts, 1998; van Vugt & Iredale, 2013). As people have a strong motivation to manage their reputations, they are keenly attuned to cues that generally indicate reputational concerns of behaviour (i.e., being watched by others) and will amend their behaviour accordingly (Vaish, Kelsey, Tripathi, & Grossmann, 2017). This has led to research that has shown if people are made to feel like they are being watched (even when there is no one actually present), then this can be used to help promote altruistic and prosocial behaviours (Haley & Fessler, 2005).

**What is the Watching-eyes Effect?**

The watching-eyes effect refers to the phenomenon that people behave more altruistically than usual when a false cue of observation (e.g. an image of eyes) is present in their environment (Matsugasaki, Tsukamoto, & Ohtsubo, 2015). In a seminal study, Haley and Fessler (2005) demonstrated that just an eye-like image on a computer screen was enough to increase an individual's prosocial behaviour in a dictator game.

Several theories which explain altruistic behaviour towards non-kin are reliant on individuals managing their prosocial reputations by signaling desirable prosocial traits. For example, as Bradley et al. (2008) maintain, for either direct or indirect reciprocity to be effective in maintaining prosociality, others need to be aware of the prosocial behaviour. Whereas CST suggests that altruistic acts signal an individual's prosocial qualities which aids them in attracting future interactive partners. In other words, people with a prosocial reputation are more likely to treated favorably by others (i.e., indirect reciprocity) (Shinohara

& Yamamoto, 2018) and participating in altruistic acts can aid in attracting potential mates or allies by signaling an attractive quality about themselves (i.e., Costly Signaling Theory) (Bereczkei et al., 2010).

These potential prosocial benefits provide a strong motivation for individuals to manage their reputations, which is where the watching-eyes effect is thought to arise from (Vaish et al., 2017). Both indirect reciprocity and CST can explain the watching-eyes effect; in circumstances where there are potential future rewards for prosocial behaviour, people should wish to advertise their prosocial qualities to increase the likelihood of being the beneficiary for those rewards (Bradley et al., 2018). Therefore, when people feel like they are being watched, this may trigger reputational concerns and incentivize that person to enhance their reputation by acting in a prosocial manner (e.g., by being generous to others). It is thought that humans have evolved to be so sensitive to reputational cues that even a false cue of being watched by others (e.g., an image of eyes) is enough to evoke reputational concern (Burnham & Hare, 2007).

Since Haley and Fessler's (2005) study was published, there has been a plethora of laboratory and applied research that has found evidence of the watching-eyes effect in a wide range of behaviours from increasing people's generosity (Fathi et al., 2014) to deterring littering (Bateson et al., 2015). However, there is a more recent and growing literature that has found little or no support for this effect (Northover et al., 2016), or found that such significant results are conditional on a range of participant, environmental, and eye cue factors (Dear et al., 2019; Northover et al., 2017). If researchers can pinpoint the characteristics of the environment and the types of stimuli that can evoke the watching-eyes effect, this could provide a simple and inexpensive method of encouraging prosocial behaviour which can be utilised in a range of practical applications from increasing charitable donations to, for example, reducing energy waste (Griskevicius et al., 2010).

***Proposed Causal Mechanism***

The watching-eyes effect falls under a branch of social psychology known as ‘priming’. Priming mechanisms are used to subconsciously influence a person’s behaviour through subtle cues of specific words, phrases or ways of viewing things (Lukkien, 2019). Well-known examples include Bargh, Chen, and Burrows (1996) demonstration that being primed with words related to the elderly make people walk slower, and Dijksterhuis and Knippenberg's (1998) finding that being primed with the word 'professor' made people perform better at cognitive tests. The watching-eyes effect posits that by subconsciously suggesting a person is being watched (i.e., by exposing them to an image of eyes), the person will experience feelings of being watched. This then triggers reputational concerns and motivates them to behave in a prosocial manner, thus causing the watching-eyes effect to emerge (Vaish et al., 2017).

Research has shown that it is not just the presence of other people that influences prosocial behaviour, but it is specifically the presence of eyes (Vaish et al., 2017) and the detection of direct (as opposed to averted) eye gaze (Hietanen, Syrjämäki, Zilliacus, & Hietanen, 2018). Eyes play a key part in human communication as they can convey valuable information about an individuals’ emotions, thoughts and intentions, which is of crucial importance in shaping people’s expectations and subsequent behaviour (Pauwels, Declerck, & Boone, 2017). Research has shown that direct-gaze cues (e.g., eye images) capture attention, enhance arousal, enhance memory, and can result in a strong effect on prosocial behaviour. Furthermore, as the key to the watching-eyes effect appears to be in making people *feel* like they are being watched (Bateson, Nettle & Roberts, 2006), the eye images do not have to be in the form of a real live person; a photo of eyes or even a symbolic image of eyes has been shown to affect human behaviour (Haley & Fessler, 2005).

The self-referential model (Conty, George, & Hietanen, 2016) proposes two stages in the processing of direct gaze leading to the watching-eyes effect. In the first stage, direct gaze automatically captures the beholder’s attention by a subcortical route and is thought to be triggered by the detection of low-level visual cues in eye gaze (e.g., luminance distribution in the eye). Then, the subcortical route engages mentalising and social brain areas (medial prefrontal cortex and temporoparietal junction) that process the belief of whether an observer is watching or not. In the second stage, if there is a belief that the observer is watching the beholder, the direct gaze will elicit self-referential processing (i.e., a heightened processing of stimuli in relation to the self), and the sense of self-involvement in the interaction will increase. This will lead to the watching-eyes effect, causing a change in behaviour in various ways, such as the promotion of prosocial behaviours (see Figure 1.1).

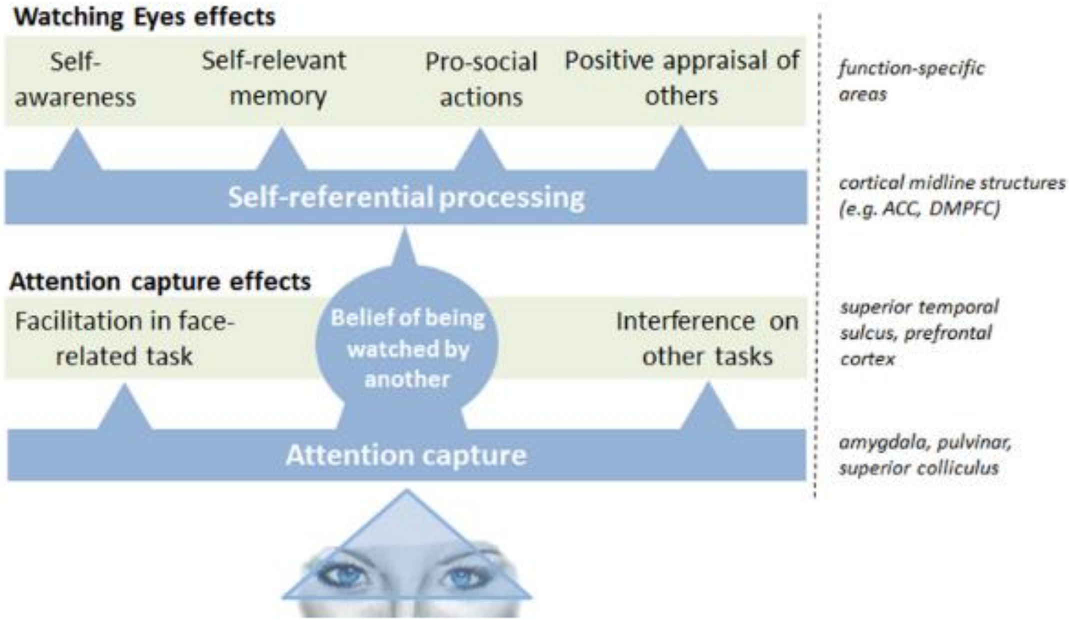
***Studies that have found a Positive Effect***

Since their seminal study, where Haley and Fessler (2005) demonstrated that just an eye-like image on a computer screen was enough to increase an individual's prosocial behaviour in a dictator game, there has been a plethora of laboratory and applied research that has found evidence of this effect in a wide range of behaviours from donating more money in a laboratory-based economic game (Burnham & Hare, 2007) to deterring bicycle theft on a university campus (Nettle et al., 2012).

Evidence for the watching-eyes effect has been found in lab-based studies which have measured prosocial behaviour. For example, in a laboratory experiment that used a public goods game to examine the watching-eyes effect, Burnham and Hare (2007) found that the presence of eyes increased contributions to the public goods game by 29% in comparison with a control. Similarly, Fathi et al. (2014) found in their lab-based experiment that participants chose to donate more money to a charity jar when there were images of watching eyes in the environment (e.g., on a nearby poster). Rigdon, Ishii, Watabe, and Kitayama (2009) found that even a minimal cue of being watched (i.e., three dots in a 'watching eyes' configuration) increased giving behaviour in a dictator game.

**Figure 1.1**

*The self-referential model of direct gaze processing (Conty et al., 2016)*




Watching-eyes studies have been highly influential in UK public policy. In particular, the watching eyes study on deterring bicycle theft conducted by Nettle et al., (2012) represents one of the baseline studies in 'watching eyes' applied research and is one of the most influential peer-reviewed research papers on the watching-eyes effect in UK public policy (Dear et al., 2019). In this study, Nettle et al. (2012) displayed images of watching eyes on posters across bicycle racks on a university campus, each image was accompanied by the message 'Cycle Thieves, We Are Watching You'. Reported bicycle thefts were monitored at the intervention and control locations for 12 months before the intervention and 12 months during, and the research found that bicycle thefts decreased by 62% at the intervention locations following the implementation of the signs. This study has paved the way for a multitude of interventions in the UK aimed at deterring a range of anti-social behaviours. For example, HMRC used images of watching eyes in their 2012-13 tax evasion campaign (Nelson, 2013; BBC News, 2015).

It has been claimed that field studies into the watching-eyes effect yield more positive results and larger effect sizes than laboratory experiments (Kelsey, Vaish, & Grossmann, 2018; Manesi & Pollet, 2017). Field studies have shown that the presence of eyes can increase donations to an honesty box (Bateson et al., 2006; Brudermann, Bartel, Fenzl, & Seebauer, 2015; Krátký, McGraw, Xygalatas, Mitkidis, & Reddish, 2016), increase charitable donations (Ekström, 2012; Fathi et al., 2014; Krupka & Croson, 2016; Oda & Ichihashi, 2016; Powell, Roberts, & Nettle, 2012), increase museum donations (Kelsey et al., 2018), increase voter turnout (Panagopoulos, 2014a; Panagopoulos, 2014b; Panagopoulos & van der Linden, 2016; Rad, Martingano, & Ginges, 2018), increase hand-washing hygiene (Beyfus et al., 2016; Kuliga, Verhoeven, & Tanja-Dijkstra, 2011; Stella et al., 2013), increase prosocial search terms (Beaumont, 2019), decrease littering (Bateson, Callow, Holmes, Redmond Roche, & Nettle, 2013; Ernest-Jones, Nettle, & Bateson, 2011; Francey & Bergmüller, 2012; Zengerink, 2013), deter theft (Nettle et al., 2012), and reduce train fare evasion (Ayal, Celse, & Hochman, 2019).

However, there is a growing body of literature that has challenged these findings on the effect of eye cues on prosocial behaviour, with the results of recent meta-analyses, multiple replication attempts and new experiments failing to find consistent evidence for the watching-eyes effect (e.g., Northover et al., 2016; Northover et al., 2017; Dear et al., 2019).

***Studies that have failed to Find an Effect***

As outlined by Northover et al. (2016), Northover et al. (2017), and Dear et al. (2019), recently there have been many failed replication attempts and new experiments failing to find consistent evidence for the watching-eyes effect (e.g., Beyfus et al., 2016; Bolton, Rivas, Prachar & Jones, 2015; Brudermann et al., 2015; Bush et al., 2016; Cai, Huang, Wu, & Kou, 2015; Carbon & Hesslinger, 2011; Fehr & Schneider, 2010; Fujii, Takagishi, Koizumi, & Okada, 2015; Huang, Liu, Zheng, Tan, & Zhao, 2015; Jolij & de Haan, 2014; Lamba & Mace, 2010; Matland & Murray, 2015; Matsugasaki et al., 2015; Meleady et al., 2017; Northover et al., 2017; Pederson, 2016; Raihani & Bshary, 2017; Kuliga et al., 2011; Sparks, 2010; Sparks & Barclay, 2015; Stella et al., 2013; Tane & Takezawa, 2011; Vogt, Efferson, Berger, & Fehr, 2015).

It has been suggested that some watching eyes studies on generosity (e.g., donations in economic games and to charity) have failed to find the watching-eyes effect because images of watching eyes do not increase mean donations, they increase the probability of donating something rather than nothing (Nettle et al., 2013). For example, Raihani and Bshary (2012) reported that images of eyes did not increase the amount of money shared by participants in a dictator game, but a subsequent re-analysis by Nettle et al. (2013) revealed that the presence of eyes increased the frequency of sharing itself. However, Northover et al. (2017) conducted a meta-analysis on 26 experiments on the effects of surveillance cues on generosity (using both donations in economic games and to charity) and their study showed no overall effect of watching eyes on generosity; neither on the amount of money given nor on the proportion of individuals who gave.

However, inconsistent results for the watching-eyes effect have also been found in studies that have not focused on generosity. In a different meta-analyses, Northover et al. (2016) found that artificial surveillance cues also had inconsistent effects on moral outcomes. All studies in these meta-analyses utilised vignettes that asked participants to rate the moral acceptability of two misdeeds; returning a lost wallet, but keeping the money (Bourrat, Baumard, & McKay, 2011), and falsifying information on a résumé (Schnall, Haidt, Clore, & Jordan, 2008). Northover et al. (2016) found that the wallet vignette meta-analysis provided no evidence that artificial surveillance cues increase reported moral judgment, whereas the résumé vignette meta-analysis provided limited evidence. In addition, Northover et al. (2016) attempted to replicate the study by Bourrat et al., (2011), who had reported that participants who were exposed to an image of watching eyes rated moral transgressions more harshly than participants exposed to an image of flowers. However, they failed to replicate the findings of this study and found no evidence for a significant watching-eyes effect.

### ***Potential Moderators***

Human behaviour is complex so it would not be surprising that, if the watching-eyes effect does exist, it may be nuanced. Many watching-eyes studies have reported moderating variables or effects conditional on features of participants, situational factors, or features of the surveillance cues (i.e., the eye images) themselves (Northover et al., 2017). It has also been suggested that the context of the watching eyes may be important as the eye images may not necessarily increase prosocial behaviour *per se*, but rather encourage people to comply with social norms (Ayal et al., 2019). Alternatively, watching eyes may be more effective in deterring anti-social rather than promoting prosocial behaviour (Dear et al., 2019).

**Participant Features.** Effects of watching eyes on participant behaviour have been reported as being dependent on a range of participant traits such as the gender of the participant (Rigdon et al., 2009), on whether the participant has a prevention-focused concern about their reputation (Keller & Pfattheicher, 2011), in the level of self-awareness of the participant (Pfattheicher & Keller, 2015) and the emotion of the participant (Horita & Takezawa, 2014).

***Participant Gender.*** As altruism may be driven by a desire to attract potential reproductive partners, and sexual selection is largely driven by female mate choice (Iredale et al., 2020), it is surprising that only a few studies have explored the effects of gender on the watching-eyes effect. Rigdon et al. (2009) found that when presenting participants with minimal social cues (i.e., three dots in a ‘watching-eyes’ configuration) in a dictator game, males were found to be highly responsive to the presence of watching eyes, giving twice as much in a dictator game, whereas female behaviour remained unchanged in the presence of eyes. Rigdon et al. (2009) argue that female donations remained unchanged in the presence of eyes as they are likely to place more importance on social interactions than males, even in anonymous one-shot interactions, and whether or not someone is 'watching' their decision would be irrelevant to their giving behaviour. However, other studies have found a larger watching-eyes effect for females in both dictator games (Nettle et al., 2013) and donations to charity (Saunders et al., 2016). Due to these inconsistent results and little evidence for any influence of gender on the watching-eyes effect, more recent studies have not attempted to explore gender differences any further (e.g., Dear, 2018).

***Reputational Concern.*** The watching-eyes effect is thought to arise from people's strong motivation to manage their reputations (Vaish et al., 2017) and reputational concerns are a leading interpretation for the watching-eyes effect (Pauwels et al., 2017). Therefore, individuals who are more concerned about their reputations (i.e., possess a strong chronic public self-awareness) should plausibly be more sensitive to cues that trigger reputational concerns (Vaish et al., 2017). Indeed, a charity donation study by Pfattheicher and Keller (2015) found that participants high in chronic public self-awareness donated more when eyes were present, but the eyes seemingly had no effect on participants who were low in chronic public self-awareness. Pfattheicher and Keller (2015) maintain that there is a relationship between individual differences in prevention focus (i.e., participants who were highly focused on preventing negative events in their lives) and reputational concerns. In a lab-based study on charitable donations, Keller and Pfattheicher (2011) found that participants high in prevention-focused self-regulation donated more money to charity in the eyes condition than those in the no-eyes condition, and participants who were low in prevention-focused self-regulation showed the opposite pattern. The first finding was replicated in two additional samples by Pfattheicher (2015) and the second finding was replicated in one sample, but not the other.

***Participant Emotion.*** Human cooperative behaviour is arguably maintained in large part by the reputational costs that individuals incur when they break cooperative norms and are punished by the group (Fehr & Gächter, 2002). In a study involving a one-shot third-party punishment game (where players punish another's selfish behaviour even when their own interests have not been harmed), Horita and Takezawa (2014) found participants in the eye-present condition punished unfair players more than participants in the control condition, but only if they felt anger toward unfair players. There do not appear to be any other studies that have considered the emotional state of the participant when testing the watching-eyes effect.

**Situational Factors.** There have been several situational factors that have been reported to be important when eliciting the watching-eyes effect such as ingroup/outgroup membership (Mifune, Hashimoto, & Yamagishi, 2010), surrounding crowd density (Bateson et al., 2013), and how long the participants are exposed to the eye cues (Sparks & Barclay, 2013).

***Ingroup and Outgroup Membership.*** Mifune et al., (2010) maintain that the watching-eyes effect is based on the fundamental assumption that sensitivity to being watched by community members is a proximate mechanism to promote human adaptation to group life. They argue that the proximate mechanism of human sensitivity to watching eyes is based on the social mechanism to promote mutual cooperation within the group and that human sensitivity to being watched is likely to be heightened when they are monitored by community members rather than by strangers. In line with this argument, Mifune et al. (2010) found that in a dictator game, when participants were told they were playing in-group members, their altruism towards other players was enhanced by the presence of eyes. However, it was unclear from their experiment whether the eyes displayed on the screen were perceived by participants as eyes of a community member. Northover et al. (2016) theorised that using an image of a familiar face (e.g., a celebrity) would induce a feeling of being watched by a member of the community. However, across their series of four moral judgement experiments, Northover et al. (2016) failed to find an effect for this potential moderator.

***Crowd Density.*** The surrounding crowd density has also been posited as a possible boundary condition for the watching-eyes effect with several studies finding that the effectiveness of eye cues seem to be dependent on the number of people in the vicinity (e.g., Ekström, 2012; Ernest-Jones et al., 2011; Powell et al., 2012). Ekström (2012) conducted a field experiment in a supermarket setting over 12 days. A picture of human eyes was posted on a recycling machine in the supermarket where participants could either earn money from recycling cans or bottles or choose to donate that amount to a charity organisation. Ekström (2012) found no general effect for the watching eyes on charitable donations, but the results showed that the picture of eyes increased donation amounts by 30 percent during days when relatively few other people visited the store. In a similar supermarket field experiment, Powell et al. (2012) displayed eye-like or non-eye-like images on charity collection buckets over an 11-week long period and found that the effect of eyes on charitable donations was significantly stronger at times when the supermarket was quiet rather than busy. These findings are also in line with a study by Ernest-Jones et al. (2011) who found that the presence of eyes halved the odds of littering a university café with a larger effect when there were fewer people in the café. These findings suggest that surveillance cues may be redundant in the presence of large numbers of people as people are receiving surveillance cues from many actual observers (Northover et al., 2017).

In contrast, in their field experiment on the effects of watching eyes on littering decisions on a university campus, Bateson et al. (2013) found that images of watching eyes reduced littering, but only when there were larger numbers of people around. Bateson et al. (2013) argue that this is because, when in a large group, people are more likely to avert their gaze from others which would increase the salience of the large signs displaying the eye images. For this reason, they posit that the watching-eyes effect may be strongest when people feel anonymous (e.g., when they are alone or blended into a large group). However, other studies have suggested that watching eyes do not seem to be effective if participants feel truly anonymous (i.e., they do not believe that that they are being observed). In two laboratory experiments, Tane and Takezawa (2011) found that the illustration of a human face on a computer screen did not increase the amount of donations in a one-shot dictator game when it was presented in a dark, soundproofed room. It has been suggested that sound mimics the presence of others and that a lack of sound may contribute to a perceived cloak of anonymity (whereby participants feel like they are truly anonymous and thus that their actions will not be seen or judged by others) which would cancel out any eye cue effects (Raihani & Bshary, 2012).

In addition, it has been suggested that sensitivity to eye cues may be weakened in online environments due to a similar cloak of perceived anonymity. Previous online studies of the watching-eyes effect (e.g., Raihani & Bshary, 2012; Saunders et al., 2016) may have failed to find significant results because the online environment is a truly anonymous setting, and if people believe that they are in an anonymous setting (e.g., their actions will not be seen by anyone else), the image of watching-eyes will fail to trigger reputational concerns (Lamba & Mace, 2010; Tane & Takezawa, 2011). It is this belief of being watched by another person which is critical in generating the watching-eyes effect (Conty et al., 2016). This suggests that as artificial surveillance cues (e.g., images of eyes) may be redundant in the presence of a large number of genuine surveillance cues and participants with total privacy may be immune to those cues (Northover et al., 2016), perhaps eye images work by reminding individuals that there are people in the area who can monitor their actions, and thus require the presence of at least some people in the area to affect behaviour.

***Exposure to Eye Cues.*** In a dictator game study conducted by Sparks and Barclay (2013), players who were shown an image of eyes only briefly were more generous than those that were shown the eyes for a longer period and those in the control condition. Sparks and Barclay (2013) argue that habituation occurs when individuals are exposed to eye images for prolonged periods of time, leading to a decrease in participant responsiveness to the eye cues. In addition, the longer a person is exposed to the eyes stimuli, the more likely it is that they will pay too much attention to the stimuli and realise that the eye images are false (Saunders et al., 2016). This would override any initial subconscious response to eye images and fail to trigger any reputational concerns thus a watching-eyes effect would not emerge (Sparks & Barclay, 2013). However, when the same authors investigated moral judgment, the duration of exposure to the eye images made no difference. In fact, in both this experiment by Sparks and Barclay (2013) and an additional two experiments exploring the effect of duration of exposure to eyes (Northover et al., 2016), no watching-eyes effect was found on the outcomes of moral judgments.

**Surveillance Cue Features.** There have been few watching eyes studies that have taken into account aspects of the eye cues themselves (Vrouwe & Balliet, 2014) but some research has shown that significant results of the watching-eyes effect could be dependent on a range of eye cues factors such as gender of the eye images (Bateson et al., 2006), perceived valence (e.g., kind vs unkind eyes) of eyes (Pauwels et al., 2017) and salience of the eye cues (Panagopoulos, 2014a).

***Gender of the Eye Cues.*** There have been few studies that have explored the effects of gender of the watching-eyes effect, regarding both the gender of the participant and the gender of the eye cue itself. In a field experiment on contributions to an honesty box, Bateson et al's. (2006) results showed that contributions to the honesty box were greater when male eyes were present. In contrast, Vrouwe and Balliet (2014) found that the watching-eyes effect on volunteering behaviour seemed to be driven by female watching eyes cues. However, in a lab-based dictator game, Nettle et al., (2013) found no significant differences between the male and females eyes. This was corroborated in a study by Panagopoulos (2014a), who found that the watching-eyes effect on voter turn-out could not be attributed to the gender of the eye stimuli used. Northover et al., (2016) also found no main effect for the apparent gender of the person in the surveillance cues in their experiment on moral judgments. Hence, the influence of the gender of the eye stimuli remains an open question (Panagopoulos, 2014b).

***Emotion.*** It has been argued that the null effects of watching eyes found in recent studies suggest that the effect of eyes could be sensitive to moderating factors such as the perceived emotion displayed by the eye cues (Pauwels et al., 2017). However, there has been limited research on the role of emotions in the watching-eyes effect and the research that has been conducted has produced mixed results.

Although emotion was not specifically tested in their experiment, Nettle et al. (2012) found that displaying a poster of angry eyes on a university campus helped to decrease bike thefts by 62%. Saunders et al. (2016) theorised that eyes expressing anger would be more threatening, entailing a potentially greater fitness consequence (i.e., reminding the observer that their actions may result in punishment or a decreased ability to attract a mate or allies) and thus generating a larger watching-eyes effect. However, in an online experiment that specifically manipulated the emotion displayed by the eye cues, Saunders et al. (2016) found that the emotion expressed in the eye stimuli did not affect charitable donation behaviour. In contrast, Pauwels et al. (2017), found that unkind (rather than kind) eyes significantly boosted cooperation in a sequential prisoner’s dilemma task. Pauwels et al. (2017) highlight that this finding is corroborated by physiological evidence from functional Magnetic Resonance Imaging (fMRI) research that shows that eyes that express anger are anxiety-inducing which heightens states of arousal (i.e., responsiveness to stimuli). However, in another online experiment using Amazon's Mechanical Turk (MTurk), Panagopoulos and van Der Linden (2017) found no evidence that staring, angry eyes increase negative emotions (e.g., anxiety). Except for studies by Saunders et al. (2016) and Pauwels et al. (2017), there have been no studies that have rigorously tested the watching-eyes effect using different emotions and from these mixed results, it is currently unclear as to whether any sensitivity to eye cues is influenced by the emotion expressed in the stimuli (Saunders et al., 2016).

***Type of Eye Cue.*** Watching eye studies have used a variety of different types of eyes cues including, photographs of real eyes (e.g., Bateson et al., 2006), stylized eyespots (e.g., Haley & Fessler, 2005) and minimal cues of being watch (e.g., Rigdon et al., 2009). Each of these distinct types of stimuli has different levels of ecological validity, social richness and potential to engage an audience effect (de Hamilton, 2016).

As discussed by de Hamilton (2016), research has shown that a simple photo of a pair of eyes has seemed to induce socially relevant changes in behaviour. For example, photos of eyes induced people to pay more for coffee in a university tea-room (Bateson et al., 2006) and to give more in a dictator game (Nettle et al., 2013). In addition, Rigdon et al. (2009) found that even the minimal cue of being watched (e.g., three dots in a 'watching-eyes' configuration) increased giving behaviour in a dictator game.

However, in many other cases, photos of eyes do not have the same effect as direct gaze from a live person. For example, autonomic responses such as skin conductance responses are enhanced in response to eye contact with a 'live' person (Pönkänen & Hietanen, 2012). In a field experiment, Krátký et al. (2016) found that using 3D eye cues was more successful in triggering contributions to an honesty box than using 2D cues. They argue that 3D cues are less sensitive to habituation or false cue detection and this higher activation of agency detection triggered stronger responses and thus amplified prosocial behaviour.

**Context of Watching Eyes.** It has been suggested that watching eyes do not necessarily increase prosocial behaviour *per se* but encourage people to comply with social norms (Ayal et al., 2019). While there is a growing consensus that there is little evidence for a robust watching-eyes effect on generosity (e.g., donations to an economic game or charity), a recent meta-analysis showed that there is a robust watching-eyes effect in reducing anti-social behaviour (Dear et al., 2019).

***Social Norms.*** Watching eyes may be more effective in reminding people to conform to social norms rather than to display prosocial tendencies. There have been multiple watching eyes experiments (e.g., Ayal et al., 2019; Brudermann et al., 2015; Kawamura & Kusumi, 2017; Oda & Ichihashi, 2016) which have suggested that it is not simply the eyes themselves that effectively nudge people towards more prosocial behaviours but it is the eyes in conjunction with social norm messaging that is effective. It is thought that cooperation between humans relies on social norms or beliefs that define how individuals should behave in certain situations and social norm adherence is modulated by awareness of being watched (i.e., the watching-eyes effect) and the expectation that deviation will result in consequences such as rewards or punishments (Ikuse et al., 2018).

In a recent watching eyes field experiment conducted by Ayal et al. (2019), they found that there was a decrease in fare evasion amongst train passengers who were exposed to an experimental eye-cue with social norm messaging (for example, "In this station, 90% of all individuals purchase and validate their ticket", p.4). The researchers concluded that although the watching eyes cues alone were not effective, exposing passengers to watching eye cues together with descriptive norm messaging could be an effective intervention. In a similar vein, Oda and Ichihashi (2016) conducted a field study examining the effects of watching eyes on charitable giving based on the amount of money visible in transparent collection boxes in an izakaya (a Japanese-style tavern) setting. They found that both the presence of an eye image and normative information facilitated prosociality. The presence of watching eyes increased the overall amount donated and more money was donated under the large-norm (i.e., when more money was visible) than under the small-norm (i.e., when less money was visible) treatment.

In contrast, in their field study on honest self-service payments for newspapers, Brudermann et al. (2015) found that eye cues, normative appeals or a combination of the both (e.g., displaying an image of eyes with the message “The majority of our readers pay for their weekend newspaper”, p.290) did not evoke a transition from low to high levels of honesty. In addition, there is no evidence that the social norms can impact the watching-eyes effect in an online environment. Kawamura and Kusumi (2017) conducted two experiments to investigate whether the watching-eyes effect changed depending on social norms: a lab-based experiment and an online replication. In the lab-based experiment, they found that watching eyes promoted donations only when a prosocial norm existed but, in the online experiment, they found that eyes did not promote generosity regardless of whether a prosocial norm existed or not.

***Anti-social Behaviour.*** The inconsistent results in the watching eyes literature may be due to the varying effects of prosocial behaviour on reputation. Eye cues may be more effective in reducing antisocial behaviour as antisocial behaviour may be more consistent in damaging reputation than prosocial behaviour is in enhancing it (Dear et al., 2019).

In contrast with a meta-analysis on the effect of eye cues on generosity, which showed an effect size close to zero (Northover et al., 2017), Dear et al. (2019) found in their meta-analysis of 15 experiments from 13 research papers, that the presence of watching eyes correlated with a 35% reduction in the risk of antisocial behaviour, whereas systematic reviews have suggested CCTV cameras reduce crime by only 16%. Dear et al. (2019) advocate that it does not matter whether an effect size is large or small according to statistical guidelines, but rather, whether the effect size is meaningful. As the authors argue, it is estimated that crime costs the UK economy between £35bn and £60bn a year and taking the lower end of the estimate, a 1% reduction in crime might be said to equate to a saving of £350,000,000. Therefore, their effect size of a 35% risk reduction for antisocial behaviour when eye cues are present, represents a large and meaningful effect in 'real-world' terms.

**Summary of the Literature Review**

Altruism has been a subject of much interest with behavioural sciences as, unusually within the animal kingdom, humans will promote the welfare of unrelated strangers even though there may not be any direct benefit to doing so. Reciprocal altruism (Trivers, 1971) has been a backbone of research on the evolution of altruistic behaviour towards non-kin, but it does not explain why people help those who cannot reciprocate. Instead, indirect reciprocity, and competitive altruism and costly signalling can help to understand this issue.

People who have prosocial (i.e., altruistic) reputations are more likely to receive help from others when needed (i.e., indirect reciprocity) (Shinohara & Yamamoto, 2018) and a prosocial reputation can aid in attracting mates or allies (i.e., CST) (Bereczkei et al., 2010). If someone feels like they are being watched, this may trigger reputational concerns and incentivise them to act in ways which enhances their prosocial reputation. Due to these social benefits of having a prosocial reputation, it is thought that humans have evolved to be highly sensitive to even false cues of being watched (Burnham & Hare, 2007).

Since Haley and Fessler (2005) demonstrated that just an eye-like image on a computer screen was enough to increase an individual's prosocial behaviour in a dictator game, there has been a plethora of laboratory and applied research that has found evidence of this effect in a wide range of prosocial behaviours from increasing people's generosity (Fathi et al., 2014) to deterring littering (Bateson et al., 2015). However, despite many earlier studies finding strong evidence for the watching-eyes effect on a range of prosocial behaviours

(Burnham & Hare, 2007; Haley & Fessler, 2005; Nettle et al., 2012), there is a growing literature that has found little or no support for this effect or which have found that such significant results are conditional on a range of participant, situational and eye cue factors (Dear et al., 2019; Northover et al., 2016; Northover et al., 2017). This suggests that if the watching-eyes effect does exist, it is nuanced and artificial monitoring effects (e.g., an image of eyes) do not influence human behaviour in a uniform way (Saunders et al., 2016). Although the watching-eyes effect may not be the panacea that perhaps early studies suggested it was, it could provide a potentially simple and cost-effective way of trying to affect human behaviour in ways that can have a meaningful impact (Dear et al., 2019).

**This research**

In principle, the watching-eyes effect requires nothing more than displaying an image of eyes on a poster or computer screen. If research can help tease out the exact nuances of the watching-eyes effect, it could potentially provide an inexpensive and straightforward way of encouraging people to act in more prosocial ways in a wide range of contexts.

One context which may be a potentially fruitful area to explore is *online* charity giving. In recent years, there has been an explosion in the number of people using the internet, with over 3.4 billion people using the internet daily for communicating with friends and family, work, shopping and entertainment (Turner, 2020). With almost half of the world's population utilising the internet for so many aspects of their day-to-day lives, it is not surprising that almost a quarter (23%) of UK charitable donations are now made online (CAF, 2019). However, according to the UK Giving 2019 report (CAF, 2019), the proportion of the British public who either gave money to charity directly or sponsored a friend or family has declined year on year since 2016. With the shift towards digital charity giving, charities can reach many more potential donors via the internet than they can via more traditional fundraising methodologies (e.g., door to door canvassing), so it is increasingly important for the sector to develop innovative methods to attract web visitors and encourage online donations (Green, 2021).

At the time of developing the research proposal for this thesis (circa 2016), the watching-eyes effect had previously been tested on charitable donations in both a laboratory (e.g., Keller & Pfattheicher, 2011) and field experiment context (e.g., Ekström, 2012) and on cooperation in a dictator game in an online environment (Raihani & Bshary, 2012), but there had been no studies into the effects of watching-eyes on online charitable donations. If the watching-eyes effect could be evoked in this context, it could pave the way for a range of low-cost, but potentially high-impact, online interventions to increase charitable donations. Moreover, the potential impacts are not limited to the charity sector; there are a range of other online issues where the watching-eyes effect could be of value, for example in deterring anti-social behaviour such as cyber-bullying (Dear et al., 2019). Thus, the first main aim of this thesis was to explore altruism as a costly signal in an online environment. Specifically, whether the watching-eyes effect could be evoked in an online context to promote donations to charity.

## Main Aim and Thesis Outline

The main aim of this thesis was to explore altruism as a costly signal in an online environment. Specifically, whether the watching-eyes effect could be evoked in an online context to promote donations to charity and if so, what were the caveats that would enable this 'watching-eyes effect'?

### *Empirical Studies*

To try to address this aim, this thesis consists of five studies written up into four empirical chapters (a more detailed overview will be provided in the next general methods chapter):

- Study One: Exploring how Watching Eyes Impact Prosocial Behaviour in an Online Environment
- Study Two: A Lab-based, Eye-tracking study on Participant Attention to Watching Eyes
- Study Three: Exploring Eye Gender, Emotion, and Salience
- Study Four A: A Field-based Exploration of the Watching-eyes Effect
- Study Four B: Keeping Britain Tidy; A Closer Look.

This thesis was designed via an iterative process, with each study planned and conducted based on the findings of the previous study to enable as robust an exploration into the watching-eyes effect as possible. Each empirical chapter consists of a focused introduction and literature review for each study, an overview of the methodology taken, the results and a discussion of those results and how they fit in with the wider literature.

***Next Chapter***

The next chapter of this thesis (Chapter Two: General Methodology) will provide a more detailed overview of the empirical studies and present an overview of the general methodological approaches used in this thesis and will provide a summary of the aspects of methods that were consistent across the empirical chapters including participant recruitment, materials, procedures, ethics, data processing and analysis.

# Chapter Two – General Methodology

The previous chapter discussed the literature in relation to this thesis, whereas this chapter will present an overview of the methodological approaches used in this thesis. In total, this thesis consists of five studies written up into four empirical chapters. Several aspects of the methods were consistent across the empirical chapters. This chapter provides a summary of these under the headings of participant recruitment, materials, procedures, ethics and data processing and analysis. The chapter begins by providing an overview of all studies conducted before outlining information on the participants and recruitment methods used in each study, followed by an explanation of the materials used (including eye stimuli used, survey questions, prosocial measures, and eye-tracking), an outline of the procedures used and the ethical considerations for each study. It concludes by providing an overview of the data processing and analysis procedures conducted.

## Overview of Empirical Chapters

In total, this thesis consists of five studies written up into four empirical chapters. This thesis was designed via an iterative process, with each study planned and conducted based on the findings of the previous study to enable as robust an exploration into the watching-eyes effect as possible (see Figure 2.1).

- Study One: Exploring how Watching Eyes Impact Prosocial Behaviour in an Online Environment
- Study Two: A Lab-based, Eye-tracking study on Participant Attention to Watching Eyes
- Study Three: Exploring Eye Gender, Emotion, and Salience
- Study Four A: A Field-based Exploration of the Watching-eyes Effect
- Study Four B: Keeping Britain Tidy; A Closer Look.

**Figure 2.1**

*Overview of empirical chapters in this thesis*

| | Study 1 | Study 2 | Study 3 | Study 4a | Study 4b |
|---|---|---|---|---|---|
| **Overview** | This study explored whether the ***salience*** of the eye cues had an impact on prosocial behaviour.<br>• Participants were randomly assigned to a condition and told they had been given small sum of money to play a public goods game where they could win an additional bonus.<br>• They were then given the opportunity to donate any amount of their winnings to charity.<br>• Participants were primed to think that their decisions were anonymous. | This study used ***eye-tracking software*** to determine whether participants paid attention to the eye stimuli.<br>• Participants completed the same task as those from study one but this time in a laboratory setting so their eye-tracking behaviour could be recorded.<br>• Participants were primed to think that their decisions were public. | This study explored whether the ***salience (inc. audio), gender*** or ***emotion*** of the eye cues had an impact on prosocial behaviour.<br>• Participants were randomly assigned to a condition and were offered the chance to enter a prize draw to win shopping vouchers.<br>• They were given the opportunity to donate some of those vouchers to charity if they were selected as the winner.<br>• The participants then completed the survey which consisted of moral dilemma questions and a public goods game to measure prosocial score. | This study tested the watching-eyes effect on prosocial behaviour in an ***online field experiment.***<br>• A Twitter ad campaign was run to promote a charity to Twitter Users via promoted Tweets.<br>• Twitter users could follow a link to a charity website to find out more about the charity and how to donate.<br>• Twitter users who clicked on the link were taken to one of three web pages containing the eye stimuli and charity information.<br>• The average donation amount per click was measured. | This study ***re-evaluated*** a previous field experiment which utilised the watching-eyes effect.<br>• As part of a critical reflection on a reportedly successful 'watching-eyes' campaign, the methodology and data from Keep Britain Tidy's "We're watching you" campaign was explored and evaluated. |
| **Method** | Online Survey<br>• Online recruitment via social research platforms | Laboratory setting<br>• Online recruitment via social media | Online Survey<br>• Mixed online and convenience sampling | Online field experiment in Twitter | Secondary data analysis |
| **IV groups** | • Static eyes<br>• Blinking eyes<br>• No-eyes | • Static eyes<br>• Blinking eyes<br>• No-eyes | • Audio<br>• Static<br>• Blinking<br>• No-eyes<br>• Happy female eyes<br>• Angry female eyes<br>• Happy male eyes<br>• Angry male eyes | • Static eyes<br>• Blinking eyes<br>• No-eyes | • Eyes only<br>• Eyes and enforcement message<br>• Eyes and positive reinforcement<br>• Eyes and peer influence |
| **DVs** | • % donated to the public goods pot<br>• % donated to charity<br>• Total number of volunteer hours | • % donated to the public goods pot<br>• % donated to charity<br>• Total number of volunteer hours<br>• Eye-tracking behaviour<br>  ◦ % total time<br>  ◦ Total fixation count<br>  ◦ Time to 1st fixation (m/s) | • Total amount donated to charity<br>• Total prosocial score<br>• Moral Dilemmas | • Average donation amount to charity per click (APCk) | • Number of dog fouls |

**Participant Recruitment**

A range of participant recruitment methods were used across the studies in this thesis, Online recruitment, including via social media sampling, was used for studies one, two and three. Convenience sampling was used for studies two and three. These methods are discussed in detail below. The recruitment methods used in studies four A and four B were specific to the individual studies so are outlined in their individual chapter method sections.

Basic demographic data were collected on participants' age, sex, ethnicity, country of residence and education (age the participant left compulsory schooling and highest educational qualification achieved). Sample sizes and participant demographic information pertaining to each experiment are outlined in the method section of the individual experiments. A minimum age requirement of 18 years was implemented in line with the University's and the British Psychological Society's code of ethics (The British Psychological Society, 2018).

***Online Recruitment***

Online recruitment involved recruiting participants via online social research platforms: Amazon's Mechanical Turk (MTurk) and Call for Participants (CfP) (as used in studies one and three) and via social media (Twitter and Facebook) (as used in studies one, two, and three).

**Online Social Research Platforms.** Since 2010, the use of online crowdsourcing work market sites such as MTurk and CfP has been growing steadily in online social science research (Paolacci, Chandler, & Ipeirotis, 2010). These social research platforms allow researchers to recruit large numbers of participants to complete tasks that computers are not able to accomplish (e.g., expressing personal sentiments) while facilitating the payment of participants for their time (Raihani & Bshary, 2012). This method of data collection is quick and inexpensive, as well as having the added benefit of being able to draw upon a more diverse population sample than traditional recruitment methods (Saunders et al., 2016).

Studies have shown that up to 96% of psychological research relies on data from exclusively Western, Educated, Industrialised, Rich and Democratic (WEIRD) samples (Rad et al., 2018). Recruiting participants to studies can be difficult and costly, so researchers often rely on convenience sampling, usually undergraduate students from the institutes that they are employed at (Schulson, 2020). However, just 12% of the world's population is represented in these WEIRD samples (Henrich, Heine, & Norensayan, 2010). By utilising mainly WEIRD samples, researchers fail to capture the vast diversity in human psychology, behaviour and norms and erroneously generalise the findings from one population to another (Apicella, Norenzayan, & Henrich, 2020). MTurk and CfP enable researchers to recruit participants from non-WEIRD populations in a relatively straightforward and cost-effective manner (Saunders et al., 2016).

MTurk is an online labour market where employees (called *workers*) are recruited by employers (called *requesters*) for the execution of tasks (called *HITs*, an acronym for Human Intelligence Tasks) in exchange for a wage (called a *reward*) (Paolacci et al., 2010). MTurk attracts workers from across the world (there are no residential requirements for the workers) making it convenient to recruit large numbers of diverse participants.

Many workers use MTurk to earn an income as there are no set hours and they can work in the comfort of their own home. There is no standard reward rate for HITs, the rewards offered can be as low as $0.01 which is problematic for both worker and requester (as Amazon is based in the United States of America (USA), workers get paid in dollars). From the workers perspective, this often means working for under the USA hourly minimum wage of $7.25 per hour (Doyle, 2021). In a 2016 report by the Pew Research Center, they found that half of the workers they sampled earned a rate less than $5 an hour meaning that a worker who completed HITs for 40 hours a week and did not take any vacation for a whole year would have only earned $10, 379.20 (Pew Research Center, 2016). From the researcher's perspectives, as well as the ethics surrounding paying low wages, the low wages can call into question the quality of the data collected. Given that MTurk workers can get paid so little, it is logical to presume that the workers may not pay attention to the task or take it very seriously, instead focusing on trying to complete as many HITs as possible to increase their income (Paolacci et al., 2010).

There are solutions to both issues. First, it is fair and ethical to pay MTurk workers the equivalent of the USA federal minimum wage. For the first study of this thesis, it was estimated that it would take participants 15 minutes to complete the survey, so MTurk participants were offered $2 to complete the survey, which was above the US federal minimum hourly wage at the time ($7.25 per hour in 2020). Second, MTurk provides the opportunity to direct a HIT to more experienced workers which can help with obtaining higher quality data. If workers do not complete the HIT to a satisfactory standard (e.g., by completing all of the survey or passing the attention checks), then a requester can refuse payment or even block a worker from completing future tasks. Other requesters can then make their HIT only available to workers who have previously completed HITs to a satisfactory level by setting requirements, such as a minimum number of HITs approved and a minimum HIT approval rate (the proportion of completed tasks that have been satisfactorily completed by the worker). Setting a minimum requirement of approved HITs (e.g., 500) helps the requester to target their HIT towards more experienced workers and depending on the type of task (surveys for example), the HIT approval rate is usually set at a minimum of 95% to enforce higher quality. However, MTurk participants may not be the best sample to test prosocial behaviours on; as participants sign up to MTurk with the specific aim of earning money, and as such, they may be reluctant to donate any of their earnings in this setting (as seen in discussions on MTurk forums[1]).

CfP is an alternative online platform that promotes academic research studies that anyone can take part in. Like MTurk, participants are from around the world (207 different countries as per the CfP website in April 2020), but unlike MTurk, participants are not necessarily compensated for their time in money. For example, when recruiting participants, researchers can choose to offer a prize draw to earn vouchers as an incentive or, if they are unable to offer financial incentives, they can just offer 'sincere gratitude'. The incentive is set by the researcher on a per study basis. Thus, as financial incentives are not the sole purpose for signing up to the platform, CfP may be a better participant source to use to explore whether eye images induce prosocial behaviour.

However, the effectiveness of participant recruitment via CfP was unknown at the time of planning this research (circa 2016). Although there are numerous published studies that have used MTurk to recruit their participants (e.g., Pfattheicher & Keller, 2015; Raihani & Bshary, 2017; Saunders et al., 2016), there appears to be a lack of published papers that have used CfP to recruit their participants. Therefore, to mitigate any potential participant recruitment risk, it was decided in study one to attempt to recruit all participants via CfP but use MTurk in the event of low recruitment numbers. In study three, participants were recruited from a mixture of online recruitment (via CfP and social media) and convenience sampling methods.

**Social Media.** Following the rise of social media giants such as Facebook and Twitter in the last two decades (Britannica, 2020; Hall, 2021), the use of social media as a recruitment

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[1] MTurk forums: https://www.reddit.com/r/mturk/comments/aw7cfg/how_much_of_your_bonus_would_you_like_to_donate/ [Accessed on 20 Feb 2021]

tool has been growing (Gelinas et al., 2018). Social media recruitment is simple, efficient, inexpensive and yields a high participant response rate (Herbell & Zauszniewski, 2018). Facebook and Twitter are amongst the largest and most popular online social networks and have become a significant part of daily life for over a billion people around the world (Kosinski, Matz, Gosling, Popov, & Stillwell, 2015).

The size and reach of these social media platforms offer researchers an unprecedented opportunity to acquire large and diverse samples of participants and one of the least expensive ways of utilising social media participant pools is by snowball sampling - encouraging Facebook and Twitter users to invite their friends and followers to join a study (Herbell & Zauszniewski, 2018). However, it is important to bear in mind that snowball sampling methods do not meet the gold standard of randomised sampling because the method can introduce biases. The first participants are likely to disproportionately affect the composition of the sample because people tend to interact with others similar to themselves. The majority of users tend to be younger, better educated and some groups may be excluded (e.g., the elderly, or isolated groups such as the Amish) (Kosinski et al., 2015). However, research has shown that data collected over the internet are no less valid than data collected from more traditional groups (e.g., university undergraduates) (Rife, Cate, Kosinski, & Stillwell, 2016). Even though alternative recruitment techniques such as MTurk boast a more diverse and representative sample than the traditionally psychological studies, even these methods are affected by self-selection bias because only certain types of people sign up to these sites (Kosinski et al., 2015). With these caveats in mind, social media samples provide an inexpensive and relatively high-quality alternative. The size and diversity of the Facebook and Twitter populations help to minimise the disadvantages of snowball sampling (Kosinski et al., 2015).

Participants were recruited via social media in studies one, two, and three. In study one, participants were recruited via social media for two small pre-tests. The pre-tests were conducted to test whether the eye stimuli correctly conveyed the intended emotion and to test whether participants understood the instructions to the public goods game correctly (see chapter three for a full overview). The pre-tests were created on Qualtrics, and a link shared via Social Media (Facebook and Twitter) via the researcher's friends and followers. Snowball sampling was then applied to secure participants. Studies two and three also used social media to recruit participants but participants were additionally recruited via convenience sampling methods.

***Convenience Sampling***

Recruiting individuals to participate in research can be time-consuming and costly and, as a result, psychology researchers have often relied on recruiting undergraduate students for their studies (Zannella, Vahedi, & Want, 2020). Psychology courses often include a component where undergraduate students are required to participate in research studies for course credit and are an important source of research participants (Elicker, McConnell, & Hall, 2010). There are clear limitations to the over-reliance on undergraduate samples, namely the previously discussed issue of an over-reliance on WEIRD samples and the lack of generalisability to the wider human population (Apicella et al., 2020). Due to these limitations, there has been a recent decline in the proportion of published studies using undergraduate students (most likely reflecting the rise of crowdsourcing platforms such as MTurk and CfP). Despite the limitations of using an undergraduate participant pool, undergraduate samples still hold value to researchers as they are convenient, cost-effective and can still assist in answering important research questions (Zannella et al., 2020).

In this thesis, convenience sampling was used to recruit participants for both studies two and three. As study two took place in a laboratory setting, it required people to take part in the experiment in person in the University's psychology labs, thus convenience sampling was the most suitable method to recruit participants. Participants were recruited via Canterbury Christ Church University's (CCCU) Research Participation Scheme (RPS) for two-course credits. In study three, the same approach was used to recruit participants to complete an online survey, but participants were additionally recruited via CfP and social media.

## Materials

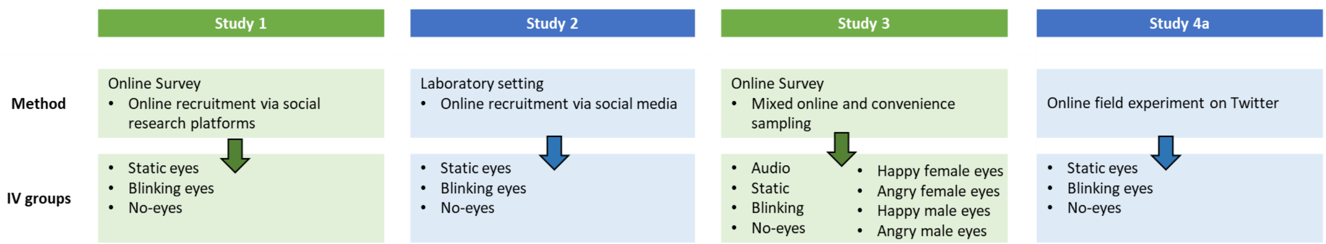
This section will give an overview of the general materials used. These include the eye stimuli (static, blinking, and no-eyes) used in the experimental tasks (as used in studies one, two, three, and four A). The survey items, which covered a range of demographic and individual differences potential associated with prosociality (as used in studies one, two, and three), and the prosocial measures (as used in studies one and two). There were three measures of prosocial behaviour used in studies one and two: 1) the amount that participants donated to the public goods pot, 2) the amount that participants donated to charity, and 3) the total number of hours that participants indicated they would be willing to volunteer their time to charity for on a monthly basis.

### *Eye Stimuli*

The same static, blinking, and no-eyes stimuli were used across studies one, two, three, and four A. Additional types of eye stimuli were tested in study three but as these stimuli were specific to study three, the stimuli are outlined in the third empirical chapter.

**Figure 2.2**

*Overview of the eye stimuli used in studies one, two, three, and four A*



As the key to the watching-eyes effect appears to be in making people *feel* like they are being watched (Bateson et al., 2006), the salience (i.e., how noticeable) of the eyes may play an important role in evoking this feeling in an online environment. In the seminal study by Haley and Fessler (2005), they found that the mere display of stylized eyespots on a computer screen significantly increased giving in a dictator game but more recent research has challenged these earlier findings, with a series of replication attempts and new experiments failing to find any consistent evidence for a watching-eyes effect (Dear et al., 2019). However, when Haley and Fessler (2005) conducted their study, it was just before the widespread use of computers and the internet and social media giants such as Facebook and Twitter were not widely used by the general public until 2006 (Britannica, 2020; Hall, 2021). In 2017 (when study one of this thesis was conducted), the near-ubiquitous use of the internet may mean that participants are exposed to eye images on an almost constant basis and therefore, could be accustomed to ignoring what they may consider to be task-irrelevant stimuli specifically designed to attract attention and/or change their behaviour (Sparks & Barclay, 2013).

If people are becoming habitualised to eye stimuli in general, then it is intuitive that the type of eye stimuli that would be effective in capturing attention would be nuanced and perhaps a 2D image of eyes is not enough to capture a person’s attention and invoke a feeling of being watched. In a field experiment, Krátký et al. (2016) found that increasing the salience of the eye stimuli by using 3D eye cues was more successful in triggering contributions to an honesty box than using 2D cues. They argue that 3D cues are less sensitive to habituation or false cue detection and this higher activation of agency detection, triggered stronger responses and thus amplified prosocial behaviour.

Due to technological restrictions, using 3D eye cues is a challenge in an online environment; although the technology exists, 3D computer displays are a niche luxury (at the time of research) that the majority of the public would not have access to (Kyrnin, 2020). Embedding a short video of blinking eyes, instead of using a static image of eyes, may make the eyes seem more realistic and assist in invoking a feeling of being watched in the participant (Manesi, van Lange, & Pollet, 2016). Therefore, studies one, two, three, and four A of this thesis examined whether there is a difference in participant prosociality (i.e., prosocial behaviour) when exposed to a static condition (a photograph of eyes), a blinking condition (a short video of blinking eyes) or a no-eyes condition (a university logo).

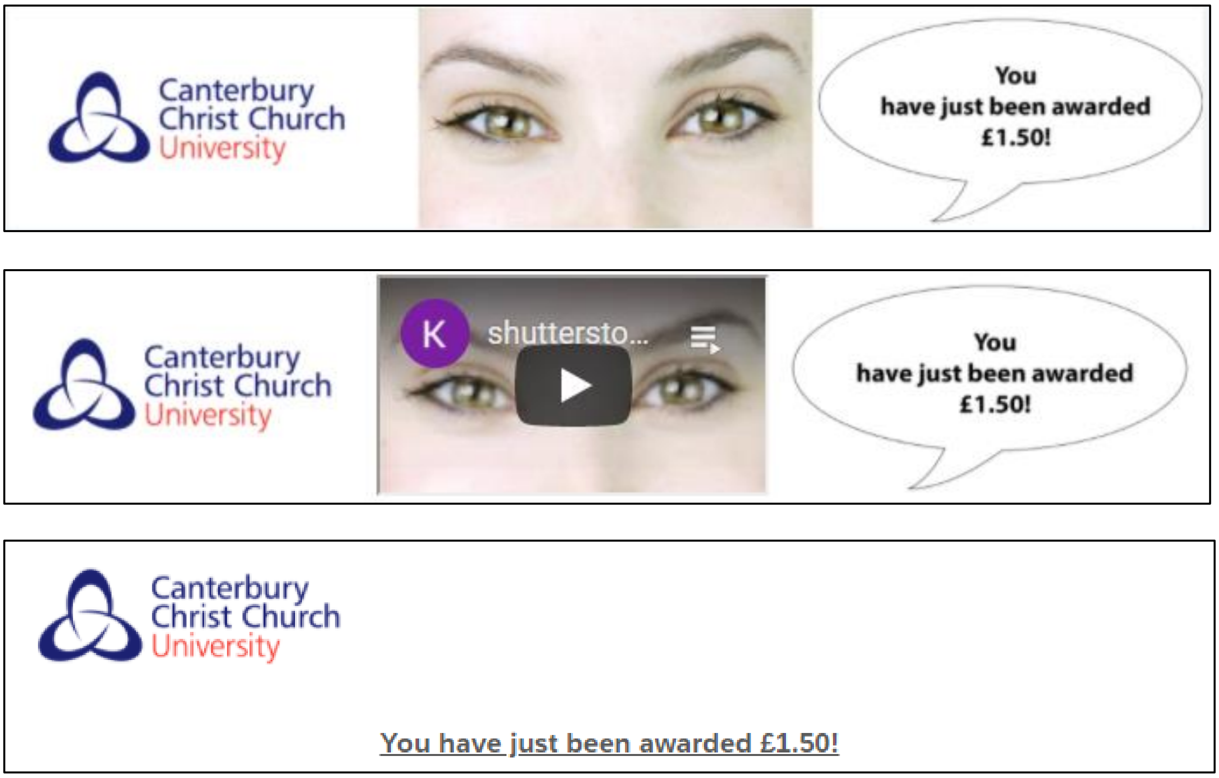
For the static and blinking conditions, eye images that depicted happy, female eyes were used. It was decided to use eyes that depicted happiness as other studies (e.g., Pauwels et al., 2017) theorised that 'kind' eyes may create the impression of trustworthiness and 'happy' faces are more salient than 'angry' faces (Becker, Anderson, Mortensen, Neufeld, & Neel, 2011). Female eyes were used rather than male ones as they are less likely to be consciously acknowledged (Conty et al., 2016) and female facial features are said to be more attractive than male facial features (Vrouwe & Balliet, 2014). As Vrouwe and Balliet (2014) posit, the increased attractiveness could lead to increased attention to feminine watching cues, which in turn could lead to an increase in prosocial behaviour. A short clip of a pair of eyes blinking was purchased from Shutterstock (copyright: Federico Marsicano) and a still image of the eyes was taken from this video. The video/image was then incorporated into a banner that displayed the University's logo and a speech bubble advising the participant that they have been awarded a bonus amount. In the 'no eyes' condition', just the University's logo was used with some text below advising that the participants had been awarded a bonus (see Figure 2.3).

### *Survey Items*

The same survey items were used across studies one, two, and three. As the main aim of this thesis was to test whether the presence of watching eyes could increase prosocial behaviour in an online environment, studies one, two, and three were conducted via an online survey, which was designed and created in Qualtrics (©2020). In addition to the experimental

**Figure 2.3**

*Screenshots of the eye stimuli used in the experimental tasks*




*Note.* The above images are screenshots of the static, blinking and no-eyes stimuli (in that order) used in the experimental tasks. The video controls shown in the blinking image above are not visible to the participants during the task.

tasks (which are discussed in the next section), participants completed survey items that covered a range of demographic and individual differences potentially associated with prosociality (e.g., Saunders et al., 2016).

Participants were asked about a range of potential covariates for prosocial behaviour including sexual orientation, relationship status, religion, income, number of dependent children, and how charitable the participant considered themselves to be. These questions were asked after the prime (e.g., after the eye stimuli were displayed) and donation opportunity so they did not interfere with the effects of the prime. As no main effect was found between the three groups (static, blinking and no-eyes) and the prosocial measures, further analysis of the potential covariates for prosocial behaviour was not conducted. However, the potential covariates included in the survey are detailed below.

Participants were asked about their sexual orientation as there is a possible sexual selection element to the watching-eyes effect. It is generally accepted within evolutionary psychology and sexual selection theory that at a high level, when it comes to reproductive strategies, human male and females are driven by different mating preferences (Gobrogge et al., 2007). In the case of heterosexual relationships, males tend to be motivated by cues of youth and fertility, whereas females tend to seek partners who are good providers so are more strongly influenced by non-physical cues such as wealth, high social status and altruism (Iredale, 2009). Given that homosexual people would in general seem likely to seek partners for reasons other than procreation, it is expected that they may exhibit different mating preferences from their heterosexual counterparts (Gobrogge et al., 2007). Therefore, it would be expected that an individual’s sexual orientation may be an important factor in the possible impact of the watching-eyes effect.

Participants were asked about their relationship status as mating preferences may differ depending on whether the individual is seeking a long-term versus short-term relationship (Gobrogge et al., 2007). Studies have shown that when considering a short-term relationship (i.e., casual sex), a physically attractive appearance is preferred by most men whereas conversely, positive, internal attributes (such as altruism) are emphasized by both sexes when considering a long-term relationship (Regan, Levin, Gate, Sprecher, & Christopher, 2000). Therefore, the participant’s relationship status could have an impact on the levels of altruistic behaviour they choose to display.

Participants were asked about their income as their individual financial situations could affect how much they would be willing to donate to charity. People may be more predisposed to charitable behaviour the more disposable income they have so participants were asked how much their household income was, how much disposable income they have and how many dependent children they have (Raihani & Bshary, 2012).

Lastly, participants were asked to rate how religious and how charitable they considered themselves to be on a seven-point Likert scale. There is a growing body of literature that suggests that religiosity may promote prosocial behaviour so to account for religious variation, participants were asked to state their religion and how religious they considered themselves to be (Saunders et al., 2016). Studies have found that there may be a self-image mechanism to charitable giving with charitable giving being positively correlated with a charitable self-image. Giving to charity is not only the result of a charitable self-image but it reinforces that image and it is this reciprocal relationship that can drive charitable donations (Bekkers & Wiepking, 2011).

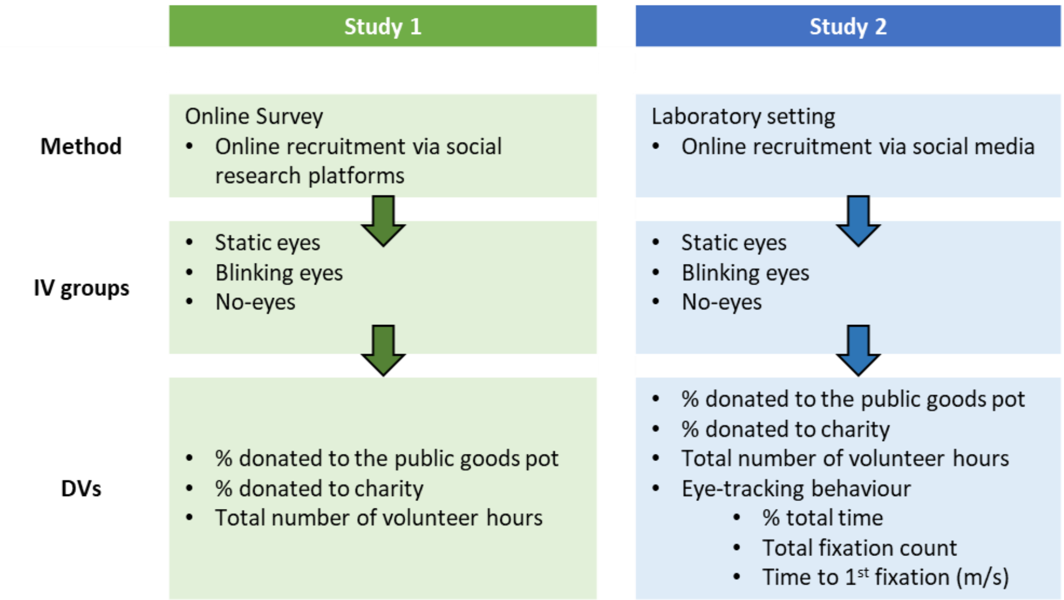
***Prosocial Measures***

There were three general measures of prosocial behaviour used in studies one and two: 1) the amount that participants donated to the public goods pot, 2) the amount that participants donated to charity, and 3) the total number of hours that participants indicated they would be willing to volunteer their time to charity for on a monthly basis.

**Public Goods Game.** As prosocial behaviour is a socially desirable trait, self-reports on altruistic behaviour in surveys are unreliable as they are prone to response biases such as a social responsibility bias or self-presentation effects (Bekkers, 2007). To overcome this, researchers have applied game theory and utilised economic games to measure a range of prosocial behaviours (e.g., see Northover et al., 2017). There are seven experimental games useful for measuring social preferences (Camerer & Fehr, 2005) including the dictator game, public goods game, prisoner’s dilemma game, ultimatum game, trust game, gift exchange game and third-party punishment game. The two games which have been utilised in the

**Figure 2.4**

*Overview of the general prosocial measures used in studies one and two*




watching eyes literature are the dictator game and the public goods game. Haley and Fessler (2005) first utilised the dictator game on the watching-eyes effect with other researchers following suit either by utilising the same game or by utilising a public goods game (e.g., Burnham & Hare, 2007).

A dictator game allows a simple investigation into human decision making in a social context (Raihani & Bshary, 2012). In a dictator game, one of two players (the dictator) receives money and decides how to allocate it among the two players. The second player merely accepts what the dictator offers (if the dictator offers anything at all). As the dictator maximizes their material payoff by keeping everything and giving nothing, they gain nothing by giving away money to the second player so any money the dictator does give away can be seen as a measure of pure altruism (Camerer & Fehr, 2005).

A public goods game is another economic game in which participants are told they are playing a game with other people and that they have the chance to earn some money. They have an initial sum of money that they can either keep for themselves or invest into a public goods pot. Whatever amount they put into the pot is doubled and then distributed evenly back to each of the players (Burnham & Hare, 2007). Like the dictator game, the participant has an incentive to be selfish but unlike the dictator game, every participant has the potential to benefit from the game. A public goods game captures key features of many common human interactions; namely, each individual would be materially better off by free-riding on others but the collective benefits more from cooperation in these interactions (Alger, 2010). As social exchanges very often involve one-shot interactions with strangers who are not able to reciprocate later (Wu, Balliet, Kou, & van Lange, 2019), a one-shot public goods game is a particularly good proxy for a real-world setting.

Studies one and two of this thesis utilised the public goods game as a measure of prosocial behaviour. Specifically, the percentage of their initial bonus that the participants choose to invest in the public goods pot was compared across the three eye conditions (static, blinking, no-eyes).

**Donations to Charity.** Some psychological and economic research has questioned the validity of measuring prosocial behaviour using an economic game approach due to game restrictions and poor external validity (Cañigueral & Hamilton, 2019a). For example, game restrictions may be caused by participants who have not understood the instructions fully or grasped the possible outcomes of the economic game. To address this, a small pretest was conducted before launching the first study in this thesis to check participant understanding of the public goods game instructions and the possible outcomes. However, another critique of using an economic game approach is poor external validity, with a recent meta-analysis concluding that economic games do a poor job of explaining behaviour in naturally occurring settings (Galizzi & Navarro-Martinez, 2019).

A complementary prosocial measure that is more analogous to a naturalistic setting and more intuitive to understand is donations to charity. This measure is also more in line (than the previous prosocial measure) with the main aim of this thesis, which was to specifically explore altruistic behaviour. Prosocial behaviour occurs when an individual acts in a manner that benefits another person or group of people whereas altruism is a kind of prosocial motivation where individuals act to promote others' welfare, even when at risk or cost to themselves (DeLamater et al., 2018). Although prosocial behaviour may be altruistically motivated, this is not a requirement for the behaviour to be considered prosocial (DeLamater et al., 2018). Although donating to the public goods pot may be a prosocial behaviour, participants in the public goods game may be motivated to participate in order to earn a bonus for themselves. Whereas there are no immediate benefits for participants to donate at least some of their bonus to charity, making any donations to charity a more altruistic act.

As an established methodology in which to measure the watching-eyes effect (e.g., Burnham & Hare, 2007), the public goods game is a useful measure of prosocial behaviour to include and has the added benefit of providing a mechanism in which participants can earn the money which they may then donate to charity. As participants would be advised of their total winning bonus before being asked how much they would like to donate to charity, using donations to charity as a prosocial measure would also negate any potential issues of some participants not fully understanding the instructions or possible outcomes of the game. Therefore, as a measure of altruistic behaviour, studies one and two provided the participants with an opportunity to donate their winnings to charity and compared the percentage of their final bonus that the participants gave away to charity across the three eye conditions.

**Volunteer Hours.** Altruistic behaviour can be displayed in many forms, including in non-monetary decisions or situations (Iredale & van Vugt, 2011). Research on the watching-eyes effect has been conducted about a variety of non-monetary forms of prosocial behaviour such as blood donation (Sénémeaud et al., 2017), voter turn-out (Panagopoulos, 2014a) and volunteering time to charity (Bereczkei et al., 2010). Bradley et al. (2018) posit that non-monetary forms of prosocial behaviour may be a more reliable signal as, at the point of donation, donating money is relatively less effortful and time-consuming than volunteering or donating blood. Therefore, as a final measure of prosocial behaviour, this study asked participants how much time per month they would be willing to volunteer to charity, and the total number of volunteer hours was compared across the three eye conditions.

**Procedure(s)**

This section will give an overview of the general experimental procedures used in studies one and two. Due to the similar nature of the studies, participants who participated in study one were not allowed to participate in study two (although due to the different settings and recruitment strategies of both studies, this was highly unlikely anyway). Using demographic data captured, it was possible to check the data for potential duplicates, and none were identified.

***Public Goods Game***

After reading an information sheet and consenting to the study, participants were randomly assigned to one of three conditions where they were exposed to either a static image of eyes (a photograph of eyes), a blinking eyes image (a short, looping video of blinking eyes), or no-eyes (a university logo), which were presented via a banner at the top of the instruction page. Underneath the banner the participants saw a set of instructions on how to play a public goods game (e.g., Burnham & Hare, 2007); the instructions were the same across all conditions.

Participants were told that they had been randomly assigned to a group with two other survey participants (in reality, there were no other participants). They were told that each ‘group member’ would shortly have to decide how much they wanted to donate to the public goods pot and how much they would like to keep for themselves. In the game instructions, participants were advised that whatever was donated to the public goods pot would be doubled and equally divided back to all players. They were advised that, therefore, how much they donated and how much they kept for themselves would affect how much money everyone else received back from the group fund.

In the public goods game, the group's total pay-off would have been maximised if everyone contributed all of their reward (Hardy & van Vugt, 2006). However, on an individual level, the best strategy was to contribute zero ensuring that the participant would have at least got the amount they started with but could have also possibly ended up with a higher amount. For example, CfP participants in study one were awarded an initial bonus of £1.50. The 'other players' contributed a pre-determined total of £1.50 to the public goods pot. If the CfP participants contributed none of their bonus to the public goods pot, all the players would have received £1 back from the public goods pot resulting in the CfP participants receiving the maximum reward of £2.50 for themselves. In contrast, if the CfP participants donated their whole bonus to the public goods pot, then they would have received the minimum reward of £2 back from the public goods pot.

The participants were then presented with a sliding scale and asked what amount of their initial bonus (in pence) they would like to donate to the public goods pot (participants could select any whole number from 0 to the maximum amount of their bonus). After donating, they were then advised of the outcome of the public good game (i.e., what their total reward amount was) and that the amount would be rounded to the nearest whole pence/cent (as applicable) and credited to their account within 72 hours.

### *Donations to Charity*

In both studies one and two, participants were given the opportunity to donate any amount of their final bonus to charity. However, the mechanism in which they could donate to charity differed between studies so is detailed within the individual chapters.

***Volunteer hours***

Participants were advised that as part of National Volunteer Month (April), information was being collected on people's willingness to participate in charitable events. They were then asked to indicate which activities they would be willing to volunteer their time to (e.g., helping to organise a fundraising event, collecting donations for charity, providing care for vulnerable groups) and also to indicate how many hours per month (in total) they would be willing to volunteer for via an open-text response.

**Ethical Considerations**

An ethics review and full risk assessment were conducted for each study. As the studies mainly took place online environment, there were only two general risks identified that were relevant to all studies: confidentiality and anonymity, and collection of information on topics of a sensitive nature (e.g., sexual orientation). Any specific risks identified for each study are discussed in the individual chapters.

To mitigate these risks, participants were advised that participation was voluntary, and all data was kept strictly confidential with no identifying information of the participant stored with the data. In line with the British Psychology Society's ethical guidelines (The British Psychological Society, 2018), consent was taken from each participant. This study was granted full ethical compliance by the Ethics Chair at Canterbury Christ Church University.

**Data Preparation and Analysis**

This section outlines the general methods used for data preparation and analysis used in this thesis. Statistical analysis was conducted using IBM SPSS Statistics 26 except for the effect size confidence intervals which could not be calculated in SPSS. These were calculated using RStudio (2022) instead. First, the testing of the manipulation checks in studies one, two, and three are outlined with an overview of the assumptions which needed to be met. Second, an outline of the data preparation prior to the main data analysis is presented, describing the measures taken to test the assumptions on the data. In studies one and two, a series of one-way ANOVAs (which compares the means of two or more independent groups) were planned to explore whether there were any significant differences across groups in the three measures of prosocial behaviour. As the data in studies one and two failed to meet the required assumptions, a non-parametric Kruskal Wallis H-test was used instead (which assesses the distribution of median scores) to analyse the prosocial measures. Finally, an overview of the effect size calculations, post hoc power analysis, marginal effects, post hoc testing, and reliability analysis approaches used in studies one, two, three, and four A are provided.

***Testing Manipulation Checks***

In studies one, two, and three, a series of chi-square tests of association were planned to explore whether the participants who had been exposed to an image of eyes correctly reported seeing an image of eyes (no/yes), correctly reported the gender of the eyes (male/female/not applicable/not sure) and whether they reported feelings of being observed whilst taking the survey (no/yes).

A chi-square test of association requires three assumptions to be met: 1) there are two categorical variables, 2) there should be independence of observations (e.g., a participant cannot be in more than one group) and 3) that all cells of the chi-square test must have expected counts greater than five. The first two assumptions relate to study design and the manipulation checks met these requirements. The last assumption was checked in SPSS before running the main analysis. In cases where the expected cell counts were less than five, a Fisher's exact test was conducted instead.

***Testing Prosocial Measures***

In studies one and two, a series of a one-way ANOVAs were planned to explore whether there were any significant differences across the three conditions (static, blinking, and no-eyes) in the amount that the participants donated to the public goods game from their initial bonus (%), the amount that they donated to charity from their final bonus (%) and the total number of hours that the participants indicated that they were willing to volunteer per month to charity activities.

**Data Preparation – Testing of Assumptions.** A one-way ANOVA requires several assumptions to be met. These include assumptions about the study design and the data produced. The first three assumptions of the one-way ANOVA relate to the study design which was met for all three prosocial measures: 1) the dependent variable is continuous, 2) the independent variable is categorical with two or more categorical groups and 3) there are independence of observations. There three remaining assumptions relate to the data: 1) there should be no significant outliers in the groups of the independent variable, 2) the dependent variable should be approximately normally distributed for each group of the independent variable, and 3) there is homogeneity of variance between groups. These are described in more detail below. If these assumptions are not met and methods to try and deal with them fail then non-parametric tests are used instead, which make less strict assumptions about the distribution of the data being analysed.

***Homogeneity of Variance.*** The homogeneity of variance assumption states that the population variance for each group of the independent variable should be similar. If the sample size in each group is similar, violation of this assumption is not serious but if sample sizes are different, a one-way ANOVA is sensitive to the violations of this assumption. Homogeneity of variance was tested using Levene's test. This tests the null hypothesis that the variances in the groups are equal, so if Levene's test is significant ($p<0.05$), then it is concluded that the null hypothesis is incorrect, and the variances are significantly different. If this happens the most common way to correct this problem is to transform the data but if this does not work (which is common) then an equivalent non-parametric test (e.g., Kruskal-Wallis test) should be used instead (Field & Hole, 2003).

***Normality.*** The normality assumption states that the dependent variable should be approximately normally distributed for each group of the independent variable. There are multiple methods in which to determine normality, in this thesis the Shapiro-Wilk test for normality was used. This is the most common numerical method used for testing normality which is advantageous as it is an objective judgement of normality (Laerd Statistics, 2021). The Shapiro-Wilk test compares the set of scores in the sample to a normally distributed set of scores with the same mean and standard deviation. If the test is significant ($p < 0.05$), then the distribution in question is significantly different from a normal distribution. If the data deviates from normality then a non-parametric test (e.g., a Kruskal-Wallis H Test) may be used instead (Field & Hole, 2003).

*Outliers.* There should be no significant outliers. This was tested by a visual inspection of box plots. Any data point more than 1.5 box lengths from the edge of their box are classified as outliers by SPSS. Any data points more than three lengths away are classified as extreme outliers. If any outliers are identified, then there are multiple ways in which to proceed (Field & Hole, 2003). First, the data should be checked for any data entry errors. If any errors are found, they should be replaced with the correct value and the tests re-run. Second, the data should be checked for measurement errors and if found, they should be removed from the analysis. Outliers should only be removed if there is a good reason to believe that the case is not from the intended sample population. However, if no data entry or measurement errors are found, then it is usually not possible to determine why there is the presence of outliers. The Kruskal-Wallis H Test is an alternative statistical test that would allow for outliers to be kept in the sample. As a non-parametric test, the Kruskal-Wallis is a ranked test, which is not sensitive to outliers (Field & Hole, 2003).

**Main Analysis.** The below is an overview of the effect size calculations, post hoc power analysis, marginal effects, post hoc testing and reliability analysis approaches used in studies one, two, three, and four A of this thesis. The methodology for study four B is detailed in the individual study.

***Effect Sizes.*** The most common method to calculate effect sizes in parametric tests (e.g., ANOVA, T-tests etc.) is Pearson's correlation coefficient (*r*), largely because commonly used statistical packages such as IBM SPSS, directly report the effect size while reporting results (Yigit & Mendes, 2018). A small effect size ($r$ = 0.10) explains 1% of the total variance, a medium effect size ($r$ = 0.30) accounts for 9% of the total variance and a large effect size ($r$ = 0.50) accounts for 25% of the variance seen within the data. A value of 1 means that the experiment completely explains the variance in the data (Cohen, 1992).

Similarly, Cramer's V can be used as an effect size measurement of the strength of association between two categorical values (e.g., in chi-square tests of association) (Field, 2009). An effect size of 0 means that the experiment had no effect as it explains 0% of the variance within the data. Cramer's V can be interpreted as follows: < .10 (very weak), < .20 (weak), < .40 (moderate), < .60 (relatively strong), < <80 (strong), > 0.8 (very strong) (Kotrlik, Williams & Jabor, 2011).

There is not a single agreed upon method of calculating the effect size for the Kruskal-Wallis test, but a common method is to follow up any significant results with a post hoc analysis using Mann-Whitney U tests for focused pair-wise comparisons. The effect sizes for these Mann-Whitney tests are then calculated using Pearson's *r* (Field & Hole, 2003). However, this method cannot be used for non-significant results as SPSS does not provide the necessary z-scores needed for the effect size calculations. An alternative effect size that could be calculated is Epsilon-squared ($\varepsilon^2$). When there is excessive deviance from normality (as seen in the prosocial measures of studies one and two), it has been suggested that Epsilon-squared ($\varepsilon^2$) gives the most unbiased effect size results (i.e., it represents the population effect size as accurately as possible) (Yigit & Mendes, 2018). Therefore, using the methodology outlined by Tomczak and Tomczak (2014), the effect sizes for any non-significant Kruskal-Wallis results were calculated using Epsilon square ($\varepsilon^2$) which provides a value from 0 (indicating no relationship) to 1 (indicating a perfect relationship). Epsilon-squared ($\varepsilon^2$) can be interpreted as follows: 0.01 to <0.08 (small), 0.08 to <0.26 (medium), and ≥ 0.26 (large) (Delvecchio et al., 2022).

***Post Hoc Power Analysis.*** Post hoc power analyses were conducted in G*Power3 (Faul, Erdfelder, Lang, & Buchner, 2007) using an alpha of 0.05 and the sample size achieved to calculate the power of each test. To conduct power analyses for non-parametric tests, a power calculation was conducted for the equivalent parametric test with 15% of the sample size removed (Develve, 2020).

The power of a test is the probability that the test will find an effect assuming that one exists in the population and the aim should be to achieve a power of 0.8, or an 80% chance of detecting an effect if one genuinely exists (Field & Hole, 2003). If a test fails to achieve a power of 0.8, this means that there is a greater chance of false-negative (type II error) and if an effect is a true but small effect, the test would have a lower probability of being able to detect it (Field & Hole, 2003). The consequences of an underpowered sample include an increased probability of a type II error (where the null hypothesis is erroneously not rejected), overestimates of effect size and low reproducibility of results (Button et al., 2013). The achieved power for each experiment is reported in each individual chapter.

***Marginal Effects.*** Statistical significance is generally accepted as $p \leq 0.05$ but it is common practice amongst psychologists that if results show a *p*-value a little greater than 0.05, then they are reported as 'marginally' significant. The argument is that if researchers use $p \leq 0.05$ as a binary cut-off for justifying scientific claims or conclusions, it can lead to erroneous beliefs and poor decision making. Results do not immediately become 'true' on one side of the divide and 'false' on the other and instead, it has been suggested that *p* values should be considered along more of a continuum (Wasserstein & Lazar, 2016). One option is to report any 'marginally significant' results, with *p* values around the region of .06 identified as acceptable to distinguish as 'marginally' significant but there is growing criticism for this practice (Pritschet, Powell, & Horne, 2016).

An alternative option for handling 'marginally' significant results is to be fully transparent in the reporting of statistical results. Under this approach, researchers should be upfront on how many and which analyses were conducted and how those analyses (including specific *p*-values) were selected for reporting (Wasserstein & Lazar, 2016). In addition, the estimation of the size of the effects in the data should be reported to supplement hypothesis testing (Iacobucci, 2005). An effect size is an objective measure of the magnitude of an observed effect and as a standardized measure, it enables a comparison of effect sizes across different studies that have measured different variables (Field & Hole, 2003). By being fully transparent and reporting all of this information in the results, it would allow for individuals reading the results to draw their own valid scientific conclusions based on *p*-values and related statistics. Therefore, in this thesis, reporting will be fully transparent with specific *p*-values and effect sizes reported for each analysis conducted.

***Equivalence tests***. A limitation of traditional hypothesis testing that it is not possible to conclude that there is no effect when p > α, - the test might simply have lacked the statistical power to detect a true effect (Lakens, Scheel, & Isager, 2017). Equivalence tests are conducted in order to explore whether any non-significant results were a true null effect or whether they were inconclusive. As the data in studies one - three failed to approximate a normal distribution, in order to conduct equivalence testing, the 90% confidence intervals (CIs) were calculated for the effect sizes with a view to compare it with the confidence intervals for the smallest effect size of interest (SESOI). If the CI of the effect size falls within the CI of the SESOI then it can be concluded that the two groups are statistically equivalent (and therefore it would strengthen any arguments for null effects) (Lakens et al. 2018). As the typical effect size seen in psychology has been reported as $r = .19$ (Gignac & Szodorai, 2016), the SESOI used in these equivalence tests are a small-medium effect size; Pearson's *r* lower: $r = -.20$, upper: $r = .20$, Cramer's *V* lower: $V = -.30$, upper: $V = .30$ (Kotrlik et al., 2011), and $\varepsilon^2$ lower: $\varepsilon^2 = -.17$, upper: $\varepsilon^2 = .17$ (Delvecchio et al., 2022).

***Post Hoc Tests.*** Post hoc tests consist of pairwise comparisons that are designed to compare all different combinations of experiment groups and are conducted after the main analysis to ascertain precisely where any possible differences lie following a significant main effect (Field, 2009). Throughout the thesis, post hoc pairwise comparisons were performed using Dunn's (1964) procedure with a Bonferroni correction for multiple comparisons. When conducting multiple tests on the same dependent variable, the chance of committing a Type I error increases, thus increasing the likelihood of coming about a significant result by pure chance. To correct for this, or protect from Type I error, a Bonferroni correction is conducted. A Bonferroni correction is a conservative correction that controls for Type 1 errors by dividing the standard acceptance criteria ($p < .05$) by the number of comparisons being made. For example, if there were three comparisons made ($0.05/3 = 0.0166$), the new acceptance criteria would be $p < .017$ (Field & Hole, 2003). It should be noted that in reducing the chance of a Type 1 error, the Bonferroni correction, by definition, increases the chance of a Type 2 error – i.e., leading to an overall more conservative assessment of significance (Perneger, 1998). Instances where the use or non-use of a Bonferroni correction had implications for the reported significance of the results have been highlighted in the results of the individual chapters.

***Reliability Analysis.*** Reliability means that a measure (in this case the nine-item public goods game used in study three) should consistently reflect the construct it is measuring (e.g., prosocial behaviour). The most common measure of reliability is Cronbach's alpha (α). Cronbach's alpha is a measure of internal consistency, that is, how closely related a set of items are as a group. Cronbach's alpha is computed by correlating the score for each scale item with the total score for each observation (usually individual survey respondents) and then comparing that to the variance for all individual item scores. The resulting α coefficient of reliability ranges from 0 to 1 in providing this overall assessment of a measure's reliability. If all of the scale items are entirely independent of one another (i.e., are not correlated or share no covariance), then $\alpha = 0$; and, if all of the items have high covariances, then α will approach 1 (Goforth, 2015). As a general rule of thumb, a value of 0.8 is seen as an acceptable value for Cronbach's alpha, with values lower than this indicating an unreliable scale (Field & Hole, 2003).

The internal consistency only needs to be assessed for one measure of prosocial behaviour: the nine-item public good game in study three. Full details are given in the specific empirical study three method section.

**Summary**

In total, this thesis consists of five studies written up into four empirical chapters. Each study was designed and constructed via an iterative process, based on the findings of the previous study to enable as robust an exploration into the watching-eyes effect as possible. This chapter provided a brief overview of the methodical approaches used in all five studies of this thesis with a summary of the aspects of the methods that were consistent across empirical chapters. This included the recruitment methods used in studies one, two, and three. The materials used in studies one, two, three, and four A. The procedures used in studies one and two, and the ethical considerations for all studies. It concluded by providing an overview of the general data processing and analysis procedures conducted across the empirical studies. There were many aspects of the methods which were specific to the individual studies so are outlined in their individual chapter method sections. This next chapter in this thesis is the first of the empirical chapters: Study One – Exploring how watching eyes impact prosocial behaviour in an online environment.

# Chapter Three – Study One: Exploring Online Prosocial Behaviour of the Watching-eyes Effect

Costly Signally Theory (CST) posits that individuals partake in behaviours that are costly to themselves (with no obvious direct benefit), such as donating time, money or even risking their life to save others, to signal a quality about themselves to potential social partners (Zahavi, 1975). As highly social creatures, humans have relied on group living to thrive and survive (van Vugt & Kameda, 2013), so any behaviour or trait that benefits the group, would help establish a positive reputation, leading to long term benefits for the individual in attracting potential mates or allies (Roberts, 1998; van Vugt & Iredale, 2013).

It has been posited that humans have evolved to be such social creatures that they possess proximate mechanisms (Burnham & Hare, 2007) that cause them to react to monitoring cues (e.g., an image of a pair of eyes) as if their reputations were at stake (Saunders et al., 2016). People have a strong motivation to manage their reputations as there are significant social costs involved when they break a cooperative norm (Fehr & Gächter, 2002). As a result, humans behave more prosocially when being watched by others (Vaish et al., 2017) and research has shown that prosocial behaviours such as generosity, morality and green behaviours are substantially enhanced even under false cues of observation (e.g. in the presence of an image of eyes) and this is known as the 'watching-eyes effect' (e.g., Bateson et al., 2015; Haley & Fessler, 2005; Sparks & Barclay, 2015).

## Previous Research

Haley and Fessler (2005) demonstrated in their seminal study that just the mere display of stylised eyes on a computer screen was enough to positively influence a person's prosocial behaviour. Since then, there have been many laboratory and field experiments using images of both real and stylised eyes (see Figure 3.1) which have supported the watching-eyes effect.

**Figure 3.1**

*The stylised eyespots used in the study by Haley and Fessler (2005)*

Significant results of the watching-eyes effect include increased donations to an honesty box (Bateson et al., 2006), increased donations to charity (Fathi et al., 2014), a reduction in littering (Bateson et al., 2013) and increased voter turn-out (Panagopoulos, 2014a). However, research has recently begun to cast doubt on the robustness of the watching-eyes effect as comprehensive meta-analyses have found no support for the watching-eyes effect on generosity (Northover et al., 2017) or moral judgment (Northover et al., 2016). Similarly, individual studies have found no effects of watching eyes on generosity (Fujii et al., 2015), reducing dishonest behaviour (Cai et al., 2015), influencing prosocial attitudes on a survey (Carbon & Hesslinger, 2011) and enhancing cooperation in a dictator game in an online environment (Raihani & Bshary, 2012). Papers that cite significant positive results report such effects as being conditional on the gender of the participant (e.g., male behaviour in a dictator game was found to be highly responsive to the presence of watching eyes whereas female behaviour was not) (Rigdon et al., 2009), chronic self-awareness of the participant (Pfattheicher & Keller, 2015), surrounding crowd density (Bateson et al., 2013), eye cue exposure length (Sparks & Barclay, 2013), perceived valence (e.g., kind vs unkind) of the eyes (Pauwels et al., 2017) and explicit over implicit eyes cues (Fehr & Schneider, 2010) to name a few (see Northover et al., 2016 for a comprehensive list). The mixed results suggest that any impact on prosocial behaviour due to watching eyes is, to some extent, dependent on the characteristics of the environment and the specific eye stimuli used. If researchers can pinpoint the characteristics of the environment and the types of stimuli that can evoke the watching-eyes effect, this would provide a simple and inexpensive method of encouraging prosocial behaviour which can be utilised in a range of applications from increasing charitable donations to, for example, reducing energy waste (Griskevicius et al., 2010).

**The Watching-eyes Effect Online**

At the time of developing the research proposal for this thesis (circa 2016), the watching-eyes effect had previously been tested on charitable donations in both a laboratory (e.g., Keller & Pfattheicher, 2011) and field experiment context (Ekström, 2012) and on cooperation in a dictator game in an online environment (Raihani & Bshary, 2012), but there had been no studies into the effects of watching-eyes on online prosocial behaviours (e.g., charitable donations). With the near-ubiquitous use of the internet in modern day-to-day life across the globe, this was deemed a novel and necessary area in which to test the watching-eyes effect.

In recent years, there has been an explosion in the number of people using the internet, with the number of global internet users increasing from 413 million in 2000 to over 3.4 billion in 2016 (Roser, Ritchie, & Ortiz-Ospina, 2015). Approximately the same number of people own a smartphone (Turner, 2020), using their devices for communicating with friends and family, work, shopping, accessing the news, entertainment and even using their phones as a digital wallet (Edmonds, 2018). With almost half of the world's population utilising the internet for so many aspects of their day-to-day lives, it is not surprising that almost a quarter (23%) of UK charitable donations are now made online (CAF, 2019). However, according to the UK Giving 2019 report (CAF, 2019), the proportion of the British public who either gave money to charity directly or sponsored a friend or family had declined year on year since 2016. This situation was only exacerbated with the COVID-19 Global pandemic in 2020 (May, 2020). With the shift towards digital charity giving, it is increasingly important for the sector to develop innovative methods to attract web visitors and encourage online donations (Green, 2021). In principle, the watching-eyes effect requires nothing more than displaying an image of eyes which is simple and straightforward to utilise in an online environment. If the watching-eyes could be evoked in this context, it could pave the way for a range of low-cost, but potentially high-impact, online interventions to increase charitable donations. Moreover, the potential impacts are not limited to the charity sector; there are a range of other online issues where the watching-eyes effect could be of value, for example in deterring anti-social behaviour such as cyber-bullying (Dear et al., 2019).

Online testing also allows for large-scale sampling of non-Western, Educated, Industrialised, Rich and Democratic (WEIRD) populations (Saunders et al., 2016). This is a key issue within psychology, as studies have shown that up to 96% of psychological research relies on data from exclusively WEIRD samples (Rad et al., 2018). By utilising mainly WEIRD samples, researchers fail to capture the diversity in human psychology, behaviour, and norms, and erroneously generalise the findings from one population to another (Apicella et al., 2020). However, there are now several online social research platforms that enable researchers to recruit participants from non-WEIRD populations in a relatively straightforward and cost-effective manner (Saunders et al., 2016). This study utilised two of these platforms, Amazon's MTurk (MTurk) and an alternative platform, 'Call for Participants' (CfP), to test the watching-eyes effect in an online environment.

Since 2010, the use of sites such as MTurk and CfP has been growing steadily in online social science research (Paolacci et al., 2010). These crowd-sourcing platforms allow researchers to recruit large numbers of participants to complete tasks that computers are not able to accomplish (e.g., expressing personal sentiments), while facilitating the payment of participants for their time (Raihani & Bshary, 2012). This method of data collection is quick and inexpensive as well as having the added benefit of being able to draw upon a more diverse population sample (Saunders et al., 2016). However, previous research which has used MTurk to explore the watching-eyes effect (e.g., Raihani & Bshary, 2012; Saunders et al., 2016) did not find any evidence that eye images influenced online prosocial behaviour. In both studies, participants were provided with the potential to earn a financial bonus and then the amount of that bonus they decided to donate to a dictator game (Raihani & Bshary, 2012) and to charity (Saunders et al., 2016) was used as the prosocial measure.

One explanation for why no positive effect was found could be that, as participants sign up to MTurk with the specific aim to earn money, it would not be surprising if they were reluctant to donate their bonuses in this setting (as seen in discussions on MTurk forums[2]). Nevertheless, an alternative platform, 'Call for Participants' (CfP) could provide a solution. CfP is an open platform that promotes academic research studies that anyone can take part in. Like MTurk, participants are from around the world (140,000 participants from over 207 countries[3]), but unlike MTurk, participants are not necessarily compensated for their time in money. For example, when recruiting participants, researchers can choose to offer a prize draw to earn vouchers as an incentive or if they are unable to offer financial incentives, they can just offer 'sincere gratitude' (the incentive is set by the researcher on a per study basis). Therefore, as financial incentives are not the sole purpose for signing up to the platform, CfP participants may be a better participant source to explore whether eye images induce prosocial behaviour. However, the effectiveness of participant recruitment via CfP was unknown at the time of planning this research (circa 2016). Even now, although there are numerous published studies that have used MTurk to recruit their participants (e.g., Pfattheicher & Keller, 2015; Raihani & Bshary, 2017; Saunders et al., 2016), there appears to be a lack of published papers that have used CfP to recruit their participants. Therefore, to

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[2]MTurk forums: https://www.reddit.com/r/mturk/comments/aw7cfg/how_much_of_your_bonus_would_you_like_to_donate/ [Accessed on 20 Feb 2021]
[3] https://www.callforparticipants.com/about [Accessed on 20 Feb 2021]

mitigate any potential participant recruitment risk, it was decided to recruit all participants via CfP but use MTurk in the event of low recruitment numbers.

Another explanation for why no evidence was found for the watching-eyes effect in the MTurk studies could be that the eye cues failed to evoke feelings of being watched in the participants. It has been suggested that previous online studies of the watching-eyes effect (e.g., Raihani & Bshary, 2012; Saunders et al., 2016) have failed to find significant results because the online environment is a truly anonymous setting and if people believe that they are in an anonymous setting (e.g., their actions will not be seen by anyone else), the image of watching-eyes will fail to trigger reputational concerns (Lamba & Mace, 2010; Tane & Takezawa, 2011). It seems it is this belief of being watched by another person which is critical in generating the watching-eyes effect (Conty et al., 2016).

***Eye Stimuli***

A key point in eliciting the watching-eyes effect appears to be in making people *feel* like they are being watched (Bateson et al., 2006), hence the salience of the eye images (i.e., how noticeable the eyes are) may play an important role in evoking this feeling. In a field experiment, Krátký et al. (2016) found that increasing the salience of the eye stimuli by using 3D eye cues was more successful in triggering contributions to an honesty box than using 2D cues. They argued that 3D cues are less sensitive to false cue detection and this higher activation of agency detection triggers stronger responses and thus amplifies prosocial behaviour. Due to technological restrictions, using 3D eye cues is a challenge in an online environment; although the technology exists, 3D computer displays at the time of this research (circa 2016) are a niche luxury that the majority of the general public would not have access to (Kyrnin, 2020). Alternatively, embedding a short video of blinking eyes instead of using a static image of eyes, may make the eyes seem more realistic and assist in evoking a feeling of being watched in the participant (Manesi et al., 2016). Therefore, the first study set out to specifically test whether the salience of eyes in an online environment has an impact on the watching-eyes effect. The study examined whether there is a difference in participant prosociality (i.e., prosocial behaviour) when exposed to either a static image of eyes (a photograph), blinking eyes (a short video of blinking eyes) or no-eyes (a university logo).

***Prosocial Measures; Public Goods Game, Charity Giving, Donating Time***

As prosocial behaviour is a socially desirable trait, self-reports on altruistic behaviour in surveys are unreliable as they are prone to response biases such as a social responsibility bias or self-presentation effects (Bekkers, 2007). To overcome this, researchers have applied game theory and utilised economic games to measure a range of prosocial behaviours (e.g., see Northover et al., 2017). Within the watching-eyes literature, Haley and Fessler (2005) first utilised the dictator game to explore how cues of being watched (i.e., eye images) impact prosocial behaviour, with other researchers following suit either by utilising the same game or a public goods game (e.g., Burnham & Hare, 2007).

As social exchanges very often involve one time interactions with strangers who are not able to reciprocate later (Wu et al., 2019), a one-shot public goods game is a particularly good proxy for a real-world setting. A public goods game captures a key qualitative feature of many common human interactions in the past (such as teamwork in food production, cooperative childrearing, and warfare); whereas the collective benefits from cooperation, each individual would be materially better off by free-riding on the others (Alger, 2010). In a public goods game, participants are told they are playing a game with other people and that they have the chance to earn some money. They are awarded an initial sum of money which they can either keep for themselves or invest into a public goods pot. Whatever amount they put into the pot is doubled and then distributed evenly back to each of the players (e.g., Burnham & Hare, 2007). Therefore, this study utilised the public goods game as the first measure of prosocial behaviour. Specifically, it compared the percentage of their initial bonus that the participants choose to invest in the public goods pot across the three eye conditions (static, blinking, no-eyes).

Some psychological and economic research has questioned the validity of measuring prosocial behaviour using an economic game approach due to game restrictions and poor external validity (Cañigueral & Hamilton, 2019a). For example, game restrictions may be caused by participants who have not understood the instructions fully or grasped the possible outcomes of the economic game. To address this in this thesis, a small pretest was conducted before launching the first study to check participant understanding of the public goods game instructions and the possible outcomes. Another consideration of using an economic game approach is poor external validity, with a recent meta-analysis concluding that economic games do a poor job of explaining behaviour in naturally occurring settings (Galizzi & Navarro-Martinez, 2019). A complementary prosocial measure that is more analogous to a naturalistic setting and more intuitive to understand is donations to charity. However, as an established methodology in which to measure the watching-eyes effect, the public goods game (e.g., Burnham & Hare, 2007) is still a useful measure of prosocial behaviour to include and has the added benefit of providing a mechanism in which participants can earn the money which they may then donate to charity. As participants would be advised of their total winning bonus before being asked how much they would like to donate to charity, using donations to charity as a prosocial measure would also negate any potential issues of some participants not fully understanding the instructions or possible outcomes of the game. Therefore, as a second measure of prosocial behaviour, this study provided the participants with an opportunity to donate their winnings to charity and compared the percentage of their final bonuses that the participants gave away to charity across the three eye conditions.

Altruistic behaviour can be displayed in many forms, including in non-monetary decisions or situations (Iredale & van Vugt, 2011). Research on the watching-eyes effect has been conducted about a variety of non-monetary forms of prosocial behaviour such as blood donation (Sénémeaud et al., 2017), voter turn-out (Panagopoulos, 2014a) and volunteering time to charity (Bereczkei et al., 2010). Bradley et al. (2018) posit that non-monetary forms of prosocial behaviour may be a more honest signal as at the point of donation, donating money is relatively less effortful and time-consuming than volunteering or donating blood. Therefore, as a final third measure of prosocial behaviour, this study asked participants how much time per month they would be willing to volunteer to charity and the total number of volunteer hours was compared across the three eye conditions.

**Aim of Study One**

The main aim of this study was to test whether the presence of eye cues positively affected prosocial behaviour in an online environment; specifically, the amount donated to a public goods pot, the amount donated to charity, and the total numbers of hours participants indicated that they would be willing to volunteer their time for. A secondary aim of this study was to test whether enhancing the salience of the eye cues (e.g., blinking vs. static eyes) had an increased impact on those prosocial behaviours.

**Hypotheses**

Based on the literature the *a priori* hypotheses were:

***Hypothesis 1:***

It was hypothesised that there would be a significant difference between conditions in the percentage of their initial bonus that participants choose to donate to the public goods game.

***Hypothesis 2:***

It was hypothesised that there would be a significant difference between conditions in the percentage of their final bonus that participants choose to donate to charity.

***Hypothesis 3:***

It was hypothesised that there would be a significant difference between conditions in the number of hours per month participants indicated they would be willing to volunteer their time for charitable activities.

## Methodology

### Participants

Participants had a minimum age requirement of 18 years old in line with the University's and the British Psychological Society's code of ethics (The British Psychological Society, 2018) and there were no other set participant criteria. Participants were recruited via CfP ($N$ = 142) and MTurk ($N$ = 61).

***Call for Participants (CfP).***

In total 142 participants from CfP were included in the analysis (37 male, 105 female, $M$age = 35.01, $SD$age = 11.33). Initially, 169 participants were recruited via CfP but only participants who completed more than 96% of the survey questions (i.e., participants who had completed the dependent variable questions) were included in the final analysis. In total 27 participants were excluded from the analysis; 26 participants were excluded for not completing the whole survey and one participant was excluded for having already completed the survey once. These exclusions were not pre-registered.

Participants represented 39 different self-reported ethnicities (see Table 3.1) from 11 different countries (see Table 3.2) and 17 different religions (see Table 3.3). In line with other similar research projects on the CfP website, the incentive for this study was an entry into a prize draw with a one in 10 chance of winning a £20 love2shop voucher (i.e., the incentive was not guaranteed) and participants were not advised of the potential bonus in advance of signing up to the study. CfP did not have the facility to enable bonus payments to the participants like some other websites (e.g., MTurk) so CfP participants were advised, before signing up to the survey, that they needed to provide an email address linked to a valid PayPal account and had to consent to this information being passed onto the University’s accounts department and the Research and Knowledge Exchange programme for research expense purposes. No participant queried this request.

***MTurk***

In the second attempt for recruitment for this study, 68 participants were recruited in total via MTurk and paid $2.00 to complete the 20-minute survey. This is above the equivalent US federal minimum hourly wage of $7.25 per hour as recommended by Amazon as fair payment to the workers (Doyle, 2021). Only participants who completed more than 96% of the survey questions (i.e., participants who had completed the dependent variable questions) were included in the final analysis; seven participants were excluded for not completing the whole survey so in total 61 participants were included in the analysis (42 male, 19 female, $M$age = 33.03, $SD$age = 7.93). This exclusion was no pre-registered. Participants represented 16 different self-reported ethnicities (see Table 3.1) from five different countries (see Table 3.2) and 14 different religions (see Table 3.3). Participants were not advised of the potential bonus prior to completing the survey.

**Table 3.1**

*Participant's self-reported ethnicity by source*

| CfP | |
|---|---|
| White British | 53 |
| Caucasian/white | 34 |
| British | 14 |
| English | 4 |
| White other | 4 |
| Greek | 3 |
| Chinese | 2 |
| Didn't Say | 2 |
| Dutch | 2 |
| English | 2 |
| Indian | 2 |
| White European | 2 |
| Anglo-Celtic | 1 |
| Asian Mixed | 1 |
| Black African | 1 |
| British Scottish | 1 |
| Caucasian Mixed | 1 |
| European | 1 |
| French | 1 |
| Irish Jew | 1 |
| Latino | 1 |
| Mediterranean white | 1 |
| Mixed Asian/white | 1 |
| Mixed Chinese/English | 1 |
| Mixed White | 1 |
| Sardinian | 1 |
| Scottish | 1 |
| Spanish | 1 |
| UK | 1 |
| White Welsh | 1 |

| MTurk | |
|---|---|
| Caucasian/white | 24 |
| Asian | 19 |
| American | 3 |
| European | 2 |
| Indian | 2 |
| African American | 1 |
| Black | 1 |
| East Asian | 1 |
| Hindu | 1 |
| Javanese | 1 |
| Mexican, Latino | 1 |
| Native American | 1 |
| Romanian | 1 |
| South Asian | 1 |
| White British | 1 |
| White USA | 1 |

*Note:* Order of ethnicities is determined by largest number first followed by alphabetical order.

**Table 3.2**

*Participant's self-reported country of residence by source*

| CfP | | MTurk | |
|---|---|---|---|
| UK | 60 | USA | 36 |
| England | 59 | India | 22 |
| USA | 11 | England | 1 |
| Wales | 3 | Indonesia | 1 |
| Italy | 3 | Romania | 1 |
| Australia | 1 | | |
| Great Britain | 1 | | |
| Netherlands | 1 | | |
| Philippines | 1 | | |
| Scotland | 1 | | |
| UAE | 1 | | |

*Note:* Order of country of residence is determined by largest number first followed by alphabetical order.

**Table 3.3**

*Participant's self-reported religion by source*

| CfP | | MTurk | |
|---|---|---|---|
| None | 58 | Hindu | 16 |
| Christian | 27 | Christian | 12 |
| Atheist | 20 | None | 8 |
| Agnostic | 13 | Agnostic | 7 |
| Spiritual | 5 | Atheist | 5 |
| Catholic | 4 | Catholic | 5 |
| Church of England | 2 | Baptist | 1 |
| Humanist | 2 | Islam | 1 |
| Pagan | 2 | Jainism | 1 |
| Scientist | 2 | Jewish | 1 |
| Buddhist | 1 | Pagan | 1 |
| Christian Orthodox | 1 | Protestant Christian | 1 |
| Islam | 1 | Sikh | 1 |
| Nominal Christian | 1 | Wiccan | 1 |
| Presbyterian | 1 | | |
| Protestant Christian | 1 | | |
| Taoism | 1 | | |

*Note:* Order of religions is determined by largest number first followed by alphabetical order.

**Pre-tests**

***Eye Stimuli and Emotion***

Research has suggested that the valence (e.g., kind/unkind) of the eye stimuli may help shape individuals' expectations and subsequent behaviour (e.g., Pauwels et al., 2017). As such, this study originally included seven different conditions, where in addition to the control condition, each eye condition displayed either 'static' or 'blinking' eyes images that were happy, angry, or sad.

A small pre-test (*N* = 48) was conducted to check that the eye images used in this study conveyed the intended emotion (please follow the link[4] to view the pre-test). Participants were shown a series of nine eye images (see Figure 3.2); three 'happy', three 'angry' and three 'sad' and were asked what emotions they thought the eyes were portraying (images were taken from Adobe stock photos - © stock.adobe.com). The images with the highest percentage of correct responses were selected to use in the main survey (see Figure 3.3). After the pre-test, it was decided to amend the conditions to just three conditions (static, blinking and no-eyes) to allow for an initial exploration of the effect of eye salience on the watching-eyes effect and to adapt to time and budget constraints. The emotion of the eyes was explored again in study three.

For the static and blinking conditions, eye images that depicted happy, female eyes were used. It was decided to use eyes that depicted happiness as other studies (e.g., Pauwels et al., 2017) theorised that 'kind' eyes may create the impression of trustworthiness and 'happy' faces are more salient than 'angry' faces (Becker et al., 2011). Female eyes were used rather than male ones as they are less likely to be consciously acknowledged (Conty et al., 2016) and studies have shown that people rate female facial features as more attractive than

[4] https://cccusocialsciences.az1.qualtrics.com/jfe/form/SV_bPetH9xObHdlY6q

male facial features (e.g., Vrouwe & Balliet, 2014). As Vrouwe and Balliet (2014) posit, the increased attractiveness could lead to increased attention to feminine watching cues, which in turn could lead to an increase in prosocial behaviour.

A short clip of a pair of eyes blinking was purchased from Shutterstock (copyright: Federico Marsicano) and a still image of the eyes was taken from this video. The video/image was then incorporated into a banner which displayed the Canterbury Christ Church University's (CCCU) logo and a speech bubble advising the participant that they have been awarded a bonus amount. In the no-eyes condition, just the University's logo was used with some text below advising that the participants had been awarded a bonus.

### *Checking Understanding of Game Instructions*

An additional pre-test was conducted (*N* = 68) to ensure that the instructions for the public goods game were clear. As the dependent variables were contingent on the results of the public goods game, it was imperative to check that the participants understood what was being asked of them. The pre-test was created on Qualtrics, and the link shared via social media (Facebook and Twitter). Participants received no incentive to complete the pilot and provided no personal data. Participants were presented with the instructions to the public goods game and a series of multiple-choice questions (see Appendix A) to ensure they had understood the instructions.

In total 68 participants (80.88%) answered all four questions correctly. The percentage of participants who answered the questions correctly did decrease from question one to question four. This would be expected to some degree as participants could only get questions two to four correct if they had got the previous questions correct. It was decided, given that 80.88% of all participants correctly answered question four (which was dependent on getting the previous three questions correct), this was a high enough proportion to indicate that participants understood the public goods task (see Figure 3.4).

**Figure 3.2**

*Images used in pre-test*

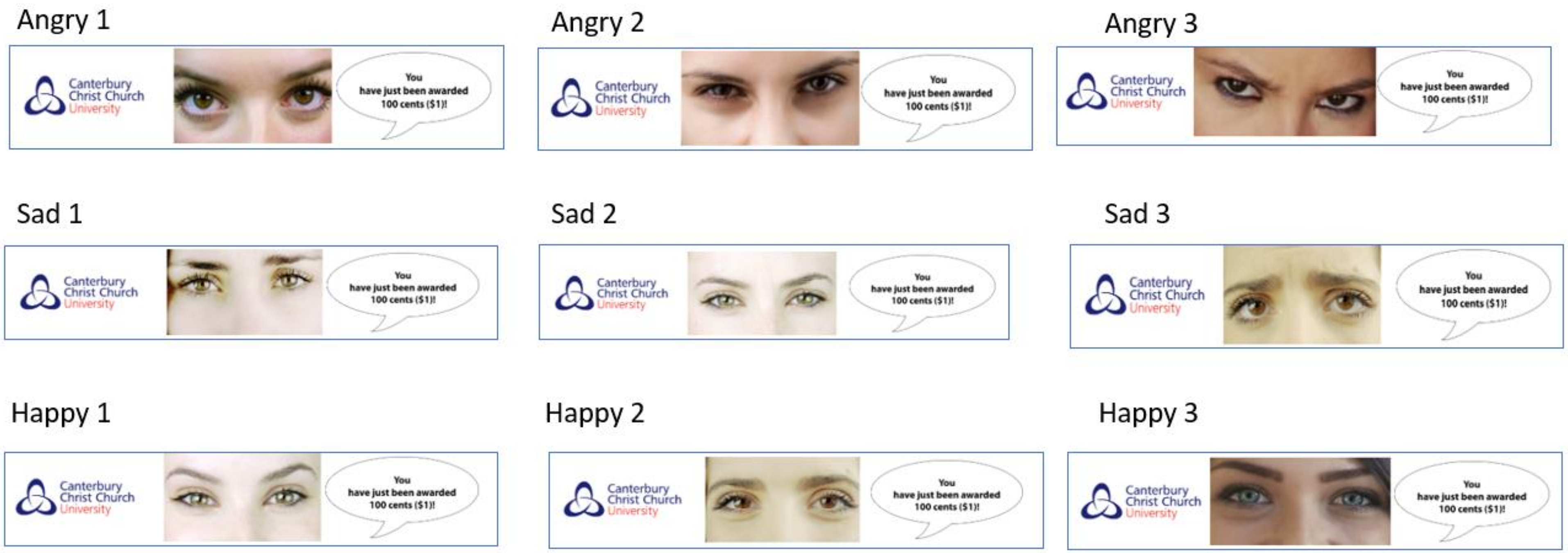


**Figure 3.3**

*Percentage of participants who correctly identified the emotion in each image*

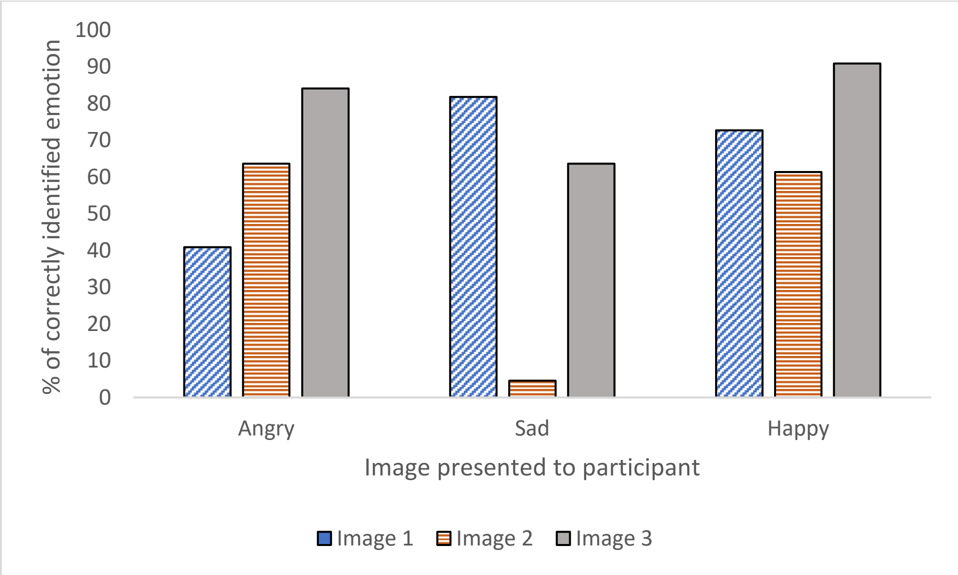
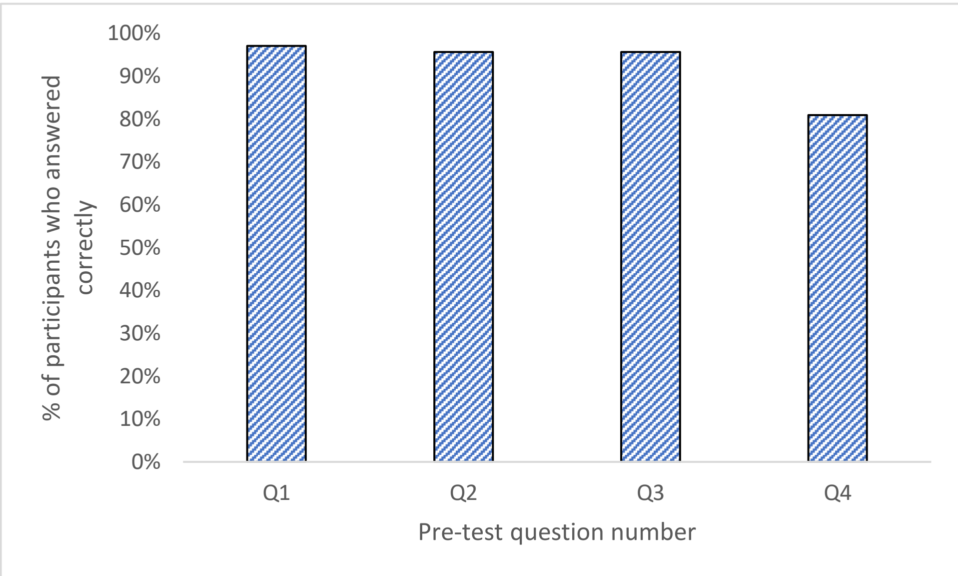



**Figure 3.4**

*Percentage of participants who answered question four correctly*



Any potential limitations caused by participants not understanding the public goods task may be mitigated by the addition of using ‘donations to charity’ as a prosocial measure. Participants were advised of their total winning bonus before being asked if and how much they would like to donate to charity which would likely negate any potential issues of some participants not understanding the instructions or possible outcomes of the public goods game correctly.

**Design**

Participants were recruited to complete an online survey where they were randomly assigned to one of three conditions (static, blinking or no-eyes) in a one-way ANOVA, between-subjects design. The dependent variables were ‘prosocial behaviour’ which consisted of 1) the percentage of their initial bonus that the participant donated to the public goods game (0 - 100%), 2) the percentage of their final bonus that the participant donated to charity (0 - 100%), and 3) the number of hours per month that the participant indicated that they were willing to volunteer for (this ranged from 0 - 120 hours via a free-text box).

**Procedure**

In both groups of participants, the survey took approximately 20 minutes to complete. CfP participants were given an initial £1.50 bonus and had the potential to win up to a maximum of £2.50, MTurk participants were given an initial $1 bonus and had the potential to win up to a maximum of $1.13 via a one-shot public goods game. The difference in potential bonus amounts between MTurk and CfP was due to the differences in bonuses generally offered on each site – each amount was reflective of the bonuses offered in other studies.

After reading an information sheet and consenting to the study, participants were randomly assigned to one of three conditions where they were exposed to either a static image of eyes (a photograph of eyes), a blinking eyes image (a short, looping video of blinking eyes), or no-eyes (the Canterbury Christ Church University logo, see Figure 3.2), which were presented via a banner at the top of the instruction page (see Figure 2.3 in Chapter Two – General Methodology). This was the only time during the survey that participants were exposed to the eye stimuli[5]. Underneath the banner the participants saw a set of instructions on how to play a public goods game (e.g., Burnham & Hare, 2007); the instructions were the same across all conditions.

Participants were then advised that they had been randomly matched up with two other survey participants (in reality, there were no other participants) and that each group member would shortly have to decide how much they wanted to donate to the group fund and how much they would like to keep for themselves. In the game instructions, participants were advised that whatever was donated to the public goods pot would be doubled and equally divided back to all players. They were told that therefore, how much they donated and how much they kept for themselves would affect how much money everyone else received back from the group fund. In the public goods game, the group's total pay-off would have been maximised if everyone contributed all of their reward (Hardy & van Vugt, 2006). However, for the individual to maximise their payoff, the best strategy was to contribute zero ensuring that the participant would at least get the amount they started with but could also possibly end up with a higher amount. For example, the CfP participants in study one were awarded an initial bonus of £1.50. The 'other players' contributed a pre-determined total of £1.50 to the public goods pot. If the CfP participants contributed none of their bonus to the public goods pot, all the players would have received £1 back from the public goods pot resulting in the CfP participants receiving the maximum reward of £2.50 for themselves. In contrast, if the CfP

[5] The length of time that the stimuli was presented to the participants for was not recorded for this study, but it is estimated that participants were exposed for less than one minute. This estimation is based on data from study two which indicated that the eyes were present at the top of the instructions page for an average of 48.54 seconds.

participants donated their whole bonus to the public goods pot, then they would have received the minimum reward of £2 back from the public goods pot.

The participants were then presented with a sliding scale asked what amount of their initial bonus that they would like to donate to the public goods pot (participants could select any whole number from 0 to the maximum amount of their bonus). After donating, they were then advised what their total reward amount was and that the amount would be rounded to the nearest whole pence/cent as applicable) and credited to their account within 72 hours.

Participants were then advised that as part of National Volunteer Month (April), they were being given the opportunity to donate some of their final bonus amount to charity (the charity was left anonymous at this stage so the choice of charity could not bias whether participants chose to donate) but any donations were completely voluntary. Participants indicated how much they would like to donate in a free-text box. Participants were then advised that as part of National Volunteer Month, information was being collected on people's willingness to participate in charitable events. They were then asked to indicate which activities they would be willing to volunteer their time to (e.g., helping to organise a fundraising event, collecting donations for charity, providing care for the elderly, and providing care for the disabled) and asked how many hours per month they would be willing to volunteer for.

The participants were taken through nine items of manipulation checks to try and ascertain whether the presence of an image of a pair of eyes was successful in making them feel like they were being observed. For example, whether they felt like they were being observed whilst completing the survey (and if so, to explain why), whether or not they saw an image of eyes during the task and if so, what was the gender of the eyes, what emotions the eyes were displaying.

Participants then completed survey items that covered a range of demographic and individual factors potentially associated with prosociality (e.g., Saunders et al., 2016). Basic demographic data were collected on age, sex, ethnicity, country of residence and education (age the participant left compulsory schooling and highest educational qualification achieved). Participants were also asked about a range of potential covariates for prosocial giving including sexual orientation, relationship status, religion, income, number of dependent children, and how charitable the participant considered themselves to be. These questions were asked after the prime (e.g., after the eye stimuli were displayed) and donation opportunity so they did not interfere with the effects of the prime (please see Section 2.3.2 of the methodology chapter for a full overview and discussion of all survey questions).

***Data Analysis***

A series of one-way ANOVAs were planned to explore whether there were significant differences across the three conditions in the amount (0-100%) that participants donated to the public goods game from their initial bonuses, the amount (0-100%) that they donated to charity from their final bonuses, and the number of hours (this ranged from 0-120 hours via free-text response) that the participants indicated that they were willing to volunteer per month to charity activities. However, the distribution of the amount that the participants donated to the public goods game and donated to charity, and the number of hours that they were willing to volunteer to charity, were found to not be normally distributed as assessed by Shapiro-Wilk's test ($p < .05$). As the data was strongly negatively skewed and strongly positively skewed (i.e., a bimodal distribution), reflect and logarithmic transformations were applied respectively but they failed to transform the data to approximate a normal distribution. Therefore, non-parametric, Kruskal-Wallis H tests were used instead. The effect sizes for any significant results were calculated using Pearson's *r* by follow-up post hoc analysis using Mann-Whitney tests for focused pairwise comparisons (Field & Hole, 2003). The effect sizes for any non-significant results were calculated using Epsilon square ($\varepsilon^2$) instead (Tomczak and Tomczak, 2014).

A series of chi-square tests were conducted between conditions (static/blinking/no-eyes) to check whether the participants who had been exposed to an image of eyes correctly reported seeing an image of eyes (no/yes/not sure), what gender they thought the eyes were (male/female/not sure/N/A) and whether they felt like they were being watched (no/yes).

An *a priori* power analysis was conducted using G*Power3 (Faul et al., for a one-way ANOVA with three groups to be able to obtain a medium effect size (0.25) with an alpha of 0.05 and power of 0.80. Results showed that a total sample of 158 participants was required to achieve a power of .80. However, due to the non-parametric tests, a post hoc power analysis for a one-way ANOVA with 15% of the sample size removed was conducted for the was conducted for the Kruskal-Wallis H tests (e.g., Develve, 2020). The results revealed that overall, the Kruskal-Wallis H tests achieved a power of 0.99 for a large effect size (0.4), 0.84 for a medium effect size (0.25) and 0.20 for a small effect size (0.1). The chi-square tests achieved a power of 0.99 for a large effect size (0.5), 0.98 for a medium effect size (0.3) and 0.23 for a small effect size (0.1), and the Mann-Whitney U tests achieved a power of 0.91 for a large effect size (0.5), 0.51 for a medium effect size (0.3) and 0.10 for a small effect size (0.1) (see Table 3.4).

**Ethics**

In line with the British Psychology Society's ethical guidelines (The British Psychological Society, 2018), consent was taken from each participant. It was made clear that participation was voluntary, and all data is kept strictly confidential with no identifying information of the participant stored with the data. This study was granted full ethical compliance by the Ethics Chair at Canterbury Christ Church University (Ref: 16/SAS/365C).

**Table 3.4**
*A summary of the power achieved for each sample*

| | Kruskal-Wallis H Tests* | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Overall ($N$ = 173) | | | CfP ($N$ = 121) | | | Mturk ($N$ = 58) | | |
| Effect Size | Small (0.1) | Medium (0.25) | Large (0.4) | Small (0.1) | Medium (0.25) | Large (0.4) | Small (0.1) | Medium (0.25) | Large (0.4) |
| Power | 0.20 | 0.84 | 0.99 | 0.15 | 0.68 | 0.98 | 0.09 | 0.36 | 0.76 |
| | **Chi-square tests** | | | | | | | | |
| | **Overall ($N$ = 203)** | | | **CfP ($N$ = 142)** | | | **MTurk ($N$ = 68)** | | |
| Effect Size | Small (0.1) | Medium (0.3) | Large (0.5) | Small (0.1) | Medium (0.3) | Large (0.5) | Small (0.1) | Medium (0.3) | Large (0.5) |
| Power | 0.23 | 0.98 | 0.99 | 0.17 | 0.90 | 0.99 | 0.10 | 0.59 | 0.97 |
| | **Mann-Whitney U Tests**** | | | | | | | | |
| | **Overall** | | | | | | | | |
| Effect Size | Small (0.1) | Medium (0.3) | Large (0.5) | | | | | | |
| Power | 0.10 | 0.51 | 0.91 | | | | | | |

*Note:* The post-hoc power analysis were conducted using G*Power 3.1.9.4, an alpha of 0.05 and the achieved sample size for each test.
*Power analysis on Kruskal-Wallis tests were calculated with 15% of the sample removed from the original sample size.
** The Mann-Whitney U tests formed part of exploratory analysis which were not pre-planned. These tests are detailed in the results section.

## Results

In total, 203 participants were randomly assigned to the 'static' ($N$=68), 'blinking' condition ($N$=67) or the 'no-eyes' condition ($N$=68). The three conditions were approximately evenly distributed across MTurk and CfP participants. Participants consisted of 124 females and 79 males ranging in age from 18 to 62 ($M$= 33.61, $SD$ = 9.83).

### Hypothesis 1 (Public Good Game):

It was hypothesised that there would be a significant difference between conditions in the percentage of their initial bonus that participants choose to donate to the public goods game.

#### *Amount donated to the public goods game*

A Kruskal-Wallis H test was conducted to explore whether there were any significant differences between conditions in the percentage of their initial bonus (0-100%) that participants donated to the public goods pot. There were no significant differences between the static (*Mdn* = 80.50, IQR = 47.75 - 100.00), blinking (*Mdn* = 100, IQR = 50.00 - 100.00) and no-eyes (*Mdn* = 100, IQR = 61.75 - 100.00) condition in the percentage donated to the public goods pot, $H(2) = 2.711$, $p = .258$, $\varepsilon^2 = 0.01$, 90% CI [0.00,1.00] (see Figure 3.5).

**Hypothesis 2 (Charitable Donations):**

It was hypothesised that there would be a significant difference between conditions in the percentage of their final bonus that participants choose to donate to charity.

***Amount Donated to Charity***

A Kruskal-Wallis H test was conducted to explore whether there were any significant differences between conditions in the percentage of their initial bonus that participants donated to charity (0 - 100%). There were no significant differences between the static (*Mdn* = 24, IQR = 0.00 – 100.00), blinking (*Mdn* = 50, IQR = 0.00 – 100.00) and no-eyes (*Mdn* = 63, IQR = 0.00 – 100.00) condition in the percentage donated to charity, $H(2) = 3.583$, $p = .167$, $\varepsilon^2 = 0.02$, 90% CI [0.00, 1.00] (see Figure 3.6).

**Hypothesis 3 (Volunteering time):**

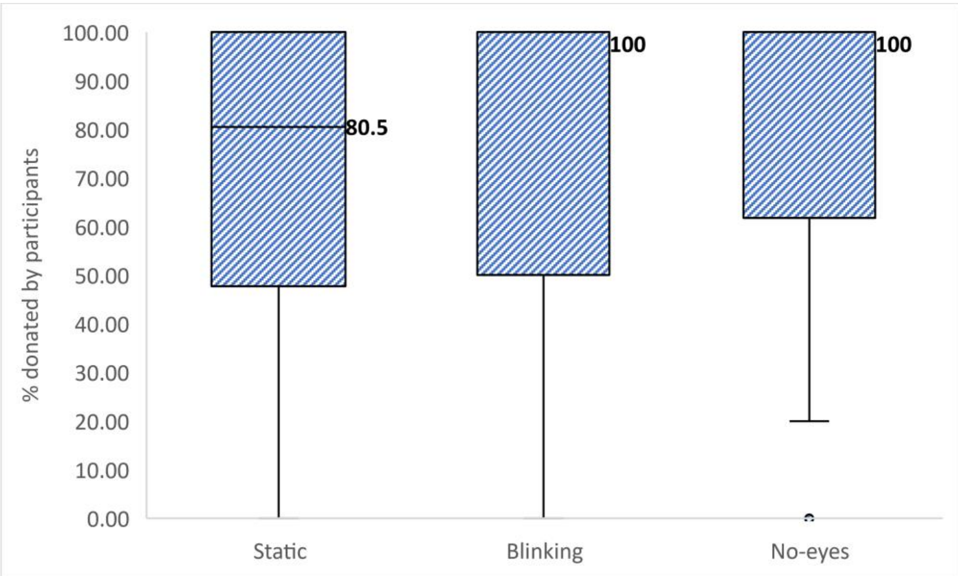
It was hypothesised that there would be a significant difference between conditions in the number of hours per month participants indicated they would be willing to volunteer their time for charitable activities.

***Amount of Volunteer Hours***

A Kruskal-Wallis H test was conducted to explore whether there were any significant differences between conditions in the number of hours per month (participant responses ranged from 0 to 120 hours via a free-text box) that participants indicated that they were willing to volunteer to charity. There was a significant difference between the static (*Mdn* = 10, IQR = 2.00 – 20.00), blinking (*Mdn* = 5, IQR = 0.00 – 10.00) and no-eyes (*Mdn* = 8, IQR = 2.25 – 19.25) condition in the number of volunteer hours, $H(2) = 6.371$, $p = .041$. Pairwise comparisons were performed using Dunn's (1964) procedure with a Bonferroni correction for multiple comparisons. Statistical significance was accepted at $p < .016$ (0.05/3). The post hoc analysis revealed a marginal significant difference in the number of volunteer hours between the blinking and static groups ($p = .052$, $r = 0.21$, 90% CI [0.07, 0.34]), but not between the blinking and no-eyes groups ($p = .164$, $r = 0.17$, 90% CI [0.03, 0.30]), and the static and no eyes groups ($p = 1.000$, $r = -0.03$, 90% CI [-0.17, 0.11]) (see Figure 3.7).

**Figure 3.**5

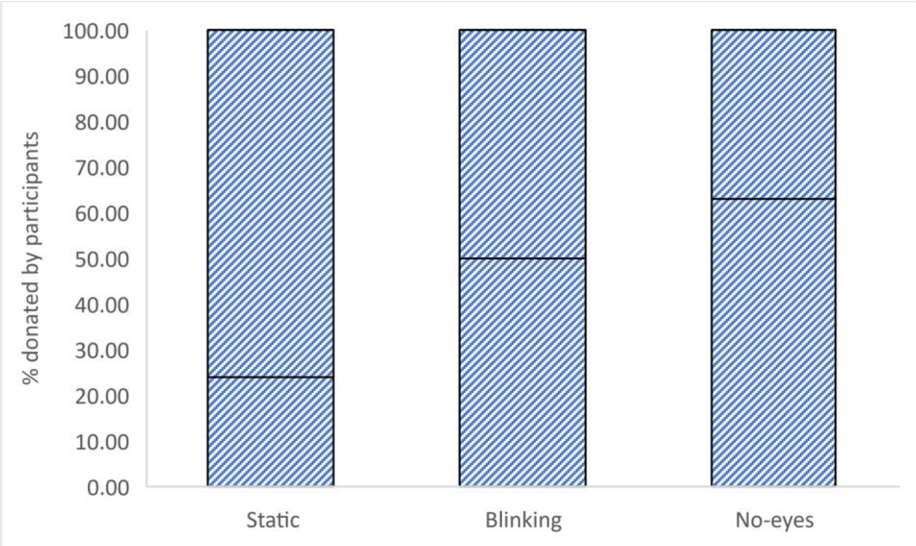
*Percentage of their initial bonus participants donated to the group fund*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show outliers. Due to ceiling effects, data labels are also used to demonstrate median.

**Figure 3.6**

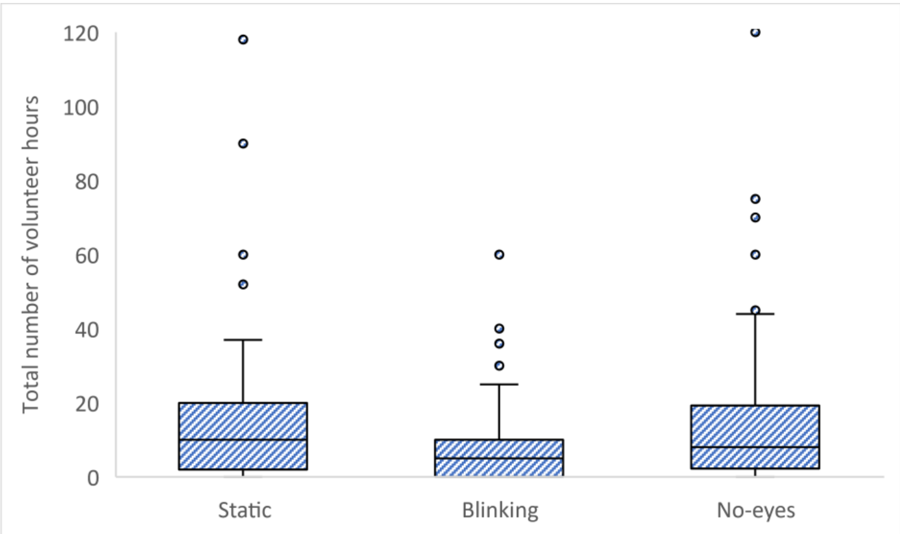
*Percentage of their final bonus participants donated to charity*




*Note.* The box spans the minimum and maximum values/25th and 75th quartiles, and the bold horizontal line indicates the median.

**Figure 3.7**

*Total number of participant volunteer hours*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show outliers.

**Manipulation Checks**

Manipulation checks included asking participants whether they saw the eyes, what gender they thought the eyes were and whether they felt like they were being watched.

To check whether the participants who had been exposed to an image of eyes correctly reported seeing an image of eyes, a chi-square test was conducted between the condition (static/blinking/no-eyes) and whether participants reported seeing an image of eyes (no/yes/not sure). As expected, there was a significantly higher proportion of participants who reported seeing an image of eyes in the blinking condition (54%), followed by the static condition (37%) and then the no-eyes condition (3%), $\chi^2$K(4, $N$ = 203) = 47.762, $p$ < .001. Post hoc analysis involved pairwise comparisons with a Bonferroni correction. Statistical significance was accepted at $p$ < .016 (0.05/3). Tests revealed there were significant differences between the static and blinking, $\chi^2$(2, $N$ = 135) = 9.201, $p$ = .01, $V$ = .261, 90% CI [0.14, 0.40], static and no-eyes, $\chi^2$(2, $N$ = 135) = 24.8, $p$ = < .001, $V$ = .427, 90% CI [0.34, 0.43], and the blinking and no-eyes groups, $\chi^2$(2, $N$ = 135) = 46.855, $p$ = < .001, $V$ = .589, 90% CI [0.50, 0.70] (see Figure 3.8).

As a further attention check, the participants were asked, if applicable, what the gender was of the eyes that they saw (male/female/not sure/N/A). As expected, there was a significantly higher proportion of participants who correctly reported seeing an image of female eyes in the blinking condition (58.3%), followed by the static condition (38.3%) and then the no-eyes condition (3.3%), $\chi^2$(6, $N$ = 203) = 54.904, $p$ < .001. Post hoc analysis involved pairwise comparisons with a Bonferroni correction. Statistical significance was accepted at $p$ < .016 (0.05/3). Tests revealed there was a marginal significant difference between the static and blinking groups, $\chi^2$(3, $N$ = 135) = 7.2048, $p$ = .065, $V$ = .231, 90% CI [0.14, 0.39], and a significant difference between the static and no-eyes, $\chi^2$(3, $N$ = 135) = 29.911, $p$ = < .001, $V$ = .469, 90% CI [0.37, 0.57], and the blinking and no-eyes groups, $\chi^2$(3, $N$ = 135) = 46.745, $p$ = < .001, $V$ = .588, 90% CI [0.50, 0.69] (see Figure 3.9).

A chi-square test was also conducted to examine the relationship between conditions and whether participants reported feelings of being watched whilst completing the survey (no/yes). Here, there were no significant differences between the static (22%), blinking (21%) and no-eyes (15%) condition $\chi^2(2, N = 203) = 1.37$, $p = .525$, $V = .082$, 90% CI [0.02, 0.21] (see Figure 3.10).

**Exploratory Data Analysis**

As feelings of being watched may be a key component in the watching-eyes effect (Batesone et al., 2006), the main survey analysis was re-run on just those participants who reported that they felt as if they were being watched during the survey (*N*=39). However, there were no significant differences between groups (static, blinking, or no-eyes) in any of the

**Figure 3.8**

*Proportions of participants who reported seeing eyes by condition*

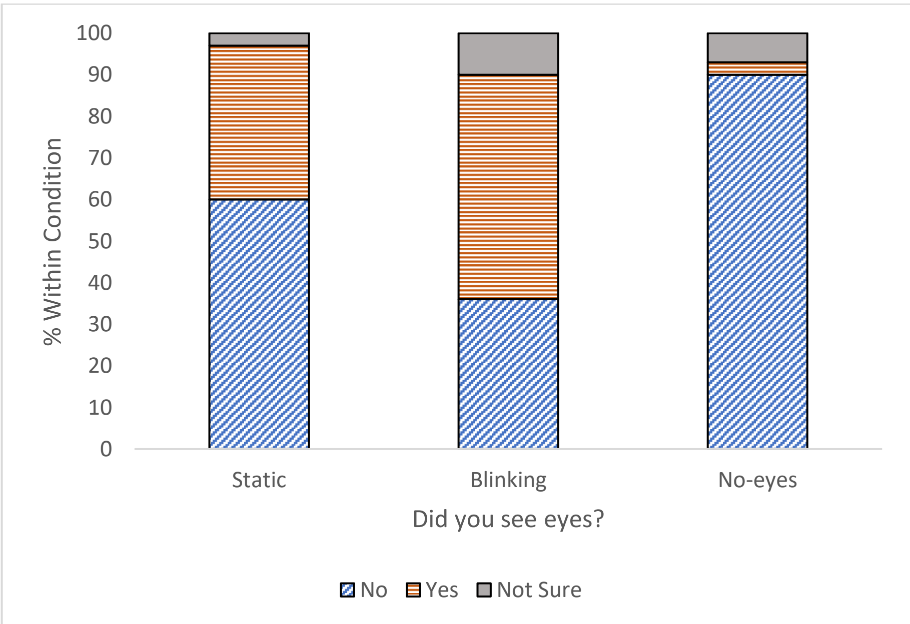


**Figure 3.9**

*Proportions of participants reporting which gender of eyes that they saw*

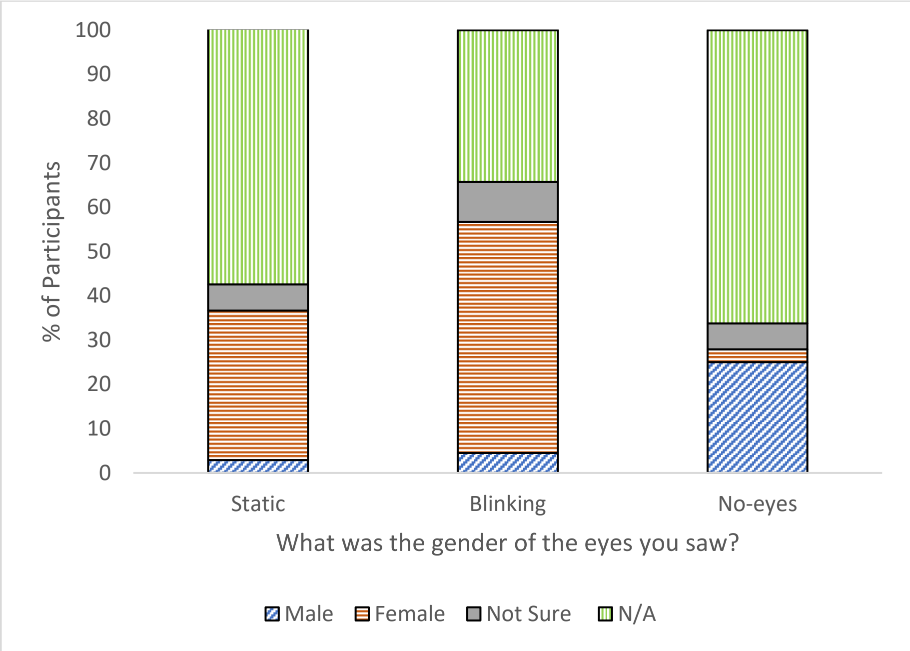
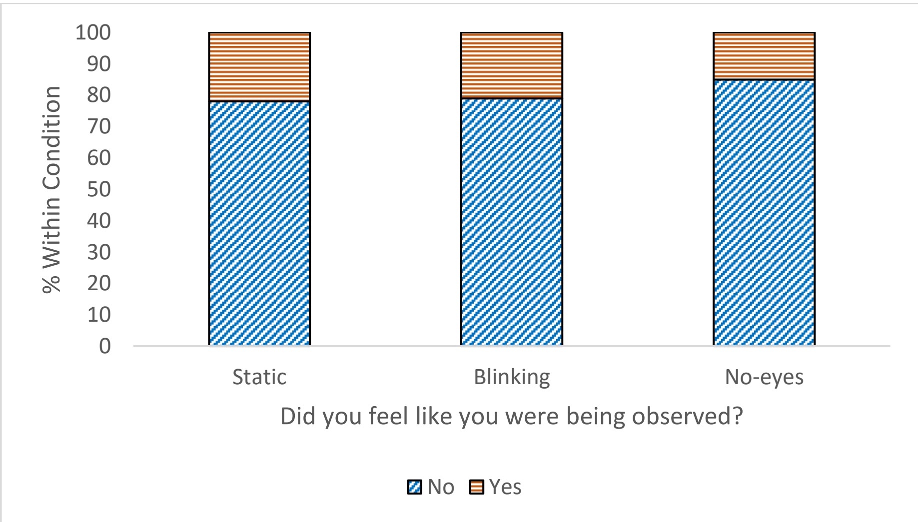



**Figure 3.10**

*Proportions of participants who felt like they were being observed*



dependent variables. There was no significant difference in the percentage donated to the public goods pot across the static (*Mdn* = 100, IQR = 33.00 – 100.00), blinking (*Mdn* = 100, IQR = 40.00 – 100.00), or no-eyes (*Mdn* = 100, IQR = 30.00 – 100.00) groups, $H(2) = .006$, $p = .997$, $\varepsilon^2 = 0.0002$, 90% CI [0.01, 1.00]. There were no significant differences between the static (*Mdn* = 50, IQR = 0.00 – 100.00), blinking (*Mdn* = 50, IQR = 0.00 – 100.00) and no-eyes (*Mdn* = 12.50, IQR = 0.00 – 63.25) conditions in the percentage donated to charity, $H(2) = .801$, $p = .670$, $\varepsilon^2 = 0.02$, 90% CI [0.01, 1.00]. There were no significant differences between the static (*Mdn* = 15, IQR = 5.00 – 20.00), blinking (*Mdn* = 8, IQR = 0.75 – 10.50), and no-eyes (*Mdn* = 18.50, IQR = 0.75 – 48.50) conditions in the number of volunteer hours, $H(2) = 3.116$, $p = .211$, $\varepsilon^2 = 0.08$, 90% CI [0.03, 1.00].

As CfP participants did not receive any initial financial incentive to take part in the study and MTurk participants did, it was speculated that CfP participants may have been more generous with their money as they would not have been primarily financially motivated. In addition, the CfP participants were exposed to the stimuli at the top of the instruction page only, whereas MTurk participants were additionally exposed to the stimuli at the top of the page which asked them how much they wanted to donate to the public goods pots. This additional exposure to the eye stimuli may have had a negative impact on their levels of prosocial behaviour as the longer a person is exposed to the eyes stimuli, the more likely it is that they will recognise the eyes to be a false cue of detection and this will fail to trigger any reputational concerns (Krátký et al., 2016).

Mann-Whitney U tests were conducted to see if there was a significant difference between the two sources of participants and the percentage of their bonus they donated to the public goods pot, the percentage of their bonus they donated to charity and the number of hours they indicated that they would be willing to volunteer their time to charity for each month. CfP participants donated significantly more of their money (*Mdn* = 100, IQR = 67.00 – 100.00) to the group fund compared to the MTurk participants (*Mdn* = 50, IQR = 3.00 – 10.00),

$U$ = 2.364.5, z = - 5.704, $p < .001$, $r$ = -0.4, 90% CI [-0.49, -0.30]. CfP participants also donated significantly more of their money (*Mdn* = 100, IQR = 0.00 – 100.00) to charity compared to the MTurk participants (*Mdn* = 0, IQR = 0.00 – 27.00), $U$ = 2229, z = - 5.844, $p < .001$, $r$ = -0.4, 90% CI [-0.51, -0.28]. However, there were no significant differences between CfP (*Mdn* = 8, IQR = 0.00 – 20.00) and MTurk participants (*Mdn* = 8, IQR = 3.00 – 17.00) in the number of hours they indicated they would volunteer their time for each month, $U$ = 4663.5, z = .873, $p$ = .376, $r$ = 0.06, 90% CI [-0.06, 0.17].

The data file was then split by 'Source' (CfP/MTurk) and the main analysis was re-run on each participant sample. Within the CfP sample, there was a significant difference in the percentage donated to the public good pot between the static (*Mdn* = 100, IQR = 67.00 – 100.00), blinking (*Mdn* = 100, IQR = 79.25 – 100.00), and no-eyes (*Mdn* = 100, IQR = 100.00 – 100.00) groups, $H(2)$ = 6.653, $p$ = .036. Pairwise comparisons were performed using Dunn's (1964) procedure with a Bonferroni correction for multiple comparisons. Statistical significance was accepted at $p < .016$ (0.05/3). The post hoc analysis revealed a significant difference between the no-eyes and static groups ($p$ = .043, $r$ = 0.21, 90% CI [0.07, 0.34]) but not between the no-eyes and blinking groups ($p$ = 1.000, $r$ = -0.04, 90% CI [-0.18, 0.10]) or the static and blinking groups ($p$ = .179, $r$ = 0.16, 90% CI [0.02, 0.29]). There were no significant differences between the static (*Mdn* = 88, IQR = 0.00 – 100.00), blinking (*Mdn* = 100, IQR = 3.25 – 100.00), and no-eyes (*Mdn* = 100, IQR = 1.00 – 100.00) conditions in the percentage donated to charity, $H(2)$ = 4.461, $p$ = .108, $\varepsilon^2$ = 0.03, 90% CI [0.01, 1.00]. There was a marginal significant difference between the static (*Mdn* = 10, IQR = 1.13 – 20.00), blinking (*Mdn* = 5, IQR = 0.00 – 10.00), and no-eyes (*Mdn* = 8, IQR = 2.00 – 20.00) conditions in the number of volunteer hours, $H(2)$ = 4.784, $p$ = .099, $\varepsilon^2$ = 0.03, 90% CI [0.01, 1.00].

Within the MTurk sample, there were no significant differences between the static (*Mdn* = 50, IQR = 5.00 – 10.00), blinking (*Mdn* = 50, IQR = 5.50 – 100.00), and no-eyes (*Mdn* = 50, IQR = 0.00 – 100.00) conditions in the percentage donated to the public goods pot, $H(2)$ =

.151, $p$ = .927, $\varepsilon^2$ = 0.003, 90% CI [0.01, 1.00]. There were no significant differences between the static ($Mdn$ = 0, IQR = 0.00 – 25.00), blinking ($Mdn$ = 0, IQR = 0.00 – 50.00), and no-eyes ($Mdn$ = 0, IQR = 0.00 – 20.00) conditions in the percentage donated to charity, $H(2)$ = .668, $p$ = .716, $\varepsilon^2$ = 0.01, 90% CI [0.01, 1.00] and there were no significant differences between the static ($Mdn$ = 15, IQR = 2.00 – 21.00), blinking ($Mdn$ = 8, IQR = 3.50 – 10.00), and no-eyes ($Mdn$ = 10, IQR = 3.00 – 15.50) conditions in the number of volunteer hours, $H(2)$ = 1.800, $p$ = .407, $\varepsilon^2$ = 0.03, 90% CI [0.01, 1.00.

## Discussion and Conclusion

### Summary

The main aim of this study was to explore whether feelings of being watched can increase *online* prosocial behaviour. The main analysis demonstrated that the presence of eyes during the survey did not significantly impact the amount that the participants donated to the public goods pot or charity. There was a significant difference between conditions in the number of volunteer hours, with a marginal significant difference and a moderate effect size seen specifically between the blinking and static conditions.

The manipulation checks indicated that there was a significant difference between conditions in the numbers of participants who correctly reported seeing an image of eyes and correctly perceived them to be female. However, being exposed to the eye images did not significantly increase participants' feelings of being watched, which has been suggested to be a key component of the watching-eyes effect (Bateson et al., 2006).

Exploratory analysis was conducted to see if there were any significant differences between groups in prosocial behaviour when the analysis was restricted to those participants who reported feelings of being watched. No significant differences were found. The sample was also split by participant source (e.g., CfP or MTurk) and the main analysis was re-run on each sample. The percentage donated to the public good pot was significantly greater in the no-eyes group within the CfP participants but not within the MTurk sample. There was a marginal significant difference within the CfP participants between conditions and the number of volunteer hours (with the number of volunteer hours again being highest in the no-eyes group) but no other significant differences were found between groups in either participant sample in the amount donated to charity.

It should be noted that there were some issues with the analysis which means that no definitive conclusions can be drawn from this study about the impact of watching eyes on prosocial behaviour. Namely, this is due to low statistical power, ceiling effects, and the results of equivalence testing. This will be discussed in the limitations section at the end of the chapter.

**Where the Results Fit with Existing Literature**

Since Haley and Fessler's (2005) seminal work, there have been a multitude of studies that have successfully demonstrated the watching-eyes effect (e.g., see Nettle et al., 2013 and Sparks & Barclay, 2013 for meta-analyses). The marginal significant differences and moderate effect sizes seen between condition and the number of volunteer hours whilst inconclusive, are in line with a previous study by Vrouwe and Baillet (2014), who were the first to use volunteering behaviour as behavioural measure for prosociality. Conversely, the results from this study did not find any conclusive evidence for the watching-eyes effect on donations to the public goods game or to charity. However, this is in line with many recent studies which have failed to find support for the watching-eyes effect (Northover et al., 2016, Northover et al., 2017). This does raise questions as to why many earlier studies found significant positive effects of watching eyes (e.g., Bateson et al., 2006; Fathi et al., 2014; Haley & Fessler, 2005) but most recent studies are struggling to replicate the effect.

One possible explanation could be that the eye cues used in more recent studies may have varied in important ways from those used in previous studies (Saunders et al., 2016). One of these possible ways could be in the length of time that participants were exposed to the eye stimuli. Sparks and Barclay (2013) posit that people are almost constantly, and often subconsciously, evaluating their social environments for cues on whether they are being watched. This process of evaluating one's environment involves the detection, interpretation and response to stimuli and the outcome of this process (i.e., whether a person believes they are being observed) serves as an input into social decision-making. Any increase in prosocial behaviour in response to eye images is likely to be an involuntary, subconscious response (Burnham & Hare, 2007) which can eventually be overridden by slower acting conscious pathways (e.g., the realisation that the eyes cues are not real). Prolonged exposure to eye stimuli can lead to a decrease in responsiveness due to individuals become habituated to the images of eyes and therefore becoming adept at ignoring task-irrelevant stimuli (Sparks & Barclay, 2013). Whereas, paying too much attention to the eye images increases the chance that a person will recognise that the stimulus is a false cue of actual human agency (e.g., by recognising that the eyes are two-dimensional and haven't moved or changed). If a false cue is detected, a person would know that there would be no repercussions to their social decision-making (positive or otherwise), thus it would fail to trigger any reputational concerns and would be unable to evoke a prosocial effect (Krátký et al., 2016).

The length of time that the stimuli was presented to the participants for in this study was not recorded but it is estimated that participants were exposed for approximately one minute (please see methodology section). However, this is around the same length of exposure (60 seconds) for other studies which have found a watching-eyes effect (Sparks and Barclay, 2013). In addition, other studies that have found null effects of the watching eyes have reported using methodologies similar or even identical to studies where positive effects have been found so it seems unlikely that the failure to replicate the effect in those studies is due to variation in eye cues (e.g., Saunders et al., 2016).

It has also been suggested that the watching-eyes effect can increase the probability of donating something rather than nothing but it does not increase the mean donation amounts (Nettle et al., 2013). Nettle et al. (2013) posit that this is because the probability of donation is a more robust outcome measure than the mean donation amount, which is likely to be affected by subtle framing or anchoring effects why could erase any effects of the experimental manipulation. The authors go on to argue that eyes do not make people more generous across the board, but they make people more resistant to extreme strategies, such as giving nothing or giving an oddly large amount. This is supported by the smaller variance in donations when eyes were present in Haley and Fessler (2005) and replicated in the Nettle et al. (2013) study. Although the results from this empirical study showed that there was a smaller variation in the donation amounts to the public good game, contrary to Nettle et al's. (2013) argument, more extreme variation was seen in the donation amounts to charity with the majority of participants donating either none of their bonus or all of their bonus to charity (with very few donation amounts in between). However, there were no differences *between* conditions in the variation seen in either donations to the public goods game or donations to charity[6].

It has also been suggested that previous online studies of the watching-eyes effect (e.g., Raihani & Bshary, 2012; Saunders et al., 2016) have failed to find significant results because the online environment is a truly anonymous setting and therefore the eyes are not effective in making people feel like they are being watched (e.g., Lamba & Mace, 2010; Tane & Takezawa, 2011). Previous research has shown three-dimensional eye cues are more effective than two-dimensional ones (Krátký et al., 2016) suggesting that the stimuli may need to be

[6] Across all conditions there was a smaller variation in the percentage of bonus donated to the public goods game (*Mdn* = 100.00, IQR = 50.00 – 100.00) than in the percentage donated to charity (*Mdn* = 50.00, IQR = 0.00 – 100.00) but as per the results, there were no significant differences *between* conditions (i.e., when the eyes were present) in the variation seen in either donation amount.

more realistic in order to elicit a watching-eyes effect in an online environment (Conty et al., 2016). Therefore, another objective of this study was to explore whether a blinking image of eyes (i.e., a more noticeable eye cue) would aid in evoking a *feeling* of being watched in the participant and thus make the watching-eyes effect more impactful when compared to a static image of eyes. The manipulation check revealed that there were significant differences between conditions in the number of participants who correctly reported seeing an image of eyes (with the highest number of participants being in the blinking conditions) but this was not reflected in the numbers of participants who reported feelings of being watched. This indicates that increasing the salience of the eyes did not impact whether participants felt watched.

**Participants in this Study**

Participants in the present study were sampled from two different online labour crowdsourcing websites; 'Call for Participants' (CfP) and 'MTurk'. As MTurk participants sign up to the platform with the specific aim of earning money, it was thought that perhaps they may be reluctant to donate their additional bonuses to charity in this setting. Conversely, as CfP participants are not necessarily compensated for their time in money, it was thought that using participants from this source to measure the effect of watching eyes on prosocial behaviour may provide a truer reflection of the altruistic levels across the eye conditions.

CfP participants did donate significantly more money to the group fund and charity than MTurk participants but there were no significant differences between the two participant samples in the number of volunteer hours. In line with previous studies, MTurk participants donated a median amount of 50% to the group fund (e.g., Raihani & Bshary, 2012) and a median amount of 0% to charity (e.g., Saunders et al., 2016). In comparison, CfP participants donated a median amount of 100% to both the group fund and to charity. As the CfP participants took part in the survey without a guarantee of receiving an incentive, this could suggest that the participants recruited via CfP may be more altruistically motivated[7]. As participants sign up to MTurk with the specific aim to earn money, MTurk participants may be more reluctant to money in this setting or the MTurk environment may prime the participants to place a high value on quite a small amount of money. As MTurk participants get paid per survey, they would be aware of how much time/how many surveys it would take to earn back their donation amounts which may be a deterrent to monetary donations.

Due to the significant differences in the amounts donated to the group fund and charity between the CfP and MTurk participants, the analysis was re-run on the separate samples to see if the difference in donation amount could be due to one group being more susceptible to the watching-eyes effect than the other. When the analysis was re-run on the separate samples, the percentage donated to the public good pot was significantly greater in the no-eyes group compared to the blinking or static groups within the CfP sample but not the MTurk sample. There was a marginal significant difference with the CfP sample in the number of volunteer hours, with those in the no-eyes group indicating the highest number of volunteer hours. However, the effect size for this result was extremely weak. There were no significant differences between the three groups in the number of volunteer hours within the MTurk sample or in the amount donated to charity in either the CfP/MTurk sample.

This is a difficult result to interpret as significantly higher donations were only seen within the CfP sample, when the eyes were not present, and only with donations to the public goods pot. As CfP participants may have been more altruistically motivated than MTurk participants, this could be a reflection on the participants' Social Value Orientation (SVO). SVO is an individual characteristic that reflects how people interact in social situations and people

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[7] As part of the survey, participants were asked to rate how charitable they considered themselves to be on a five-point Likert. A follow-up analysis revealed that there were no significant differences between the MTurk (*Mdn* = 4, IQR = 3.00 – 5.00) and CfP (*Mdn* = 4, IQR = 3.00 – 5.00) participants in how charitable they considered themselves to be, $H(1) = .357$, $p = .550$, $\varepsilon^2 = 0.002$, 90% CI [0.00, 1.00].

are typically categorised as prosocial or proself (Millet & Aydinli, 2019). Those who are prosocially orientated tend to be especially sensitive and concerned about how they appear in the eyes of others (i.e., public self-awareness) and public self-awareness is strongly correlated with social anxiety (Pfattheicher & Keller, 2015). Pfattheicher and Keller (2015) posit that individuals with relatively strong chronic public self-awareness behave more prosocially under watching eyes because they are anxious about and motivated to avoid the negative judgements and evaluations of others.

It is difficult to explain why there were significantly higher donations when the eyes were not present. This could be due to a dual effect of the control image evoking a sense of authority (Dolan et al., 2012) and the eye images evoking negative emotions (Yu, Duan, & Zhou, 2017). Raihani and Bshary (2012) found that donations were significantly higher in the *absence* of eye stimuli in their study on watching eyes and donation behaviour in a dictator game. They argue that these findings were due to a type of messenger effect; in their case, an established association between the control stimuli they used (i.e., an image of flowers) and a positive emotional state, which in turn could have led to more prosocial behaviour. There is evidence that signals of authority can generate compliant behaviour (Dolan et al., 2012) so it is possible that the university logo used as the control image in the ‘no-eyes’ group could have provided a sense of authority and credibility which would have led to higher donations (Park & Reiner, 2019). It is also possible that that the presence of eyes may have triggered negative emotions such as anxiety about being judged which in turn could have lead participants to avoid looking at the eye stimuli (Yu et al., 2017). If the eye images did trigger negative emotions within the participants, it may have also triggered higher levels of anxiety in those who were more prosocial orientated (i.e., the CfP participants). This heightened anxiety could have led to higher avoidance behaviour and if the CfP participants only glanced at the eye images, they may not have paid enough attention to the eye images for the watching-eyes effect to emerge.

In addition, a prosocial SVO could explain why statistically significant differences were only seen in donations to the public goods pot. A prosocial SVO has strong associations with Social Mindfulness (SoMi), defined as seeing and considering the needs and wishes of others before making a decision (Manesi, van Lange, van Doesum, & Pollet, 2019), and previous studies have shown that those who are prosocially orientated tend to consider the impact of their behaviour on others and strive to maximize joint outcomes (Wei, Zhao, & Zheng, 2016). In the public goods game, the group's total pay-off would have been maximised if everyone contributed all of their reward (Hardy & van Vugt, 2006) which means it would be expected to see higher donations amongst prosocial participants (i.e., CfP participants) than proself (i.e., MTurk participants).

Nevertheless, this possible explanation to why significantly higher donations were only seen within the CfP sample, when the eyes were not present, and only with donations to the public goods pot, is just conjecture and would need to be investigated systematically (Berger, 2019). Given that this empirical result is difficult to rationalise, no other meaningful results found between groups in prosocial behaviour, and the low statistical power of this analysis, it is possible this finding is just simply a statistical anomaly (Burnham & Hare, 2007).

Saunders et al. (2016) question whether the null findings in their study using an MTurk sample was due to MTurk participants having considerable experience in research which could result in higher levels of automatic responding. As MTurk workers get paid per survey, it is logical to presume that they may not be motivated to pay attention to the task but instead focus on strategies that would allow them to complete as many surveys as they can, as quickly as they can, to increase their income (Paolacci et al., 2010). An increasing prevalence of economic game experiments makes it likely that highly experienced MTurk workers will be familiar more generally with experimental paradigms and will have had an opportunity to adjust their default responses to fit these games (Rand et al., 2014). However, sourcing participants from CfP mitigates this potential factor, suggesting that the lack of evidence for the watching-eyes effect in this study was not due to the recruitment approach.

**Emotion**

This study originally included seven conditions that would have allowed for an exploration into the role of the emotion portrayed by the eyes and its impact on participant behaviour. It has been argued that the null effects of watching eyes found in recent studies suggest that the effect of eyes is sensitive to moderating factors such as the perceived emotion displayed by the eye cues (Pauwels et al., 2017). Although there has been limited research on the role of emotions in the watching-eyes effect, the research that has been conducted has produced mixed results. For instance, Pauwels et al. (2017) recently investigated the previously unexamined area of the valence of the eye cues (kind vs. unkind eyes) and found that unkind eyes increased cooperation and Nettle et al. (2017) found that displaying an image of angry eyes helped to deter bike thieves. This is possibly because negative emotions would result in a heightened arousal response (e.g., anxiety), which would trigger reputational concerns and increase the effects of watching eyes. However, Panagopoulos and van der Linden (2017) found no evidence that staring, angry eyes can increase negative emotions (e.g., anxiety). Although a positive emotional state has been linked to increases in sociable and cooperative behaviour (Raihani & Bshary, 2012), it has been suggested that eyes increase adherence to a social norm rather than increase prosocial behaviour *per se* (Oda, Kato & Hiraishi, 2015). Therefore, concerns about one's reputation may not be triggered in a context in which participants simply increase their profit but it may be triggered in a social context, where eye stimuli portraying negative emotions may remind participants that they may be negatively affected by deviating from social norms (Pauwels et al., 2017). The potential moderating effect of the emotion portrayed by the eye stimuli is re-visited in study three of this thesis.

**Strengths**

This study had three main strengths: its diverse range of participants, the neutrality of the control condition used and, at the time of research, the novel area in which the watching-eyes effect was being tested. As discussed in the introduction of this study, a common problem in psychology is that researchers often rely on opportunity sampling and when research is conducted in a university setting, this means recruiting almost exclusively from a WEIRD sample which does not represent the whole range of human emotion, behaviour, and preferences. Using online crowd-sourcing platforms mitigates this by allowing researchers to draw upon a diverse and therefore a more representative population sample (Saunders et al., 2016). Another strength of this study was the neutrality of the control condition (e.g., a University logo). Previous studies used images of flowers as a ‘neutral’ control condition (e.g., Raihani & Bshary, 2012) and the neutrality of this has been called into question (Manesi, van Lange, & Pollet, 2015). For example, Raihani and Bshary (2012) discuss how an image of flowers could evoke a feeling of pleasure and enhance attention which could explain why their participants in the control condition made higher donations to the dictator game than those who were exposed to eyes. There have been calls for future research to juxtapose the effect of eyes against various other controls, which could offer insights into the boundary conditions of the watching-eyes effect (Manesi et al., 2016). Lastly, although the watching-eyes effect has previously been tested on charitable donations (e.g., Fathi et al., 2014), at the time of developing the research proposal for this thesis (circa 2016), there had been no studies into the effects of watching eyes on *online* charitable donations providing a novel area to explore the watching-eyes effect and pave the way for potential future low-cost but high-impact interventions (Dear et al., 2019).

**Limitations**

There were some issues identified with this analysis that means overall, no definitive conclusions can be drawn for or against the watching-eyes effect in an online environment. These issues include the presence of ceiling effects, low statistical power, and the outcomes of equivalence testing. The first issue identified was that across the main and exploratory analysis, participants in the no-eyes group donated the maximum amount possible (100%) of their initial bonus to the public goods game. Given this ceiling effect in the control group, the presence of watching eyes could not have made an additional impact (Pfattheicher et al., 2018).

The second issue identified was that post-hoc power analysis revealed that the analysis for this study was underpowered. The manipulation checks (i.e., chi-square tests) only had enough power to detect a medium or large effect size when using the CfP sample or all participants. Whereas the main analysis (i.e., the Kruskal-Wallis H tests) only had enough statistical power to detect a medium or large effect when using CfP and MTurk participants combined. The exploratory analysis only had enough power to detect a large effect size and only when using all participants. As discussed in the general methodology section of this thesis, if a test fails to achieve the minimum power required, this means that there is a greater chance of false-negative (type II error) and if an effect is a true but small effect, the test would have a lower probability of being able to detect it (Field & Hole, 2003). This means that the ability to draw conclusions from any null results in this study is limited.

The low statistical power also has implications for interpreting some of the moderate effect sizes seen within the results. For example, in the post hoc analysis there was a non-significant difference between the blinking and no-eyes groups in the number of volunteer hours and an effect size of $r$ = .17. As the results were not significant, this moderate effect size could be indicative of a type II error - i.e., the non-significant results are possibly not due to a true null result but rather from a lack of statistical power to detect the effect. In addition, there were several instances of marginal significant differences after post-hoc pairwise comparisons using a Bonferroni correction (e.g., the differences between blinking and static groups in the number of volunteer hours was $p = 0.52$). Bonferroni corrections are generally conservative (Perneger, 1998) and coupled with the low statistical power, it cannot be ruled out that there may been an effect, it was potentially there but with low confidence.

Equivalence tests were conducted in order to explore whether the non-significant results were a true null effect or whether they were inconclusive. As detailed in the general methodology chapter, 90% confidence intervals (CIs) were calculated for the effect sizes and compared to the confidence intervals for the smallest effect size of interest (SESOI). As the CIs for each effect size in this analysis was outside of the boundaries for the SESOI, this means that it cannot be determined that the two groups are statistically equivalent. Therefore, this means that the results of this analysis are inconclusive as it cannot be concluded either way whether there was an effect of watching eyes.

Another limitation of this study is that the participants did not seem to notice the eyes. This study used the blinking eye stimuli to try and increase the salience of the eyes. It was thought that by increasing the salience of the eyes, it would increase feelings of being watched and therefore positively impact prosocial behaviour via the watching-eyes effect. The results show that as could be expected, there was a significantly higher proportion of participants who reported seeing an image of eyes in the blinking condition (i.e., the condition where the eyes are presumably most noticeable), followed by the static condition, and then the no-eyes condition. However, it still only means that 54% of participants in the blinking condition and 37% of participants in the static condition reported seeing an image of eyes. The non-significant findings of this study are perhaps not surprising, people may not be affected by eyes if they are not noticing them.

Another potential limitation of this study is that the CfP participants and MTurk participants were exposed to the eye stimuli for different amounts of time. When participant recruitment via CfP stalled, a decision was made to recruit additional participants from MTurk. Due to an error when preparing the survey for MTurk participants, MTurk participants were exposed to the eye stimuli in two separate places of the survey, whereas the CfP participants were only exposed once. However, there were high standard deviations within both the CfP and MTurk samples in the percent donated to the public good pot, the percent donated to charity and the number of volunteer hours. Although this could mean that both groups were 'very' noisy which would have drowned out a potential watching-eyes effect if it existed, it also means no group was 'noisier' than the other suggesting that there were no fundamental differences between the two groups. The literature on the habituation of the watching-eyes effect suggests that if habituation does occur, it occurs surprisingly quickly (Vogt et al., 2015). Therefore, it is possible that if habituation occurred in this empirical study, perhaps both the MTurk and CfP participants became habituated and the difference in exposure time did not have any systematic effect on the prosocial behaviour of either sample.

It is possible that the images of eyes were too obvious, and the participants paid too much attention to them. This would result in the participants becoming habituated to the eyes which in turn may have reduced their impact (Sparks & Barclay, 2013). This could explain why MTurk participants donated less amounts of their bonus to the group fund and charity than CfP participants as the MTurk participants were exposed to the eye images more times. A potentially fruitful avenue for future research (which was explored in the next empirical study) would be to replicate this study in a laboratory environment using eye-tracking software (Vaish et al., 2017). Studying where people look has become one of the standard vehicles for trying to understand attention and the principal means for studying looking behaviour is eye tracking. This would provide a useful vantage point in determining whether the participants are paying attention to the eye stimuli.

A final potential limitation of this study was that the CfP and MTurk participants may have had different perceptions of anonymity. In order to be able to pay the CfP participants their final bonus, they were required to provide an email address linked to a valid PayPal account. It was necessary to request this information before the participant took the survey which may have meant that the participants may not have felt truly anonymous. This could have triggered reputational concerns leading to the CfP participants donating more to the group fund and charity than the MTurk participants. However, as the participants were advised that the task itself was anonymous after providing this information, it is not likely that providing an email address would have triggered any reputational concerns for CfP participants[8] (Raihani & Bshary, 2012).

Reputational concerns are the leading interpretation for the watching-eyes effect (Pauwels et al., 2017) with previous studies finding the association between attentiveness to eyes and donation behaviour significant only in a public donation context (Lamba & Mace, 2010; Vaish et al., 2017). If the presence of eyes does promote cooperation, the underlying mechanism could be reputational concerns (Vaish et al., 2017). By leading the participants in this first empirical study to believe that their actions would remain completely anonymous, this may have contributed to a cloak of perceived anonymity (Raihani & Bshary, 2012) and therefore failed to trigger any reputational concerns. Therefore, another interesting avenue for future research would be to try and specifically evoke reputational concerns in the participants (e.g., by explicitly telling them that their donation amounts would be shared with the other players and with charity) and comparing the results of this public setting with the anonymous setting on this study.

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[8] This best measure in the survey to test this was whether participants reported feelings of being watched. A follow-up analysis revealed that conversely, there were significantly more MTurk participants (31.1%) than CfP participants (14.1%) who reported feelings of being watched, $\chi^1(N = 203) = 8.004$, $p < 0.01$, $V = .199$, 90%CI [0.07, 0.33].

**Next steps**

The next study built on this one by re-running the experiment but this time in a laboratory setting to allow the use of eye-tracking equipment. The results of manipulation checks in this study indicated that being exposed to the eye images did not significantly increase participants' feelings of being watched, which has been suggested to be a key component of the watching-eyes effect (Bateson et al., 2006). However, as any changes in behaviour in response to eye images is likely to be an involuntary, subconscious response (Burnham & Hare, 2007), it is possible that *conscious* feelings of being watched are not important. Participants may still be paying attention to the images of eyes even if they are not aware of them and as eye-tracking equipment can detect even subconscious observations, it is an important tool to try to ascertain whether participants paid attention to the different eye stimuli during the tasks (Weggelaar-Jansen et al., 2016).

As a limitation of this first empirical study was that participants were explicitly led to believe that their actions would remain completely anonymous, the next study built on the first study by attempting to evoke reputational concerns. The study included an added element of deception whereby participants were specifically led to believe that they are playing against 'Jesse' and 'Sam'. By adding this 'interactive' element, it was hoped that the participants would believe they were playing the public goods game in real-time and with real people which in theory would increase reputational concerns and therefore it may increase the effectiveness of the stimuli (Pauwels et al., 2017). Lastly, as a result of lessons learnt in this study, more careful attention was devoted in subsequent studies to ensure materials were consistent across participants.

**Conclusion**

Although the results of this analysis were inconclusive, the findings from this study indicate that they are in line with the growing body of literature which suggests that artificial monitoring effects (e.g., an image of eyes) do not influence human behaviour in a uniform way (Saunders et al., 2016). It seems that if the watching-eyes effect does exist, it is nuanced. The results of this study, when situated within the mixed results in the literature, do not negate the premise that cooperative behaviours are sensitive to the subtle cues of being watched, but further research is needed to uncover the nuances of the watching- eyes effect whilst allowing for the development of novel techniques to promote prosocial behaviours in online environments (Saunders et al, 2016).

# Chapter Four – Study Two: A Lab-based, Eye-tracking Study on Participant Attention to Watching Eyes

The first study of this thesis explored whether the presence of eyes positively affected prosocial behaviour in an online environment. Analysis of the data showed no significant differences between the eye stimuli groups in the three measures of prosocial behaviour and the results of the manipulation checks indicated that the eye cues were not effective in making the participants feel like they were being watched. One of the limitations identified in study one was that the participants in the eye conditions did not seem to notice the eye stimuli. There is a question of whether the level of attention that participants pay to the eye stimuli is an important component to the watching-eyes effect emerging so to try and address this question, the aim of this empirical study two was to explore whether participants paid attention to the eye images. In addition, an additional limitation identified in study one was that participants were led to believe that their actions were anonymous, which may have failed to trigger reputational concerns. To rectify this, the participants in this study were led to believe they were playing against other ‘real-life’ participants and that their donations to the public good pot and to charity would be made public. It was theorised that this would help make the participant believe that their behaviour was being watched by others and provide a situation in which they could enhance their prosocial behaviours, if they so desired.

## Previous Study

The aim of the first empirical study of this thesis was to explore whether the presence of eyes positively affects prosocial behaviour in an online environment. Despite the rapid growth of the watching-eyes literature, at the time of research, there had been no studies exploring the effects of watching eyes on *online* charitable behaviour. Due to the simple and inexpensive ways in which the watching-eyes could be utilised in this context, this seemed an important area to explore.

The main analysis demonstrated that the presence of eyes during the survey did not significantly impact the amount that the participants donated to the public goods pot or charity and there was only a marginal significant difference between conditions in the number of volunteer hours. One possibility as to why a watching-eyes effect was not found in study one, is that participants may not have been affected by eyes because they simply did not notice them. The results showed that only 54% of participants in the blinking condition and 37% of participants in the static condition reported seeing an image of eyes, and being exposed to eye images did not significantly increase the participants' feelings of being watched. Raihani and Bshary (2012) suggest that sensitivity to eye cues may be weakened in online environments due to a sense of anonymity; cues of being watched are less likely to trigger the reputational concerns needed for the watching eyes to influence prosocial behaviour.

As the salience of the eye cues may be the key to capturing and maintaining attention, another objective of the first study was to explore whether using a short, looping video of blinking eyes (i.e., a more noticeable eye cue) would result in an increased level of prosocial behaviour. It was thought that using more salient eye cues (e.g., blinking eye images) may be more effective at capturing and maintaining participants' attention and therefore evoking a feeling of being watched. However, the attention checks in study one demonstrated that the salience of the eye cues had no impact on whether participants reported feelings of being watched.

Another possible reason to why a watching-eyes effect was not found in study one is that there is a possibility that participants simply became habituated to the images of eyes (Sparks & Barclay, 2013). With modern life involving near-constant use of computers and televisions, people are frequently bombarded with images of people (and therefore eye images) so it is possible that in an online environment, people may have become accustomed to seeing constant images of eyes. If participants were exposed to the eye images for too long, they could have become habituated to them, leading to a decrease in responsiveness and thus a watching-eyes effect would not have emerged (Oda, 2019; Sparks & Barclay, 2013). This could possibly explain why in the first study, participants recruited via Amazon's Mechanical Turk (MTurk) donated less amounts of their bonus to charity than those recruited from 'Call for Participants' (CfP) as the MTurk participants were exposed to the eye images more times than the CfP participants.

**This Study (Empirical Study Two)**

If, as theorised by the watching eyes literature, the simple presence of eyes or eye-like stimuli increases people's prosocial behaviour, there is a question of whether the level of attention that participants pay to the eye stimuli is an important component to the watching-eyes effect emerging (Sparks & Barclay, 2013, Vaish et al., 2017). To try and address this question, the aim of empirical study two was to explore whether participants paid attention to the eye images. Studying where people look has become one of the standard vehicles for trying to understand attention, and the principal means for studying where people look is by utilising eye-tracking technology (Vaish et al., 2017). However, the study by Vaish et al. (2017) is seemingly the only study to date that has utilised eye-tracking when explicitly testing the watching-eyes effect.

Vaish et al. (2017) demonstrated that it is the presence of eyes specifically (but not other human features such as ears or hands) that is correlated with greater generosity when donating to a charitable cause but only in a reputation-relevant context (e.g., when donations are public). This suggests that, although other human features may indicate the presence of others, the eyes are unique in their monitoring function and therefore are especially relevant for reputation management. This eyes-specific association indicates that it is not the sensitivity to the presence of others, but rather the sensitivity to reputation-related cues (e.g., the images of eyes) that is pivotal in the watching-eyes effect (Vaish et al., 2017). In addition, research has shown that 3D eyes images are more effective than 2D ones in triggering the watching-eyes effect (Krátký et al., 2016). It could be that that people are more sensitive to eye cues when they are more 'realistic' and perhaps this is because they are more effective in capturing and maintain attention (Manesi et al., 2016). Therefore, an individual's propensity to pay attention to eye cues could be an indicator of the watching-eyes effect in action (Conty et al., 2016).

In this second empirical study, participants completed the survey in a lab-based setting to allow for the collection of eye-tracking data. This enabled an analysis of where the participants looked at on the screen and for how long, providing an insight into whether the participants noticed and paid attention to the image of eyes, building on the findings by Vaish et al., (2017), and on the previous study in this thesis.

Eye trackers provide a moment-to-moment record of where an individual is looking, providing an invaluable tool for studying attentiveness (Risko & Kingstone, 2011). When exploring gaze behaviour, a main measurement is 'fixations', which are the periods of time when the eyes are locked onto an area of interest (AOI). Within this measurement, there are a range of metrics used by researchers such as duration of fixations, number of fixations and time to first fixation. In this study two metrics were used; percentage of total time spent observing the AOI and time to first fixation on the AOI.

The percentage of total time spent observing the AOI was used to measure the levels of attentiveness to the eye cues. As previously mentioned, there is seemingly only one previous specific watching eyes study which has examined gaze behaviour (e.g., Vaish et al., 2017) and this study used total gaze durations in seconds as the main dependent variable. Conversely, Bojko and Adamczyk (2015) recommend that the number of fixation counts is used to measure attentiveness as the gaze duration time can indicate difficulty with processing information. However, in this study, each participant was allowed to work through the survey and tasks in their own time, which meant that the total number of fixation counts was not appropriate, as it could not be directly compared between participants.  Instead, 'observation length' (the total duration of time participants fixated on the AOI) was used to calculate the percentage of total time the participants spent observing the AOI. This allowed a standardised comparison across the groups in how much attention the participants were paying to the stimuli. This metric is listed as one of the most common eye tracking metrics by Jacob and Karn (2003) and has been used more recently in other eye-tracking studies on prosocial behaviour (e.g. Cañigueral & Hamilton, 2019; Yu, Duan, & Zhou, 2017). The time to first fixation was used to measure how quickly each participant first paid attention to the AOI and therefore whether the ‘blinking eyes’ stimuli were more effective in capturing participant attention than the ‘static’ or ‘no-eyes’ stimuli (e.g., Borys & Plechawska-Wójcik, 2017; Cañigueral Vila, 2020).

**Reputational Concerns**

A limitation of study one was that it did not account for reputational concerns, which could be another possible key component of the watching-eyes effect (e.g., Pauwels et al., 2017). Altruistic behaviour usually leads to a good reputation and an individual with a good reputation tends to obtain future rewards such as a positive interaction with a partner or romantic mate(s). These reputational benefits serve as an incentive for altruistic behaviour, and this incentive is known as ‘reputational concern’ (Kawamura & Kusumi, 2018). Previous research has shown that attentiveness to eyes predicts prosocial behaviour, but only when reputation is at stake (Vaish et al. 2017). In the first empirical study, the participants were told that they had been grouped with two other anonymous people and advised that their donations to the public goods pot and charity would remain completely anonymous. Therefore, this could have contributed to the cloak of perceived anonymity (Raihani & Bshary, 2012) and failed to trigger any reputational concerns.

To rectify this, the participants in this second empirical study were led to believe they were playing against other real-life participants, where in reality these other participants did not exist. Participants in study two were recruited to complete an online survey in a lab-based setting (to allow the collection of eye-tracking data) and, as with the first study, were randomly allocated to one of three groups (static, blinking, or no-eyes). They were exposed to either a static image of eyes (a photograph of eyes), a blinking eyes image (a short, looping video of blinking eyes), or no-eyes (a university logo). The same three measures of prosocial behaviour (donation to the public goods game, donation to charity, and the number of volunteer hours) were compared across the three groups. However, in this study, the participants were specifically told via the survey instructions that they were playing against two other players called 'Jesse' and 'Sam' and that whatever amount they decided to donate to the public goods pot would be displayed to the other players at the end of the game. By giving the other 'participants' a name, it was hoped that this would make the deception more believable, thus providing the actual participant with a situation in which they could enhance their reputation (Vaish et al., 2017) if they were motivated to do so by the presence of the watching eyes. In addition, instead of participants simply being asked whether they wanted to donate any of their final bonus to an anonymous charity (as with study one), this time participants were taken to a mock-up of a real charity website and advised that any donations would be made public to the charity in question. It was hoped that utilising this method to measure charitable donations would help make the participant believe that their behaviour was observable by others and provide a situation in which they could enhance their prosocial behaviours, if they so desired.

**Aim**

The first aim of this study was to test whether the presence of eye cues positively affected prosocial behaviour in an online environment. This study built on study one by attempting to evoke reputational concerns by leading the participants to believe that their behaviour would be witnessed by others.

The second aim of this study was to determine whether the participants paid attention to the image of eyes whilst completing the prosocial tasks. This study again built on study one by additionally collecting data on participants' gaze behaviour whilst the participants completed an online survey that measured levels of prosocial behaviour.

**Hypotheses**

***Prosocial Behaviour***

**Hypothesis 1:** There will be a significant difference between conditions in the percentage of their initial bonus that participants choose to donate to the public goods pot ('Donation to Public Goods Pot').

**Hypothesis 2:** There will be a significant difference between conditions in the percentage of their final bonus that participants choose to donate to charity ('Donation to Charity').

**Hypothesis 3:** There will be a significant difference between conditions in the number of hours per month participants indicate they would be willing to volunteer their time to charity for ('Total Volunteer Hours').

***Gaze behaviour***

**Hypothesis 4:** There will be a significant difference between groups in the percentage of total time participants spend observing the Area of Interest (AOI) ('% Total Time').

**Hypothesis 5:** There will be a significant difference between groups in the time to first fixation (ms) on the AOI ('Time to 1$^{st}$ Fixation').

## Methodology

### Participants

Participants had a minimum age requirement of 18 years old in line with the University's and the British Psychological Society's code of ethics (The British Psychological Society, 2018) and participants were advised they needed a valid email address which was linked to a PayPal account to be eligible for the survey. This criterion was required so that participants could be paid their final bonus amount. As participants were not informed of the potential bonus prior to the survey, no explanation was provided to why this information was needed. No participant queried this request and there were no other participant criteria.

Participants were recruited via Canterbury Christ Church's Research Participation Scheme (for two course credits) and via the University's temporary job website (for a £10 Love2shop voucher). Due to university guidelines, participants could only complete the study for course credits or a voucher but not both. Participants had authorised their names and email addresses to be taken for account purposes; no participant queried this request.

### Stimuli

The survey used in this study was a replication of the survey used in study one, which was conducted using Qualtrics survey software. It utilised the same static, blinking and no-eyes stimuli (see Figure 2.3. in Chapter Two – General Methods). However, to expand on study one by trying to evoke reputational concerns, participants were primed with information to make them feel as if their donation to the group fund would be made public to the other players. After playing the public goods games, participants were advised that they would be taken to a charity webpage where they could donate any amount of their final bonus that they liked and that their charitable donations (if any) would be made public to the charity.

**Design**

In a laboratory setting using eye-tracking equipment, participants were required to complete an online survey that measured prosocial behaviour. The survey was designed and created in Qualtrics, and participants were randomly assigned to one of three eye stimuli groups (static, blinking or no-eyes) in a one-way ANOVA, between-subjects design. There were three measures of prosocial behaviour which were compared across the three groups; the percentage of their initial bonus that participants donated to the public goods pot, the percentage of their final bonus that they donated to charity and the number of hours that they indicated they would be willing to volunteer their time to charity for on a monthly basis.

In addition, using the ManGold software suite (Lab Suite Version 2017), the participants' gaze behaviour was measured and compared across the three groups. Areas of Interest (AOIs) on the screen were defined so that the programme could record the desired eye gaze metrics. The AOIs in this study were at the locations in the survey at which participants were exposed to the stimuli, which were at the top of the public goods game instructions (location one), at the point where participants were playing the public goods game (location two), and at the point of donating to charity (location three).

The focus of the AOIs for the static and blinking groups were the eye stimuli at all three locations in the survey. The focus of the AOIs for the no-eyes group at locations one and two was the blank area of the screen where the eyes would have been in in the other two conditions. This was to enable a direct comparison of eyes versus no eyes. At location three, a decision was made to use a Canterbury Christ Church University (CCCU) logo as the control stimuli instead of a blank space (see Figure 4.1). This was due to a decision to place the eye group stimuli above the donation box to help draw attention to the donation box (see discussion in the 'Stimuli' section).

The webpage itself was a pseudo webpage (see Figure 4.1) created, with the permission of the charity, as it would not have been possible to track any donations from the study via the charity's real webpage. The intent was to make the pseudo webpage as close to real webpage as possible, therefore it was decided to not place the different group stimuli at the top of the page (as with the other locations in the survey) as this would have involved changing too many aspects of the charity's webpage. Instead, the different group stimuli were placed above the donation box to help draw attention to the latter. The donation box itself was also marked as an area of interest, although this was purely to separate the attention paid to the different group stimuli from the attention paid to the donation box as the latter was not of interest in this study.

The pseudo webpage was made by Canterbury Christ Church University's (CCCU) Psychology technicians using HTML within Qualtrics. The webpage was not accessible outside of the survey.

**Procedure**

In a laboratory setting, participants took approximately 30 minutes to complete an online survey on a laptop equipped with non-invasive eye-tracking equipment. The participants completed this survey alone, with no other people in the room. A chin and headrest were used to place the participant approximately 60cm away from a 15.6" monitor with a resolution of 1920 by 1080 pixels. The eye-tracking unit (ManGold, Lab Suite Version 2017) was positioned below the monitor and participants eye movements were measured at a frequency of 100-200 Hz (indicating how many times the eye is captured in a second). At the beginning of the session, each participant completed a 9-point calibration test to check the accuracy and performance of the eye tracker. Once the calibration test started, participants saw a red dot appear on the screen and were instructed to follow the dot with their eyes as it moved across the screen 9 times. The ManGold software had a built-in minimum eye gaze accuracy requirement with participants only being able to progress onto the survey once the calibration reached a minimum of 95% accuracy.

As part of the survey, participants were awarded an initial £1.50 bonus which they could use in a public goods game to win up to a total £2.50 bonus (participants were not informed of this prior to taking the survey) and were offered a chance to donate their winnings to a charity. As it was not possible to enable electronic payments via the pseudo webpage, any money owed to the charity was added up across all participants at the end of the data collection period and donated directly to the charity via their official website. Any money owed to the participant was paid via PayPal within three working days of them completing the survey.

As with study one, the group-specific stimuli (static, blinking or no-eyes) was presented at the top of the public goods game instruction sheet (location one) which informed the participants that they had been awarded an initial bonus of £1.50 and provided a set of instructions on how to play the public goods game (based on the methodology used by Burnham & Hare, 2007)[9]. Building on study one, to attempt to make reputational concerns more salient, participants were led to believe they were interacting in real-time with two other participants, in another room from the same session and that their decisions and donation amounts would be made public to other players. For example, they were told in the instructions for the public goods game that “everyone will know how much each other decides to donate to the group fund”. In reality, there were no other participants, which the real participants were informed of at the end of the survey. To try and make this deception believable, the fake participants were given names: ‘Jesse’ and ‘Sam’. These names were chosen as they could be perceived as gender-neutral to try and minimise any effect of gender of the audience on prosocial behaviour (Mulcahy, 1999).

[9]The eye-tracking data shows that the eyes were on the screen at location one for an average of 48.54 seconds across participants.

The participants then saw the group-specific stimuli at the top of the public goods game itself (location two)[10]. Participants were then advised that each group member would shortly have to decide how much they wanted to donate to the group fund and how much they would keep for themselves. They were advised that whatever was donated to the group fund would be doubled and equally divided back to all players; therefore, how much they donated and how much they kept for themselves would affect how much money everyone else received back from the group fund. They were then given the option to donate any amount (up to the maximum of £1.50) of their initial bonus to the group fund via a sliding scale.

On the next page of the survey, the participants were advised of their total bonus amount (rounded to the nearest whole pence). The participants were then advised that as supporters of the charity INAS (International Sports Federation for Persons with Intellectual Disability), the School of Psychology at CCCU wanted to give them the opportunity to donate some of their total bonus to the charity and that any donation they gave would be made public to the charity. For example, they were advised that "Any donation will be deducted from your bonus amount and your donation will be made public to INAS".

The participants were then taken to an offline pseudo webpage (INAS granted full permission for the webpage to be made) where the participants could find out more about the charity and make a donation if they so wished. They then saw the group-specific stimuli for the third and final time[11]; above the donation box (location three) on the pseudo charity webpage. The participants were advised that any donation would be deducted from their bonus amount and their donation made public to INAS.

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[10] The eye-tracking data shows that the eyes were on the screen at location two for an average of 31.05 seconds across participants.

[11] The eye-tracking data shows that the eyes were present on the screen at location three for an average of 54.75 seconds across participants.

On the next screen, participants were advised that as part of the School of Psychology's ongoing work with charities, the school was collecting information on people's willingness to participate in charitable activities and the participants were asked to indicate which activities they would be willing to volunteer their time to (e.g., helping to organise a fundraising event, collecting donations for charity, providing care for the elderly and providing care for the disabled) and asked how many hours per month (in total) they would be willing to volunteer via an open-text response. As in study one, this was to try to capture different types of altruism as the desire to be altruistic can be displayed in many forms, not necessarily just monetary (Iredale & van Vugt, 2011).

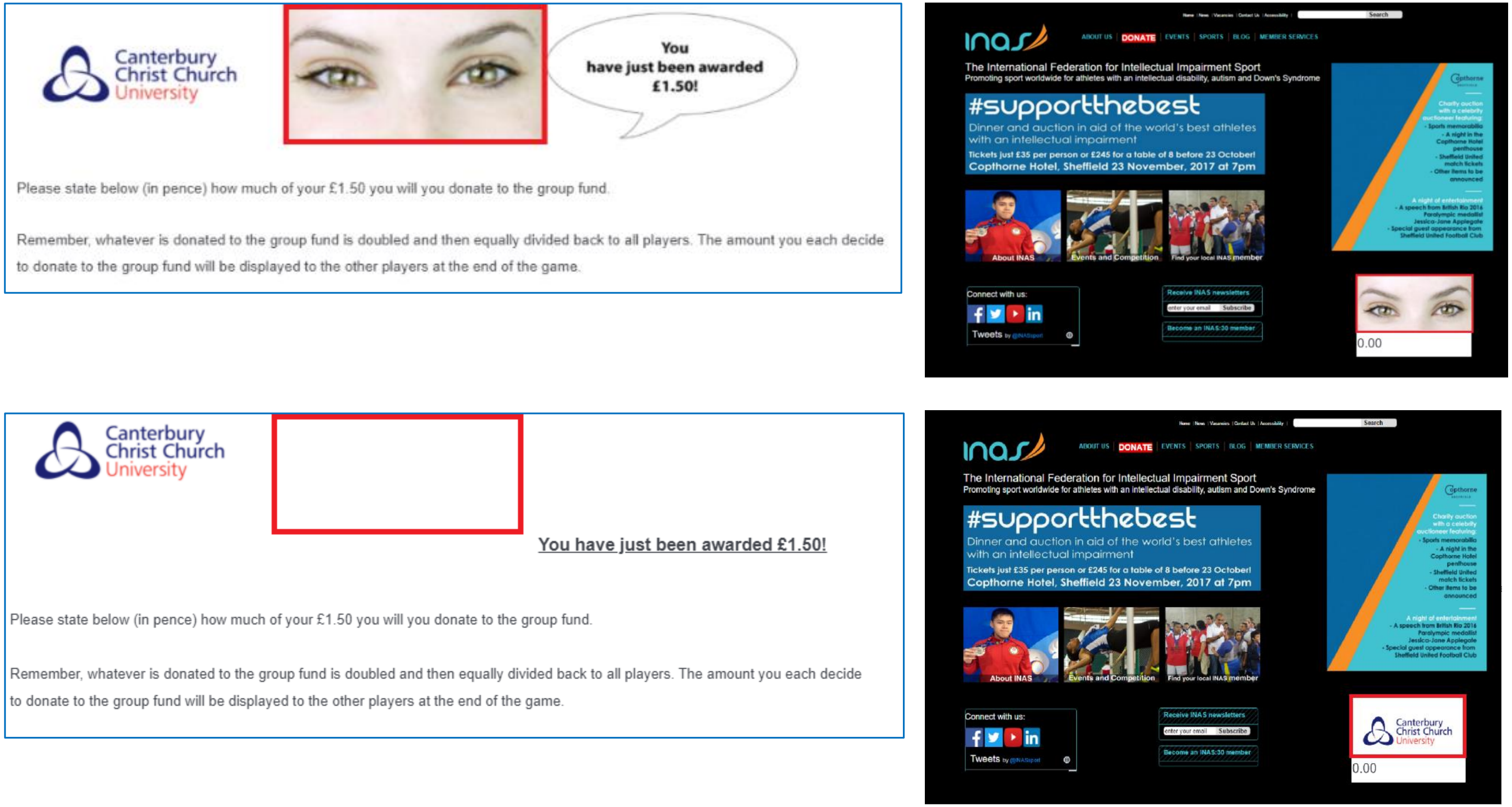
The participants were then taken through a series of manipulation checks to try to ascertain whether the presence of an image of a pair of eyes was successful in making them feel like they were being observed. For example, participants were asked whether they felt like they were being observed at all whilst completing the survey (and if so, to explain why), whether or not they saw an image of eyes during the task and if so, what was the gender of the eyes, and what emotions the eyes were displaying.

Participants then completed survey items that covered a range of demographic and individual differences that are potentially associated with prosociality (e.g., Saunders et al., 2016). Basic demographic data were collected on age, gender, ethnicity, and country of residence. Data were also collected on a range of potential covariates for the watching-eyes effect such as how charitable they considered themselves to be, and what they thought the study was about. These questions were asked after the prime (e.g., after the eye stimuli were displayed) and donation opportunity so they did not interfere with the effects of the prime (see general methodology chapter for a full overview and discussion of all survey

**Figure 4.1**

*Example of the areas of interest (AOI) for each condition*

**Location two (the public goods game)** **Location three (pseudo webpage/donations to charity)**



questions). As in study one, the demographic and possible covariate data was only intended to be explored in more detail if evidence for the watching-eyes effect was found in the first place.

### *Data Analysis*

**Survey data.** A series of one-way ANOVAs were planned to explore whether there were significant differences across three groups (static, blinking and no-eyes) in three measures of prosocial behaviour: 1) the percentage of their initial bonus that participants donated to the public goods pot, 2) the percentage of their final bonus that participants donated to charity and 3) the number of hours that the participants indicated that they were willing to volunteer per month to charity activities. The distribution of the amount that the participants donated to the public goods pot and donated to charity, and the number of hours that they were willing to volunteer to charity, were found to not be normally distributed as assessed by Shapiro-Wilk's test ($p < .05$). As the data had a bimodal distribution, reflect and logarithmic and logarithmic transformations were applied respectively but they failed to transform the data to approximate a normal distribution. Therefore, non-parametric Kruskal-Wallis H tests were conducted instead, which assessed the distribution of median scores. The effect sizes for any significant results were calculated using Pearson's *r* by follow-up post hoc analysis using Mann-Whitney tests for focused pairwise comparisons (Field & Hole, 2003). The effect sizes for any non-significant results were calculated using Epsilon square (ε2) instead (Tomczak and Tomczak, 2014).

As feelings of being watched may be a key component in the watching-eyes effect (Bateson et al., 2006), in study one, the main survey analysis was re-run on just those participants who reported that they felt as if they were being watched during the survey. However, due to low sample size numbers in study two, the analysis could not be conducted on the participants in this study. As study two was specifically designed to address the anonymity limitation identified in study one, it could be expected that levels of prosocial behaviour would be higher in study two when compared to study one, therefore post hoc exploratory analysis was conducted using a series of Mann-Whitney U tests to see if there were any significant differences in the percentage of initial bonus donated to the public goods pot, the percentage of total bonus donated to charity and the total volunteer hours between the two studies.

**Eye-tracking.** Participants were exposed to the different eye group stimuli at three different locations during the survey; 1) in a banner at the top of the instructions to the public goods game page, 2) in a banner at the top of the page when the participants were asked how much they wanted to donate to the public goods game, and 3) on top of the donation box on the pseudo charity website. In this study, there were two eye-tracking metrics used to assess gaze behaviour; time to first fixation (ms) on the AOI ('Time to 1$^{st}$ Fixation') and percentage of total time (ms) spent observing the AOI ('%Total Time')

To explore attentiveness to the eye images, a one-way ANOVA was planned to assess whether there was a significant difference between conditions (static, blinking or no-eyes) in the percentage of total time (ms) spent looking at the AOI. To explore whether the 'blinking eyes' stimuli were more salient (i.e., more effective in capturing participant attention) than the 'static' or 'no-eyes' stimuli, a series of one-way ANOVAs were planned to assess whether there was a significant difference between conditions (static, blinking or no-eyes) in the time to first fixation (ms) on the AOI at locations one, two and three. However, as the dependent variables were not normally distributed in either test, again non-parametric Kruskal Wallis H tests were conducted instead, which assessed the distribution of median scores. The effect sizes for any significant results were calculated using Pearson's *r* by follow-up post hoc analysis using Mann-Whitney tests for focused pairwise comparisons (Field & Hole, 2003). The effect sizes for any non-significant results were calculated using Epsilon square ($\varepsilon^2$) instead (Tomczak and Tomczak, 2014).

As habituation to eye images is one possible explanation to why a watching-eyes effect was not found in the first empirical study (Sparks & Barclay, 2013), post hoc exploratory analysis was conducted using a series of one-way repeated measures tests which compared the percentage of total time the participants looked at the three AOIs, i.e., location one (at the top of the public goods game instructions), location two (at the point where participants were playing the public goods game), and location three (at the point of donating to charity), over time. A decrease in the percentage of total time looking at the stimuli across the locations (i.e., time) could be an indication of habituation (Sparks & Barclay, 2013). As the dependent variables were not normally distributed, non-parametric Friedman tests were conducted on the static and blinking groups (e.g., the groups that were exposed to the eye stimuli), which assessed the distribution of median scores. These tests were followed up with Wilcoxon tests (from which z-scores were extracted for each comparison) to calculate an effect size using Pearson's *r* (Field, 2009).

**Power Analysis.** An *a priori* power analysis was conducted using G*Power3 (Faul et al., 2007) for a one-way ANOVA with three groups to be able to obtain a large effect size (.50), with an alpha of .05 and power of 0.80. Results showed that a total sample of 64 participants was required to achieve a power of .80. However, post hoc power analyses were conducted for both the survey and eye-tracking data as the study failed to obtain the required number of participants.

The results revealed that for the survey data, the Kruskal-Wallis H tests achieved a power of 0.60 for a large effect size (0.4), 0.27 for a medium effect size (0.25) and 0.08 for a small effect size (0.1). The chi-square tests achieved a power of 0.89 for a large effect size (0.5), 0.45 for a medium effect size (0.3) and 0.08 for a small effect size (0.1). The Mann-Whitney U tests achieved a power of 0.86 for a large effect size (0.5), 0.45 for a medium effect size (0.3) and 0.09 for a small effect size (0.1) (see Table 4.1).

For the eye-tracking data, the Kruskal-Wallis H tests achieved a power of 0.58 for a large effect size (0.4), 0.25 for a medium effect size (0.25) and 0.08 for a small effect size (0.1) (see Table 4.1). The Friedman tests for the static group ($n$ = 14) achieved a power of 0.98 for a large effect size (0.5), 0.63 for a medium effect size (0.3) and 0.11 for a small effect size (0.1). The sample for the blinking group ($n$ = 15) for the Friedman tests achieved a power of 0.99 for a large effect size (0.5), 0.67 for a medium effect size (0.3) and 0.12 for a small effect size (0.1) (see Table 4.1).

**Manipulation Checks.** As part of manipulation and attention checks, chi-square tests were planned to examine the relationship between the different eye groups and the eye stimuli. First, a chi-square test was conducted between the groups (static/blinking/no-eyes) and whether participants reported seeing an image of eyes (no/yes/not sure). Second, as a further attention check, the participants were asked that if applicable, what was the gender of the eyes that they saw (male/female/not sure/N/A). Lastly, a chi-square test was also conducted to examine the relationship between conditions and whether participants reported feelings of being watched whilst completing the survey (no/yes).

**Table 4.1**
*A summary of the power achieved for each sample*

| | Survey Analysis | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Chi-square ($N$ = 49) | | | Mann-Whitney | | | Kruskal-Wallis* (N = 42) | | |
| Effect Size | Small (0.1) | Medium (0.3) | Large (0.5) | Small (0.1) | Medium (0.3) | Large (0.5) | Small (0.1) | Medium (0.25) | Large (0.4) |
| Power | 0.08 | 0.45 | 0.89 | 0.09 | 0.45 | 0.86 | 0.08 | 0.27 | 0.60 |

| | Eye-tracking Analysis | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Kruskal-Wallis* (between groups) | | | Friedman (within groups) | | | | | |
| | ($N$ = 40) | | | Static group ($N$ = 14) | | | Blinking group ($N$ = 15) | | |
| Effect Size | Small (0.1) | Medium (0.25) | Large (0.4) | Small (0.1) | Medium (0.3) | Large (0.5) | Small (0.1) | Medium (0.3) | Large (0.5) |
| Power | 0.08 | 0.25 | 0.58 | 0.11 | 0.63 | 0.98 | 0.12 | 0.67 | 0.99 |

*Note:* The post-hoc power analysis were conducted using G*Power 3.1.9.4, an alpha of 0.05 and the achieved sample size for each test.
*Power analysis on Kruskal-Wallis tests were calculated with 15% of the sample removed from the original sample size for the

**Ethics**

In line with the British Psychology Society's ethical guidelines (The British Psychological Society, 2018), consent was taken from each participant. It was made clear that participation was voluntary, and all data is kept strictly confidential with no identifying information of the participant stored with the data. This study was granted full ethical compliance by the Ethics Chair at Canterbury Christ Church University (Ref: Jenner-18/SAS/20F).

## Results

In total, the survey consisted of 49 participants who were randomly assigned to the 'static' ($N$ = 16), 'blinking' ($N$ = 16) or the 'no-eyes' ($N$ = 17) condition. Participants consisted of 35 females and 14 males ranging in age from 18 to 58 ($M$ = 27.43, $SD$ = 10.49). Initially, 63 participants were recruited for study two, but there were technical issues with the survey loading and saving onto the laboratory computer which meant that the data did not save for all participants. Overall, 14 participants' survey data and 16 participants' eye-tracking data was lost.

**Prosocial Behaviour**

***Hypothesis 1.*** There will be a significant difference between conditions in the percentage of their initial bonus that participants choose to donate to the public goods game.

A Kruskal-Wallis H test was conducted to explore whether there were any significant differences between conditions in the percentage (0-100%) of their initial bonus that participants donated to the public goods pot. There were no significant differences between the static ($Mdn$ = 83.33, IQR = 52.50 – 100.00), blinking ($Mdn$ = 63.33, IQR = 47.5 - 96.00), and no-eyes ($Mdn$ = 66.67, IQR = 33.03 – 100.00) groups, $H(2) = 1.168$ $p = .445$, $\varepsilon^2 = 0.03$, 90% CI [0.01, 1.00] (see Figure 4.2).

***Hypothesis 2.*** There will be a significant difference between conditions in the percentage of their final bonus that participants choose to donate to charity.

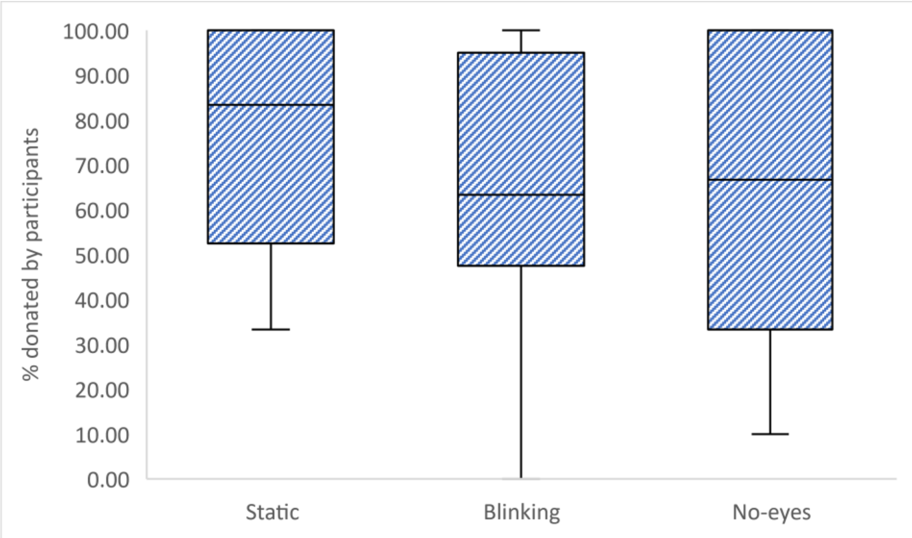
A Kruskal-Wallis H test was conducted to explore whether there were any significant differences between conditions in the percentage (0 – 100%) of their final bonus that participants donated to charity. There was a marginal significant difference between the static (*Mdn* = 51, IQR = 0.00 – 100.00), blinking (*Mdn* = 0, IQR = 0.00 – 0.00), and no-eyes (*Mdn* = 41, IQR = 0.00 – 93.50) groups, $H(2) = 5.189$ $p = .055$, $\varepsilon^2 = 0.12$, , 90% CI [0.04, 1.00] (see Figure 4.3).

***Hypothesis 3.*** There will be a significant difference between conditions in the number of hours per month participants indicated they would be willing to volunteer their time for charitable activities.

A Kruskal-Wallis H test was conducted to explore whether there were any significant differences between conditions in the total number of hours (participant responses ranged from 0 to 120 hours via an open-text response) that participants indicated that they would be willing to volunteer for charitable activities. There were no significant differences between the static (*Mdn* = 15, IQR = 10.50 – 35.75), blinking (*Mdn* = 19, IQR = 8.00 – 48.00) and no-eyes (*Mdn* = 14, IQR = 6.00 – 44.00) groups, $H(2) = .177$, $p = .915$, $\varepsilon^2 = 0.003$, 90% CI [0.00, 1.00] (see Figure 4.4).

**Figure 4.2**

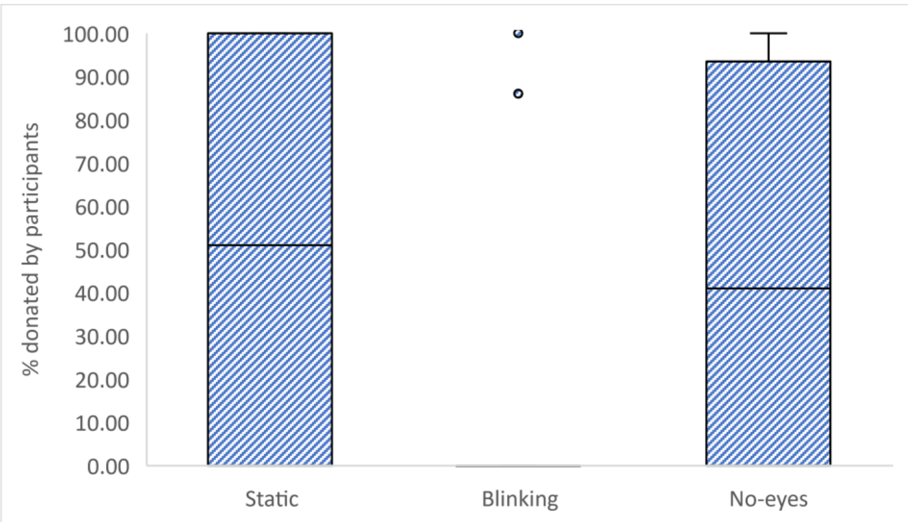
*Percentage of their initial bonus participants donated to the group fund*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, and the bold horizontal line indicates the median.

**Figure 4.3**

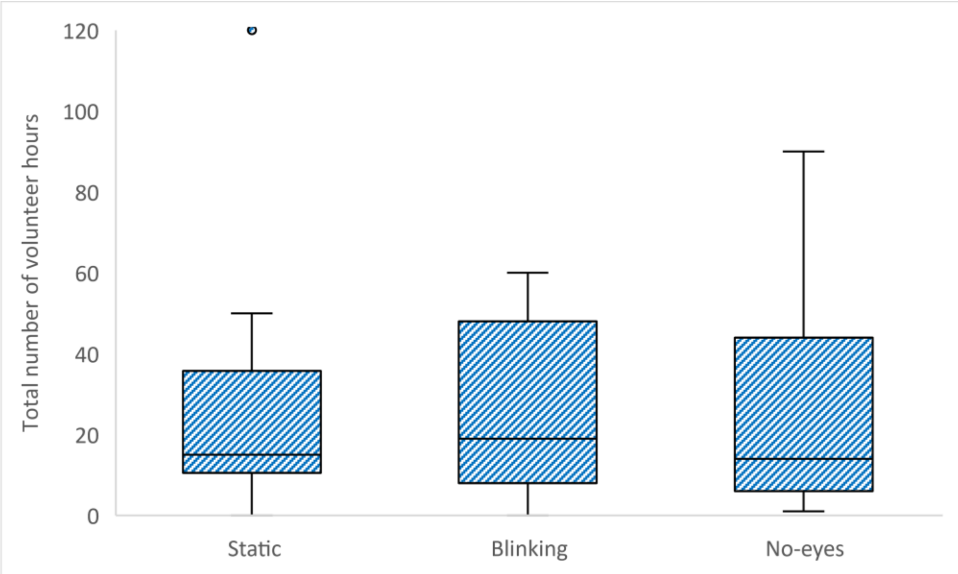
*Percentage of their initial bonus participants donated to charity*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show outliers.

**Figure 4.4**

*Total number of participant volunteer hours*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show outliers.

## Gaze Behaviour

In total, the eye-tracking data consisted of 47 participants who were randomly assigned to the 'static' (*N*=15), 'blinking' condition (*N*=16) or the 'no-eyes' condition (*N*=16). The eye-tracking data was captured separately from the survey data, so it is not possible to summarise participant gender and age for the eye-tracking data.

***Hypothesis 4*[12].** There will be a significant difference between conditions in the percentage of total time (ms) spent looking at the AOI at location point location one (at the

[12] Due to differences in AOIs across locations (e.g., amount of time there were present on the screen), a post-hoc analysis was conducted using total time (ms) as a metric instead of percentage of total time (ms). The analysis using this new dependent variable did not significantly change the results. Please see APPENDIX D.

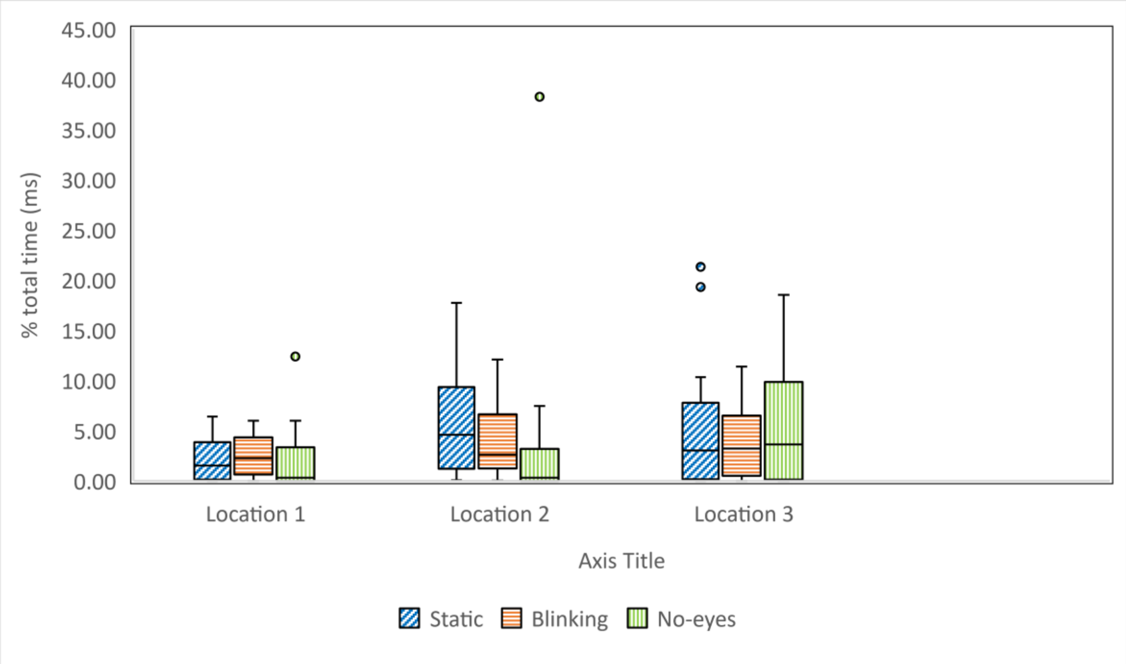
top of the public goods game instructions), location two (at the point where participants were playing the public goods game), and location three (at the point of donating to charity).

A series of Kruskal-Wallis H tests were conducted to explore whether there were any significant differences between conditions in the percentage of total time (ms) that participants spent looking at the AOI at locations one (total time ranged from 0 to 12.40%), two (total time ranged from 0 to 38.25%), and three (total time ranged from 0 to 21.33%).

There were no significant differences at location one (the instructions page) between the static (*Mdn* = 1.54, IQR = 0.13 – 3.88), blinking (*Mdn* = 2.45, IQR = 1.09 – 4.70) and no-eyes (*Mdn* = 0.33, IQR = 0.01 – 2.66) groups, $H(2) = 4.102$, $p = .129$, $\varepsilon^2 = 0.09$, 90% CI [0.03, 1.00]. However, those in the static (*Mdn* = 4.60, IQR = 1.23 – 9.34) and blinking (*Mdn* = 1.72, IQR = 0.96 – 5.28) groups spent significantly more time looking at the AOIs at location two (during the public goods game) than those in the no-eyes (*Mdn* = 0.28, IQR = 0.13 – 1.75) groups, $H(2) = 10.582$, $p = .004$. Post hoc Mann-Whitney U test revealed that the percentage of total time was significantly different between the no-eyes and static groups, $p = .003$, $r = -0.47$, 90% CI [-0.64, -0.26], and there were marginally significant differences between the no-eyes and blinking groups, $p = .06$, $r = 0.33$, 90% CI [0.09, 0.53] and between the static and blinking groups, $p = 0.84$, $r = -0.15$, [-0.38, 0.10]. The percentage of total time at location three (at the point of donating to charity) did not differ significantly between the static (*Mdn* = 3.06, IQR = 0.19 – 7.78), blinking (*Mdn* = 3.65, IQR = 0.00 – 7.15) and no-eyes (*Mdn* = 4.73, IQR = 2.54 – 11.59) groups, $H(2) = 1.914$, $p = .384$, $\varepsilon^2 = 0.04$, 95% CI [0.01, 1.00] (see Figure 4.5).

**Figure 4.5**

*Percentage of time (ms) participants spent looking at stimuli in each location*



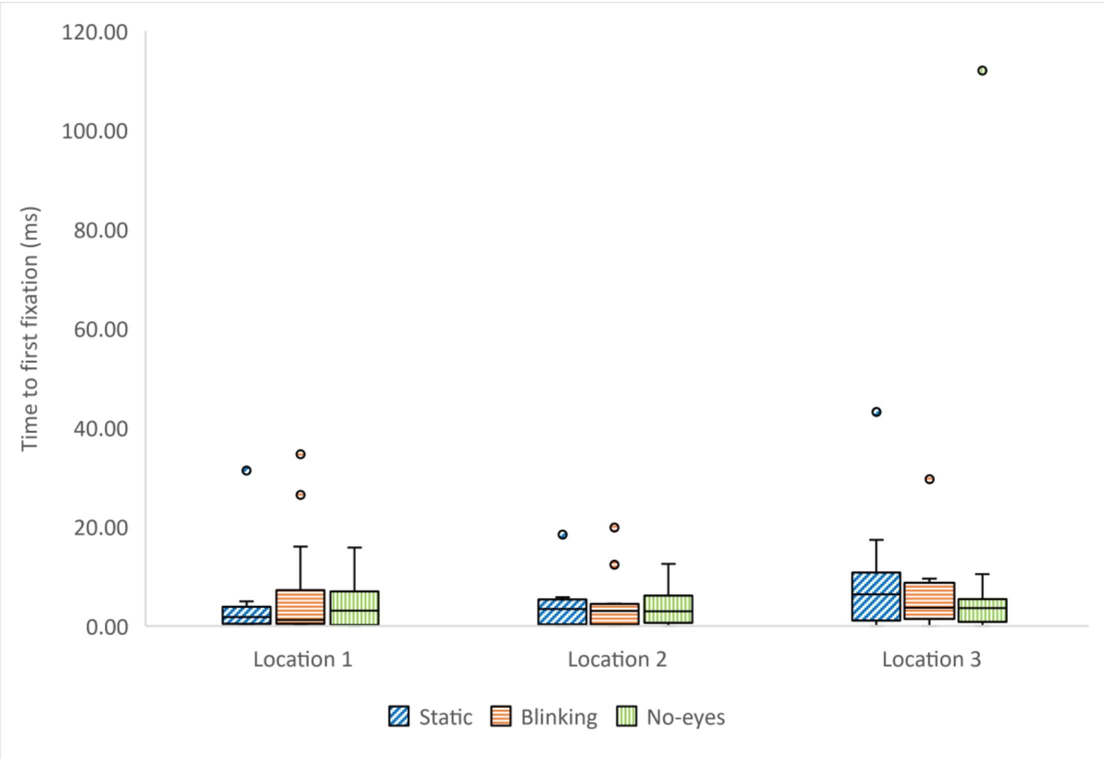

*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and the circles show the outliers.

***Hypothesis 5.*** There will be a significant difference between conditions in the time to first fixation (ms) on the AOI at location one (at the top of the public goods game instructions), location two (at the point where participants were playing the public goods game), and location three (at the point of donating to charity).

A series of Kruskal-Wallis H tests were conducted to explore whether there were any significant differences between conditions in the time to first fixation (ms) to the AOI at locations one, two and three. The time to first fixation at location one ( time to first fixation ranged from 0 to 34.66 ms) did not differ significantly between the static (*Mdn* = 1.82, IQR = 0.48 – 3.89), blinking (*Mdn* = 1.23, IQR = 0.43 – 7.24) and no-eyes (*Mdn* = 3.11, IQR = 0.21 – 6.98) groups, $H(2) = .104$, $p = .949$, $\varepsilon^2 = 0.003$, 90% CI [0.00, 1.00]. The time to first fixation also did not differ significantly at location two (time to first fixation ranged from 0 to 19.86 ms) between the static (*Mdn* = 3.54, IQR = 0.33 – 5.36), blinking (*Mdn* = 2.62, IQR = 0.30 – 4.40) and no-eyes (*Mdn* = 2.94, IQR = 0.62 – 6.14) groups, $H(2) = .024$, $p = .902$, $\varepsilon^2 = 0.004$, 90% CI [0.01, 1.00], or at location three (time to first fixation ranged from 0 to 111.98 ms) between the static (*Mdn* = 6.66, IQR = 1.71 – 10.84), blinking (*Mdn* = 3.62, IQR = 0.70 – 8.06) and no-eyes (*Mdn* = 3.60, IQR = 0.83 – 5.43) groups, $H(2) = .549$, $p = .460$, $\varepsilon^2 = 0.04$, 90% CI [0.01, 1.00] (see Figure 4.6).

**Figure 4.6**

*Time to first fixation (ms)*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show outliers.

**Manipulation Checks**

To check whether the participants who had been exposed to an image of eyes correctly reported seeing an image of eyes, a chi-square test was conducted between condition (static/blinking/no-eyes) and whether participants reported seeing an image of eyes (no/yes/not sure). There was a significantly higher proportion of participants who reported seeing an image of eyes within the blinking condition (93.8%), followed by the static condition (68.8%) and then the no-eyes condition (0%), as assessed by a Fisher's exact test, $p < .001$. Post hoc analysis involved pairwise comparisons with a Bonferroni correction. Statistical significance was accepted at $p < .016$ (0.05/3). Tests revealed there were significant differences between the static and blinking, $\chi2(4, N = 32) = 3.614$, $p = .016$, $V = .336$, 90% CI [0.22, 0.55], static and no-eyes, $\chi2(4, N = 33) = 18.987$ $p = < .001$, $V = .759$, 90% CI [0.61, 0.94], and the blinking and no-eyes groups, $\chi2(2, N = 135) = 29.247$, $p = < .001$, $V = .941$, 90% CI [0.84, 1.00] (see Figure 4.7).

The participants were then asked, if applicable, what was the gender of the eyes that they saw (male/female/not sure/N/A). There was a marginally significantly difference in the number of participants who correctly reported seeing an image of female eyes within the blinking condition (86.7%), compared to those in the static condition (70%) as assessed by a Fisher's exact test, $p = 0.061$, $V = .417$, 90% CI [0.21, 0.68] No participant in the no-eyes group erroneously reported seeing an image of eyes (see Figure 4.8).

In total, 24.5% of participants reported feelings of being watched whilst completing the survey. A chi-square test was conducted to examine the relationship between conditions and whether participants reported feelings of being watched whilst completing the survey (no/yes). There were no significant differences between the static (18.8%), blinking (37.5%) and no-eyes (18.8%) condition in the proportions of participants who felt like they were being watched, as assessed by a Fisher's exact test, $p = .530$, $V = .20$. 90% CI [0.16, 0.40] (see Figure 4.9).

**Figure 4.7**

*Proportions of participants who reported seeing eyes by condition*

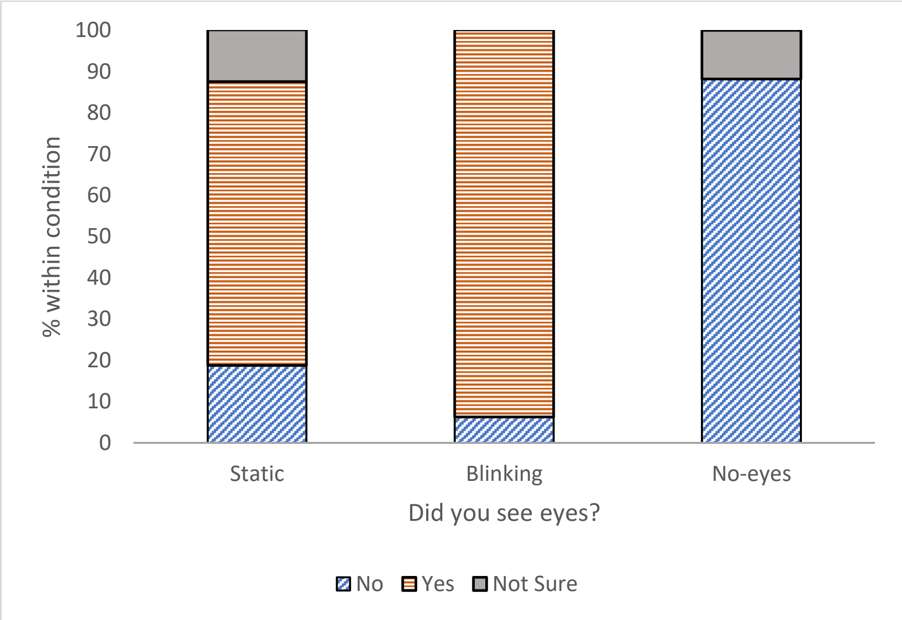



**Figure 4.8**

*Proportions of participants reporting which gender of eyes they saw*

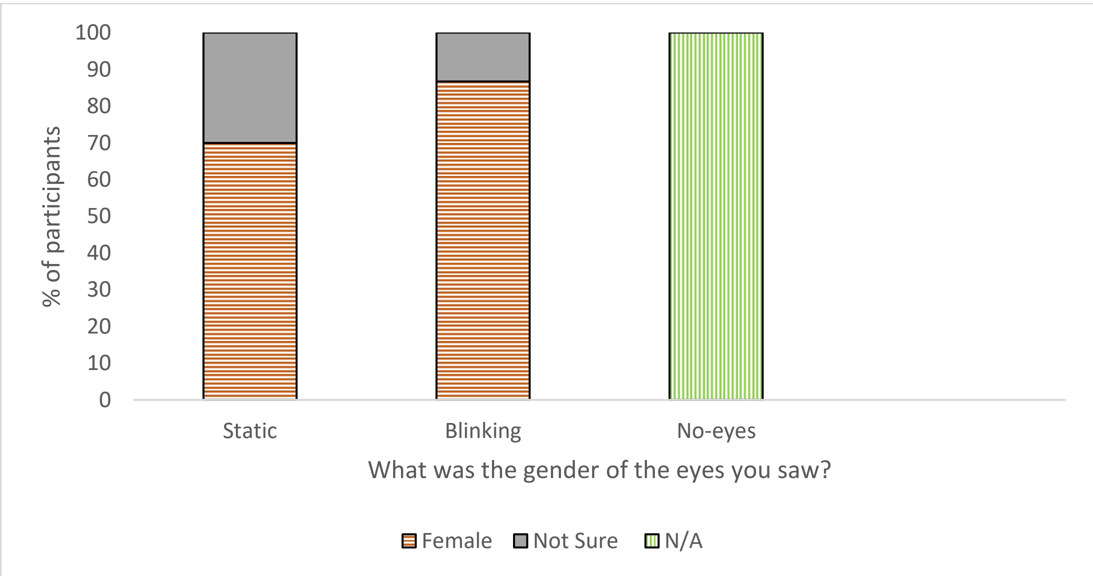
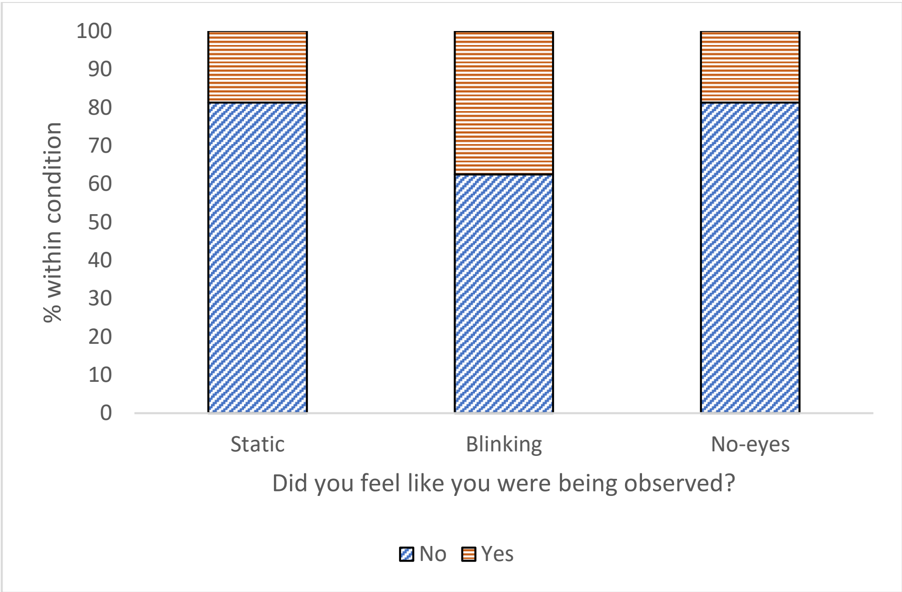


**Figure 4.9**

*Proportions of participants who felt like they were being observed*



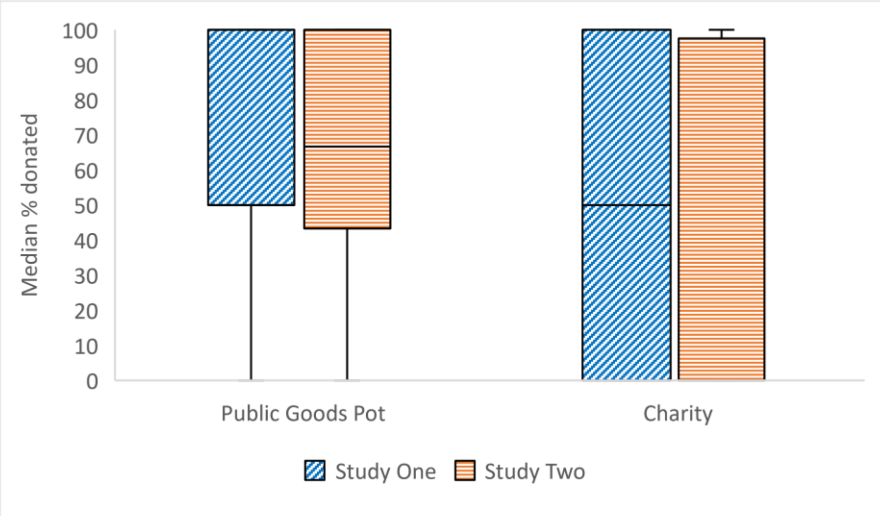

**Exploratory data analysis**

***Comparing Study 1 and Study 2 Measures of Pro-social Behaviour.*** A series of Mann-Whitney U-tests were conducted to see if there were any significant differences in the percentage of initial bonus donated to the public goods pot (0-100%), the percentage of total bonus donated to charity (0-100%) and the total volunteer hours between study one and study two (participant response range from 0 -120 hours via an open-text response).

Results show that participants in study one donated a significantly higher percentage of their initial bonus to the public goods pot (*Mdn* = 100, IQR = 50.00 – 10.00) than those in study two (*Mdn* = 66.67, IQR = 43.34 – 100.00), *U* = 4070, *z* = -2.150, *p* =.032, *r* = 0.14, 90% CI [0.04, 0.24], and donated significantly higher percentage of their final bonus to charity (*Mdn* = 50, IQR = 0.00 – 100.00) than those in study two (*Mdn* = 0, IQR = 0.00 – 97.50), *U* = 4060, *z* = -2.126, *p* = .033, *r* = 0.13, 90% CI [0.03, 0.23] (see Figure 4.10). However, participants

**Figure 4.10**

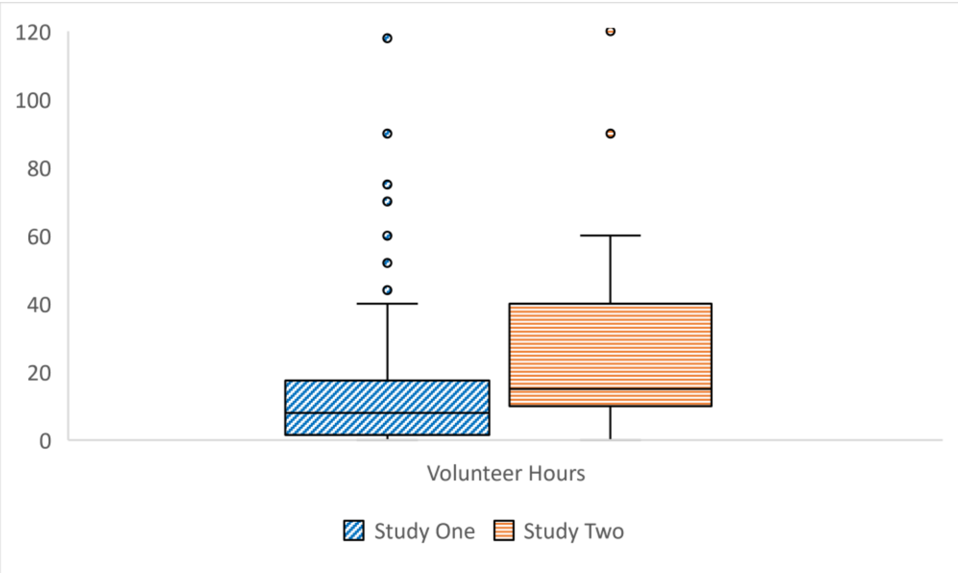
*Median percentage of initial bonus donated to the public goods pot and to charity*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, and the bold horizontal line indicates the median.

**Figure 4.11**

*The median number of volunteer hours per month*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show the outliers.

in study two indicated that they would be willing to volunteer significantly more hours per week (*Mdn* = 15, IQR = 9.50 – 44.00) than those in study one (*Mdn* = 8, IQR = 1.00 – 18.00), $U$ = 6948, $z$ = 4.331, $p$ < .001, $r$ = -0.27, 90% CI [-0.36, -0.17] (see Figure 4.11).

***Testing habituation to the eye images***[13]**.** Non-parametric Friedman tests were conducted to assess whether there were any significant differences between the static and blinking groups in the percentage of total time spent observing the corresponding AOI across location one (at the top of the public goods game instructions), location two (at the point where participants were playing the public goods game), and location three (at the point of donating to charity).

The percentage of time that participants in the static eyes group spent looking at the AOI did not significantly change between location one (*Mdn* = 1.54, IQR = 0.13 – 3.88) and location two (*Mdn* = 4.60, IQR = 1.23 – 9.34), location one and location three (*Mdn* = 3.06, IQR = 0.19 – 7.78), or location two and location three, $\chi^2(2, N = 14) = 4.429$, $p$ = .109, $r$ = 0.38, 90% CI [0.25, 0.50].

The percentage of time that participants in the blinking group spent looking at the AOI also did not significantly change between location one (*Mdn* = 2.45, IQR = 1.09 – 4.70) and location two (*Mdn* = 1.72, IQR = 0.96 – 5.28), location one and location three (*Mdn* = 3.65, IQR = 0.00 – 7.15), or location two and location three, $\chi^2(2, N = 15) = .933$, $p$ = .627, $r$ = 0.08, 90% CI [-0.08, 0.22] (see Figure 4.12).

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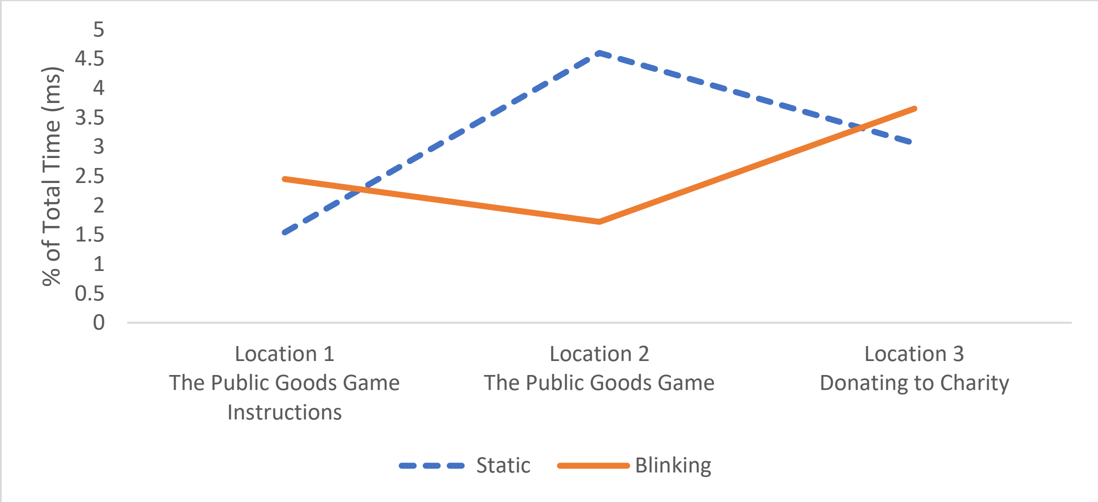
[13] As with hypothesis 4, due to differences in AOIs across locations (e.g., amount of time there were present on the screen), a post-hoc analysis was conducted using total time (ms) as a metric instead of percentage of total time (ms). The analysis using this new dependent variable also did not significantly change these results. Please see APPENDIX D.

**Figure 4.12**

*Percentage of total time participants spent looking at the eye stimuli at each location*




## Discussion and Conclusion

### Summary

The first aim of this second empirical study was to test whether the presence of eye cues positively affected prosocial behaviour in an online environment when trying to specifically evoke reputational concerns (as opposed to leading the participants to believe their actions were anonymous like in study one). Results demonstrated that as with study one, the presence of eyes during the survey did not significantly impact the amount that the participants donated to the public goods pot, or the number of volunteer hours. There was a marginal significant difference and moderate effect size between conditions in the amount donated to charity. In line with the first empirical study of this thesis and other recently published studies (e.g., Northover et al., 2017), this study does not provide evidence that supports the notion that either the presence or salience of eyes positively affects prosocial behaviour.

The second aim of this second empirical study was to determine whether the participants paid attention to the image of eyes whilst completing prosocial tasks. The percentage of total time ('% Total Time') that participants spent looking at the area of interest

(AOI) was used to measure how much attention the participants were paying to the stimuli at location one (the public goods game instructions), location two (the point where participants were playing the public goods game), and location three (the point of donating to charity). Results showed that the '% Total Time' did not significantly differ between the static, blinking and no-eyes groups at locations one, or three. However, at location two, at which point participants were playing the public goods game, participants in the static group spent significantly more time looking at the AOI compared to the no-eyes group. There were marginal significant differences and moderate effect sizes between the blinking and no-eyes group, and the static and blinking groups. However, this increased attentiveness to the eye images at location two did not result in an increased level of prosocial behaviour.

The time to first fixation (ms) on the AOI ('Time to 1$^{st}$ Fixation') at locations one, two, and three was used to determine if participants looked at the blinking eyes quicker than those who looked at the static eyes or no-eyes stimuli. This was to ascertain whether blinking eyes were more salient than static eyes as theorised. Results showed that the 'Time to 1$^{st}$ Fixation' did not significantly differ between the static, blinking and no-eyes groups at locations one, two, or three, suggesting that the blinking eye stimuli were not more salient than the static eye stimuli and that the two types of eye stimuli were not more salient than the control.

Exploratory analysis was conducted to see if there were any significant differences in prosocial behaviour between study one and study two. In all prosocial measures, there were significant differences in prosocial behaviour between the two studies. Participants from study one donated significantly more money to both the public goods pot and to charity than those in study two. However, participants from study two indicated that they were willing to volunteer their time to charity for significantly more hours than those in study one. This suggests that if reputational concern does have a positive impact on the watching-eyes effect and prosocial behaviour, it may be dependent on the type of prosocial behaviour (Dear et al., 2019). Contrary to much of the literature which posits that the mechanisms of the watching-eyes effect lie in reputational concern (Cañigueral & Hamilton, 2019b), as there were no significant differences between groups *within* study two, this study does not provide any evidence that attempting to lead the participants to believe that their actions would be made public has an effect on watching eyes and prosocial behaviour.

Exploratory analysis was also conducted to see if there were any significant differences within the static and blinking groups in the percentage of total time ('% Total Time) spent observing the eye stimuli across location one (the public goods game instructions), location two (the point where participants were playing the publics good game), and location three (the point of donating to charity), which may have been an indicator of participants becoming habitualised to the eye stimuli. Studies have shown that habituation occurs when individuals are exposed to eye images for prolonged periods of time, leading to a decrease in participant responsiveness to the eye cues (Oda, 2019). Results showed that although there was a moderate effect size for the static group, there were no significant differences in the '% Total Time' that participants spent looking at the eye stimuli across locations one, two, and three, for either the static or blinking group suggesting that the lack of evidence for the watching-eyes effect in this study could not be attributed to participants becoming habitualised to the eye images.

Overall, this study does not provide evidence that the 'blinking eyes' stimuli were more effective in capturing participant attention than the 'static' or 'no-eyes' stimuli and it does not provide evidence that participants became habitualised to the eye images over time. As the participants in the static and blinking groups spent significantly more time looking at the AOI during the public goods game than those in the control group, this study provides some evidence that participants were aware of and paid attention to the eye images but this did not translate into an increase in prosocial behaviour. However, as with study one, issues were identified with low statistical power and equivalence testing which means that these conclusions are not definitive. This will be discussed further in the limitations section at the end of the chapter.

**Prosocial behaviour**

***Differences between this Study and Empirical Study One***

Research has shown that people behave more cooperatively when their identifiable reputations are at stake than when their identities are anonymous (Milinski et al., 2002). Therefore, it could be anticipated that the watching-eyes effect would have had a greater impact on prosocial behaviour within study two, which specifically tried to evoke reputational concerns, when compared to study one, where participants were told that their actions would remain anonymous. However, the results did not provide any evidence that the presence or salience of eyes positively impacted prosocial behaviour.

In fact, participants in study two donated significantly less to both the public goods pot and to charity than those in study one. The differences in donation behaviour between the two studies could be explained by the different samples used in each study (Vaish et al., 2017) and a range of factors such as cultural differences (Bekkers, 2017), different motivations to donate (Henrich et al., 2010) and the different recipients of the public goods money and charitable donations (Bekkers, 2017).

**Different Participant Samples.** In study one, participants were recruited from the online crowdsourcing websites, MTurk and CfP, which allowed for a more heterogeneous sample of participants to be recruited. As study two took place in a laboratory setting, it required people to take part in the experiment in person in the University's psychology labs, thus convenience sampling was used to recruit participants. As discussed in the methodology chapter (see chapter two), this meant relying on psychology undergraduate students from Canterbury Christ Church University (CCCU) who were, as with most Psychology university students in Britain, predominantly white, young and female (e.g., Bourrat et al., 2011) and thus a more homogenous sample.

Studies conducted using a more diverse subject pool have often found stark differences in social behaviour when cultural differences are taken into account (e.g., Krupp et al., 2005; Raihani & Bshary, 2012). For example, in a public goods experiment comparing Canadian, mainland Chinese, and Hong Kong students, low anonymity conditions led Chinese students to behave cooperatively but those conditions had no effect on Canadians (Krupp et al., 2005).

Alternatively, there may have been key differences in motivations to donate between the two study samples (Henrich et al., 2010). In the previous chapter, it was speculated that MTurk participants were less likely to give money as they were likely to be financially motivated to participate in the study whereas the CfP participants, who were not paid to participate in the study, were likely to give more money as they were theorised to be more altruistically motivated. In this study, as participants were mainly recruited through the University’s Research Participation Scheme (RPS), it is possible that the student sample used would be even less likely to be generous with their money than the participants in study one, as they were motivated to participate in this study as a course requirement. This could have meant that there was no financial or altruistic motivation to encourage any donations to the public goods game or to charity.

It should be considered that there was a difference in the recipients of the public goods game and charity donations between the two studies. In study one, participants were led to believe that they were playing the public goods game against anonymous participants. In this study, participants were led to believe that they were playing against other players. However, even if the participants did believe that they were playing against real people, they had no information about those people. In their watching eyes experiment, Mifune et al., (2010) found that eye cues promote altruistic behaviour toward in-group but not out-group members, which suggests that the lack of information on group membership in this study could have been a contributing factor to the lower public goods pot donations observed.

**Differences between the Surveys.** Although the surveys between study one and study two were largely the same, there were some key differences which may have been confounding factors such as the differences in the way the charitable donations were collected, in the information provided on the recipient of the charitable donations, and between the environment that participants completed the survey in.

In study one, as part of the survey, participants were asked if they wanted to donate any of their winnings to charity. It was decided to leave the recipient of the charitable donations anonymous so the choice of charity could not bias whether participants chose to donate. In this study, participants were taken to a mock-up of a real charity website to make any donations to charity and were specifically advised that any donations would be made public to the charity in question. It was hoped that utilising this method to measure charitable donations would help make the participant believe that their behaviour was observable by others and provide a situation in which they could enhance their prosocial behaviour if they so desired. To be able to make a mock-up of a real charity website, permission was needed from the charity. A decision was made to approach the charity INAS (International Sports Federation for Persons with Intellectual Disability) as the School of Psychology at CCCU already had an existing relationship with them. It is possible that as a relatively small charity with a niche cause, participants may not have heard of or related to INAS which could have biased their donation decisions. People often support organisations that promote their own preferences, that help people they feel some affinity with, and support causes that relate to their own life experiences (Breeze, 2013).

It should also be noted that although the participants in study one were asked to ensure that they were completing the survey alone and with no one else in the room, there is no way of knowing for certain whether the participants were in fact alone. If the majority of the participants in study one did have other people in the room while they were completing their survey, this could explain why they donated significantly more money to both the public goods pot and to charity than those in study two as it has been suggested that participants with total privacy (e.g., the participants in study two) may be immune to cues of being watched (Northover et al., 2016). However, even if the presence of others in the room increased donations to the public goods pot and to charity in study one overall, this did not result in a watching-eyes effect emerging

***Measurements of Prosocial Behaviour***

Studies involving economic games have shown that donation levels are higher when the initial endowment is obtained through a 'windfall' than when it is earned (Vaish et al., 2017). This was supported by the results of this study; the median amount donated to the group fund (the windfall) was higher than the median amount donated to charity (the earned amount). However, despite the higher donation amounts seen in the public goods game, the analysis of the effects of watching eyes on the public goods game in this study showed there were no significant differences between groups in the amount of their initial bonus that the participants chose to donate to the public goods pot.

It has been suggested that economic games may not be very suitable for measuring the watching-eyes effect as such games with low stakes often lead to the majority of participants donating (Nettle et al., 2013) and these potential 'ceiling effects' may overshadow potential differences across conditions (Manesi et al., 2016). In addition, a recent systematic review by Galizzi and Navarro-Martinez (2019) found that economic games (specifically one-shot games, as used in studies one and two) have poor external validity. However, as with study one, although donations to the public goods game were not the main measurement of prosociality. The public goods game provided a mechanism in which participants could earn a bonus in that they could then go on to donate to charity if they so wished.

As discussed in chapter two, the main aim of this thesis was to explore altruism (as opposed to prosocial behaviour) as a costly signal and arguably charity donations could be seen as a more reliable measure of altruism than the donations to the public goods game. Participants in the public goods game could have been motivated to participate in order to earn a bonus for themselves. Whereas there are no immediate benefits for participants to donate at least some of their bonus to charity, making any donations to charity a more altruistic act.

When examining the effects of the eye stimuli on donations to charity, there were no significant differences between groups in the amount of their final bonus that the participants chose to donate to charity. This in line with Raihani and Bshary (2012) who found that donations to a one-shot dictator game were not significantly different when presented with eye images in an online environment. Raihani and Bshary (2012) argued that in an online environment, their participants may have felt truly anonymous and therefore the eye images would not have been sufficient in triggering reputational concerns and therefore failed to have demonstrable effects on cooperative behaviour. It was anticipated that this would not be the case in study two as the participants were explicitly told that their actions would be made public, whereas the participants in Raihani and Bshary (2012) played a dictator game under completely anonymous conditions. There is a possibility that the participants may simply not have believed that the other participants were real and that their own actions would be made public. However, participants were provided with a free-text box at the end of the survey where they could provide comments on the study and no participant commented that they thought the other participants were not real.

It has been suggested that non-monetary forms (e.g., volunteering) of prosocial behaviour may be a reliable honest signal than donating to charity as at the point of donation, donating money is relatively less effortful and time-consuming than volunteering (Bradley et al., 2018). Therefore, the final measure of prosocial behaviour in this study was the number of hours per month that participants indicated they would be willing to volunteer their time to charity for. In line with similar studies (e.g., Bereczkei, Birkas, & Kerekes, 2010), participants in study two indicated that they would volunteer for significantly more hours than the participants in study one but there were no significant differences in volunteer hours between groups *within* study two. This may be because the self-reported intentions to volunteer may not a reliable prosocial signal as it is too easy to fake intention (Bradley et al., 2018) and self-reported prosocial intentions are sensitive to social desirability bias (the tendency to give socially desirable answers instead of answers that reflect the true feeling of the individual) (Tussyadiah & Miller, 2019). Future research into this area would benefit from testing whether social desirability bias mediates any watching-eyes effect on volunteering behaviour (Miyazaki, 2017).

***Feelings of Being watched***

Although the participants completed this study alone (i.e., there was no one else in the room with them) and were only exposed to the eye stimuli in an online environment, the participants were still in a laboratory setting. A limitation of laboratory-based studies is the difficulty in avoiding participants feeling watched even when they are not under surveillance (Dear et al., 2019). The very nature of participating in a lab-based study means that the participant is likely to be aware that their actions will be scrutinised by the researcher. Such a possibility should have led to a greater level of donations.

However, this is not supported by the results of this study. Despite the manipulation checks showing that the majority of participants noticed the eye stimuli when they were exposed to them, the manipulation checks found that just a quarter of all participants in study two reported feelings of being watched with no significant relationship found between condition and whether participants reported that they felt watched. This suggests that the eye images were not effective in evoking feelings of being watched within the participants.

***Messenger Effect***

Not only did Raihani and Bshary (2012) not find a relationship between watching eyes and donation behaviour, they found that donations were significantly higher in the *absence* of eye stimuli. They argue that these findings were due to a type of messenger effect; in their case, an established association between the control stimuli they used (i.e., an image of flowers) and a positive emotional state, which in turn could have led to more prosocial behaviour. There is evidence that signals of authority can generate compliant behaviour and people are more likely to act on information when a messenger has similar characteristics to them (Dolan et al., 2012). As the majority of participants in study two were students from CCCU, the university logo used as the control image at location three (at the point of donating to charity) in the 'no-eyes' group could have theoretically provided a sense of shared identity, authority and credibility which would have led to higher donations (Park & Reiner, 2019). However, this was not reflected in the results as there were no significant differences between the three groups in the percentage of total time spent looking at the AOI in location three and no differences between the three groups in the amount donated to charity.

Alternatively, the type of messenger used could have evoked strong negative emotions which could negate any incentive to act in a prosocial manner (Park & Reiner, 2019). For example, if someone has developed a distrust of government interventions, they may be less likely to listen to messages that they perceive to come from the government (Dolan et al., 2012). 'Watching-eyes' images often draw comparisons with George Orwell's 1984 (1949), and the infamous line "big brother is watching you" (e.g., Nelson, 2013) which has dark connotations of authoritarian societies and the erosion of personal privacy (Dear, 2018). The watching eyes images could evoke negative feelings of judgement (Yu et al., 2017) which could lead to avoidance behaviour (de Hamilton, 2016) and therefore it could be expected that the participants in the eye groups would make lower donations than those in the no-eyes group. However, as there were no significant differences in donation amounts between the three groups and the exploratory analysis on gaze behaviour showed that there was not a significant decrease in the percentage of total time that participants spent looking at the eye stimuli over time, this suggests that the lack of effect of watching eyes seen in this study cannot be attributed to participant avoidance behaviour.

The messenger effect works by promoting a certain message, product or outcome (Park & Reiner, 2019) which was not provided to the participants in this study. It has been suggested that watching eyes do not necessarily increase prosocial behaviour *per se* but encourage people to comply with social norms (Ayal et al., 2019). Prosocial behaviour can largely depend on social norms and beliefs that define how individuals should behave in certain situations and the expectation of deviation resulting in consequences such as rewards or punishments (Ikuse et al., 2018). Although presumably, the participants would have inferred that they were being encouraged to donate money to charity, only an option to donate was provided rather than an explicit message advising them to do so. This lack of social norm provision could be one reason why this study did not find any evidence for the watching-eyes effect.

**Gaze Behaviour**

The second aim of this study was to explore participants' gaze behaviour to try to ascertain whether they paid attention to the different eye stimuli during the prosocial tasks. Vaish et al. (2017) argue that an individuals' attentiveness to eyes, in so far as it reflects participants' reputational concerns, should be associated with prosocial behaviour in a public context. In other words, attentiveness to eye images should be an indicator of prosocial behaviour in a public setting.

Analysis was conducted to explore 1) whether there were any significant differences between groups (static, blinking or no-eyes) in how quickly participants first fixated on the AOI and 2) whether there were any significant differences between groups in the percentage of total time (ms) that participants spent looking at the AOI at locations one (the public goods game instructions), two (the point where participants were playing the publics good game), and three (the point of donating to charity).

Results showed that there were no significant differences between groups in how long it first took the participant to look at the AOI at locations one, two, or three. This measure was included to explore whether, as theorised, blinking eyes may be more salient than static eyes at capturing participant attention. These results indicate that not only were blinking eyes no more effective than static eyes at capturing attention but the eye stimuli, in general, were no more effective than the control stimuli in capturing the participants' attention.

The results also showed that there were no significant differences between groups in the percentage of total time that participants spent looking at the AOI at locations one or three. This result may seem counter intuitive as it could be expected that participants would look at an area on the screen with eye stimuli for a longer period than they would look at an area on the screen which contained no stimuli. However, this could be due to the other information on the screen. At location one (at the top of the instructions page), participants may have been focusing on the public good instructions and at the third location, a CCCU logo was presented instead of a blank space which meant that the participants had something else to look at.

However, at location two, where the participants had to make the prosocial decision, results showed that participants in the static group spent a significantly longer amount of time looking at the AOI when compared to the no-eyes group. One interpretation of these results could be that the eye stimuli were more effective than no stimuli (i.e., a blank space) at maintaining participant attention at the point of making a public goods game decision, but they were not more effective at capturing participant attention than alternative stimuli (e.g., the University logo) at the point of making a charitable donation.

***Attentiveness to Eye Cues***

Previous eye-tracking studies have found that human attention to social features, such as eyes, is reflexive and subsequently results in additional effort being employed to respond to gaze cues (Rösler, End, & Gamer, 2017) but this was not seen in the results of this study. The results from the manipulation tests of this study show that participants are noticing the eyes and paying enough attention to them to correctly identify the gender of the eye images but overall, the eye-tracking results suggest the eye images were generally not effective in maintaining participant attention throughout the whole survey.

This significant difference in attention to stimuli at location two (i.e., the public goods game) could be an indication that participants may have believed that their donation amounts would be shared in the public goods game but not when donating money to charity. Previous eye-tracking studies have shown that people who believe that they are being watched will modify their natural-looking behaviour (Nasiopoulos, Risko, Foulsham, & Kingstone, 2015) and when present, will usually orient their attention to eye images over other stimuli (Vaish et al., 2017). In the public goods game, the participants were told that they were playing with two other people, they would be told how much each participant donated to the game and how much they donated would directly impact the amount of final bonus each participant would receive. This could have all contributed to a belief of being watched which may have impacted participant attentiveness to the eye images.

In contrast, although participants were told their charitable donations would be made public to the charity, they would most likely be aware that this would not have been communicated in real-time and that there would be no immediate and obvious impact of their donations to charity. Therefore, at this point of the survey (i.e., location three), even if participants felt like they were being observed, they may have had less of a concern regarding the impact of their actions on their reputation, which may have been reflected in the amount of attention they paid to the eyes. Nevertheless, even if the participants had enough concern about their reputation to amend their gaze behaviour at location two, this was not enough to significantly impact their subsequent prosocial behaviour.

***Presentation of the Stimuli***

There could be additional factors in play that could have affected how much attention participants paid to the eye images, such as the presence of images with faces on the INAS website and the position of the stimuli on the screen. Research has shown that watching eyes are more effective when the eye cues are more salient and more realistic (e.g., Krátký et al., 2016; Manesi et al., 2016). The experimental eye stimuli used on the pseudo webpage used an image that just consisted of a pair of eyes with no other visual element (e.g., other facial features) whereas the existing images on the pseudo web page included photos of people in a naturalistic setting. Therefore, the presence of other eyes in the existing images on the web page may have detracted from the main eye stimuli.

The positions of the eye stimuli may have also impacted how much attention participants paid to the eye images. At location three, they were placed on the bottom-right of the screen. As people's gaze generally follows a left to right, top to bottom pattern (Farnsworth, 2020), the stimuli were already where people would typically look on the screen, meaning that no matter which condition the participant was allocated to, every participant would have most likely ended up looking at the bottom-right of the screen. However, in what appears to be the only watching eyes study that has considered the position of the eye stimuli on the screen, Northover et al., (2016) found that the surveillance cue's location did not impact the watching-eyes effect on moral judgements. In addition, previous eye-tracking studies have found that when exposed to social features (e.g., eye images), people prioritise their attention to these social features independent of the physical saliency of the features and above the non-social features on the screen (Rösler et al., 2017). Therefore, if the eye stimuli were more effective than the control stimuli in capturing the participants’ attention, it would be expected that the participants in the eye groups would attend to the AOI quicker than the participants in the control group. As there seems to be only one study (Northover et al., 2016) that has considered the possible importance of the position of the eye stimuli in evoking the watching-eyes, it would be an interesting avenue for future research especially in the context of trying to evoke the watching-eyes effect in an online environment.

***Habituation***

One prominent theory within the literature is that any effect of watching eyes on prosocial behaviour may decline when individuals are presented with eye stimulus for a prolonged period of time. This prolonged exposure leads to a decrease in responsiveness due to individuals become habituated to the images of eyes (Oda, 2019). Prolonged exposure to eye stimuli increases the chance that a person will recognise that stimuli to be a false cue of actual human agency and therefore would be unable to evoke a prosocial effect (Krátký et al., 2016). In addition, with the near-ubiquitous use of the internet and constant exposure to advertisements, people may have generally become habitualised to eye stimuli, having ample experience at ignoring task-irrelevant stimuli specifically designed to attract attention and change their behaviour (Sparks & Barclay, 2013).

The exploratory analysis on gaze behaviour in this study showed that there was no significant decrease in the percentage of total time that participants spent looking at the eye stimuli across the three locations in the survey. This indicates that the lack of evidence for the watching-eyes effect in this study cannot be attributed to participants becoming habituated to the eye images.

***Avoiding the eye cues***

An interesting observation was made during the data analysis on the eye-tracking data; participants seemed to be avoiding looking at the eye stimuli. The ManGold software collected data on thousands of fixation points across approximately 30 minutes which demonstrated where participants were looking at on the screen during the task. When watching the videos back, the gaze point paths were generally following a typical left to right, top to bottom pattern (Farnsworth, 2020) but when reaching the eye stimuli, the gaze points either seemed to skip over the eyes (when they were placed in the middle at the top of the screen at locations one and two) or stop short before reaching the eyes (when they were on top of the donation box at location three). As there is seemingly only one previous eye-tracking study that has specifically tested the watching-eyes effect, there are no reports in the published literature on how long participants typically spend looking at the eye stimuli but the results from this study show that the participants spent less than 5% of their time looking at the eyes which meant that for the majority of the time they spent looking at the screen, their attention was elsewhere.

In a recent eye-tracking study by Yu et al. (2017), it was found that participants fixated less on the eye region of the face when they were experiencing negative emotions. Yu et al. (2017) posit that eye contact avoidance indicates that the person looking has perceived a social threat and it has triggered negative emotions such as anxiety and fear. A unifying theory within the watching eyes literature is that eye cues capture attention, triggering a self-referential process that heightens concern in how we are socially evaluated and causes us to moderate our behaviour (Conty et al., 2016). The inconsistent results in the watching eyes literature may be due to the varying effects of prosocial behaviour on reputation. Eye cues may be more effective in reducing antisocial behaviour as antisocial behaviour may be more consistent in damaging reputation than prosocial behaviour is in enhancing it (Dear et al., 2019). The watching eyes images in this study may have induced anxiety about being judged, leading to avoidance behaviour (de Hamilton, 2016) and disrupting the regulation of behaviour (Meleady et al., 2017).

It was not possible to determine whether the participants in this study were avoiding looking at the images of eyes (though total gaze time was generally less than 5%). The data collected in this study demonstrated gaze behaviour when the participants were looking at the eye stimuli, but it could not provide any data on where participants were *not* looking. A common method for presenting visual attention is via a heat map, which can provide a visual representation of where whole groups of people are focusing their attention (Borys & Plechawska-Wójcik, 2017). Unfortunately, it was not possible to produce a heat map in this experiment to explore this observation further due to the data being collected via a video format. As heatmaps are built from an accumulation of gaze points on a static image, it is not possible to create a single heatmap of a video; each video in this study consisted of 25 frames per second across the 30-minute survey which translates to approximately 45,000 heat maps per video. In addition, the survey pages did not fit on one screen, so participants had to scroll down to view the whole page. As each participant was able to complete the survey in their own time, there was no possible way to calibrate the survey frames across participants to form a single representative heat map (ManGold, personal communication, April 2020). An interesting avenue for future research would be to try and visually capture whether participants do avoid looking at the eye stimuli and whether this avoidance behaviour is due to negative emotions triggered by the stimuli.

**Limitations**

The first issue identified was that post-hoc power analysis revealed that the analysis for this study (as with study one) was underpowered. When initially designing the experiment an *a priori* power calculation determined that the minimum sample size should be 64 to detect a large effect size (0.5) with an alpha of 0.05 and a power of 0.8. To try and ensure that the study achieved the minimum sample size needed to detect a large effect size, the study was designed with an aim to recruit 90 participants. This was the maximum number possible with the research budget of this study (this was to allow for each participant to potentially earn up to the maximum bonus of £2.50 each). However, this was an erroneous calculation, and it should have been calculated with a small-medium effect size as in study one. In addition, the sample had failed to meet the minimum size to detect even a large effect. In total, only 63 participants were recruited and, due to technical issues, 14 participants' data was lost resulting in the small sample size of 49 participants. This resulted in the main analysis (i.e., Kruskal-Wallis H tests) failing to reach the minimum statistical power to detect *any* effect, and the manipulation checks (i.e., chi-square tests) and the exploratory analysis (i.e. Mann-Whitney U and Friedman tests) only having enough power to detect a large effect.This also has an impact on the instances where the results demonstrated a marginal significant difference between groups with a moderate effect size for non-significant (differences (e.g., in the amount donated to charity). The non-significant results are possibly not due to a true null result but rather from a lack of statistical power to detect the effect resulting in an increased chance of a Type II error.

As with study two, the 90% CI for effect sizes were calculated in order to conduct equivalence tests to explore whether the non-significant results were a true null effect or whether they were inconclusive. Again, the CIs for each effect size in this analysis was outside of the boundaries for the SESOI (please general methodology chapter). Therefore, this means that no definitive conclusion can be made in this study on the effects of the presence or absence of eyes on prosocial or gaze behaviour. However, despite the lack of power in the analysis and inconclusive results, the actual values can assist with exploring the trend and patterns within the data which can help with building a bigger picture of the effects of the watching-eyes effect (Ogborn, 2014).

Another limitation was that the experimental design could have possibly benefitted from a different measure of non-monetary prosocial behaviour. As previously discussed, non-monetary forms of prosocial behaviour could provide a more reliable signal (Bradley et al., 2018) as it is likely that an *indication* of willingness to volunteer may not be a reliable because it could be too easy to fake intention. Alternative measures include social value orientation (SVO), a stable trait that reflects an intrinsic prosocial willingness (Pauwels et al., 2017). There are also other potential behavioural outcomes such as moral judgments (Northover et al, 2016) which the best studied of these include the trolley dilemma (Hauser, Cushman, Young, Kang-Xing Jin, & Mikhail, 2007), a moral thought experiment in which the reader must decide whether they would save the lives of five people tied to a main train track by flipping a switch to sacrifice the life of one person tied to the alternate track (Bleske-Rechek, Nelson, Baker, Remiker, & Brandt, 2010).

**Future Research**

Given the findings and the considerations of the limitations of this empirical study and the first empirical study of this thesis, the next empirical study will utilise alternative prosocial measures (e.g., moral judgments) as well as including a different focus on the eye stimuli characteristics. Both study one and study two explored whether increasing the salience of the eye images (e.g., by using a short 'blinking' video) would increase the watching-eyes effect. The results of both studies have not provided any conclusive evidence that increasing the salience of the watching eye images increases prosocial behaviour but with internet usage being near-ubiquitous, participants may be used to being bombarded with images of people and therefore it is possible that the blinking video was not salient enough.

One possibility for increasing the effectiveness of the eye stimuli could be by including an audio cue such as a pre-recorded message with participant instructions (Jansen, Giebels, van Rompay, & Junger, 2018). It has been suggested that a lack of sound which mimics the presence of others (particularly in an online environment) may contribute to a perception of anonymity which would cancel out any eye cue effects (Raihani & Bshary, 2012). Despite an extensive literature search for this thesis, it appears that the use of audio cues has not been explored in the context of the watching-eyes effect. Therefore, the first change to the eye stimuli focus will be to include an audio element to test whether this will increase the salience of the eyes.

Research has suggested that the gender (Rigdon et al., 2009) or perceived emotion displayed by the eye cues (Pauwels et al., 2017) may help shape our expectations and subsequent behaviour. A focus on the gender and emotion of the eye stimuli was originally planned in study one but was not deemed possible under the study's time and budget allocation. The findings from study one and two suggest that if the watching-eyes effect does exist, it is nuanced (Saunders et al., 2016) and warrants a closer examination of the eye stimuli themselves so the gender and emotion portrayed by the eye images will be the second and third (and final) changes to eye stimuli focus in the next study.

**Conclusion**

A strength of this study was that it has added to the small watching eyes literature which has examined gaze behaviour (e.g., Vaish et al., 2017) and the effects of reputational context on prosocial behaviour. Along with study one, it is also the first study to specifically test whether the salience of eyes in an online environment has an impact on the watching-eyes effect.

As with study one, this study does not provide evidence that the presence of eyes positively affects prosocial behaviour or that manipulating the salience of eyes or reputational concerns has an impact on the effects of the eye stimuli. However, this study is the first study to find that participants paid significantly more attention to the eye stimuli at the point of making a prosocial decision. Contrary to the findings by Vaish et al. (2017), however, attentiveness to the eye stimuli was not associated with greater generosity even in a reputation-relevant context. This study also did not find any evidence to support the theory that any null effects of watching eyes were due to the participants becoming habitualised to the eye images over time. There were some limitations to the study such as low statistical power, possibly problematic prosocial measures, and software restrictions (e.g., not able to produce heatmaps) which means that no definitive conclusions can be made but the study has provided some insight into nuances of the watching-eyes effect and highlighted some fruitful future research avenues.

## Chapter Five – Study Three: Exploring the Watching-eyes Effect of Gender, Emotion and Salience

This thesis so far has not provided evidence that the presence of eye images can increase prosocial behaviour in an online environment. In the first empirical study, participants were recruited via online crowdsourcing platforms to take an online survey which had three measures of prosocial behaviour. The main analysis indicated that were no significant differences between the three conditions in the amount that the participants donated to the public goods pot or charity. There was a marginal significant difference between conditions in the number of volunteer hours, specifically between the blinking and static conditions.

The second empirical study built on study one by attempting to evoke reputational concerns. Participants completed an online survey that included the same three measures of prosocial behaviour but were advised that the amount they donated to the public goods pot would be made public to other participants in the game and that their charitable donations would be made public to the charity receiving the donations. This was to address the limitation in the first empirical study whereby participants were led to believe that their prosocial decisions were completely anonymous. However, as with study one, there were no significant differences between the three groups in the amount that the participants donated to the public goods pot, or in the number of volunteer hours. There was a marginal significant difference and moderate effect size between conditions in the amount participants donated to charity.

The second empirical study also extended the first one by collecting additional data on participants' eye-tracking behaviour when completing the online survey in a lab-based setting. As the results of study one showed that being exposed to eye images did not significantly increase the participants' feelings of being watched, the second aim of study two was to determine whether the participants paid attention to the image of eyes whilst completing the prosocial tasks. Results showed that the '% Total Time' did not significantly differ between the conditions for two out of three points that the stimuli were on the screen. However, at the point at which participants were playing the public goods game, participants in the static group spent significantly more time looking at the AOI than those in the no-eyes group (there was a marginal significant difference and moderate effect size for the blinking and no-eyes, and static and blinking groups). However, this increased attentiveness to the eye images at location two did not result in an increased level of prosocial behaviour.

The time to first fixation (ms) on the AOI was used to determine if participants looked at the blinking eyes quicker than those who looked at the static eyes or no-eyes stimuli. Results showed that the 'Time to 1$^{st}$ Fixation' did not significantly differ between conditions suggesting that the blinking eye stimuli were no more salient than the static eyes stimuli and that the two types of eye stimuli were not more salient than the control.

However, due to the limitations identified in studies one and two, such as the low statistical power of both analyses, it should be noted that no definitive conclusions about the presence or absence of a watching-eyes effect can be made so far.

## This Study – Empirical Study Three

### *Eye Stimuli*

There is a growing body of evidence that artificial monitoring cues (i.e., eye images) do not influence human prosociality in a uniform way (Saunders et al., 2016) so it is intuitive that the type of eye stimuli that would be effective in capturing and maintaining attention (and thus affecting prosocial behaviour) may be nuanced. Research has shown that the watching-eyes effect could be dependent on a range of factors such as gender of the eye images (Rigdon et al., 2009), perceived valence (e.g., kind vs unkind eyes) of eyes (Pauwels et al., 2017) and salience of the eye cues (Panagopoulos, 2014a). Replication and extension are essential for good science and can help shed light on the nuances of social intervention and bolster external validity (Panagopoulos, 2014b). Therefore, in this study, it was decided to expand the range of eye stimuli in the experiments to include an exploration of the gender, the emotion expressed and the salience of the eye images to explore whether specific nuances and qualities of the eyes themselves may trigger prosocial behaviour.

**Gender.** The watching-eyes effect is commonly explained by CST, an evolutionary mechanism that may have generated the sensitivity to cues of being watched (Bekkers, 2017). CST posits that individuals partake in costly behaviour, such as donating time or money, to signal a quality about themselves which aids in attracting potential mates or allies (Griskevicius et al., 2007). This being the case, the gender of the eyes may be particularly important. For example, research has shown that males, rather than females, are more likely to display altruism as a mating display, and that females are more generous than males (e.g., Saunders et al., 2016). However, males have found to be highly responsive to the presence of watching eyes, giving twice as much in a dictator game whereas female behaviour remained unchanged in the presence of eyes (Rigdon et al., 2009) and the presence of female observers have been found to increase male generosity (Iredale, van Vugt & Dunbar, 2008). Therefore, it might be expected that gender may be an important factor in generating the watching-eyes effect. In studies one and two of this thesis, the demographic data of the participants was collected (e.g., their gender) but as no evidence was found in either study for the watching-eyes effect, this was not explored any further. The lack of evidence for the watching-eyes effect suggests that a closer expectation of the demographic data was not warranted. For this reason, in this final study, it was decided to focus on exploring the effects of gender of the eye stimuli and not consider the effects of the gender of the participant.

However, there have been few watching eyes studies that have taken into account specific social aspects of the eye cues such as gender (Vrouwe & Balliet, 2014). In a field experiment on contributions to an honesty box, Bateson et al.'s (2006) results showed that contributions to the honesty box were greater when male eyes were present. In contrast, Vrouwe and Balliet (2014) found that the watching-eyes effect on volunteering behaviour seemed to be driven by female watching eyes cues. However, in a lab-based dictator game Nettle et al., (2013) found no significant differences between the male and female eyes. This was corroborated in a study by Panagopoulos (2014), who found that the watching-eyes effect on voter turn-out could not be attributed to the gender of the eye stimuli used. Northover et al., (2016) also found no effect for the apparent gender of surveillance cues in their experiment on moral judgments. The influence of the gender of the eye stimuli remains an open question (Panagopoulos, 2014b) which this study aimed to address.

**Emotion.** Eyes play a key part in human communication as they can convey valuable information about an individuals' emotions, thoughts and intentions which is of crucial importance in shaping people's expectations and subsequent behaviour (Pauwels et al., 2017). It is currently unclear as to whether any sensitivity to eye cues is influenced by the emotion expressed in the stimuli (Saunders et al., 2016). Saunders et al. (2016) theorised that in the context of the watching-eyes effect, eyes expressing anger would be more threatening, entailing a potentially greater fitness consequence (i.e., reminding the observer that their actions may result in punishment or a decreased ability to attract a mate or allies) and thus generating a larger monitoring (i.e., watching-eyes) effect. Although Saunders et al. (2016) did not find that the emotion expressed in the eye stimuli affected donation behaviour, Pauwels et al. (2017), found that unkind rather than kind eyes boosted cooperation in a sequential prisoner's dilemma task. Pauwels et al. (2017) highlight that this finding is corroborated by physiological evidence from functional Magnetic Resonance Imaging (fMRI) research that shows that eyes that express anger are anxiety-inducing which heightens states of arousal (i.e., responsiveness to stimuli). Research has suggested that people are more sensitive to criticism than praise; people learn faster from negative experiences rather than positive and prioritise the processing of negative over positive information (Dear et al., 2019). As Dear et al. (2019) maintain, this suggests that negative emotions and emotional distress could have a greater and longer-lasting influence on humans than positive affect and pleasant emotions. Therefore, it could be predicted that 'angry' or 'unkind' eye images may be more likely to positively affect prosocial behaviour than eyes depicting positive emotions such as 'happiness'. However, except for the studies by Saunders et al. (2016) and Pauwels et al. (2017), no other study has rigorously distinguished between eyes of different emotions which again, this study aimed to address.

**Salience and Audio Cues.** The salience of the eye images (i.e., how noticeable they are) may be a key factor in capturing and maintaining people's attention and evoking feelings of being watched. It has been suggested that previous online studies of the watching-eyes effect have failed to find significant results because the online environment is a truly anonymous setting and therefore the eyes are not effective in making people feel like they are being watched (e.g., Raihani & Bshary, 2012; Saunders et al., 2016). In addition, eye images are likely to evoke the feeling of being watched much less effectively than real people do (Ernest-Jones et al., 2011).

The manipulation checks in empirical studies one and two revealed that there were significantly more participants in the blinking condition that correctly reported seeing an image of eyes, but this was not reflected in the numbers of participants who reported feelings of being watched. This indicates that increasing the salience of the eyes (e.g., by using blinking eyes) did not impact whether participants felt watched. With the near-ubiquitous use of the internet in modern day-to-day life for many people across the globe, individuals are exposed to eye images on an almost constant basis and therefore, could be accustomed to ignoring what they may consider to be task-irrelevant stimuli (Sparks & Barclay, 2013). This suggests that perhaps the eye stimuli somehow need to be more salient to capture participants' attention (Ernest-Jones et al., 2011).

There is a question to how to make eye images more salient in an online environment. Manipulating the movement of the eyes did not result in increased levels of prosocial behaviour in either empirical study one or two. The eye-tracking data in study two did not provide any evidence that the 'blinking eyes' stimulus was more effective in capturing participant attention than the 'static' or no-eyes' stimuli. As discussed in the methodology chapter (see chapter two), it is not feasible to display 3D eyes in an online environment due to technological restrictions. However, in a study on the effects of indoor surveillance cues, Jansen et al. (2018) suggest that the effectiveness of surveillance cues could be improved by the use of audio messages as audio cues may mimic the presence of others.

In the seminal watching eyes study by Haley and Fessler (2005), they included silent and non-silent conditions as well as eyes present and eyes not present conditions. For each of their experimental sessions, Haley and Fessler (2005) recruited approximately 20 participants and seated them randomly in a computer laboratory. In the silent conditions, participants wore sound-reducing earmuffs and in the non-silent condition, no earmuffs were provided. Results showed that the participants gave significantly more money in a dictator game when they were in the non-silent/eyes present condition. Tane and Takezawa (2011) replicated this study and found that the illustration of a human face on a computer screen did not increase the donation amounts in a dictator game when presented in a dark, soundproof room. It has been suggested that the lack of sound may have contributed to a perceived cloak of anonymity which may have cancelled out any surveillance cue effects (Raihani & Bshary, 2012). Although these studies explored the watching-eyes effect in a soundproof environment, they did not specifically test whether the use of sound influenced the watching-eyes effect. Despite an extensive literature search for this thesis, it appears that the use of audio cues has not been explored in the context of the watching-eyes effect. This is the final eye stimuli consideration that this study attempted to address.

### *Three Behavioural Outcomes: Donations to Charity, Total Prosocial Score and Moral Dilemmas*

Three measures of behaviour were included as dependent variables in this study, specifically the amount donated to charity, participant total prosocial score (a modified version of the public goods game) and the outcomes of moral dilemmas scenarios. These measures differ from the previous studies in order to improve on the measures of prosocial behaviour and are discussed in the order that they were presented to the participants.

**Donations to Charity.** The first measure used in this empirical study was a modified version of the 'donations to charity' used in empirical studies one and two. As discussed in the methodology chapter, the main aim of this thesis was to explore altruism (as opposed to prosocial behaviour) as a costly signal, and donations to charity could be seen as a more reliable measure of altruism than the donations to the public goods game. Prosocial behaviour occurs when an individual acts in a manner that benefits another person or group of people whereas altruism is a kind of prosocial motivation where individuals act to promote someone else's welfare, even when at risk or cost to themselves (DeLamater et al., 2018). Although participants may donate to a public goods pot for altruistic reasons, they may also be motivated by the potential to earn a bonus for themselves. There are, however, no immediate benefits to participants from donating at least some of their bonus to charity, making any donations to charity a more altruistic act. Therefore, 'donations to charity' has been used throughout this thesis so far as a measure of altruistic behaviour, as opposed to just using prosocial measures, and will be used again in this empirical study.

To improve this measure (donation to charity) from the previous studies of this thesis, a higher potential financial incentive was offered to the participants which meant they could potentially donate a higher amount to charity and demonstrate higher levels of altruistic behaviour. A potential limitation of the first two empirical studies was the small bonus amounts awarded to participants (Raihani & Bshary, 2012). In a meta-analysis of 129 economic games, the mean stake size used was $21.8 (Engel, 2011), whereas in studies one and two, the mean stake awarded to participants was $3.75[14] for the public goods game and $1.95[15] for the charitable donations. It has been suggested that economic games with low stakes typically lead to a high proportion of players making a non-zero donation meaning the additional effects of eye images on giving behaviour are difficult to detect (e.g., Nettle et al., 2013). This was seen in both study one and study two with the majority of participants donating at least something in both the public goods game (93.7%) and to a lesser extent charity (59%). In addition, undergraduate students, the most frequently sampled demographic in psychology studies (and the demographic recruited in this third empirical study), often have lower disposable income and make lower donations (Raihani & Bshary, 2012) which can also make it difficult to detect an effect.

To address these possible limitations, the incentive offered was entry into a prize draw where participants could potentially win £40 of Love2Shop vouchers. Participants were then offered an opportunity to donate any amount of their vouchers to charity in the eventuality they were selected as the prize draw winner. This had three benefits: first, as the incentive was a 'windfall', it would not leave participants out of pocket and may encourage greater donation levels (Vaish et al., 2017). Second, as the incentive was not guaranteed, this meant that

[14] This is based on the initial £2.50/$3,47 bonus for 'Call for Participants'/lab-based participants (based on the £/$ exchange rate on 23rd April 2021) and the $1 bonus for MTurk participants.
[15] This is based on the total amount won by participants from the public goods game. The total amount won by the CfP/lab-based participants was converted into US dollars and then combined with the MTurk participants to get a mean.

participants were not necessarily participating in the research for financial incentives. Third, the potential bonus amount was much higher than the previous studies in this thesis, so any amount donated to charity from this bigger potential endowment could be a truer reflection of the participants' altruistic levels (Bekkers, 2007).

As self-reported measures are sensitive to social desirability bias (Tussyadiah & Miller, 2019), the self-reported intention to donate an amount of theoretical winnings to charity may not a reliable prosocial signal as there is only a *potential* cost involved to the participant and it is easy to fake intention (Bradley et al., 2018). To provide a comparison point, participants were also asked how much they would have been willing to donate to this charity even if they did not win any vouchers. As this question represents zero risk to the participant (i.e., there is no risk of them having to donate anything), it would be expected that participants would report intentions of donating higher amounts to charity in this scenario.

**Moral Dilemmas.** The second measure used in this study was the participant's response to a series of moral dilemmas. In response to the growing literature which suggests limited evidence for the effects of watching eyes on generosity, it has been noted that more work should be done to investigate additional behavioural outcomes such as moral outcomes (Northover et al., 2016).

Moral judgments play a critical role in motivating and enforcing human cooperation (Everett et al., 2016) yet empirical investigations of how people modify their behaviour when they know or feel like they are being observed have neglected moral judgements (Bouratt et al, 2011). As Bouratt et al. (2011) maintain, expressing our opinions about the morality of certain acts is a key means of advertising our cooperative disposition. According to research, people who make deontological decisions (i.e., rule-based morality) in moral dilemmas are rated as more empathetic and as having higher moral qualities than those who make utilitarian decisions (i.e., outcome-based morality) (Jin & Peng, 2021). In a series of five studies on moral judgments, Everett et al. (2016) found that people who make deontological judgments were more likely to be chosen as social partners and are considered more moral, likeable, and trustworthy. In an experiment which explored the effect of observability and on prosocial behaviours (specifically judgements in sacrificial moral dilemmas), Anderson et al. (2020) found that revealing decisions in public did not affect altruistic behavior, but it increased cooperation and made subjects less likely to make utilitarian judgments in sacrificial dilemmas (i.e., trolley problems). In their meta-analysis, Northover et al. (2016) found that artificial surveillance cues had inconsistent effects or possibly no effects on moral outcomes. The studies in this meta-analysis all utilised vignettes which asked participants to rate the moral acceptability of two misdeeds: returning a lost wallet but keeping the money (Bourrat et al., 2011), and falsifying information on a résumé (Schnall et al., 2008). However, it appears that there has been no research into the impact of watching eyes on moral judgements outside of the research discussed in Northover et al's. (2016) meta-analysis.

In this study, the trolley problem was used as a measure of moral judgment, which is a modified version of the classic moral dilemma developed by philosophers to explore intuitions about the permissibility of harming or helping others (Hauser et al., 2007). Taken from Hauser et al. (2007), participants were presented with a series of five moral dilemmas in random order. Each moral dilemma presented a choice between action and inaction which could result in lives saved or lives lost:

- Scenario 1 – ‘Switch Track’: Participants were asked whether it was morally permissible to switch a train to a side track and kill one person in order to avoid killing five people.
- Scenario 2 – ‘Physically Push’: Participants were asked whether it was morally permissible to kill a person by personally shoving them onto the track in order to save the lives of five other people.

- Scenario 3 – 'Back Turned': Participants were asked whether it was morally permissible to switch a train onto a side track to kill one person with their back turned to save five people who were walking across the main track.
- Scenario 4 – 'Life-saving Drug': Participants were asked whether they thought it was morally permissible to give a patient a life-saving drug that there is an unlimited supply of.
- Scenario 5 – 'Clear Track': Participants were asked whether it was morally permissible to switch a train to a side track that is completely clear of people, to avoid killing five people.

The latter two scenarios were included as attention checks as the suggested action in each of the scenarios was clearly morally permissible and any participant who failed these attention checks were excluded from the analysis. Each choice was categorized as either '0' for not moral or '1' for moral which allowed for tests of association to be conducted between condition (static, blinking, and no-eyes) and whether participants agreed that the suggested actions in the moral dilemma scenarios were morally permissible. Although sacrificial dilemmas (i.e., trolley problems) have been researched extensively, studies on the influence of observability on sacrificial dilemmas are lacking (Andersson, Erlandsson, Västfjäll, & Tinghög, 2020) thus utilising a trolley problem has provided a novel approach in which to explore the potential influence of the watching-eyes effect.

**Total Prosocial Score.** The third and last measure used in this study was the participants' total prosocial score calculated from the results of a nine-item public goods game (taken from Messick & McClintock, 1968). In empirical studies one and two, the percentage of the participants' initial bonus donated to the public goods pot was used as a single-item measure of prosocial behaviour. Although research has shown that carefully crafted single-item measures (e.g., where both the object of measurement and the attribute of measurement are clear and unambiguous) can be at least as valid as multi-item measures of the same constructs, the use of single-item measures can still raise concerns around low content validity and sensitivity (Bergkvist & Rossiter, 2007). Single-item measures also require a larger sample size than multiple items as they are limited in their capability to provide enough points of discrimination (Sauro, 2018).

To address this limitation, a nine-item measure of prosocial behaviour was utilised in this study (e.g., Diamantopoulos, Sarstedt, Fuchs, Wilczynski, & Kaiser, 2012). Based on the methodology used in Iredale (2009), participants played a public goods game where they were presented with nine questions and were asked to distribute a sum of money (the participants did not play with real money) between themselves and an 'other'. Each answer the participant gave was then scored as either '0' (selfish) or '1' (prosocial) and the score for the nine-items were then combined. Each participant could score anything between 0 (selfish) and 9 (very prosocial). The total prosocial scores were then compared across the conditions (static, blinking and no-eyes). Using nine items to measure prosocial behaviour increased construct validity and reliability (Diamantopoulos et al., 2012) by providing more points of discrimination, increasing the sensitivity of the measure and allowing Cronbach's Alpha, a measure of reliability to be calculated (Sauro, 2018).

**Aim**

The main aim of this study was to expand on empirical studies one and two, which tested whether the presence of eye cues positively affected prosocial behaviour, by further exploring the range of eye stimuli in three areas: Gender, emotion, and salience.

**Hypotheses:**

***Hypothesis 1 – Gender:***

There will be a significant difference between gender conditions of the eye stimuli (male, female, and no-eyes) in the three measures of prosocial behaviour.

***Hypothesis 2 - Emotion:***

There will be a significant difference between emotion conditions (angry, happy and no-eyes) in the three measures of prosocial behaviour.

***Hypothesis 3 - Salience:***

There will be a significant difference between salience conditions (static, blinking, audio and no-eyes) in the three measures of prosocial behaviour.

## Methodology

### Participants

Participants were recruited via convenience sampling, utilising Canterbury Christ Church University's Research Participant Scheme (RPS), where participants could receive two course credits for completing the survey. Due to the nature of the prosocial variables being measured (i.e., donations to charity), there was an additional incentive of a chance to win a £40 love2shop voucher. This meant that the survey could also be advertised outside of the RPS, and further participants were recruited from the crowdsourcing website 'Call for Participants' (CfP) and from social media (e.g., Facebook and Twitter). A break-down of the number of participants recruited via RPS, CfP and social media was not possible as the same survey link was used for all three participant sources and there was no way to determine numbers from the anonymised data collected.

**Materials: Eye Stimuli**

Each eye stimulus (see Figure 5.1) was displayed to the participant via a banner at the top of the survey webpage (please follow the link[16] to listen to the audio cue condition). As the results of study two indicated that participants paid more attention to the eyes stimuli at the point of making a decision in the public goods game, it was decided in this study to only present the stimuli to the participant at the point where they had to make prosocial decisions (the charity donation amount questions, the moral dilemma scenarios, and the public goods game)[17].

Overall, one control image (i.e., a university logo) and six images of eyes were used, of which three were male and three were female. Within each gender, there was an image of angry eyes and an image of happy eyes.  The effects of the gender, emotion, and salience of the eyes cues on prosocial behaviour were explored by conducting planned comparisons between the groups of each condition: gender (male, female, and no-eyes), emotion (angry, happy, and no-eyes), and salience (static, blinking, audio, and no-eyes).

The eye stimuli used in the salience condition (static, blinking and audio) were taken from the first empirical study where a short clip of a pair of 'happy female' blinking eyes was purchased from Shutterstock (copyright: Federico Marsicano) to use for the 'blinking 'condition. A still image of the eyes was taken from this video to use in the 'static' and 'audio' conditions. The video/image was then incorporated into a banner that displayed the University's logo and a speech bubble reminding the participants to complete all of the questions in the survey. In the audio condition, a voice recording was made which asked the

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[16] https://cccusocialsciences.az1.qualtrics.com/jfe/form/SV_0k1TFybGXAMAzT8

[17] The length of time that the stimuli was presented to the participants for was not recorded for this study, but it is estimated that participants were exposed for less than one minute. This estimation is based on data from study two which indicated that the eyes were present at location three (where participants chose how much to donate to charity) for an average of 54.75 seconds.

participants to complete all of the questions in the survey which was played to the participants each time they were on a new survey webpage that had the eye stimuli banner.

**Design and Procedure**

Participants were randomly assigned to one of seven conditions (angry male, happy male, angry female, happy female, static, blinking, audio, and no-eyes) in a one-way ANOVA, between-subjects design. Participants were required to complete an online survey in which levels of prosocial behaviour were measured. Prosocial behaviour was determined by three measures: 1) the amount donated to charity (participants could donate any amount in whole pounds up to the maximum voucher value of £40), 2) the outcomes of three moral dilemmas (participants who failed the two attention check moral dilemmas were excluded from the whole analysis), and 3) the total prosocial score (taken from a 9-item public goods questionnaire).

Participants completed the survey via an online Qualtrics link. After reading an information sheet and consenting to the study, participants were informed they were being entered into a prize draw where they could potentially win £40 of Love2Shop vouchers. They were then advised that in light of the devastating bushfires in Australia, the School of Psychology at Canterbury Christ Church University (CCCU) were raising funds to help with the ongoing support of bushfire affected communities and were asked if they were selected as the winner of the prize draw, whether they would like to donate some of their winnings and if so, how much. If they were selected as the winner, the amount they indicated would be donated directly to the charity and the remainder would be rounded to the nearest whole pound and given to the participant in vouchers. The winner was chosen at random and contacted via email. The participants were then asked, “for information purposes only”, how much they would have been willing to donate to the charity even if they did not win any vouchers.

It should be noted that at the start of participant recruitment (February 2020), there had been catastrophic bushfires in Australia which had burned more than 11 million hectares of bush, forest and parks across Australia (BBC, 2020). These events were prior to World Health Organisation (WHO) declaring COVID-19 as a pandemic on 11th March 2020 (WHO, 2020). As at this stage COVID-19 was relatively unknown in the United Kingdom, the Australian bushfires had dominated the media which is why it was selected as the recipient charity.

The participants were then presented with five moral dilemmas (taken from Hauser et al., 2007) with each moral dilemma presenting a choice between action and inaction which could result in lives saved or lives lost:

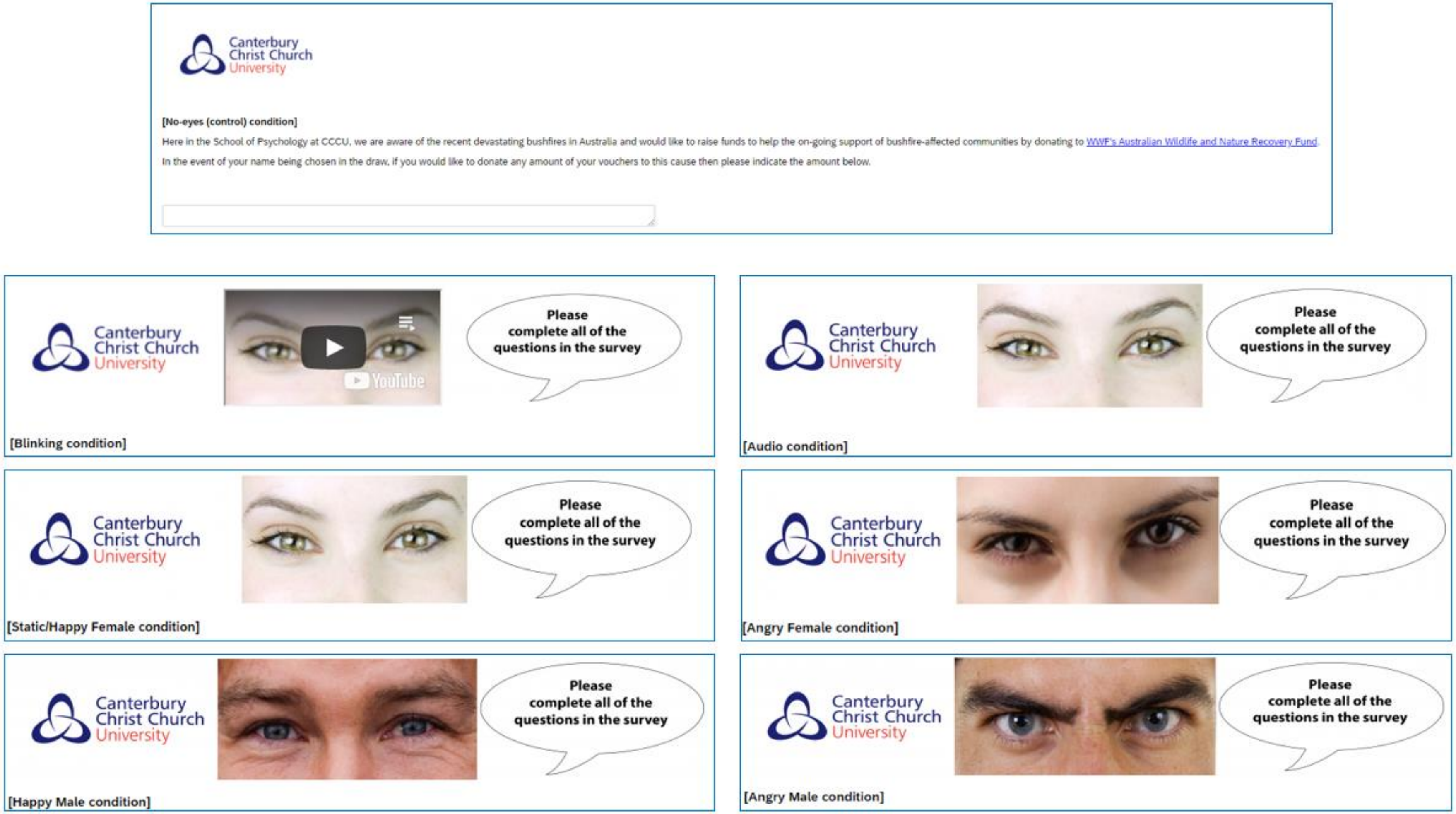
- Scenario 1 – 'Switch Track': Participants were asked whether it was morally permissible to switch a train to a side track and kill one person in order to avoid killing five people.
- Scenario 2 – 'Physically Push': Participants were asked whether it was morally permissible to kill a person by personally shoving them onto the track in order to save the lives of five other people.
- Scenario 3 – 'Back Turned': Participants were asked whether it was morally permissible to switch a train onto a side track to kill one person with their back turned to save five people who were walking across the main track.
- Scenario 4 – 'Life-saving Drug': Participants were asked whether they thought it was morally permissible to give a patient a life-saving drug that there is an unlimited supply of.
- Scenario 5 – 'Clear Track': Participants were asked whether it was morally permissible to switch a train to a side track that is completely clear of people, to avoid killing five people.

To avoid order bias, each participant was presented with the five scenarios in random order and the five dilemmas were presented on separate pages. The participants were asked to read through each scenario and answer the corresponding question about whether a particular action was morally permissible (no/yes). Two scenarios were included as attention checks as the suggested action in each of the scenarios was clearly morally permissible (i.e., the action saved the lives of the people in the scenario with no negative consequences) and any participant who failed these attention checks were excluded from the analysis. Therefore, only the outcomes of three moral dilemma scenarios will be reported. Each choice was categorized as either '0' for not moral or '1' for moral which allowed for tests of association to be conducted between condition (static, blinking, and no-eyes) and whether participants agreed that the suggested actions in the moral dilemma scenarios were morally permissible. In the next task, participants were introduced to the public goods game questionnaire (based on the methodology in Messick & McClintock, 1968). They were asked to imagine they had been randomly paired with another person, referred to as the 'other' and were told that would be making decisions on how to allocate points between themselves and the other person. The participants were then presented with nine items, each with three options for how to distribute the points between themselves and the 'other'. For each item, they had to choose the option that they most preferred. In the analysis, each question answer was either scored as '0' (selfish) or '1' (prosocial). These scores were then added up across the nine items to calculate a total prosocial score for the participant. Reliability analysis was conducted on the nine items and the scale had a high level of internal consistency, as determined by a Cronbach's alpha (α) of 0.963.

On the next screen, the participants were no longer exposed to the eye stimuli and the participants were asked a series of seven questions of attention checks such as: whether they got the sense they were being watched, whether they remembered seeing an image of eyes, and if so, what emotion they thought the eyes were portraying. Lastly, the participants then

**Figure 5.1**

*Screenshots of the eye stimuli used in the experimental tasks*



answered seven demographic questions about themselves. These questions were asked after the participants completed the task, so they did not interfere with the effects of the stimuli (e.g., Saunders et al., 2016). The survey took approximately 30 minutes to complete in total.

**Ethics**

In line with the British Psychology Society's ethical guidelines (The British Psychological Society, 2018), consent was taken from each participant. It was made clear that participation was voluntary, and all data was kept strictly confidential with no identifying information of the participant stored with the data. This study was granted full ethical compliance by the Ethics Chair at Canterbury Christ Church University (Ref: ETH1920-0057).

**Data Analysis**

A series of statistical tests were planned between the gender (male, female and no-eyes), the emotion (angry, happy and no-eyes) and salience (static, blinking, audio and no-eyes) conditions across the three measures of prosocial behaviour; donations to charity, the moral dilemmas, and participants' total prosocial score. Participants who, in either of the two control moral dilemma scenarios, judged that it was permissible to choose a course of action that resulted in death even though there was a costless alternative, were excluded from the analysis.

A series of one-way ANOVAs and independent T-tests were planned to explore whether there were significant differences across the gender, emotion, and salience conditions in the three measures of prosocial behaviour. The first measures of prosocial behaviour were the amounts the participant indicated that they would donate to charity; both if they were selected as the winner of the prize draw and if they were not. The next measure of prosocial behaviour was the participants' total prosocial score from the nine-item public goods game. However, the distributions of these dependent variables did not approximate a normal distribution as assessed by Shapiro-Wilk's test ($p < .05$). Transformations failed to approximate a normal distribution so non-parametric, Kruskal-Wallis H tests and Mann-Whitney U tests were conducted instead. Outliers were kept in the sample as both the Kruskal-Wallis and Mann-Whitney U tests are ranked tests, which are not sensitive to outliers (Field & Hole, 2003).

The effect sizes for any non-significant Kruskal-Wallis results were calculated using Epsilon square ($\varepsilon^2$) (Tomczak & Tomczak, 2014). The effect sizes for any significant Kruskal-Wallis H test results were calculated by follow-up post hoc analysis using Mann-Whitney tests for focused pairwise comparisons and all Mann-Whitney test effect sizes were calculated using Pearson's *r* (Field & Hole, 2003). Chi-square tests of homogeneity with Cramer's *V* calculations were planned to explore whether there were significant differences across the gender, emotion, and salience conditions in whether participants agreed that the suggested actions in the moral dilemma scenarios were morally permissible.

*A priori* power analyses were conducted using G*Power3 (Faul et al., 2007) with an alpha of 0.05 and power of 0.80. Results showed that the desired sample size was $N = 225$ for medium effect size (0.25) for a two-way ANOVA with seven groups, and $N = 152$ for a medium effect size (0.3) for the chi-square tests. However, due to the non-parametric tests, a post hoc power analysis for a one-way ANOVA with 15% of the sample size removed was conducted for the was conducted for the Kruskal-Wallis H tests (e.g., Develve, 2020). The results revealed overall, the Kruskal-Wallis H tests achieved a power of 0.97 for a large effect size (0.4), 0.59 for a medium effect size (0.25) and 0.12 for a small effect size (0.1). The chi-square tests achieved a power of 0.99 for a large effect size (0.5), 0.87 for a medium effect size (0.3) and 0.13 for a small effect size (0.1). The Wilcoxon tests achieved a power of 0.99 for a large effect size (0.8), 0.96 for a medium effect size (0.5) and 0.32 for a small effect size (0.2) (see Table 5.1).

**Table 5.1**

*A summary of the power achieved for each sample*

| | Kruskal-Wallis H Tests* | | | chi-square tests | | | Wilcoxon** | | |
|---|---|---|---|---|---|---|---|---|---|
| Effect Size | Small (0.1) | Medium (0.25) | Large (0.4) | Small (0.1) | Medium (0.3) | Large (0.5) | Small (0.2) | Medium (0.5) | Large (0.8) |
| Power | 0.12 | 0.59 | 0.97 | 0.13 | 0.87 | 0.99 | 0.32 | 0.96 | 0.99 |

*Note:* The post-hoc power analysis were conducted using G*Power 3.1.9.4, an alpha of 0.05 and the achieved sample size for each test.

*Power analysis on Kruskal-Wallis tests were calculated on *n* = 150, which is 15% of the sample removed from the original sample size (*n* = 176)

** The Wilcoxon tests formed part of exploratory analysis which were not pre-planned. These tests are detailed in the results section.

## Results

Originally, 210 participants were recruited, however, 34 participants were removed for failing the moral dilemma attention checks where the suggested action in each of the attention checks was clearly morally permissible (i.e., the action saved the lives of the people in the scenario with no negative consequences). In total, 176 participants were included in the analysis and were randomly allocated to one of seven conditions: angry male ($n$ = 23), happy male ($n$ = 25), angry female ($n$ = 20), happy female/static ($n$ = 21), blinking ($n$ = 21), audio ($n$ = 22), and no-eyes ($n$ = 20) conditions. Participant ages ranged from 18 to 60 years old ($M$ = 31.18, $SD$ = 11.87).

### Hypothesis 1 – Gender:

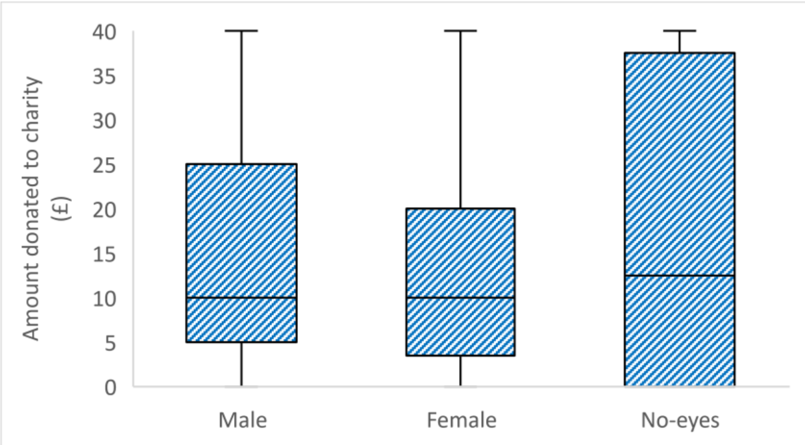
There will be a significant difference between gender conditions of the eye stimuli (male, female, and no-eyes) in the measures of prosocial behaviour.

#### *Donations to Charity*

A Kruskal-Wallis H test was conducted to determine if there were any significant differences in the amount (£0-£40) the participants indicated that they would donate to charity, if they were selected as the winner of the prize draw, between gender conditions

**Figure 5.2**

*Median donations to charity by gender if selected as the prize draw winner*



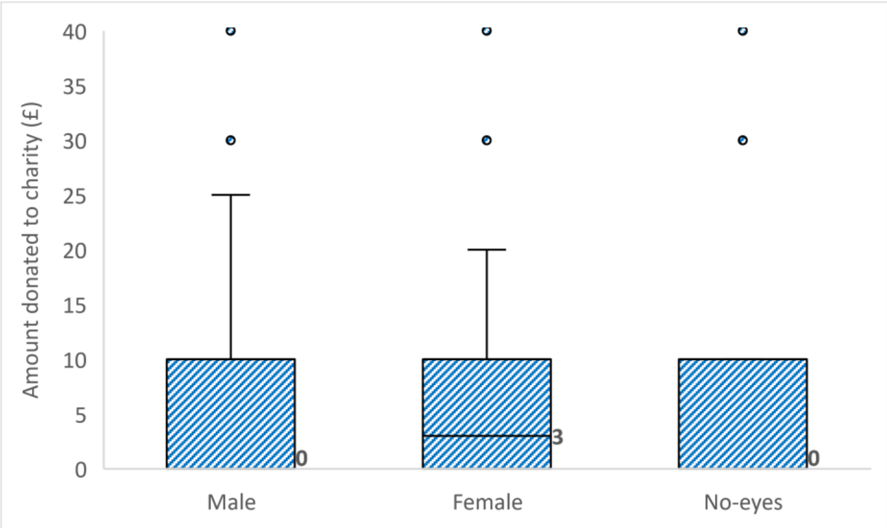

*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, and the bold horizontal line indicates the median.

(male, female, and no-eyes). There were no significant differences in donation amounts between the male (*Mdn* = 10, IQR = 5.00 – 32.50*),* female (*Mdn* = 10, IQR = 2.5 – 20.00*),* and no-eyes (*Mdn* = 12.5, IQR = 0.00 – 40.00*)* groups, $H(2) = 1.267$, $p = .531$, $\varepsilon^2 = 0.01$, 90% CI [0.01, 1.00] (see Figure 5.2).

A further Kruskal-Wallis H test was conducted to determine if there were any significant differences in the amount the participant indicated that they would donate to charity, even if they were not selected as the winner of the prize draw (£0-£40, between gender conditions (male, female, and no-eyes). There were no significant differences in donation amounts between the male (*Mdn* = 0, IQR = 0.00 – 10.00), female (*Mdn* = 3, IQR = 0.00 – 10.00) or no-eyes (*Mdn* = 0, IQR = 0.00 – 10.00) groups, $H(2) = .825$, $p = .662$, $\varepsilon^2 = 0.006$, 90% CI [0.00, 1.00]  (see Figure 5.3).

**Figure 5.3**

*Median donations to charity by gender if **not** selected as the prize draw winner*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show outliers. Due to floor effects, data labels are also used to demonstrate median.

### *Moral Dilemmas*

A series of chi-square tests were conducted between the gender conditions (male, female, and no-eyes) for the three moral dilemma scenarios. In the first scenario ('Switch Track'), participants were asked whether it was morally permissible to switch a train to a sidetrack and kill one person in order to avoid killing five. There were no significant differences in the percentages of participants who thought it was morally permissible between the male (70.2%), female (68.8%) or no-eyes (80.8%) groups, $\chi2(2, N = 176) = 1.440$, $p = .487$, $V = .090$, 90% CI [0.03, 0.23]

In the second scenario ('Physically Push'), participants were asked whether it was morally permissible to kill a person by personally pushing them onto the track in order to save five other people. There was a significantly lower proportion of participants in the the female (20.4%) and male (8.8%) groups who thought it was morally permissible compared to those in the no-eyes group (38.5%), $\chi2(2, N = 176) = 10.254$ $p = .006$. Post hoc analysis involved pairwise comparisons with a Bonferroni correction (0.05/3). Statistical significance was accepted at $p < .016$ (0.05/3). Tests revealed there were significantly more participants in the no-eyes group who thought that the second scenario was morally permissible compared to those in the male group, $\chi2(2, N = 83) = 10.630$, $p = .001$, $V = .358$, 90% CI [0.17, 0.54], and marginal significant differences between the no-eyes and female groups, $\chi2$ (2, $N = 119$) = 3.584, $p = .058$, $V = .174$, 90% CI [0.03, 0.35] or the male and female groups, $\chi2$ (2, $N = 150$) = 3.574, $p = .059$, $V = .154$, 90% CI [0.04, 0.27].

In the final scenario ('Back Turned'), participants were asked whether they thought it was morally permissible to switch a train onto a sidetrack to kill one person with their back turned to save five people who were walking across the main track. There were no significant differences in the percentages of participants in the male (50.9%), female (58.1%) and no-eyes (64%) groups, $\chi2(2, N = 175) = 1.398$, $p = .497$, $V = .089$, 90% CI [0.03, 0.23] (see Figure 5.4).

**Figure 5.4**

*Participants who agreed each scenario was morally permissible within the gender condition*

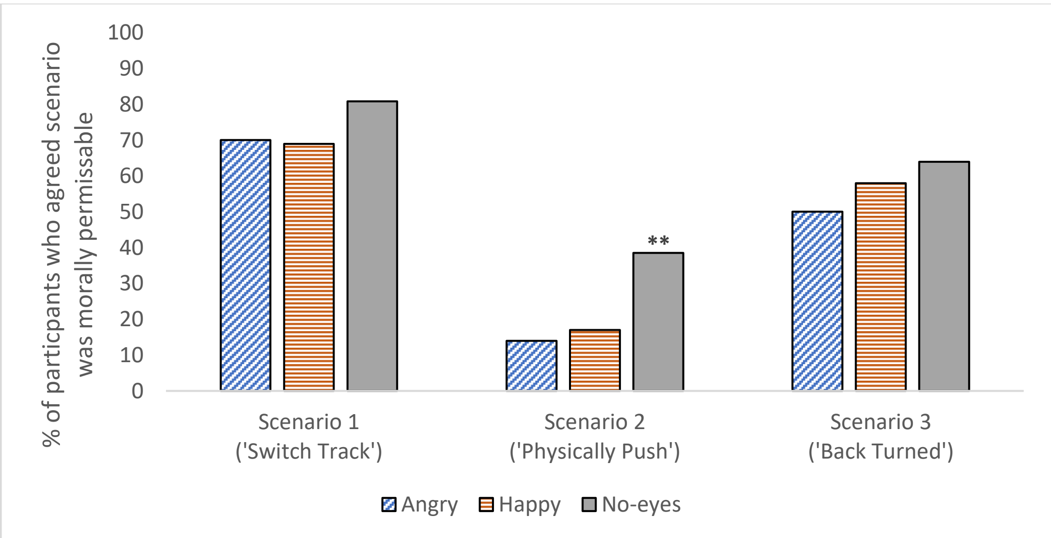
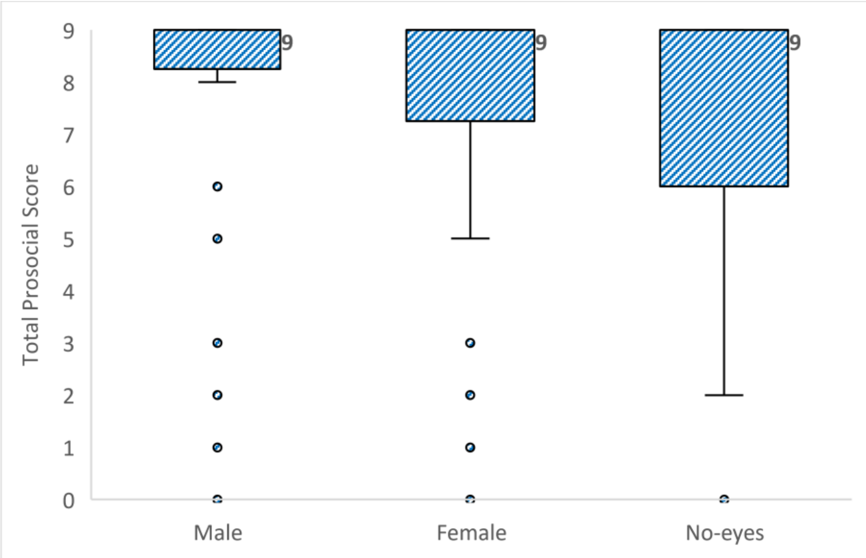


**Figure 5.5**

*Median Total Prosocial Scores across the gender condition*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show outliers. Due to ceiling effects, data labels are also used to demonstrate median.

*** p* < 0.016 adjusted for Bonferroni correction (0.05/3)

***Prosocial score***

A Kruskal-Wallis H test was conducted to determine if there were any significant differences in the participants' total prosocial score (0-9) between gender conditions (male, female, and no-eyes). There were no significant differences in donation amounts across the male (*Mdn* = 9, IQR = 8.25 – 9.00), female (*Mdn* = 9, IQR = 7.25 – 9.00) and no-eyes (*Mdn* = 9, IQR = 6.00 – 9.00) groups, $H(2) = .120$, $p = .942$, $\varepsilon^2 = 0.0007$, 90% CI [0.00, 1.00] (see Figure 5.5).

**Hypothesis 2 - Emotion:**

There will be a significant difference between emotion conditions (angry, happy and no-eyes) in the measures of prosocial behaviour.

***Donations to Charity***

A Kruskal-Wallis H test was conducted to determine if there were any significant differences in the amount (£) the participants indicated that they would donate to charity if they were selected as the winner of the prize draw between the emotion conditions (angry, happy, and no-eyes). There were no significant differences in donation amounts between the angry (*Mdn* = 10, IQR = 10,00 – 30.00*),* happy (*Mdn* = 10, IQR = 0.00 – 21.25*),* and no-eyes (*Mdn* = 12.5, IQR = 0.00 – 40.00*)* groups, $H(2) = 1.816$, $p = .403$, $\varepsilon^2 = 0.02$, 90% CI [0.01, 1.00] (see Figure 5.6).

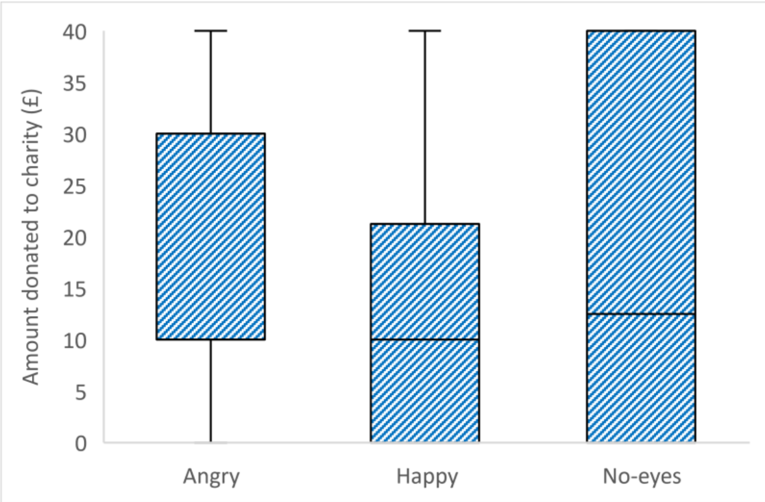
A further Kruskal-Wallis H test was conducted to determine if there were any significant differences in the amount the participant indicated that they would donate to charity, even if they were not selected as the winner of the prize draw, between emotion conditions (angry, happy, and no-eyes). There were no significant differences in donation amounts between the angry (*Mdn* = 5, IQR = 0.00 – 15.00), happy (*Mdn* = 0, IQR = 0.00 – 10.00) or no-eyes (*Mdn* = 0, IQR = 0.00 – 10.00) groups, $H(2) = 2.146$, $p = .342$, $\varepsilon^2 = 0.02$, 90% CI [0.00, 1.00] (see Figure 5.7).

***Moral Dilemmas***

A series of chi-square tests were conducted between the emotion conditions (angry, happy, and no-eyes) for the three moral dilemma scenarios. In the first scenario (‘Switch Track’), participants were asked whether it was morally permissible to switch a train to a sidetrack and kill one person in order to avoid killing five. There were no significant differences in the percentages of participants who thought this was morally permissible between the angry (70%), the happy (69%) or no-eyes (80.8%) conditions, $\chi2(2, N = 176) = 1.424$, $p = .491$, $V = .09$, 90% CI [0.04, 0.22].

**Figure 5.6**

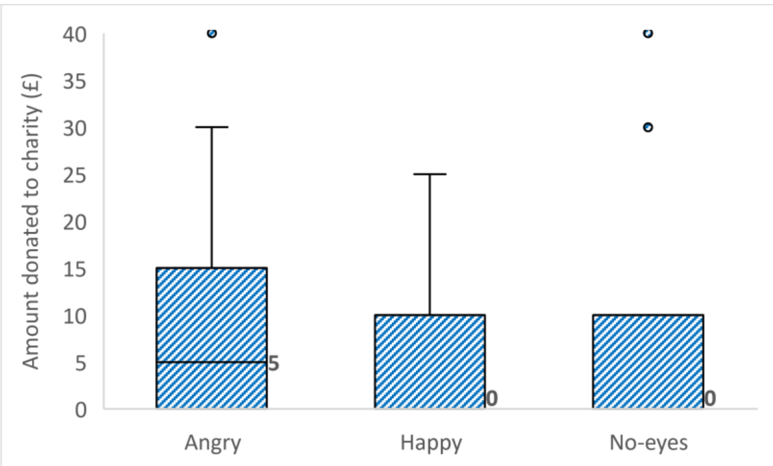
*Median donations to charity by emotion if selected as the prize draw winner*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, and the bold horizontal line indicates the median.

**Figure 5.7**

*Median donations to charity by emotion, if not selected as the prize draw winner*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show outliers. Due to floor effects, data labels are also used to demonstrate median.

In the second scenario (‘Physically Push’), participants were asked whether it was morally permissible to kill a person by personally pushing them onto the track in order to save five other people. There were a significantly lower proportion of participants in the angry (14%) and happy (17%) groups who thought it was morally permissible compared to those in in the no-eyes (38.5%) group, $\chi 2$ (2, $N$ = 176) = 7.365, $p$ = .025. Post hoc analysis involved pairwise comparisons with a Bonferroni correction. Statistical significance was accepted at $p <$ .016 (0.05/3). Tests revealed that there were significantly more participants in the no-eyes group who thought that the second scenario was morally permissible compared to those in the angry group, $\chi 2$(2, $N$ = 76) = 5.894, $p$ = .015, $V$ = .278, 90% CI [0.08, 0.49], marginal significant differences between the no-eyes and happy groups, $\chi 2$ (2, $N$ = 126) = 5.645, $p$ = .018, $V$ = .212, 90% CI [0.05, 0.37] and no significant differences between the angry and happy groups, $\chi 2$(2, $N$ = 150) = .223, $p$ = .637, $V$ = .039, 90% CI [0.01, 0.17].

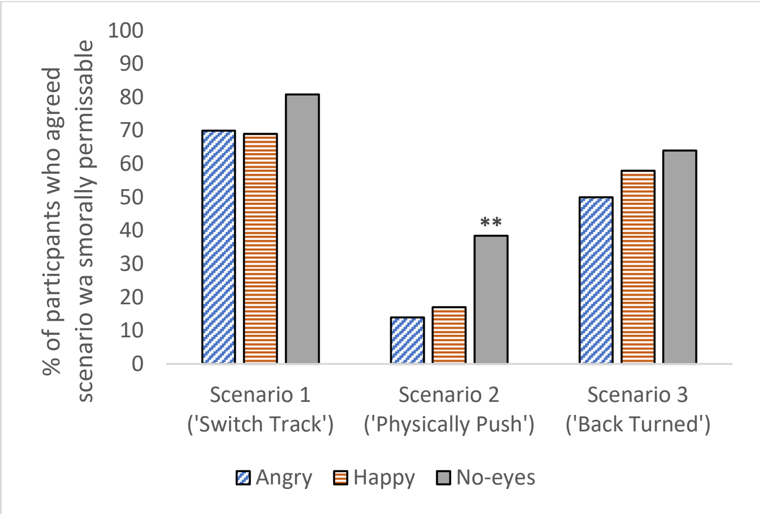
In the final scenario (‘Back Turned’), participants were asked whether they thought it was morally permissible to switch a train onto a sidetrack to kill one person with their back turned to save five people who were walking across the main track. There were no significant differences in the percentages of participants in the angry (50%), happy (58%) and no-eyes (64%) groups, $\chi 2$(2, $N$ = 175) = 1.523, $p$ = .467, $V$ = .093, 90% CI [0.03, 0.24] (see Figure 5.8).

***Prosocial score***

A Kruskal-Wallis H test was conducted to determine if there were any significant differences in the participants’ total prosocial score between emotion conditions (angry, happy, and no-eyes). There were no significant differences in donation amounts across the angry (*Mdn* = 9, IQR = 6.00 – 9.00), happy (*Mdn* = 9, IQR = 8.00 – 9.00) and no-eyes (*Mdn* = 9, IQR = 6.00 – 9.00) groups, $H(2)$ = .094, $p$ = .954, $\varepsilon^2$ = 0.001, 90% CI [0.00, 1.00] (see Figure 5.9).

**Figure 5.8**

*Participants who agreed each scenario was morally permissible within the emotion condition*




*Note.* ** $p < .016$ adjusted for Bonferroni correction (0.05/3)

**Figure 5.9**

*Median Total Prosocial Scores across emotion condition*


*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show outliers. Due to ceiling effects, data labels are also used to demonstrate median.

**Hypothesis 3 - Salience:**

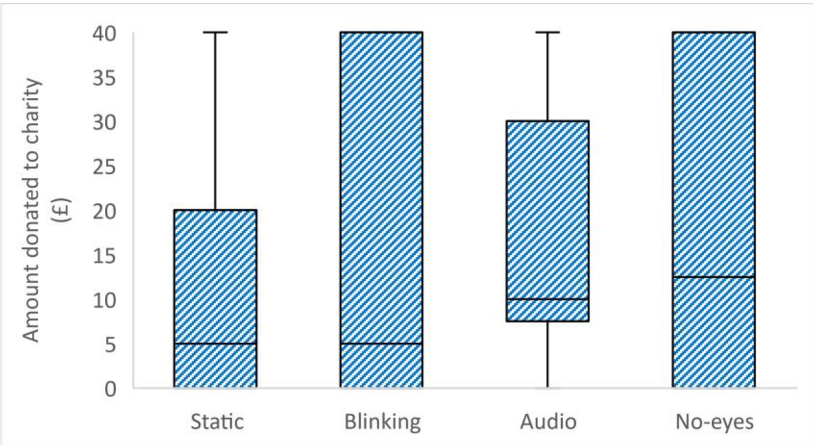
There will be a significant difference between salience conditions (static, blinking, audio, and no-eyes) in the three measures of prosocial behaviour.

***Donations to Charity***

A Kruskal-Wallis H test was conducted to determine if there were any significant differences in the amount (£) the participants indicated that they would donate to charity, if they were selected as the winner of the prize draw, between salience conditions (static, blinking, audio, and no-eyes). There were no significant differences in donation amounts between the static (*Mdn* = 5, IQR = 0.00 – 20.00*),* blinking (*Mdn* = 5, IQR = 0.00 – 40.00*),* audio (*Mdn* = 10, IQR = 7.50 – 30.00*)* and no-eyes (*Mdn* = 12.5, IQR = 0.00 – 40.00*)* groups, $H(2) = 3.290$, $p = .349$, $\varepsilon^2 = 0.06$, 90% CI [0.03, 1.00] (see Figure 5.10).

**Figure 5.10**

*Median donations to charity by salience condition, if selected as the prize draw winner*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, and the bold horizontal line indicates the median.

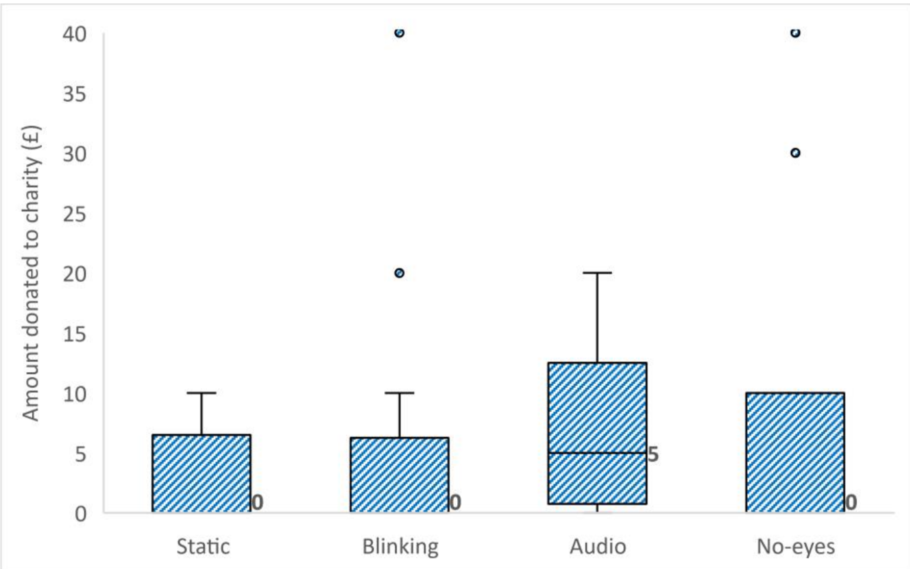
A further Kruskal-Wallis H test was conducted to determine if there were any significant differences in the amount (£0-£40) the participant indicated that they would donate to charity even if they were not selected as the winner of the prize draw between emotion conditions (static, blinking, audio, and no-eyes). There were no significant differences in donation amounts between the static (*Mdn* = 0, IQR = 0.00 – 6.50), blinking (*Mdn* = 0, IQR = 0.00 – 6.25), audio (*Mdn* = 5, IQR = 0.75 – 12.50) and no-eyes (*Mdn* = 0, IQR = 0.00 – 10.00) groups, $H(2) = 5.748$, $p = .125$, $\varepsilon^2 = 0.8$, 90% CI [0.05, 1.00] (see Figure 5.11).

***Moral Dilemmas***

A series of chi-square tests were conducted between the emotion conditions (static, blinking, audio, and no-eyes) for the three moral dilemma scenarios. In the first scenario

**Figure 5.11**

*Median donations to charity by salience condition, if not selected as the prize draw winner*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show outliers. Due to floor effects, data labels are also used to demonstrate median.

('Switch Track'), participants were asked whether it was morally permissible to switch a train to a sidetrack and kill one person in order to avoid killing five (no/yes). There were no significant differences in the percentages of participants who thought this was morally permissible between the static (63.6%), blinking (77.3%), audio (26.3%) and no-eyes (80.8%) groups, $\chi 2(2, N = 95) = 3.629$, $p = .304$, $V = .195$, 90% CI [0.10, 0.39].

In the second scenario ('Physically Push'), participants were asked whether it was morally permissible to kill a person by personally pushing them onto the track in order to save five other people (no/yes). There were no significant differences in the percentages of participants who thought it was morally permissible between the static (27.3%), blinking (22.7%), audio (12%) and no-eyes (38.5%) groups, $\chi 2(2, N = 95) = 4.850$, $p = .183$, $V = .226$, 90% CI [0.14, 0.41].

In the final scenario ('Back Turned'), participants were asked whether they thought it was morally permissible to switch a train onto a sidetrack to kill one person with their back turned to save five people who were walking across the main track (no/yes). There were no significant differences in the percentages of participants who thought it was morally permissible between the static (63.6%), blinking (59.1%), audio (56%) and no-eyes (64%) groups, $\chi 2(2, N = 94) = .449$, $p = .950$, $V = .069$, 90% CI [ 0.07, 0.31] (see Figure 5.12).

***Prosocial score***

A Kruskal-Wallis H test was conducted to determine if there were any significant differences in the participants' total prosocial score (0-9) between the salience conditions (static, $n = 21$; blinking, $n = 21$; audio, $n = 22$; and no-eyes, $n = 21$). There were no significant differences in donation amounts across the static ($Mdn = 9$, IQR = 6.50 – 9.00), blinking ($Mdn = 9$, IQR = 7.00 – 9.00), audio ($Mdn = 9$, IQR = 9.00 – 9.00) and no-eyes groups ($Mdn = 9$, IQR = 6.00 – 9.00), $H(3) = 2.666$, $p = .446$, $\varepsilon^2 = 0.03$, 90% CI [0.02, 1.00] (see Figure 5.13).

**Figure 5.12**

*Participants who agreed each scenario was morally permissible within the salience condition*

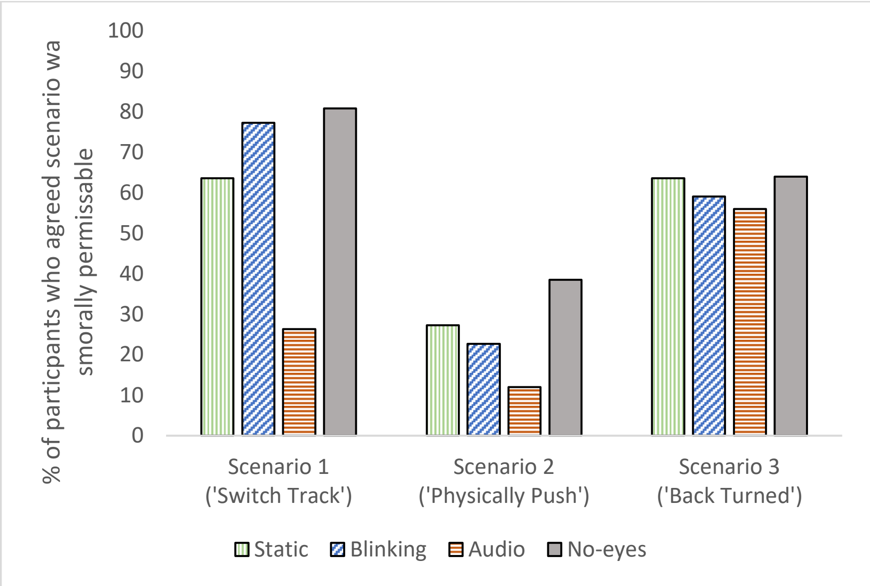
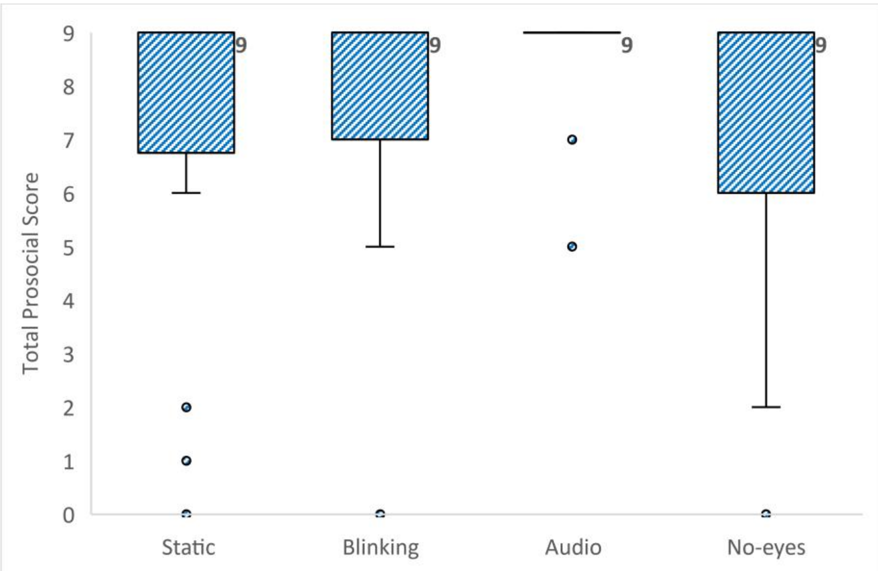



**Figure 5.13**

*Median Total Prosocial Scores across the salience condition*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show outliers.

Due to ceiling effects, data labels are also used to demonstrate median.

**Manipulation Checks**

Manipulation checks were included to ask the participants whether they saw the eyes and whether they felt like they were being watched during the survey. To check whether the participants who had been exposed to an image of eyes correctly reported seeing an image of eyes, a series of chi-square tests were conducted between the gender (male, female, and no-eyes), the emotion (angry, happy, and no-eyes), and the salience (static, blinking, audio, and no-eyes) conditions and whether participants reported seeing an image of eyes (no/yes).

Within the gender condition, there were significantly more participants in the female group (71.4%) who reported seeing an image of eyes when compared to the male (57.4%) or no-eyes (30%) groups, $\chi^2(2, N = 151) = 12.215$, $p = .002$. Post hoc analysis involved pairwise comparisons with a Bonferroni correction. Statistical significance was accepted at $p < .016$ (0.05/3). Tests revealed there were significantly more participants in the female group who reported seeing eyes compared to those in the no-eyes group, $\chi2(2, N = 104) = 11.957$, $p = .001$, $V = .339$, 90% CI [0.17, 0.49], there were marginal significant differences between the male and no-eyes groups, $\chi2(2, N = 67) = 4.229$, $p = .040$, $V = .251$, 90% CI [ 0.05, 0.44], and no significant differences between the female and male groups, $\chi2(2, N = 131) = 2.641$, $p = .104$, $V = .142$, 90% CI [0.02, 0.28].

Within the emotion condition, there were significantly fewer participants in the no-eyes (30%) group who reported seeing an image of eyes when compared to those in the angry (69%) and happy (65.2%) groups, $\chi^2(2, N = 151) = 9.906$, $p = .007$. Post hoc analysis involved pairwise comparisons with a Bonferroni correction. Statistical significance was accepted at $p < .016$ (0.05/3). Tests revealed there were significantly more participants in the angry group, $\chi2(2, N = 62) = 8.403$ $p = .004$, $V = .368$, 90% CI [0.18, 0.57], and in the happy group, $\chi2(2, N = 109) = 8.332$ $p = .004$, $V = .276$, 90% CI [0.11, 0.43]who reported seeing eyes compared to those in the no-eyes group but there were no significant differences between the angry and happy groups, $\chi2(2, N = 131) = .192$, $p = .661$, $V = .038$, 90% CI [0.01, 0.18].

Within the salience condition, there were significantly less participants in the no-eyes (30%) group who reported seeing an image of eyes when compared to those in the static (71.4%), blinking (71.4%) and audio (68.2%) groups, $\chi^2(3, N = 84) = 10.446$, $p = .015$. Post hoc analysis involved pairwise comparisons with a Bonferroni correction. Statistical significance was accepted at $p < .016$ (0.05/3). Tests revealed there were significantly more participants in the audio group, $\chi2(2, N = 42) = 6.109$ $p = .013$, $V = .381$, 90% CI [0.21, 0.59], in the blinking group, $\chi2(2, N = 41) = 7.037$, $p = .008$, $V = .392$, 90% CI [0.23, 0.59], and in the static group, $\chi2(2, N = 41) = 7.037$, $p = .008$, $V = .414$, 90% CI [0.17, 0.66], who reported seeing eyes compared to those in the no-eyes group. However, there were no significant differences between the audio and blinking groups, $\chi2(2, N = 43) = .054$, $p = .817$, $V = .035$, 90% CI [0.04, 0.31], the audio and static groups, $\chi2(2, N = 43) = .054$, $p = .817$, $V = .035$, 90% CI [0.01, 0.30], or the blinking and static groups, $\chi2(2, N = 42) = .000$, $p = 1.000$, $V = .000$, 90% CI [0.01, 0.32].

A series of chi-square tests were also conducted to examine the relationships between the gender (male, female, and no-eyes), the emotion (angry, happy, and no-eyes), and the salience (static, blinking, audio, and no-eyes) conditions and whether participants reported feelings of being watched (no/yes) (see Figure 5.15). There were no significant differences between the male (20.8%), female (26.2%), and no-eyes (23.8%) groups in the proportions of participants who felt like they were being watched during the survey, $\chi^2(2, N = 153) = .480$, $p =.787$, $V = .056$, 90% CI [ 0.03, 0.21]. There were no significant differences between the angry (14%), happy (29.2%), and no-eyes (23.8%) groups in the proportions of participants who felt like they were being watched during the survey, $\chi^2(2, N = 153) = 3.684$, $p =.158$, $V = .155$, 90% CI [0.06, 0.29]. There were also no significant differences between the static (28.6%), blinking (28.6%), audio (22.7%) and no-eyes (23.8%) groups in the proportions of participants who felt like they were being watched during the survey, $\chi^2(3, N = 85) = .320$, $p =.956$, $V = .061$, 90% CI [ 0.07, 0.32].

**Figure 5.14**

*Proportions of participants who reported seeing an image of eyes*

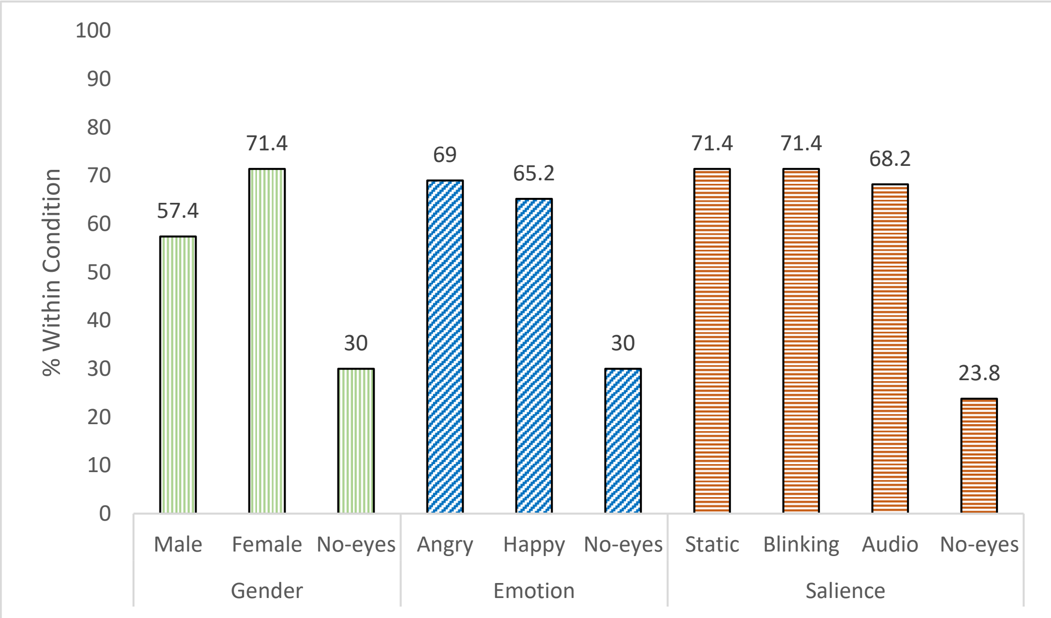
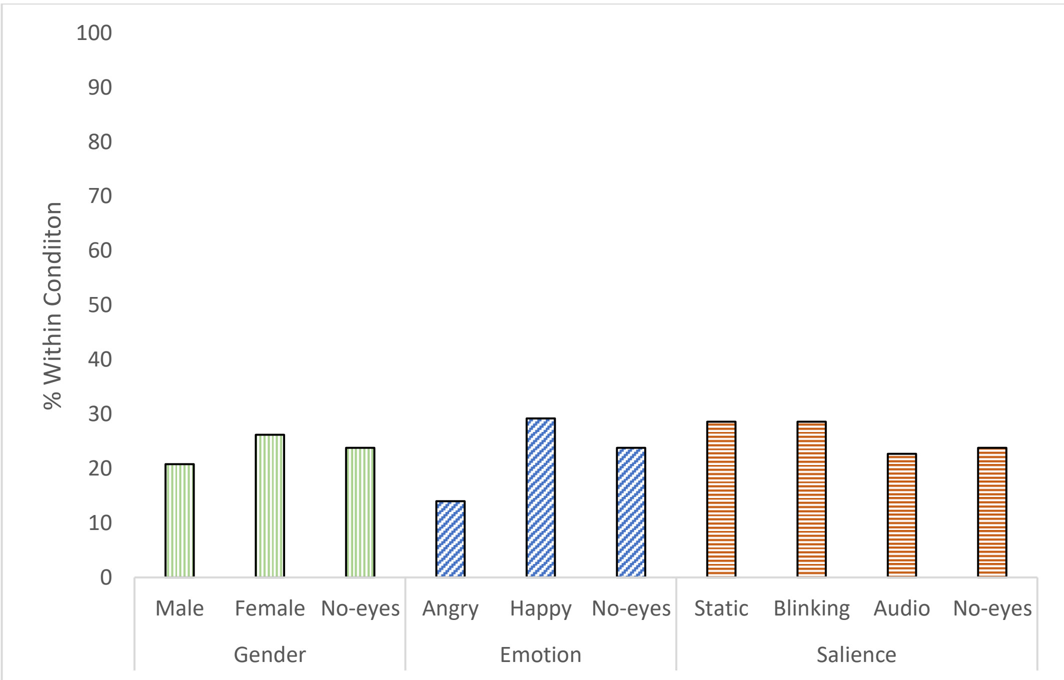



**Figure 5.15**

*Proportions of participants who felt like they were being observed*



**Exploratory Analysis**

Exploratory analysis was conducted to see 1) if there were any significant differences between those participants who reported feelings of being watched and those who didn't, and 2) whether there were any significant differences in the amounts donated to charity if the participant was selected as the prize draw winner (i.e., representing an actual cost) compared to if the participants were not selected as the prize draw winner (i.e., representing a potential cost).

In the first empirical study, as feelings of being watched are thought to be a key component of the watching-eyes effect (Bateson et al., 2006), exploratory analysis was conducted on just those participants who reported feelings of being watched during the survey. This could not be conducted in the second empirical study due to the low sample sizes. In this third empirical study, sample sizes in each condition were again too low to repeat the exploratory analysis from study one but it was decided to compare the prosocial outcomes between those who felt watched and those who didn't. A series of Mann-Whitney U tests were run on the three measures of prosocial behaviour between those who felt watched and those who did not.

The amount donated to charity if selected as a prize draw winner (£0-£40) for those who felt watched (*Mdn* = 10.00, IQR = 5.00 – 36.25) was not statistically different from those who did not feel watched (*Mdn* = 10.00, IQR = 0.00 – 20.00), $U$ = 828.5000, $z$ = .704, $p$ = .482, $r$ = 0.05, 90% CI [-0.10, 0.20]. The amount donated to charity if not selected as a prize draw winner (£0-£40) for those who felt watched (*Mdn* = 5.00, IQR = 0.00 – 10.00) were not statistically different from those who did not feel watched (*Mdn* = 10.00, IQR = 0.00 – 10.00), $U$ = 1322, $z$ = .754, $p$ = .451, $r$ = 0.07. The total prosocial score (0-9) for those who felt watched (*Mdn* = 9.00, IQR = 2.00 – 9.00) was marginally statistically different from those who did not feel watched (*Mdn* = 9.00, IQR = 9.00 – 9.00), $U$ = 1793.5, $z$ = -1.928, $p$ = .054, $r$ = -0.15, 90% CI [-0.29, 0.00].

A series of chi-square tests were conducted to compare the proportions of participants who thought each of the moral dilemma scenarios was morally permissible (no/yes) between those who did and did not feel watched. In the first scenario, there were no significant differences in the proportions of participants who felt it was morally permissible between the participants who did (70.3%) and did not feel watched (74.1%) , $\chi2(2, N = 153) = .214$, $p = .644$, $V = .037$, 90% CI [0.004, 0.19]. However, in the second scenario, there were significantly more participants who reported that they felt like they were being watched who thought it was morally permissible (32.4%) than those who did not feel like they were being watched (14.7%), $\chi2(2, N = 153) = 5.771$, $p = .016$, $V = .194$, 90% CI [0.05, 0.35]. In the final scenario, there were no significant differences in the proportions of participants who felt it was morally permissible between the participants who did (56.8%) and did not feel watched (58.6%) , $\chi2(2, N = 153) = .040$, $p = .841$, $V = .016$, 90% CI [0.01, 0.17].

A Wilcoxon signed-rank test showed that participants were willing to donate a significantly higher amount to charity if they were selected as winners of the prize drawer (*Mdn* = 10) (representing a potential actual cost) than if they were not selected as winners (*Mdn* = 0.5) (representing a potential theoretical cost), $T = 65.00$, $p = < 0.01$, $r = -0.48$, 90% CI [-0.59, -0.36].

## Discussion and Conclusion

The main aim of this study was to expand on the first two empirical studies of this thesis, which tested whether the presence of eye cues positively affected prosocial behaviour in an online environment by further manipulating eye stimuli in three areas: Gender, emotion, and salience. As with studies one and two, due to issues with low statistical power, and the presence of floor and ceiling effects, no definitive conclusions can be drawn from this study about the impact of watching eyes on prosocial behaviour. This will be discussed in the limitations section at the end of the chapter.

**Summary**

The main analysis revealed that the gender (male, female, and no-eyes), emotion (angry, happy, and no-eyes) and salience (static, blinking, audio, and no-eyes) of the eye images did not significantly impact the amounts donated to charity (both if the participant was selected as the winner of the prize draw and if they were not). Nor did these factors impact the participant's total prosocial score.

There were three moral dilemma scenarios reported that were presented to the participants. Participants were asked 1) whether it was morally permissible to switch a train to a side track and kill one person to avoid killing five, 2) whether it was morally permissible to kill one person by personally pushing them onto the track in order to save five other people, and 3) whether it was morally permissible to switch a train onto a side track to kill one person with their back turned to save five people who were walking across the main track. There were no significant differences between groups within the gender, emotion, or salience conditions in the percentages of participants who thought scenario one and scenario three were morally permissible. However, the results showed that participants who were exposed to the eye images within the emotion and gender conditions were significantly less likely to agree with a more controversial moral dilemma (e.g., the willingness to cause direct physical harm to one person to save the lives of others).

The manipulation checks indicated that there was a significant difference between conditions in the numbers of participants who but as with studies one and two, being exposed to the eye images did not significantly increase participant reports of feeling watched.

Exploratory analysis was conducted to see 1) if there were any significant differences between those participants who reported feelings of being watched and those who didn't, and 2) whether there were any significant differences in the amounts donated to charity if the participant was selected as the prize draw winner (i.e., representing an actual cost) compared to if the participants were not selected as the prize draw winner (i.e., representing a potential cost). The results showed that there were marginally significant differences and moderate effects sizes in the total prosocial score but not in the amount donated to charity between those who reported feelings of being watched and those who did not. There were also no significant differences in the proportions of participants who felt that scenario one and scenario three of the moral dilemmas were morally permissible. There were significantly more participants who reported feelings of being watched that felt that scenario two was morally permissible than those who did not report feelings of being watched. The results also demonstrated that there was a significantly higher amount donated to charity if the participant was selected as the prize draw winner compared to the amount donated if they were not selected as the winner.

**Gender of the Eye Stimuli**

As research has shown that males, rather than females, use altruism (i.e., a form of prosocial behaviour) as a mating display (Iredale et al., 2008) and males are more affected by eye images than females (Rigdon et al., 2009), it was anticipated that the gender of the eye stimuli would be an important factor in evoking the watching-eyes effect on prosocial behaviour. However, the results from this analysis did not find evidence that the presence and gender of the eye stimuli increased levels of prosocial behaviour in an online environment.

This is in line with previous studies by Nettle et al. (2013), Panagopoulos, (2014a), and Northover et al., (2016) who found no significant differences in the watching-eyes effect generated by male and female eyes. Any effect of watching-eyes may be moderated by further nuances such as the perceived attractiveness (as opposed to simply the gender) of the eye cue (Panagopoulos, 2014a). If a person finds an eye image particularly attractive, this could lead to an increase in attention to the watching-eyes, which in turn could lead to an increase in prosocial behaviour (Vrouwe & Balliet, 2014). However, in a meta-analysis that explored possible watching-eyes moderators, Northover et al. (2016) found that the gender and perceived attractiveness of the eyes cued did not moderate the effects of watching-eyes. With more recent studies (including this study) failing to find any evidence of a main effect for the gender of the eye cues on prosocial behaviour, it is possible that the positive results found in Bateson et al., (2006) and Vrouwe and Balliet (2014) were false positives (Northover et al., 2016).

**The Emotion Portrayed by the Eye Stimuli**

The eyes play a key role in social interactions as they aid in inferring others' attentional, emotional, and or mental states (Vaish et al., 2017) which is a key factor in shaping people's expectations and subsequent behaviour (Pauwels et al., 2017). However, it is unclear within the literature whether any sensitivity to eye cues is influenced by the emotion expressed in the stimuli as there have been few studies that have specifically explored the impact of the emotion portrayed by the eye stimuli on the watching-eyes effect (e.g., Pauwels et al, 2017; Saunders et al., 2016) and these few studies have found conflicting results. As behaviour in economic games has been found to be substantially affected by factors such as the emotional expression of partners (as discussed in Haley & Fessler, 2005) and previous research on the watching-eyes effect have found that unkind rather than kind eyes boost cooperation in a prisoner's dilemma game (Pauwels et al., 2017), it was anticipated that the emotion portrayed by the eye stimuli (i.e., angry or happy) would be an important factor in evoking the watching-eyes effect on prosocial behaviour.

However, in line with Saunders et al. (2016), the results from this analysis did not find evidence that the presence and gender of the eye stimuli increased levels of prosocial behaviour in an online environment. One explanation for this could be due to the lack of social norm messages within the prosocial messages in this study. There has been a suggestion within the literature that the watching-eyes effect does not work by directionally affecting desirable behaviour (either positively or negatively) *per se* but rather works by increasing adherence to the social norm (Hietanen et al., 2018). In their study which explicitly explored the emotion portrayed by the eye stimuli in the watching-eyes effect, Pauwels et al. (2017) posit that rather than encouraging prosocial behaviour, unkind eyes boost cooperation due to a ‘policing effect’ by coercing participants to conform to a prosocial norm.

Another possible explanation for the lack of evidence in this study is that any potential effects of watching-eyes are so weak they do not translate into an online environment (Raihani & Bshary, 2012; Saunders et al., 2016). As discussed in study one, any increase in prosocial behaviour in response to eye images is likely to be an involuntary, subconscious response (Burnham & Hare, 2007) which can eventually be overridden by slower acting conscious pathways (e.g., the realisation that the eyes cues are not real). Exposing the participants to the eye stimuli on a screen means that the participants are viewing the eye stimuli more directly, with fewer distractions and for possibly longer periods of times than in other settings (e.g., if they were just walking past a poster of watching eyes). Any emotional response subconsciously triggered by the eye stimuli would be presumably overridden by the quick and conscious realisation that the eye images are not real and there is nobody around to watch their behaviour. If the participants are aware that the cues of being watched are false, they would know that there would be no repercussions (positive or otherwise) to the decisions they make, thus it would fail to trigger any reputational concerns (Krátký et al., 2016).

**The Salience of the Eye Stimuli**

It has been suggested that online studies of the watching-eyes effect have failed to find significant results because the online environment is a truly anonymous setting (Raihani & Bshary, 2012) and therefore the eyes are not effective in making people feel like they are being watched (Lamba & Mace, 2010). This is supported with the findings of the manipulation checks for empirical studies one, two and three of this study which found no relationship between the different eye groups and reported feelings of being watched. With the near-constant use of the internet in modern day-to-day life for many people across the world, people may have become used to seeing eye images on an almost constant basis and have become accustomed to ignoring them (Sparks & Barclay, 2013). In this study, it was thought that perhaps the eye stimuli somehow needed to be more salient to capture participants' attention. Research has suggested that the effectiveness of surveillance cues (i.e., the eye images) could be improved by the use of audio messages (Jansen et al., 2018), and it was therefore anticipated that increasing the salience of the eyes to include an audio element (which would mimic the presence of other people) would help to evoke the watching-eyes effect on prosocial behaviour.

There were no significant differences found between groups in donations to charity or in total prosocial score. These results, combined with the findings from the first two empirical studies, suggest that increasing the salience of the eye cues (i.e., how noticeable they are) does not increase prosocial behaviour in an online environment. One possible explanation for this is in a laboratory or online experimental setting, participants are aware that they are being manipulated and observed even if indirectly which could make the effect of the eye stimuli less salient (Dear et al., 2019). It has been claimed that positive results and larger effects sizes for watching-eyes have predominantly been found in field studies rather than laboratory experiments (Kelsey et al., 2018; Manesi & Pollet, 2017). To date, there are no known online field experiments in which the watching-eyes effect has been tested. Online field experiments are experiments that leverage platforms or systems that already exist on the internet to study the motivations and behaviours of individuals (Muise & Pan, 2019). This could an interesting future avenue to explore.

**The Prosocial Measures**

As highlighted above, there were no significant differences between groups within the gender, emotion or salience conditions in the amounts donated to charity or in the participants' total prosocial score. This is in line with many recent studies and meta-analyses which have failed to find evidence that cues of being watched can increase altruistic behaviour (e.g., Northover et al., 2017).

***Donations to Charity***

As discussed in the introduction of this study, the amount donated to charity could arguably be a more reliable measure of altruistic behaviour to use when testing the watching-eyes effect (i.e., when compared to the public goods game). Although participants may donate to a public goods pot for altruistic reasons, they may also be motivated by the potential to earn a bonus for themselves, whereas there are no immediate benefits for participants to donate at least some of their bonus to charity, making any donations to charity a more altruistic behaviour.

However, there were no significant differences between groups within the gender, emotion or salience conditions in the amounts donated to charity. In fact, multiple groups across all conditions, had a median donation of £0 which indicates a floor effect. The floor effect was seen in both the no-eyes group and in the eye's present groups (e.g., male, happy, static, and blinking) so any impact of the presence of eyes cannot be determined.

The exploratory analysis demonstrated that participants were willing to donate a significantly higher amount to charity if they were selected as winners of the prize draw (representing a potential actual cost) than if they were not selected as winners (representing a potential theoretical cost). This could suggest that, in line with CST (which maintains that a behaviour must be costly to act as a signal), people are more likely to signal their prosocial behaviour if there is an actual cost involved or it could simply mean that the participants were more willing to donate as they were given the money in the first place to give away. In line with a growing body of literature (e.g., Northover et al., 2017), the presence of eye images (even with the gender, emotion and salience distinctions) did not significantly increase the amounts donated to charity.

***Moral Judgements***

The participants in this study were presented with three moral dilemmas: 1) participants were asked whether it was morally permissible to switch a train to a sidetrack and kill one person to avoid killing five, 2) whether it was morally permissible to kill a person by personally shoving them onto the track in order to save the other five people, and 3) whether they thought it was morally permissible to switch a train onto a side track to kill one person with their back turned to save five people who were walking across the main track. According to Hauser et al. (2007), it could be expected that scenario one ('switch track') would have elicited a significantly higher proportion of permissibility judgments than scenario two ('physically push') as it is less permissible to cause harm by introducing a new threat (e.g., pushing a man) than by redirecting an existing threat (e.g., switching the train track). It could also be expected that there would be a higher proportion of participants in scenario three (back turned) who would agree that the situation was morally permissible as the person it would kill had their back turned (and therefore, not realise what was happening).

There were no significant differences between groups within the gender, emotion, or salience conditions in the numbers of participants who agreed that scenarios one and three were morally permissible. However, with the second scenario, there were significantly fewer participants in the eyes group within the emotion and gender conditions that thought it was morally permissible to personally push a person onto a track to save five people. This was also true for those who reported feelings of being watched when compared to those who did not.

Scenario two differs from scenarios one and three in two ways. First, the action to save lives involves introducing a new threat (e.g., pushing a person) rather than redirecting an existing threat (e.g., switching a train onto a side-track and into the path of a person).

Secondly, the action in scenario two is personal rather than impersonal as it involves causing harm by direct physical contact rather than by an indirect means (Hauser et al., 2007). Although participants may feel like it was morally permissible to cause the death of one person to save the lives of five others, scenario two represents a more controversial option of the three options.

The results of the manipulation checks showed that there was no relationship between the groups in the gender, emotion, or salience conditions and whether the participants reported feelings of being watched. There is a general consensus within the literature that if watching eyes are effective at changing behaviour, it is because they make us feel watched (Dear, 2018) yet it has been seen in both in the literature (e.g., Zengerink, 2013) and in empirical studies one, two, and three of this thesis that eye images do not significantly evoke general self-reported feelings of being watched.

Hauser et al. (2007) maintain that moral judgments are not based solely on conscious reasoning; when people make certain kinds of moral judgments, they may do so without consciously applying explicitly understood principles. In addition, although little attention has been paid to the question of whether the watching-eyes effect requires a direct gaze to be consciously perceived, there is some evidence that visual awareness of a direct gaze is not required for the watching-eyes effect to occur (Conty et al., 2016). It is possible that both the feelings of being watched and the reputation management mechanism it triggers could be occurring at a sub-conscious level (Matland & Murray, 2015) which may not be reflected in participants' self-reported measures of whether they felt observed.

The presence of eyes may have given the participants an unconscious perception that they were being watched which would have activated a reputation maintenance mechanism and this reputation management mechanism ultimately regulates the public expression of moral judgements (Bourrat et al., 2011). The action in scenario two is a more extreme action than the actions in scenarios one and three, representing a larger deviation in behaviour (compared to inaction). The watching-eyes effect may not work by directionally affecting desirable behaviour (e.g., increasing charitable donations) but rather by increasing adherence to the social norm (Hietanen et al., 2018). Although these moral dilemma scenarios did not include any social norm messages, in the study in which these moral dilemma scenarios were originally adapted from, it was determined that it is less permissible to cause harm by direct physical contact than by indirect means (Hauser et al., 2007). In this particular case, the results from scenario two could suggest that the participants in the eyes present groups felt less able to communicate controversial ideas (e.g., the willingness to cause direct physical harm) due to the more extreme deviation from the norm which could result in a fear of punishment or social judgment (Dear et al., 2019).

However, it is not possible to be certain that the participants in this study would have known which was the 'correct' moral judgement in these particular scenarios in order for them express their cooperative disposition and therefore it is not clear which moral decision (if either) would be reputation enhancing or what effect reputation will have on those decisions. In the more ambiguous scenarios one and three, if it is unclear what participants 'should' do, then it is also unclear on whether people will be more or less judgmental in the presence of watching eyes. Specific situational cues might encourage one response or another (Sparks and Barclay, 2015). If the predictions of the effect of reputation on moral judgment aren't clear, then as reputational concerns are the leading interpretation for the watching-eyes effect (Pauwels et al. 2017), the lack of results for these scenarios is perhaps unsurprising. The presence of eyes cannot moderate effect if there is no effect of reputation on moral judgements.

***Total Prosocial Score***

In line with the literature  (e.g., see Dear et al., 2019 for an overview) and the previous empirical studies in this thesis, this study included a public goods game as a measure of prosocial behaviour. A public goods game simulates a key common feature of many human interactions (such as cooperative food production or child-rearing): whereas the collective of individuals benefits from cooperation in these interactions, each individual would be materially better off by free-riding on others (Alger, 2010). Viewing this through the lens of CST, contributing to a public good provides an excellent platform in which the giver can advertise their qualities (e.g., resources or generosity) as a potential interactive partner, group member or sexual mate (van Vugt & Hardy, 2009). However, in empirical studies one and two, a single-item was used as a measure of prosocial behaviour (the percentage of the participants' initial bonus donate to the public goods pot) which raises concerns around a potential lack of sensitivity (Bergkvist & Rossiter, 2007). Using a single-item measure means that there were fewer points of discrimination in which to detect a watching-eyes effect, which is particularly problematic in small sample sizes (Sauro, 2018).

To address this issue, participants in this study played a nine-item public goods game, based on the methodology by Messick and McClintock (1968) and used by Iredale (2009). Each item was scored as either '0' (selfish) or '1' (prosocial) in order for a total prosocial score across the nine items to be calculated and compared across groups. It was hoped that providing more points of discrimination would increase the sensitivity of the measure but despite these additional items in the prosocial measure, there were no significant differences between groups within the gender, emotion, or salience conditions in the total prosocial scores. In fact, across all groups and conditions, there was a median prosocial score of nine (the maximum amount available). This is indicative of a ceiling effect which could be masking any effect of gaze on overall task performance (Manesi et al., 2016). As this ceiling effect is present across all groups, the presence of watching eyes could not have made an additional impact (Pfattheicher et al., 2018).

One possible explanation for these findings is that, in the public good games in this study, the participants were asked to allocate points to themselves and an 'other' which did not represent any actual cost to the participant. As discussed earlier in relation to donations to charity, a core underlying principle of CST and the watching-eyes effect is that if a person feels like they are being watched, they are incentivised to signal a quality about themselves via prosocial behaviours as this may provide a fitness advantage. It is possible that the prosocial decisions made in the public goods game of this study were not a reliable prosocial signal as there was no actual cost to the participants and it would be easy for the participant to fake intention (Bradley et al., 2018).

Another possible explanation for these findings could be that prosociality is the wrong context in which to examine the watching-eyes effect. A recent meta-analysis showed that there is a robust watching-eyes effect in reducing anti-social behaviour, Dear et al. (2019) maintains that the inconsistent results in the watching eyes literature could be due to the inconsistent effects of prosocial behaviour on enhancing reputation. Eye cues may more reliably reduce antisocial behaviour as anti-social behaviour is more consistently reputation damaging. Pauwels et al. (2017) posit that the eye gaze (particularly of unkind eyes) provides a type of policing effect, compelling individuals to conform to prosocial norms or risk the anger or disappointment of their interaction partner. The earlier findings in this study showed that the participants in the eyes present groups were less likely to agree with a controversial moral dilemma (e.g., the willingness to cause direct physical harm to one person to save the lives of others), perhaps due to the more extreme deviation from the norm which could result in a fear of punishment or social judgment (Dear et al., 2019). Therefore it is likely that watching eyes may be more effective in reminding people to conform to social norms and that they risk punishment if they do not (Hietanen et al., 2018). With the growing consensus that, if there is a watching-eyes effect, its effect on human generosity is weak (Shinohara & Yamamoto, 2018), it is possible that a public goods game may not be sensitive enough to capture a watching-eyes effect on prosocial behaviour in an online environment, if one exists at all.

**Limitations**

Although this study had a larger sample size than previous studies in this thesis, a major limitation is that as with studies one and two, this analysis was underpowered which impacts the interpretation of any non-significant results. The manipulation checks (e.g., chi-square tests) only had enough power to detect medium and large effects whereas the main analysis (e.g., Kruskal-Wallis H tests) and the exploratory tests (e.g., Wilcoxon tests) only had enough power to detect a large effect size. Gignac and Szodorai (2016) report that in psychology the typical effect size is $r$ = .19, therefore it would have been ideal to have had enough power to reach a small-medium effect size. This means that the analysis had a greater chance of type II error, the effect could have been there but there was not enough statistical power to detect it.

This would also have an impact in cases where marginal significant differences and moderate effect sizes were seen after Bonferroni corrections were applied (e.g., the difference between gender groups who though moral dilemma two was permissible). Bonferroni corrections are generally conservative (Perneger, 1998) and coupled with low statistical power, this means there may still be an effect even when any significant differences failed to pass the Bonferroni threshold (such as the female and no-eyes groups in the above scenario). In addition, as with studies one and two, equivalence tests were conducted and the CIs for each effect size was outside of the boundaries for the SESOI. Therefore, this means that the results of this analysis are inconclusive as it cannot be concluded either way whether there was an effect of watching eyes on prosocial behaviour.

There was also an issue identified with ceiling effects in Total Prosocial Score across all groups and conditions. In addition, floor effects were also seen in the amount donated to charity (if not a winner) across all conditions. The presence of these effects mean that the watching-eyes effect could not have made an additional impact so conclusions cannot be drawn from these results (Pfattheicher et al., 2018). There is a possibility that, if the watching-eyes effect does work, then prosociality is not the right context in which to explore the effect. Watching eyes may be more effective in reminding people to conform to social norms rather than to display prosocial tendencies (Hietanen et al., 2018) so it could achieve more consistent results in deterring anti-social behaviour rather than promote prosocial behaviour (Dear et al., 2019). It has also been suggested that the watching-eyes effect may produce a more robust effect in the field rather than in laboratory experiments (Kelsey et al., 2018; Manesi & Pollet, 2017). This is perhaps because, in an experimental context, participants are aware that they are being manipulated and observed by the researcher, even if indirectly, which could make the effect of the eye stimuli less salient (Dear et al., 2019). There are no known online field experiments in which the watching-eyes effect has been tested.

A final potential limitation of this study is that as with study one, fewer participants reported noticing the eye stimuli. In particular, only 57.4% of participants who were exposed to the male eye stimuli reported seeing an image of eyes whereas nearly a third of participants in the no-eyes groups erroneously reported seeing an image of eyes. This was not an issue in study two, which was the only laboratory-based study of this thesis. This suggests that perhaps when participants complete a survey online (outside of a laboratory environment) there may be extraneous variables which impact a person’s ability to pay attention to the eye stimuli, whereas conditions in a laboratory setting are more tightly controlled and therefore will be less subject to the influence of extraneous variables (Bradley et al. 2018).

**Future Studies**

Despite the widely reported replication issues of the watching-eyes effect, it remains a popular research topic. Although the watching-eyes effect is not the panacea that perhaps early studies suggested it was, it does provide a potentially simple and cost-effective way of trying to affect human behaviour in ways that can have a meaningful impact (Dear et al., 2019). An interesting avenue to explore further is the potential impact of the watching-eyes effect beyond the somewhat artificially controlled conditions of the laboratory. The work by Nettle et al., (2012) represents one of the baseline studies in the watching-eyes research and is one of the most influential peer-reviewed research papers on the watching-eyes effect on public policy in the United Kingdom (Dear et al., 2019). Since its publication, there have been multiple examples within UK public policy of utilising watching eyes images to reduce a range of antisocial behaviour including high profile campaigns by HMRC (Nelson, 2013; BBC News, 2015) discouraging tax evasion and by 'Keep Britain Tidy' in reducing the number of dog fouls left on public streets (Keep Britain Tidy, 2014). If watching eyes does produce a more robust effect in the field, then it would be worth exploring why this could be. In addition, to date, there are no known online field experiments in which the watching-eyes effect has been tested so both of these considerations will be explored in the final empirical chapter of this thesis.

**Conclusion**

A strength of this third empirical study was that it built on the first two empirical studies of this thesis by expanding on the range of eye stimuli to examine possible effects of gender, emotion, and salience of the eye images to explore the specific nuances and qualities of the eyes themselves that may trigger prosocial behaviour in an online context. In addition to replicating and extending previous research, an additional strength was that it included a novel aspect, the use of audio cues to mimic the presence of others and aid in capturing participant attention, which had not been seen in the watching eyes literature before.

Overall, there were no significant differences between the gender, emotion, salience or watched conditions in the levels of prosocial behaviour across prosocial measures except for scenario two of the moral dilemmas. Results showed that in the presence of eyes, the participants in the gender and emotion conditions were less likely to report that they thought it was morally permissible to cause direct physical harm to one person to save the lives of five others. It was argued that the participants in the eye conditions felt less able to communicate controversial ideas (e.g., the willingness to cause direct physical harm) due to the more extreme deviation from the social norm. Conversely, there were a higher number of participants who reported that they felt that the action from scenario two was morally permissible when they had felt like they were being observed during the survey but it was argued that both the feelings of observation and the reputation management mechanism it triggers could be occurring at a sub-conscious level (Matland & Murray, 2015).

This thesis so far has provided an in-depth exploration into the possible facets of the watching-eyes effect. The first empirical study explored the effects of the salience of the eye cues on prosocial behaviour in an online survey. The second empirical study attempted to manipulate the reputational concerns of the participants and explore the possible effects whilst collecting additional data on eye gaze behaviour to ascertain whether participants were paying attention to the eye images. This third empirical study explored further social aspects (i.e., gender, emotion, and salience) of the eye images and their potential impact on prosocial behaviour. However, due to low statistical power in each of these studies, no conclusive evidence has been found to support or reject the idea that the presence of watching eyes increases prosocial behaviour. The final empirical chapter of this thesis will attempt to address the further questions identified in this study: whether the watching-eyes effect can be elicited in an online field experiment and whether the watching-eyes effect is more effective at deterring anti-social behaviour. The first part of the chapter will focus on addressing the apparent lack of online field experiments which have tested the watching-eyes effect and the second part will focus on re-evaluating a previous field experiment that utilised the watching-eyes effect in deterring anti-social behaviour. Overall, the final empirical chapter will contribute to the overall rigorous approach undertaken in this thesis to exploring whether the watching-eyes effect can be utilised in an online environment.

## Chapter Six – A field-based exploration of the watching-eyes effect

This thesis so far has not found any evidence that the presence of eyes increases prosocial behaviour but the results from study three indicate that the presence of eyes may deter people from agreeing with controversial moral dilemmas (i.e., possibly representing a strong deviation from the norm). Up to now, this thesis has explored the watching-eyes effect in an online survey (empirical study one), in a laboratory setting with the collection of additional data on eye gaze behaviour (empirical study two) and in a further online study exploring the effects of the gender, emotion and salience of the eye stimuli on prosocial behaviour (empirical study three).

In each of these studies, no evidence has been found for the watching-eyes effect on prosocial behaviour. This corresponds with other recent experimental studies which have failed to replicate the watching-eyes effect on prosocial behaviour (Dear et al., 2019). Yet, Dear et al. (2019) found in their meta-analysis that the presence of watching-eyes was associated with a 35% reduction in the risk of antisocial behaviour and the findings from the previous study of this thesis showed that the participants were significantly less likely to agree with a controversial moral dilemma (e.g., the willingness to cause direct physical harm to one person to save the lives of others) in the presence of watching eyes. These mixed results suggest that the watching-eyes effect should be interpreted cautiously (Northover et al., 2017) but the possible meaningful effect of watching-eyes in the right context (e.g., reducing anti-social behaviour) warrants further research (Dear et al., 2019).

The watching eyes study on deterring bicycle theft conducted by Nettle et al., (2012) represents one of the baseline studies in ‘watching eyes’ research and is one of the most influential peer-reviewed research papers on the watching-eyes effect in UK public policy (Dear et al., 2019). It has paved the way for a multitude of interventions in the UK aimed at deterring a range of anti-social behaviours. Examples of ‘watching eyes’ in public policy include attempts by West Midlands Police (2006) to deter theft, Hucknall Police (Flanagan, 2013) in reducing crime, the Forest of Dean (Knapton, 2016) in reducing littering, HMRC (Nelson, 2013; BBC News, 2015) in discouraging tax evasion, British Transport Police (Basildon Canvey Echo, 2013) in deterring bicycle thieves, and motorway service stations (Keep Britain Tidy, 2015) in reducing littering. In fact, the use of ‘watching eyes’ is so well established in UK public policy that the UK Government’s (2017) National Anti-Littering Strategy recommends the use of ‘watching eyes’ interventions to reduce littering (as discussed in Dear et al., 2019).

However, the data available on the *outcomes* of these applied interventions is limited at best. For example, in 2013, multiple news articles (Reilly, 2013; BBC News, 2013) reported that the British Transport Police were going to trial a ‘watching eyes’ intervention with the train operating company c2c. However, despite an extensive web search and a request for information sent to the British Transport Police, it has not been possible to obtain any information on the watching eyes intervention between the British Transport Police and c2c.

A notable exception is Keep Britain Tidy (2014) with their “We’re watching you” campaign, a specifically designed social experiment to combat dog fouling. Keep Britain Tidy (2014) spearheaded a nationwide campaign in partnership with 17 local partner councils (LPCs) which utilised images of watching eyes on anti-dog fouling posters. Each LPC monitored the average number of dog fouls three weeks prior to the watching eyes posters being erected and for three weeks whilst the posters were displayed. Overall, Keep Britain Tidy reported a significant decrease of 46% in dog fouling across all sites. Following this initial iteration of the social experiment, Keep Britain Tidy now offer a ‘watching eyes’ package which land managers can purchase to help tackle issues with dog fouling and they report that “hundreds of partners are already successfully using the campaign and making a real difference in their area” (Keep Britain Tidy, n.d.).

Public policy interventions are seemingly highly successful, with the majority reporting a reduction in the anti-social behaviour targeted using an inexpensive and simple methodology (i.e., a simple display of eye images). However, except for the campaign by Keep Britain Tidy

(2014), these interventions have not been designed in a robust experimental way, so it is difficult to interpret whether it is the watching-eyes themselves that is causing a reduction in anti-social behaviour or some other factor, such as the messaging included on the signs, or the simple display of a sign itself (Dear et al., 2019). Nevertheless, there are multiple academic experiments in field settings that do robustly test for the watching-eyes effect. In their recent meta-analysis, Dear et al. (2019) provided what they claim to be the most comprehensive database of watching-eyes studies yet compiled. From their database, Dear et al. (2019) listed 35 field experiments across 31 articles, of which 24 reported finding a positive effect (68.57%).

It has been claimed that positive results and larger effects sizes for watching eyes have predominantly been found in field studies rather than laboratory experiments (Kelsey et al., 2018; Manesi & Pollet, 2017). The field experiments in Dear et al's (2019) meta-analysis behaviour cover a range of prosocial behaviours including increasing donations to an honesty box (Bateson et al., 2006; Brudermann et al., 2015; Krátký et al., 2016), increasing charitable donations (Ekström, 2012; Fathi et al., 2014; Krupka & Croson, 2016; Oda & Ichihashi, 2016; Powell et al., 2012), increasing museum donations (Gaiani & Rose, n.d.; Kelsey et al., 2018), increasing voter turnout (Matland & Murray, 2015; Panagopoulos, 2014b, 2014a; Panagopoulos & van der Linden, 2016; Rad et al., 2018), increasing hand-washing hygiene (Beyfus et al., 2016; Bolton, Rivas, Prachar, & Jones, 2015; Kuliga, Verhoeven, & Tanja-Dijkstra, 2011; Stella et al., 2013), increasing political compliance (Bush et al., 2016), increasing survey participation (Pedersen, 2016), increasing prosocial search terms (Beaumont, 2019), decreasing littering (Bateson et al., 2013; Ernest-Jones et al., 2011; Francey & Bergmüller, 2012; Zengerink, 2013), deterring theft (Nettle et al., 2012), preventing engine idling (Meleady et al., 2017) and reducing fare evasion (Ayal et al., 2019).

To date, online lab experiments have found no evidence for the watching-eyes effect on generosity and there are no known online field experiments (i.e., experiments that leverage platforms or systems that already exist on the internet) in which the watching-eyes have been tested. It has been suggested that the effect of eye cues may be weakened or non-existent in an online environment (Saunders et al., 2016) as the eye cues are not effective in making people feel like they are being observed (e.g., Lamba & Mace, 2010; Tane & Takezawa, 2011), which is hypothesised to be a key component in generating the watching-eyes effect (Conty et al., 2016). Perhaps one explanation for this is that in a laboratory or online experimental setting participants are aware that they are being manipulated and observed, even if indirectly, which makes the effect of the eye images less salient (Dear et al., 2019).

In behavioural science, people's judgement is thought to be largely driven by intuition (i.e., system one) or deliberate thought (i.e., system two) (Kahneman, 2011). The watching-eyes effect falls under a branch of social psychology known as 'priming' which is used to implicitly influence a person's behaviour through subtle cues of specific words, phrases or ways of viewing things (Lukkien, 2019). However, as Lukkien (2019) maintains, there is a debate amongst those in the field on whether priming interventions only work when the person being influenced is unaware of the prime (e.g., an eye cue). For instance, individuals engage in many automatic behaviours without a great deal of thought or attention to them (system one). However, when they are aware of the nudges (i.e., priming cues), this activates system two which overrides system one and can sometimes result in 'backfiring effects' whereby the person then demonstrates the opposite behaviour to what is desired (Bolton et al., 2015). It is possible that in an experimental setting, participants would be anticipating some sort of manipulation or test and therefore may be more aware of watching eyes than they would in a naturalistic setting. Therefore, the first aim of this chapter was to conduct an online field experiment in a naturalistic setting where participants may be less likely to be consciously aware of the prime (i.e., the eye images).

Although field experiments on the watching-eyes effect seem more successful than lab experiments, there is a difficulty with replication. For example, one of the earliest watching-eyes field experiments was conducted by Bateson et al., (2006) who found that donations to an honesty box increased in the presence of a pair of watching-eyes. This experiment was later re-visited by Carbon and Hesslinger, (2011) who upon a systematic re-analysis found several shortcomings including the definition of key terms, unspecified methodology, confounding variables, invalid statistical analyses and a lack of insight into factors modulating the effect. Most importantly, in their replication experiment Carbon and Hesslinger (2011) failed to replicate the findings from Bateson et al. (2006). Due to the lack of consistent evidence in both lab and field experiments in recent years, there has been a call for more replication studies to examine the effectiveness of the watching-eyes effect (e.g., Oda & Ichihashi, 2016).

Due to the inconclusive findings of this thesis so far, the second aim of this chapter was to re-evaluate a successful field experiment to take a closer look at the methodology, analysis, and results to assess the reliability of their claims, similar to the approach undertaken by Carbon and Hesslinger (2011). It was decided to explore the Keep Britain Tidy (2014) "we're watching you" campaign as it was the only published public policy social experiment in which the raw data could be obtained. As a non-academic piece of research, the Keep Britain Tidy (2014) campaign did not go through the same peer-review process and was not subject to the same level of external scrutiny that the majority of watching-eyes studies are subject to. This re-evaluation will be the first to provide a methodological critical reflection of the campaign.

In this chapter, there were two approaches undertaken to try and explore why there seems to be a discord in the success of eliciting the watching-eyes effect between lab-based and field experiments. First, an online social experiment (Study A - Eyes in the Real-World; an online field-based exploration of the watching-eye effect) was conducted in collaboration with an international charity to try and increase online charitable donations. Second, as part of a critical reflection on a reportedly successful watching eyes campaign, the methodology and data from a field study (Study B – Keeping Britain Tidy; A Closer Look) was explored and evaluated.

## Study Four A - Eyes Online: An Online Field Experiment

This online field-based experiment took place as a collaboration between The International Federation for Athletes with Intellectual Impairments (INAS), the researcher (Keli Jenner) and colleagues at Canterbury Christ Church University (CCCU). The first aim of this study (Ethics: Jenner- 18-SAS-20F) was to build on the results from study one, study two, and study three to explore whether the presence of watching eyes can increase online charitable donations in a naturalistic setting. A secondary aim was to explore whether the web visitors that are exposed to the blinking-eye stimuli donate more money to charity than those who are exposed to either a static image of eyes or no image of eyes at all.

### Methodology

#### Stimuli

A series of three single web pages were created by INAS which mimicked the INAS website at the time (see Figure 6.1). Each webpage had information about the INAS Global Games, a key event in INAS' calendar, and contained a Charities Aid Foundation (CAF) link to donate money directly to the charity. What differed between the webpages is whether the web visitors saw a static image of eyes, a short looping video of blinking eyes, or no eye images on the top of the web page (there was one web page per condition with an individual URL address for each webpage).

#### Procedure

Three Twitter advertising (ad) campaigns were created, one for each of the conditions (static, blinking, and no-eyes) to promote the charity to Twitter users and encourage those who do visit the web pages to donate to the charity. Twitter ad campaigns work by the account user creating a 'landing card' which contains some basic information about the purpose of the webpage (see Figure 6.2) and a link to the relevant webpage (Twitter, 2020).

**Figure 6.1**

*A screenshot of the specially created INAS web page (static eyes)*

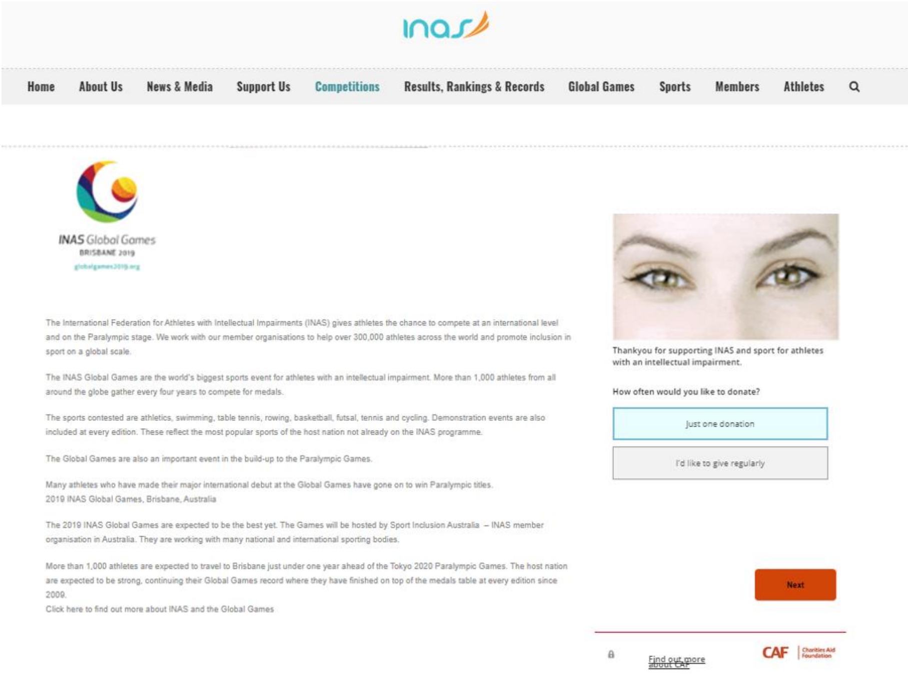



**Figure 6.2**

*The Twitter ad Campaign text*

| |
|---|
| **Tweet1 - Ad group: Donate1**<br><br>Did you know that the INAS Global Games are the world's biggest high-performance sports event for athletes with an intellectual impairment?<br><br>Find out more and help support our work here 👉👉 http://bit.ly/2mua9u4<br><br>#Brisbane2019 #GlobalGames |

Twitter then shares this landing card via a promoted Tweet on the Twitter feeds of users that fit the criteria set out by the campaign creator. The only criteria that were set out in this experiment were that users, in line with the British Psychology Society's ethical guidelines (The British Psychological Society, 2018), had to be over the age of 18 years old. There are several different types of Twitter ad campaigns and in this case, a ‘website visits’ campaign was created whereby the campaign is only charged if a Twitter user clicks on the ad and visits the website. With this type of campaign, the account user/campaign creator must specify how much they are willing to pay for a website click. When the ad campaign starts, the campaign enters a Twitter auction where it will compete against other advertisers who are targeting the same audience. Whichever advertiser has the highest bid will generally win the auction and have the ad served (i.e., displayed to the target audience). The winning bid, in theory, can be as low as 1p if no other advertisers are competing for your audience but there is no way to know this in advance of launching the campaign, the only option is to set a maximum amount per bid to prevent excessive costs. With a research budget of £150 and following Twitter ad campaign guidance, the bid was set at a maximum of £1 per website click with each of the three campaigns capped at £50. The aim was to collect data on a minimum of 50 participants per condition (150 in total). A power analysis was conducted and will be discussed in the following section.

Each Twitter ad campaign was identical except for the web page link. Each link was shortened using Bitly, a URL shortening service. This was to mask the actual address of each web page and make it less obvious that there were different web pages in the event that a Twitter User saw more than one of the Twitter campaigns. The web pages were not accessible from anywhere else on the INAS website, only via the link from the Twitter campaign. The Twitter campaign was launched from the INAS Twitter account for authenticity and ran for one calendar month (October 2019). This coincided with the INAS Global Games (12th to 19th October 2019) to try and take advantage of the increased INAS web traffic around this time. In addition, the charity was undergoing a merger and rebranding at the end of the month so they had stipulated that the experiment must be completed before this happened.

INAS agreed that at the end of the campaigns, they would share a summary report on the number of website visits for each condition, the individual donation amount per click, the country of donation and the total amount donated for each condition. In line with other field experiments for the watching-eyes effect, and to adhere to GDPR requirements, it was agreed that no additional data on the participants would be recorded and the researcher/CCCU would not have access to any additional or identifying information.

**Data**

This was a between-subjects experimental design with the condition (static, blinking, and no-eyes) as the independent variable (IV) and the average donation amount per click (APCk) as the dependent variable (DV). It was planned that the data would be analysed using a one-way Analysis of Variance (ANOVA). An *a priori* power analysis was conducted in G*Power. The power of a test is the probability that the test will find an effect assuming that one exists in the population and the aim should be to achieve a power of 0.8, or an 80% chance of detecting an effect if one genuinely exists (Field & Hole, 2003). It was determined that the desired sample size for a one-way ANOVA with three groups was 159 to be able to see a medium effect size (0.25) with an alpha of 0.05 and a power of 0.80 (e.g., Faul et al., 2007). As it was a possibility that each winning bid would cost less than the maximum cap of £1, it was hoped that the campaign budget of £150 would be enough to recruit the minimum total sample size.

## Results

There was a total of 33, 302 visits to the web pages across all three Twitter campaigns:

- Campaign A - Static condition: http://bit.ly/2oTyLgV - 11, 562 total clicks

- Campaign B - Blinking condition: http://bit.ly/2mua9u4 - 11, 112 total clicks
- Campaign C- No-eyes condition: http://bit.ly/2mwl6eN - 10, 628 total clicks

As visitors to these web pages were only exposed to the eye stimuli once they had already clicked on the link, the difference in the number of clicks across the conditions cannot be attributed to the eye stimuli at all. At the end of the campaign, despite over 33,000 web clicks, no donations were received via any of the three web pages. This will be evaluated in the following discussion section.

**Discussion**

The three Twitter ad campaigns created for this experiment were due to run for one calendar month from the 1st to the 31st October 2019. The campaign budget for each condition (static, blinking, and no-eyes) was capped at £50 per campaign with the view of paying a maximum bid of £1 per website click and collecting data on 50 participants per condition (a minimum of 150 participants in total across the three conditions). As Twitter is a powerful advertising tool used by many institutions, organisations and charities (van Dijck, 2012), it was expected that there would be some cost competition in winning the bids for ad servers and that it would be best to allow up to one calendar month for the budget of £150 to be spent. However, the campaign budget was maxed out by the 8th October 2019, just eight days after the start of the campaign and there were 33, 302 web visitors recorded. It was not known how many web visitors the charity would usually attract on their main web page in the same time period. The additional data provided by Bitly (the URL shortening service) showed at least 10, 000 web visitors per page but INAS reported no donations.

This was substantially more website clicks than the 150 that were envisioned across the entire calendar month and due to the sheer number of web visitors, it was unexpected that not a single person chose to donate. The average click to donation conversion rate for a non-profit organisation is 17% (Montalto, 2021) so if 17% of the 33, 302 visitors had donated, some 5661 donations could have been expected. Due to the absence of donation data, additional data on the web page Key Performance Indicators (KPIs) was requested (e.g., average time on page) to see if the presence of eyes affected web user behaviour at all. However, due to the charity undergoing a merger and rebranding at the end of the campaign, they no longer had access to the original Twitter or Google Analytics accounts, and it was not possible to obtain any additional data.

In discussions with INAS when creating the experiment, they had advised that they did not typically receive many donations via their website and had expressed concern that the usual lack of donations via the website may undermine the project. However, the charity could not say why they did not typically receive many donations via their website. This could have been due to a lack of initial visitors to the donation page or perhaps the cause was too specific (Bekkers & Wiepking, 2011). Those who are not involved with people with intellectual impairments may have not been aware of the charity and the support that they need, and they may feel that other charities, which they identify more with, may be of greater need. Any people already involved with individuals with intellectual impairments may already be engaged with specific charities (e.g., already making regular donations) so may simply ignore any additional charitable donation requests. In this experiment, the purpose of the Twitter ad campaigns was to specifically drive people to a page where they could donate to the charity. Each landing card created for the campaigns specifically invited participants to click on the link to donate money to help the charity’s cause, so if a participant had clicked on the link, then it is indicative that they were interested in the charity’s cause and there may have been an intention to donate to the charity.

**Potential Click Fraud**

The large volume of web page visitors for such a small budget campaign in a relatively short amount of time raised some red flags. One explanation for this data is Twitter's known 'bot' problem (Collins, 2018). A 'bot' (an internet robot) is a type of software that is designed to perform actions such as tweeting, re-tweeting, liking, following, unfollowing, commenting on posts or directly messaging other accounts (Chu, Gianvecchio, Wang, & Jajodia, 2012). Proper usage of bots includes broadcasting helpful information, automatically generating interesting or creative content, and automatically replying to users via direct message. However, a growing problem for Twitter campaigns is that is becoming increasingly more difficult to tell if you are interacting with a real person on Twitter which contributes to click fraud, a big issue in digital advertising (Collins, 2018).

Twitter ad campaigns are a type of pay-per-click online advertising whereby companies get paid an amount of money determined on how many visitors to their site click on the ads. Click fraud occurs when either a real person or bot imitates a legitimate user on the site, clicking on the ad without having any interest in meaningfully engaging with the ad (or in the case of the bot, without having the ability to) and it is reported that about 20% of bots contribute to click fraud (Dovaston, 2019). Robotic traffic is driven by code, not humans so there is no meaningful engagement, but bots are smart enough to mimic human behaviour, making them difficult to detect (Auty, 2018). As Auty (2018) points out, this results in companies wasting large amounts of money based on fraudulent Twitter impressions (the total tally of all the times the Tweet has been seen). An abnormally large click-through rate for advertising campaigns is seen as a key indicator of the influence of bots.

Online advice (e.g., Auty, 2018; Dovaston, 2019) is for businesses to adopt a fraud prevention strategy which includes monitoring user behaviour, targeting industry-specific keywords, and using a fraud prevention scheme. In this online field experiment, click fraud was not an issue identified by the researcher or the charity before the campaign launch, so these solutions were unfortunately outside of the scope of this project. However, they have provided some valuable lessons which could be implemented when designing future online interventions. For example, it is important to work with the organisation that is being promoted to make sure that there is a user monitoring strategy set out to understand user engagement and identify unusual and suspicious behaviour early on. This would enable the campaign to be paused in order to deal with any problematic issues, saving time and money and assuring the collection of more accurate data (Collins, 2018).

Using target-specific keywords enables the campaign to attract the right kind of users and ensure that most of the engagement comes from authentic web users. In this experiment, a deliberate decision was made to have the Tweets targeting anyone over the age of 18 years old with no other audience criteria being set. By using keywords such as ‘charity’ or ‘disability’, the promoted tweets would have been targeted specifically at users who have expressed an interest in either sport or charity previously in the past which could lead to biased sampling for this intervention. However, this decision may have backfired as without stipulating specific keywords in the campaign criteria, this left the ad campaigns more open to click fraud (Dovaston, 2019).

Although the campaign budget had been capped at £150, this was not a defensive strategy against click fraud but instead was due to the small research budget available. This turned out to be fortuitous as if there had not been a defined campaign budget, unlimited amounts of money could have been charged to the campaign for web clicks by click fraudsters (Auty, 2018). There are multiple services that are designed to specifically help businesses reduce the risk of falling victim to click fraud by allowing easy monitoring of their ad campaigns in real-time which allows them to quickly spot and act on click fraud. However, as with all advice on click fraud, this is a required preventative action. When designing this experiment, click fraud was an unknown risk. When designing future online interventions, it would be beneficial to consult a third party with experience in social media campaigns to mitigate potential risks and issues such as click fraud.

## Alternative Explanations for the lack of Donations

It is important to consider what other alternative reasons there may be to why the webpages failed to attract a single donation amongst web users as, although it appears likely that most visitors to the charity's web pages were click fraudsters, there is no way to determine what number (if any) were genuine web visits. Due to the unavailability of follow-up data, there is also no way to know, if there were genuine web visits, what number of web users engaged with the experimental pages and how they engaged.

### *The Importance of a Pilot Study*

A pilot study was not considered for this study as the experiment was purposefully designed to mimic the existing charity website and any changes were quite minimal in order to specifically test the effect of the presence of eyes on donation behaviour. After the end of the Twitter campaigns, a representative of the charity suggested that the text on the web pages may not have been persuasive enough and suggested editing it to something shorter and more emotive to generate the required support (personal communication, 11 October 2019). Due to a limited research budget, it was not possible to edit the text and re-launch the campaigns.

In hindsight, the issues experienced in this experiment highlight the importance of conducting a pilot study, especially as the experiment was the first known online field experiment and in addition was utilising a novel approach (i.e., the Twitter ad campaigns). An advantage of conducting a pilot study is that it may have given an advance warning on some of the practical problems in the research approach (e.g., the potential threat of Twitter bots). A pilot study would have allowed a shorter, less costly way of testing the effectiveness of the research approach and an opportunity to make improvements (van Teijlingen, Edwin & Hundley, 2010).

### *Social Norms*

There have been multiple watching eyes experiments (e.g., Ayal et al., 2019; Brudermann et al.; Kawamura & Kusumi, 2017; Oda et al., 2015) which have suggested that it is not simply the eyes themselves that effectively nudge people towards more prosocial behaviours but it is the eyes in conjunction with social norm messaging that is effective. It is thought that cooperation between humans relies on social norms or beliefs that define how individuals should behave in certain situations and social norm adherence is modulated by awareness of being watched (i.e., the watching-eyes effect) and the expectation that deviation will result in consequences such as rewards or punishments (Ikuse et al., 2018).

Humans are a fundamentally social species, and we treat our social environments as a source of information on how to act (i.e., social proof). For example, if we see other people running in the opposite direction to where we are headed, looking panicked, we are likely to copy their behaviour without stopping to think or assess the situation (a system one reaction). This type of normative social influence can cause people to conform to social norms without changing their private beliefs – e.g., donating more to charity when others around us do so. Much of our behaviour is determined by social norms and the expectations and actions of our peers.

Evidence suggests that interventions which manipulate the social setting of behaviour (such as perceived social norms) are often highly effective at inducing the desired behaviours (Park & Reiner, 2019). In a recent watching-eyes field experiment conducted by Ayal et al. (2019), they found that there was a decrease in fare evasion amongst passengers who were exposed to an experimental eye-cue with a social norm messaging (for example, “In this station, 90% of all individuals purchase and validate their ticket”, p.4). The researchers concluded that although the watching-eyes cues alone were not effective, exposing passengers to watching eye cues together with descriptive norm messaging could be an effective intervention. This could mean that, as the charity suggested, the messages in the campaigns were not persuasive enough and they could have been more persuasive by explicitly mentioning a social norm. However, this may not be the case with *online* studies. Kawamura and Kusumi (2017) conducted two experiments to investigate whether the watching-eyes effect changed depending on social norms: a lab-based experiment and an online replication. In the lab-based experiment, they found that watching eyes promoted donations only when a prosocial norm existed but, in the online experiment, they found that eyes did not promote generosity regardless of whether prosocial norm existed or not. These inconsistent results suggest that if there is a watching-eyes effect, it is significantly weakened by various other factors, especially in an online environment. This experiment by Kawamura and Kusumi (2017) was the only *online* watching-eyes experiment testing social norms found in the literature search for this thesis, which highlights the need for further exploration.

***Reputational Concerns***

In an online setting, there is limited evidence of how much an image can make us feel like we are being observed. If people do not feel like they are being observed, then their reputational concerns are unlikely to be triggered and they could lack motivation to act prosocially (Shinohara & Yamamoto, 2018) which could be one explanation for the absence of donations in this experiment. Although Pfattheicher and Keller (2015) found in their online study that subtle cues of being watched can induce a sense of being seen, subsequent online studies have suggested that the effect of monitoring cues may be weakened or non-existent in an online environment (Saunders et al., 2016; White, 2015) possibly due to the perception of constant online monitoring. If participants already feel like they are being monitored simply by being online then this may override any effect of monitoring cues (Saunders et al., 2016).

Conversely, another suggestion as to why 'eyes on a screen' may not be effective is that participants themselves may feel anonymous in an online setting (Raihani & Bshary, 2012). Perhaps artificial surveillance cues remind individuals that there are people in the area who can monitor their actions, and thus surveillance cues require the presence of at least some people in the area to affect behaviour. Studies have shown that the number of people in an area can moderate the watching-eyes effect (Bateson et al., 2013) so participants with total privacy, may be immune to artificial surveillance effects (Northover et al., 2016).

Mifune et al., (2010) found in their experiments that pictures of eyes make people act more prosocially only towards members of their own in-group which was replicated in an online setting by Baillon, Selim, and van Dolder (2013) and supported by Sisco and Weber (2019) who examined online charitable donations and found that the donors gave significantly more to recipients who had the same last name as them. So, if the Twitter web visitors in this experiment did not consider the recipients of the charity's work to be in the same in-group as them, they may not have felt compelled to act prosocially and donate any money. The likelihood of this was increased by not using target specific keywords when selecting the audience for the Twitter Ad campaigns. As more recent watching-eyes experiments are published (Dear et al., 2019; Northover et al., 2016; Northover et al., 2017), it is becoming clear that any effect of the watching-eyes is greatly nuanced by a range of other mediating factors such as gender (Bateson et al., 2006), in-group not out-group membership (Mifune et al., 2010), chronic self-awareness (Pfattheicher & Keller, 2015), crowd density (Bateson et al., 2013), cue exposure length (Sparks & Barclay, 2013), perceived valence (e.g., kind vs unkind eyes) of eyes (Pauwels et al., 2017) and explicit over implicit cues (Fehr & Schneider, 2010). There is a growing body of evidence that artificial monitoring cues do not influence human prosociality in a uniform way (Saunders et al., 2016). However, what is still intriguing is the great success of the watching-eyes effect in public policy interventions such as Keep Britain Tidy (2014) as it would stand to reason that any limitations of the watching-eyes effect in laboratory or online experiments would also exist in a naturalistic setting, although in the field, there may be more opportunity for other mediating factors to influence behavioural outcomes due to the lack of strict controls (Reichardt, 2019).

**Conclusion**

A strength of this study is that it is the first known online field experiment that has tested the watching-eyes effect. In addition, it utilised a novel approach (i.e., the Twitter ad campaigns) to test the impact of watching eyes on donation behaviour. Reflecting on this experiment, several potential design improvements could have been included. For instance, it would have been beneficial to have included either a pilot study in order to identify and deal with any issues such as Twitter bots. It would have also been beneficial to have included a third party in the design process to help identify any unknown risks or potential additional data that may be required later on. It was not anticipated that the experiment would fail to receive a single donation so it was not identified in advance that the website KPIs would have been useful additional data. It would have also been beneficial to have trialled the use of social norms in conjunction with the eyes images to attract more people to make donations. It remains uncertain whether the watching-eyes effect is simply too weak when portrayed in an online environment to evoke the sense of being watched, or perhaps the watching-eyes effect is too nuanced to influence human prosocial behaviour uniformly. The next part of this chapter will investigate why public policy interventions, in particular, the approach taken by Keep Britain Tidy (2014) reported such success when, in the lab or online, researchers are struggling to replicate the watching-eyes effect.

**Study Four B – Keeping Britain Tidy; A Closer Look**

Between December 2013 and March 2014, the charity Keep Britain Tidy ran their award-winning “We’re Watching You” campaign. In conjunction with 17 LPCs the charity developed and delivered a social experiment that tested the effectiveness of watching eyes posters on the prevention of dog fouling. Keep Britain Tidy (2014) reported that overall, the ‘watching eyes’ posters were highly effective in reducing dog fouling at both the target and potential displacement sites with an average 46% reduction in the number of dog fouls. Keep Britain Tidy (2014) also reported that the posters were effective in achieving reductions in dog fouling incidents at the target sites without simply displacing the problem to an area nearby, with 75% of target sites and 56% of displacement sites experiencing a decrease in dog fouling incidents following implementation of the posters. This seemingly highly successful intervention is in discord with growing academic literature which has found little or no support for the watching-eyes effect in recent years (Northover et al., 2016) and therefore warrants a closer inspection to assess its reliability.

Following the approach set out by Carbon and Hesslinger (2011), this study provided a critical reflection on the stimuli, procedure, and sample used in the Keep Britain Tidy (2014) social experiment. A request for a copy of the raw data was submitted and refused by Keep Britain Tidy. The charity advised that they were not in a position to share the data with any external bodies but did not offer further explanation. Each listed LPC was then approached individually, and a freedom of information request was submitted for a copy of their individual data. In total, five LPCs out of 17 provided their raw data, the other 12 advised that they no longer had the raw data. As Keep Britain Tidy (2014) did not report the statistical analysis methods used in their research, this study will provide a re-analysis of the obtained experiment data and compare it to the conclusions drawn by Keep Britain Tidy (2014) as a means of data verification (Ellis, Carette, Anseel, & Lievens, 2014).

This study also expanded on the original results reported in Keep Britain Tidy (2014) by examining whether any watching-eyes effect (if one exists) decreased over time. There has been some suggestion in the literature that the effect of watching eyes may decrease over time due to habituation (Sparks & Barclay, 2013). This was supported anecdotally by partnership councils in discussions when obtaining the raw data. One council (anonymised by request) had reported that they felt that the effect of the watching eyes they had observed was only short-term and that members of the public had become habituated to the eyes. Therefore, this study will aim to examine this possibility.

**Analysis of the Stimuli used**

For the “We’re Watching You” campaign, Keep Britain Tidy created four versions of A3-size watching eyes posters which were made with a luminescent film, this film used solar energy to charge up during the day in order to glow in the dark. Each version of the poster had an image of eyes, a Keep Britain Tidy logo and displayed the messages “Thoughtless dog owners, we’re watching you” and as a call to action “Bag that poo, any rubbish bin will do”. Three of the posters displayed additional supporting messages (see Figure 6.3):

1) ‘Enforcement’ – “Walk your dog away from a fine of £80”

2) ‘Positive reinforcement’ - “9 out of 10 dog owners clean up after their dog, are you the one who doesn’t?”

3) ‘Peer influence’ – “report those who don’t clean up after their dog to the council”

Keep Britain Tidy (2014) reported that overall, the watching eyes posters were highly effective in reducing dog fouling at both the target and displacement sites with the overall average reduction in incidents of dog fouling per site showing a 46% decrease. This is in line with what could be expected from the watching eyes literature as the eye stimuli used on the posters were male looking eyes which depicted an angry, threatening look. In a field

**Figure 6.3**

*Each version of poster used in the "We're Watching You" campaign*


experiment on contributions to an honesty box, Bateson et al's., (2006) results showed that contributions to the honesty box were greater when male eyes were present and Pauwels et al. (2017) found that it was unkind rather than kind eyes which promote prosocial behaviour. Pauwels et al. (2017) posit that rather than encouraging prosocial behaviour, unkind eyes boost cooperation due to a 'policing effect' by coercing participants to conform to a prosocial norm which was explicitly advised via the text on the posters.

In addition, the watching eyes posters were highly salient as they were luminescent (i.e., they glowed in the dark). In previous chapters of this thesis, the salience of the eyes was manipulated to see if that would increase any effect of watching eyes. The results of these manipulations did not find evidence that increasing the salience of the eye cues (i.e., how noticeable they are) increased prosocial behaviour in an online environment. It was posited that perhaps the eye stimuli somehow needs to be even more salient (Ernest-Jones et al., 2011) or that the effect of watching-eyes is so weak it does not translate to an online environment (Raihani & Bshary, 2012). It is possible that a large A3 glow in the dark poster of eyes, in the context of a naturalistic setting (as opposed to an online environment) would be much more salient than the eyes cues used in the earlier studies of this thesis. Therefore, they may have been more effective at capturing and maintaining participants' attention, allowing the watching-eyes effect to emerge.

However, all four posters used in this social experiment included an image of eyes; there was no control poster (i.e., one without an image of eyes) that could be used as a comparison. It could be argued that the display text, the logos, or the two in combination caused the reduction in dog fouls and not the eyes themselves. In fact, it has been found that just the mere display of (any) signs alone can reduce antisocial behaviour (Dear et al., 2019). It would be interesting to see if the display of text and/or logos on a control poster (i.e., with no eye images) would have had the same impact. In this social experiment, it is questionable to what extent the reduction in dog fouls can be attributed to the presence of eyes alone.

Of the four posters used, Keep Britain Tidy (2014) reported that the 'positive reinforcement' poster was the most effective in decreasing incidents of dog fouling across target and displacement sites (49% reduction overall). The 'positive reinforcement' poster contained a social norm message "9 out of 10 dog owners clean up after their dog, are you the one who doesn't?". In the previous study (study four A) of this chapter, it was suggested that the watching eyes in the online field experiment may not have been effective in encouraging donations to charity due to the lack of any social norm information accompanying the watching eyes images. In a recent field experiment focusing on the watching-eyes effect and fare evasion at French railway stations, Ayal et al., (2019) found that watching eye cues alone were not effective in reducing fare evasion but in line with the findings of other research (e.g., Brudermann et al.; Kawamura & Kusumi, 2017; Oda et al., 2015), the effect was strengthened when delivered along with descriptive social norm messages. However, it should be noted that although Keep Britain Tidy reported a reduction in dog fouling across all four versions of the poster types (ranging from 43% to 49%), the differences in results across the four posters did not reach statistical significance.

Keep Britain Tidy (2014) also reported that there was strong evidence that tailoring specific posters to specific land use areas would be more effective (e.g., the ‘eyes only’ poster appeared to be more effective in alleyways compared to social housing areas). However, this result should possibly be interpreted with some caution as the impact on dog fouling by poster message at each land type used was reported significant at the 90% confidence level and no effect sizes were reported, whereas the other significant results in the report were reported at the 95% significance level. As discussed in the methodology chapter although the p-value does not represent a binary cut-off where results become immediately true or false on either side of the divide, statistical significance is generally accepted at the 95% confidence (0.05 alpha) level (particularly within the social sciences). Researchers can decrease the significance level from the generally accepted .05 alpha level to the .10 level when there is a small sample that has low power to detect an effect. Increasing the alpha to .10 makes the statistical test more sensitive to detecting differences but it also increase the chance of a false positive and therefore being wrong about the estimated coefficient being different form zero (Hair, Black, Bain, & Anderson, 2014).

**Analysis of Procedure**

In total, there were 17 LPCs from across England who teamed up with Keep Britain Tidy to conduct the “We’re watching you” social experiment. Each LPC identified eight dog fouling hotspots in their area (i.e., target sites) with the aim to display one version of the watching eyes posters at each site (with each poster being displayed at two sites). As it was thought that incidents of dog fouling may simply be displaced to another nearby site following the implementation of the intervention, the LPCs also identified a potential displacement site (either adjacent to or up to a maximum of 100m away from the original target site) for each of the target sites.

In a quasi-experimental, pretest-posttest design, each LPC compared the average rates of dog fouling at each monitoring site (target and displacement) before and after the installation of the posters, taken over a period of three weeks on either side. In week one of the experiment, each LPC conducted a baseline cleanse at each monitoring site. In each of the following three weeks, they went back to the monitoring site to record the number of dog fouls up to a maximum of three times per week (depending on their normal schedule). In week four, each LPC conducted another cleanse at each monitoring site, put up the posters at the target sites and then repeated the dog foul monitoring for the following three weeks (see Figure 6.4).

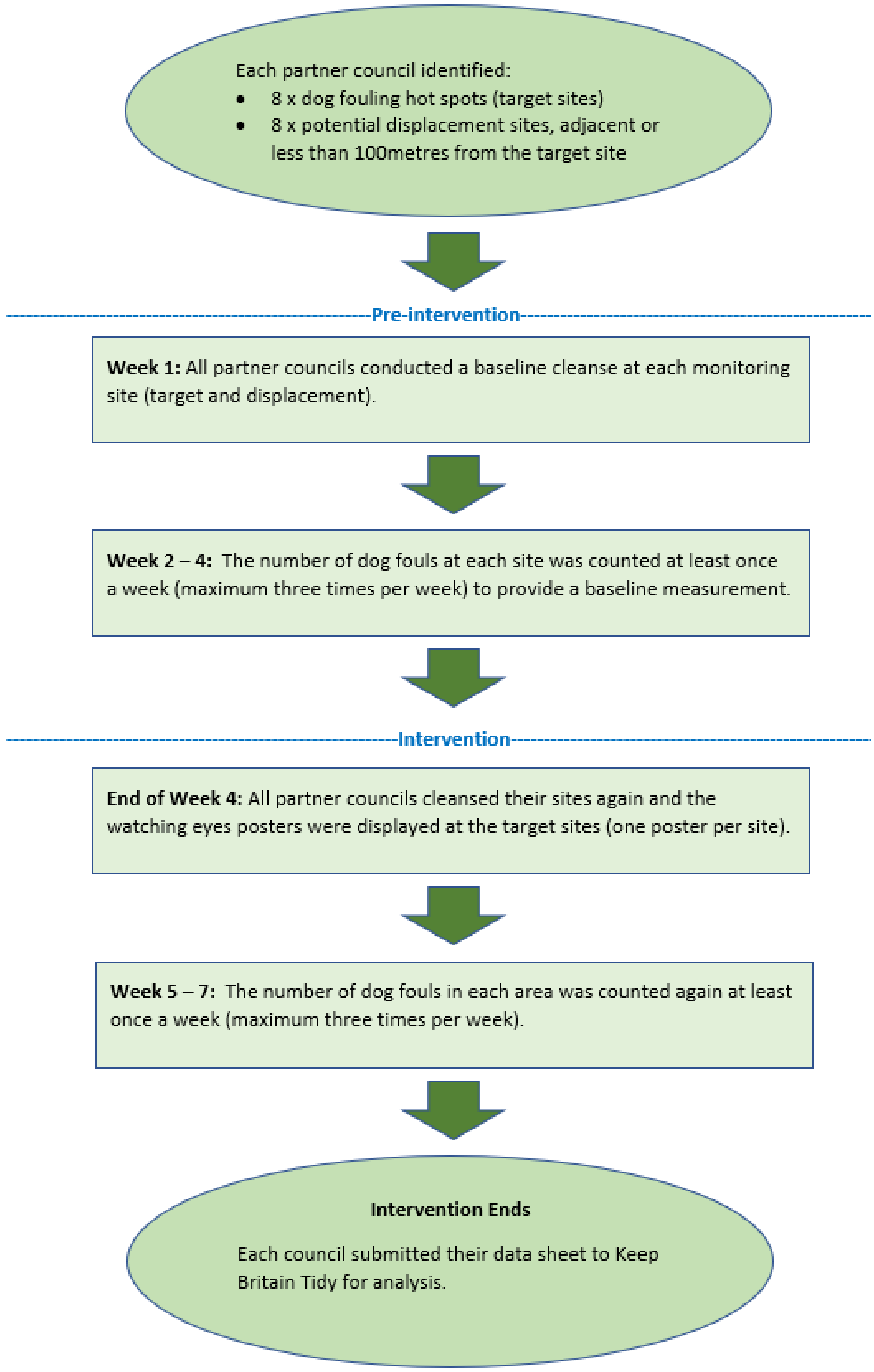
As discussed earlier, Keep Britain Tidy's (2014) results indicated that overall, the watching eyes posters approach were highly effective in reducing dog fouling at both the target and potential displacement sites. Each site saw an average 46% reduction in the number of dog fouls. Keep Britain Tidy (2014) also reported that the posters were effective in achieving reductions in dog fouling incidents at the target sites without simply displacing the problem to an area nearby, with 75% of target sites and 56% of displacement sites experiencing a decrease in dog fouling incidents following implementation of the posters.

### ***Issues with Measurement***

Rigorous evaluations are at the cornerstone of any public policy intervention and Randomized Controlled Trials (RCTs) have long been considered the gold standard design for evaluating the effectiveness of an intervention (Hudson, Fielding, & Ramsay, 2019). Randomisation of participants to either a control or an experimental condition reduces bias and provides a rigorous tool to examine cause-effect relationships (Hariton & Locascio, 2018).

**Figure 6.4**

*Keep Britain Tidy's data collection method*



However, where RCTs cannot be used (e.g., due to time, cost or ethical constraints), a quasi-experimental pretest-posttest design (as used in this social experiment) is a widely used alternative in behavioural research (Dimitrov & Rumrill, 2003). This type of design is easy to implement, and the results are easy for even the layperson to understand (e.g., especially if the data is visualised via graphs) but this type of design can be susceptible to threats to internal validity (i.e., the extent to which a study establishes a trustworthy cause-and-effect relationship between an intervention and an outcome) and external validity (i.e., how well the outcome of the study can be expected to apply to other settings) (Reichardt, 2019).

**Threats to Internal Validity.** Threats to the internal validity of this intervention could include historical effects and possible differences in weather which may have biased results (Reichardt, 2019). For example, installation of more dog waste bins, increased vigilance by the council for not picking up dog fouls, or information on the “we’re watching you” campaign discussed in the press or on social media prior to the intervention. If any of these incidents had occurred, they may have influenced whether people may have picked up dog fouls in those areas or not meaning that any reduction in dog fouls cannot be attributed solely to the poster campaign. In addition, the weather experienced during the intervention could have biased results. For example, if there was a sunny day in the pre-test period, this could have resulted in a higher number of dog walkers in the area and thus an elevated number of dog fouls. If the same site then experienced a week with bad weather in the post-test period, this could have led to a decreased number of dog walkers and thus a reduction in dog fouls which would have had nothing to do with the presence of the watching eyes posters.

**Threats to External Validity.** Threats to the external validity of an intervention such as this could include seasonality, and other time-varying confounds (Reichardt, 2019). Data collection for this intervention was conducted over the end of winter and the beginning of spring (December to mid-March 2014). Although the number of dog walkers was not counted in the experiment, it is possible that there were generally fewer dog walkers (and therefore fewer dog fouls) due to typically colder and wetter conditions at that time of the year compared to later seasons which makes it difficult to generalise the results to the rest of year (Bernal, Cummins, & Gasparrini, 2017). There could have also been other time-varying confounders such as the number of days between counts. Keep Britain Tidy (2014) did not report what day of the week the dog fouls were counted on (in fact there was no option to do this on the data collection forms), so there may not have been an equal number of days between counts. This could account for some of the variation seen week to week in the number of dog fouls.

**Differences between sites.** Keep Britain Tidy (2014) noted that not every LPC in their social experiment followed the same methodology and key differences in the posters displayed across sites and the cleansing and dog foul monitoring procedures were identified across some locations. Each LPC was asked to display only one version of the poster at each site, with each version of the poster being displayed at two different target sites in their area. However, multiple LPCs chose to test some versions of the poster at more than two sites and some at less (for example, one partner tested poster one at no sites, poster two at one site, poster three at two sites and poster four at five sites). This limits the ability to differentiate between the effects of each different type of poster on the number of dog fouls.

During the trial, dog fouls at the test sites were periodically cleansed and the number of dog fouls counted. No standard protocol was used for this, instead LPCs used their usual dog foul cleaning routines. This meant that there were differences in the frequencies of dog foul cleansing between locations. Some LPCs cleansed their sites twice, once at the beginning of the pre-intervention period and once at the beginning of the watching eyes intervention and other LPCs cleansed each time they visited their sites to count the dog fouls. Those sites which had not been cleansed after each site visit were required to be able to differentiate between the dog fouls that were already there and any new dog fouling incidents. In addition, some LPCs counted dog fouls at each site up to three times per week where others only counted once per week. This relied on the dog fouls not disappearing between counts (e.g., by bad weather washing away the fog fouls, grass cutting, being walked through etc.). Due to these differences in methodologies, and frequencies of dog foul cleansing and monitoring, the accuracy of the data collected could be called into question.

***Potential mitigations***

Keep Britain Tidy (2014) identified some of these issues and either attempted to mitigate them or made suggestions as to how the issue would be overcome. For example, Keep Britain Tidy (2014) tried to account for possible anomalies in the data caused by the different frequencies in the number of dog foul counts by using the average number of dog fouls across the three-week monitoring period. They also sought to minimise any impacts of further variability between sites by including a large number of test sites across a wide geographic range (i.e., 272 sites across 17 locations).

However, Keep Britain Tidy (2014) acknowledge that several improvements could have been made to the study to make it more robust. Namely, by including a control site alongside the target and displacement sites to allow for other potential confounding factors (e.g., weather differences between pre and post-intervention which may influence the number of dog walkers) to be controlled for. Keep Britain Tidy (2014) also suggest that they could have increased the length of their monitoring time from three to six weeks to gain a better indication of the impact of the posters and to minimise the influence of potential confounding such as the weather. This would be particularly pertinent as pretest-posttest designs with few data points may be underpowered (Bernal et al., 2017) and Keep Britain Tidy did not report on any power analysis conducted.

Keep Britain Tidy (2014) also suggested that future iterations of the experiment should add a section to the monitoring form to record approximate dawn and dusk times as rates of dog fouling increased with increased hours of darkness (i.e., when the clocks return to Greenwich Mean Time after Summer). However, it may have also been useful to add a section on the weather for each week of the count. The forms did include a section on the weather at the time of the count, but this would not have revealed anything about the weather between dog foul counts.

The results of this intervention reported by Keep Britain Tidy are in line with the claim that positive results for the watching-eyes effect are predominantly found in field studies rather than laboratory experiments (Kelsey et al., 2018; Manesi & Pollet, 2017). However, the issues raised above suggests that some caution should be taken in generalising the effects of this intervention across other potential sites.

**Analysis of the Sample**

Keep Britain Tidy (2014) could not report how many people were ‘exposed’ to the experimental conditions, but still chose not to pick up the dog fouls, they could only report the frequency of antisocial behaviour across locations. This means there is no way of knowing whether the reduction in dog fouls seen across the sites was a result of one person being deterred by the posters or for example, 10 people being deterred. A common drawback encountered in watching eyes field studies (e.g., Nettle et al., 2012) is that interventions cannot be evaluated on an individual level because of the demands of a natural setting (Carbon & Hesslinger, 2011); it is more difficult to implement optimal control conditions and monitor the experiment fully (e.g., it would not be possible to monitor the total number of people who visit the site across the six week monitor period). This matters as research shows people cannot be ‘nudged’ into doing something that is not already in their behavioural repertoire so individual characteristics matter (Latham, 2020). If a person is not prosocial by nature and genuinely does not care about the consequences of dog fouling, then an image of a pair of eyes may be unlikely to change their behaviour and the design of this intervention means that individual characteristics cannot be examined or accounted for.

Keep Britain Tidy (2014), however, were specifically interested in the reduction of dog fouls across sites not the number of people that were deterred from leaving dog fouls (i.e., so it wouldn't matter if one or 10 people were influenced by the watching eyes posters as long as the overall number of dog fouls decreased). The sample in Keep Britain Tidy's (2014) social experiment consisted of the number of sites across the locations. A large number of sites across a wide geographic area (i.e., 272 sites across 17 locations) was used to minimise any impacts of variability between sites. However, there may have been a number of differences in the test sites which could have possibly biased the results and effect how generalisable the results would be to a wider population. For example, there may have been differences in the type of site material (e.g., grass vs pavement), the size of the site, proximity to a dog waste bin and the immediate population density, both in the number of dog walkers that could be potentially leaving the dog fouls and in the number of other people in the vicinity.

It has been suggested that the watching-eyes effect may be strongest when there are relatively few real people in the vicinity (Bateson et al., 2013) as eye images are likely to evoke the feeling of being watched much less effectively than real people do (Ernest-Jones et al., 2011). Eye images may remind individuals that there are people in the area who can monitor their actions, and thus surveillance cues require the presence of at least some people in the area to affect behaviour. Studies have shown that the number of people in an area can moderate the watching-eyes effect (e.g., Bateson et al., 2013) so participants with total privacy, may be immune to artificial surveillance effects (Northover et al., 2016). This is somewhat supported by Keep Britain Tidy (2014) reporting that the posters were least effective when used on public footpaths or social housing sites when compared to sites such as rural roads.

The external validity of this intervention is of particular importance as following this initial iteration of the social experiment, Keep Britain Tidy now offer a 'watching eyes' package that local councils can purchase to help tackle issues with dog fouling. Keep Britain Tidy report that "hundreds of partners are already successfully using the campaign and making a real difference in their area" (Keep Britain Tidy, n.d.). However, it is not clear from the Keep Britain Tidy website whether they have implemented any of their suggested changes following this experiment or if data is still being collected to further evaluate the effectiveness of the campaigns.

Although the overall intervention was highly successful with 75% of target sites and 56% of displacement sites experiencing a decrease in dog fouling incidents following the implementation of the posters, not all LPCs or sites within LPCs saw a reduction in the number of dog fouls. Keep Britain Tidy (2014) report that 8% of target sites and 18% of displacement sites showed no change, and 17% of target sites and 27% of displacement sites experienced an increase. The Keep Britain Tidy (2014) report did not discuss why the watching eyes campaign may not have been successful at these sites which may be an important consideration for each LPC considering signing -up to the campaign.

**Analysis of the Statistical Analyses**

As the Keep Britain Tidy (2014) report did not include specific details on the statistical approach they used, it cannot be ascertained to what degree the analysis in this study differs from the original report. The following analysis includes considerations of the most appropriate statistical methodology and full reporting (including power analysis and effect sizes) which the original report did not contain.

In total, five LPCs provided their raw data via Freedom of Information (FOI) requests. This was re-examined below. Due to restrictions set by the FOI requests, it is not possible to name the councils in this write-up. Instead, they will be referred to by regions, the highest tier of sub-national division in England (e.g., South-East) (ONS, 2016). As previously discussed, each council selected eight areas, each with a target and displacement site (e.g., 16 sites in total) to monitor the number of dog fouls for three weeks pre-intervention and for three weeks during the intervention. Each council had their own methodology for counting dog fouls (e.g., some cleansed the area after each count, and some did not) so to account for any anomalies in dog fouls counting (e.g., accidentally counting the same dog foul twice), the average, rather than total, counts of dog fouling taken over each three-week monitoring period is used here in this re-analysis.

***Aims***

1) To test whether there was a watching-eyes effect by comparing the number of dog fouls recorded at the target sites pre-intervention versus during the intervention.

2) To test whether there was a displacement effect (i.e., whether people simply walked past the eye images and deposited the dog fouls elsewhere) by comparing the number of dog fouls recorded at the displacement sites pre-intervention versus during the intervention.

3) To test whether any effect of the watching eyes decreased over time, by comparing the number of dog fouls across time at the target and displacement sites.

***Data analysis***

Due to the within-subjects factor of time (the number of dog fouls pre-intervention vs. during intervention) and the between-subjects factor of intervention site (target vs. displacement), a 2 x 2 mixed ANOVA was first attempted to analyse this data. However, the distribution of dog fouling counts significantly deviated from normality as determined by a Shapiro-Wilk test ($p$ <0.05). Inspection of a histogram revealed that the data were positively skewed. Square root, logarithmic, and inverse transformations were applied to the data (Laerd

Statistics, 2015) but they failed to transform the data to approximate a normal distribution. In addition, multiple outliers were detected as assessed by inspection of a boxplot, but the removal of the outliers resulted in masking (the removal of outliers generated new outliers). Due to these violations of assumptions, non-parametric tests were used instead. The following analysis was conducted:

**Aim 1 and 2.** A series of non-parametric, Wilcoxon Signed-Rank tests (which assessed the distribution of median scores) were conducted to test whether there were any significant differences in the number of dog fouls pre-intervention versus during intervention, and whether there were any significant differences in the number of dog fouls between the target versus displacement sites.

**Aim 3.** A series of Friedman tests (which assessed the distribution of median scores) were conducted on the target and displacement sites (separately) during the intervention, to test whether any effect of watching eyes decreased over time which could be an indication of habituation (e.g., Sparks & Barclay, 2013). As a comparison, further exploratory analysis was conducted on the number of dog fouls at the target and displacement sites over time pre-intervention, also using a series of Friedman tests. Based on the methodology in Tomczak and Tomczak (2014), effect sizes were calculated using the Kendall's *W* coefficient which assumes the value from 0 (indicating no relationship) to 1 (indicating a perfect relationship).

## Results

### Aim 1

To test whether there was a watching-eyes effect by comparing the number of dog fouls recorded at the target sites pre-intervention versus during the intervention.

Overall, there were significantly fewer dog fouls recorded during the poster intervention when compared to pre-intervention across the target sites for all five locations combined and for four of the five locations. However, for Location D (West Midlands), there was a non-significant increase in the number of dog fouls recorded during the poster intervention when compared to pre-intervention across the target sites (see Table 6.1 and Figure 6.5).

**Aim 2**

To test whether there was a displacement effect (i.e., whether people simply walked past the eye images and deposited the dog fouls elsewhere) by comparing the number of dog fouls recorded at the displacement sites pre-intervention versus during the intervention.

Prior to the watching eyes intervention, the number of dog fouls for the target sites was significantly higher (*Mdn* = 2) than for the displacement sites (*Mdn* = 0.33) pre-intervention, z = -4.530, $p < .01$, $r = -0.51$. During the intervention, the median number of dog

**Table 6.1**

*The median number of dog fouls at the target sites pre-intervention compared to during the intervention*

| Location | *Mdn* (IQR) Pre-intervention | *Mdn* (IQR) During intervention | % change | *N* | *z* score | *p*-value | *r* | 90% CI (*r*) |
|---|---|---|---|---|---|---|---|---|
| All Locations | 2 (1.27 - 5.00) | 1.59 (0.54 - 1.59) | -20.5 | 40 | -3.463 | < .01 | -0.39 | -0.59, -0.14 |
| Location A (Yorkshire and the Humber) | 1.84 (1.16 - 3.50) | 1.38 (1.00 - 2.38) | -25 | 8 | -2.284 | 0.028 | -0.57 | -0.88, 0.09 |
| Location B (East of England) | 1.88 (1.25 - 4.75) | 0.38 (0.00 - 1.75) | -79.79 | 8 | -2.384 | 0.017 | -0.6 | -0.89, 0.04 |
| Location C (East Midlands) | 11.33 (4.08 - 19.67) | 4.33 (1.25 - 6.12) | -61.78 | 8 | -2.521 | 0.012 | -0.63 | -0.90, -0.01 |
| Location D (West Midlands) | 1.5 (0.67 - 2.59) | 3 (0.92 - 5.83) | 100 | 8 | 1.439 | 0.15 | 0.36 | -0.34, 0.80 |
| Location E (North-West) | 1.83 (1.33 - 3.58) | 1.33 (0.08 - 1.92) | -27.32 | 8 | -2.207 | 0.027 | -0.55 | -0.87, 0.12 |

*Note.* IQR: Interquartile range

**Figure 6.5**

*Median number of dog fouls at the target sites pre and during the intervention by location*


*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show the outliers.

** Differences between pre and during intervention median number of dog fouls are significant at $p < 0.01$.

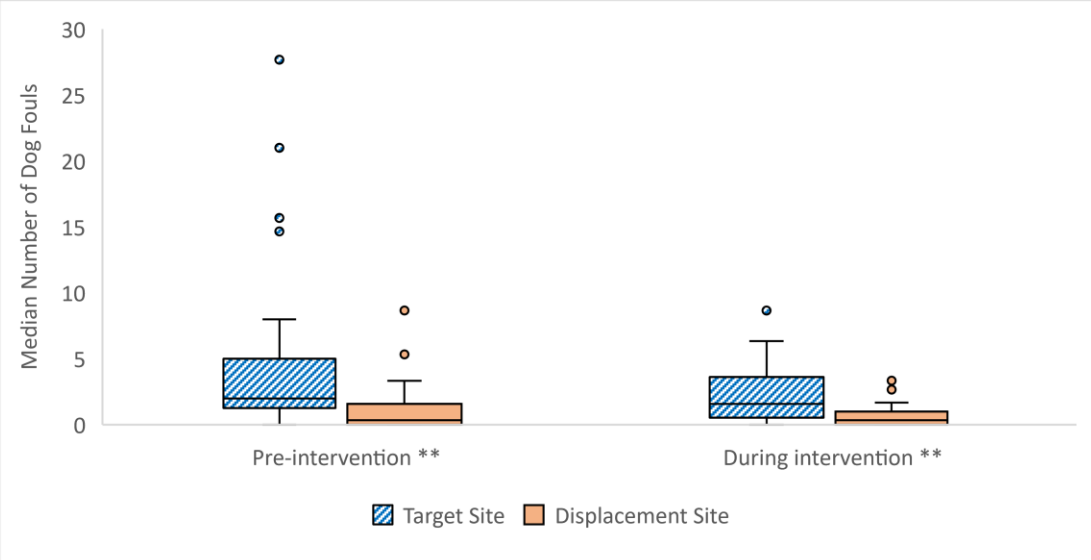
* Differences are significant at $p < 0.05$.

fouls for the target sites (*Mdn* = 1.59) was also significantly higher than the median number of dog fouls for the displacement sites (*Mdn* = 0.33) during the intervention, $z = -3.708$, $p < .01$, $r = -0.41$ (see Figure 6.6).

Overall, there were no changes in the median number of dog fouls at the displacement sites during the poster intervention when compared to pre-intervention across the displacement sites for all five locations combined. When looking at the locations individually, two locations saw a non-significant increase in the median number of dog fouls. Two locations saw a non-significant decrease and one location did see a significant decrease in the median

**Figure 6.6**

*Median number of dog fouls at the target and displacement sites overall*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show the outliers. ** Differences between pre and during intervention median number of dog fouls are significant at $p < 0.01$

number of dog fouls during the poster intervention when compared to pre-intervention (see Table 6.2 and Figure 6.7).

Overall, there were no changes in the median number of dog fouls at the displacement sites during the poster intervention when compared to pre-intervention across the displacement sites for all five locations combined. When looking at the locations individually, two locations saw a non-significant increase in the median number of dog fouls. Two locations saw a non-significant decrease and one location did see a significant decrease in the median number of dog fouls during the poster intervention when compared to pre-intervention (see Table 6.2 and Figure 6.7).

**Table 6.2**

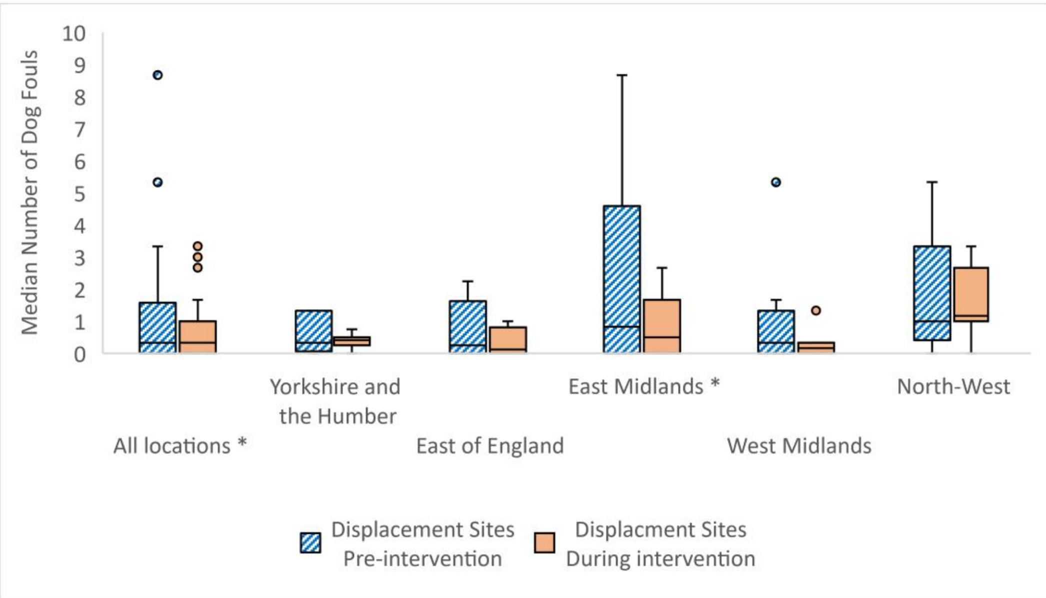
*The median number of dog fouls at the displacement sites pre-intervention compared to during intervention*

| | *Mdn* (IQR) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Location | Pre-intervention | During intervention | % change | *N* | *z* score | p-value | r | 90% CI (*r*) |
| All Locations | 0.33 (0.00 - 1.59) | 0.33 (0.00 - 1.00) | 0 | 40 | -2.337 | 0.019 | -0.26 | -0.49, 0.00 |
| Location A (Yorkshire and the Humber) | 0.33 (0.62 - 1.33) | 0.42 (0.25 - 0.50) | 27.27 | 8 | -0.912 | 0.362 | -0.23 | -0.75, 0.46 |
| Location B (East of England) | 0.25 (0.00 - 1.63) | 0.13 (0.00 - 0.81) | -48 | 8 | -1.095 | 0.273 | -0.27 | -0.77, 0.43 |
| Location C (East Midlands) | 0.84 (0.00 - 4.58) | 0.84 (0.00 - 1.67) | -40.48 | 8 | -2.023 | 0.043 | -0.51 | -0.86, 0.17 |
| Location D (West Midlands) | 0.33 (0.00 - 1.34) | 0.17 (0.00 - 0.33) | -48.48 | 8 | -1.289 | 0.197 | -0.32 | -0.79, 0.38 |
| Location E (North-West) | 1 (0.42 - 3.33) | 1.17 (1.00 - 2.67) | 17 | 8 | -0.493 | 0.622 | -0.12 | -0.69, 0.55 |

*Note.* IQR: Interquartile range

**Figure 6.7**

*Median number of dog fouls at the displacement sites by location*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show the outliers.

* Differences between pre and during intervention median number of dog fouls are significant at $p < 0.05$.

**Aim 3**

To test whether any effect of the eye images effect decreased over time, by comparing the number of dog fouls across time at the target and displacement sites.

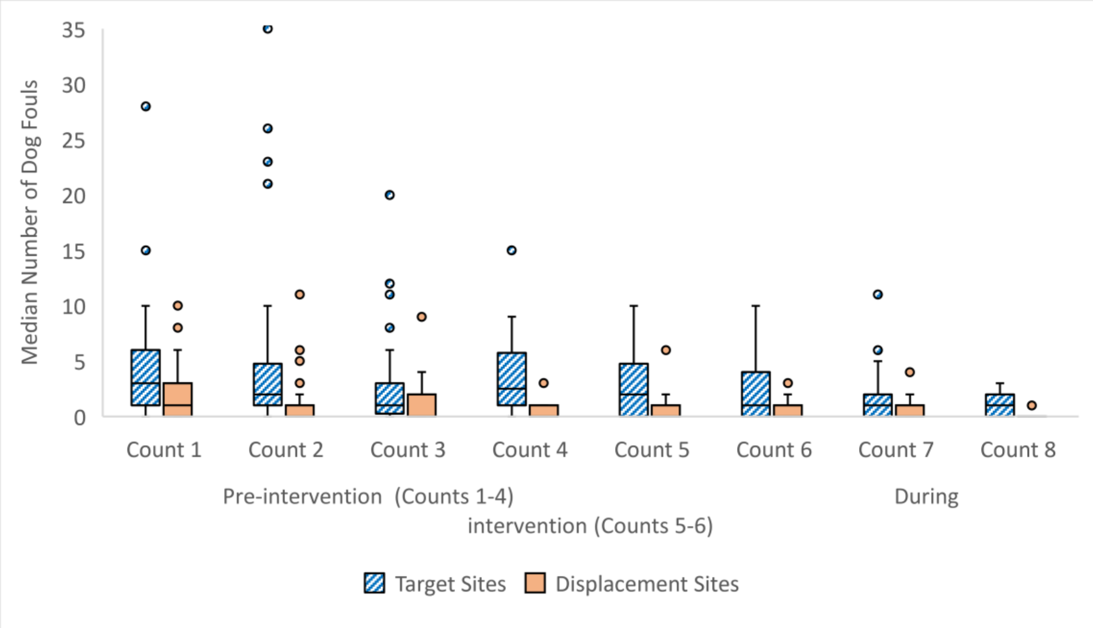
Analysis was conducted on the number of dog fouls recorded over time during the watching eyes intervention. Across all of the target sites for all five locations (*N* = 40), there was a marginal significant difference in the median number of dog fouls over time; Count 1 (*Mdn* = 2, IQR = 0.00 – 4.75), Count 2 (*Mdn* = 1, IQR = 0.00 – 4.00), Count 3 (*Mdn* = 1, IQR = 0.00 – 2.00) and Count 4 (*Mdn* = 1, IQR = 0.00 – 1.75), $\chi^2(3) = 7.258$, *p* = .064, *W* = 0.06, 90% CI [-0.21, 0.32]. There was significant difference over time across the displacement sites for all five locations (*N* = 40); Count 1 (*Mdn* = 0, IQR = 0.00 – 1.00), Count 2 (*Mdn* = 0, IQR = 0.00 – 1.00), Count 3 (*Mdn* = 0, IQR = 0.00 – 1.00) and Count 4 (*Mdn* = 0, IQR = 0.00 – 0.00), $\chi^2(3) =$ 9.930, *p* = 0.19, *W* = 0.08, 90% CI [-0.19, 0.34). However, there were no statistically significant post hoc pairwise comparisons, most likely due to the sample being underpowered (xlstat.com) (see Figure 6.8).

**Exploratory Analysis**

Further exploratory analysis was conducted on the number of dog fouls over time pre-intervention as a comparison and there were no significant differences in the number of dog fouls over time in either the target or displacement sites. Across all of the target sites for all five locations (*N* = 40), there were no significant differences in the median number of dog fouls over time: Count 1 (*Mdn* = 3, IQR = 1.00 – 6.00), Count 2 (*Mdn* = 2, IQR = 1.00 – 4.75), Count 3 (*Mdn* = 1, IQR = 1.00 – 3.00) and Count 4 (*Mdn* = 2.5, IQR = 1.00 – 5.75) , $\chi^2(3) = 5.459$, *p* = .141, *W* = 0.15, 90% CI [-0.12, 0.40]. Across all of the displacement sites (*N* = 40), there were also no significant differences in the median number of dog fouls decreased from Count 1 (*Mdn* = 1, IQR = 0.00 – 3.00) to Count 2 (*Mdn* = 0, IQR = 0.00 – 1.00), Count 3 ( *Mdn* = 0, IQR = 0.00 –

**Figure 6.8**

*The median number of dog fouls over time for target and displacement sites*




*Note.* Whiskers demonstrate the minimum and maximum value, the box spans the 25th and 75th quartiles, the bold horizontal line indicates the median, and circles show the outliers.

2.00) and Count 4 (*Mdn* = 0, IQR = 0.00 – 1.00), $\chi^2(3) = 3.393$, *p* = .349, *W* = 0.09, 90% CI [-0.18, 0.35] (see Figure 6.8).

***Power analysis***

A post hoc power analysis on the individual locations (*n* = 16) achieved a power of 0.83 for a large effect size (0.8), 0.45 for a medium effect size (0.5) and 0.11 for a small effect size (0.2) for a Wilcoxon signed-rank two-tailed test. A post hoc power analysis on all locations combined revealed that this sample (*n* = 80) achieved a power of 0.99 for a large effect size (0.8), 0.99 for a medium effect size (0.5) and 0.41 for a small effect size (0.1) for a Wilcoxon signed-rank test. The Friedman tests (*n* = 40) achieved 1.00 for a large effect size (0.5), 0.99 for a medium effect size (0.3) and 0.28 for a small effect size (0.1).

## Discussion

In a field experiment conducted by the charity Keep Britain Tidy (2014), 17 local councils partnered up with the charity to test whether the display of a pair of eyes would be effective in reducing the number of dog fouling incidents at eight identified dog fouling hotspots. Following the approach set out by (Carbon & Hesslinger, 2011), this study has re-evaluated the social experiment by Keep Britain Tidy (2014) from five out of 17 councils. It has provided critical reflections on the stimuli, procedure, and sample used in Keep Britain Tidy's social experiment as well as providing a re-analysis of the statistical analyses used in the report and supports the watching eyes to be effective in reducing dog fouling.

### Overview of the Critical reflection of the Stimuli Used

Keep Britain Tidy (2014) reported that the use of watching eyes posters was highly effective in reducing the number of dog fouls across all sites. The posters used in this social experiment depicted angry, male eyes (see Figure 6.3). Although the existing watching eyes literature is not clear on whether the gender or the emotion of the eye cues is an important nuance of any watching-eyes effect (Panagopoulos, 2014a; Pauwels et al., 2017), it has been shown that it is unkind rather than kind eyes that promote prosocial behaviour. Possibly because the eyes serve as a reminder that a person's behaviour is being watched and will be judged accordingly (Pauwels et al., 2017). However, as there was no control poster (i.e., one without an image of eyes) used, the extent to which the reduction in dog fouls can be attributed to the presence of eyes alone was questioned.

Keep Britain Tidy (2014) suggested that the poster which included a social norm was the most effective. This would have been in line with recent field studies (e.g., Ayal et al., 2019) finding that the watching-eyes effect was strengthened when delivered along with descriptive social norm messages but the differences in the number of dog fouls between posters did not reach statistical significance. In addition, although Keep Britain Tidy (2014) reported that there was ‘strong evidence’ that tailoring specific posters to specific land use areas would be more effective (e.g., the ‘eyes only’ poster appeared to be more effective in alleyways compared to areas with social housing), this was only significant at the 90% confidence level. As discussed previously, this result should possibly be interpreted with some caution as this was the only statistical test reported at the 90% significance level and although increasing the alpha to 0.10 (90%) makes it easier to detect differences, it also allows for a larger chance at being wrong (Hair et al., 2014).

**Overview of the Critical reflections of the Procedure**

In the analysis of the procedure, several considerations were highlighted about the pretest-posttest design approach. Randomized controlled trials (RCTs) have long been considered the gold standard design for evaluating the effectiveness of an intervention but where RCTs cannot be used, a quasi-experimental pretest-posttest design (as used in this social experiment) is a widely used alternative in behavioural research (Dimitrov & Rumrill, 2003). However, this type of approach can be susceptible to multiple threats to internal validity including historical effects, seasonality, and other time-varying confounders such as the day of the week the counts were conducted on, variability in daylight hours, and most likely in the weather experienced as well (Reichardt, 2019).

It was noted that not every LPC followed the same poster display methodology which limits the ability to differentiate between the effects of each different type of poster on the number of dog fouls. In addition, there were differences in the frequencies of dog foul cleansing and monitoring between locations which could cast some doubt on the accuracy of the data collected. Those sites which had not been cleansed after each site visit were required to be able to differentiate between the dog fouls that were already there and any new dog fouling incidents. In addition, there was also a reliance on dog fouls not disappearing between counts (e.g., by bad weather washing away the fog fouls, grass cutting, being walked through etc.).

Keep Britain Tidy (2014) did identify and attempt to mitigate some of the issues discussed (e.g., by using average rather than the total number of dog fouls to account for the difference in count frequencies) but Keep Britain Tidy themselves acknowledge that several potential improvements could be made to the intervention. Namely, by using a control site alongside the target and displacement sites, increasing the length of their monitoring time and collecting additional data such as the number of daylight hours during each monitoring week. Keep Britain Tidy's (2014) reported results suggest that the watching eyes campaign has been highly effective but the potential threats to internal validity should be considered as they also pose a threat as to how well the outcome of the study can be expected to apply to other settings (Tussyadiah & Miller, 2019).

**Overview of the Critical reflections of the Sample**

In the analysis of the sample, it was highlighted that the Keep Britain Tidy (2014) experiment could only report the frequency of antisocial behaviour across locations, not how frequently the watching-eyes was effective in causing people to decide against acting antisocially (e.g., there is no way of knowing whether the reduction in dog fouls seen across the sites was a result of one person being deterred by the posters or for example, 10 people being deterred). Instead, the sample consisted of a large number of sites across a wide geographic area which would have helped to minimise any impact of variability between sites.

There could have been a number of differences in those locations which could have potentially biased the results and effect how generalisable the results would be to a wider population. This is of particular importance as based on these results, Keep Britain Tidy is teaming up with more LPCs to help tackle issues with dog fouling in their areas. As the results from this intervention indicate that not all LPCs or sites saw a reduction in the number of dog fouls, the potential threats to internal and external validity could be an important consideration for councils considering signing up to this campaign.

**Overview of the Re-analysis**

The re-analysis of the LPC data corroborated the results reported by Keep Britain Tidy (2014). There were significantly fewer dog fouls recorded during the poster intervention when compared to pre-intervention across the target sites for all five locations combined and for four of the five locations. This suggests that as per Keep Britain Tidy's (2014) results, the number of dog fouls in the dog fouling hot spots significantly reduced in the presence of the watching-eyes posters and this effect was seen in the majority of the sites.

The results show that there were a higher median number of dog fouls across target sites when compared to displacement sites both prior to and during the watching eyes intervention. As the target sites were selected as dog fouling hotspots, this indicates that they were well chosen. The re-analysis also corroborated Keep Britain Tidy's (2014) assertions that people did not simply walk past the watching eyes poster and deposit their dog fouls nearby. There were no significant changes in the median number of dog fouls at the displacement sites both prior to and during the watching eyes intervention. If there had been a displacement effect it would be expected that there would be a higher number of dog fouls in the displacement sites, rather than the target sites, during the watching eyes intervention. However, the possibility that the dog walkers may still have deposited the dog fouls at different displacement sites other than the ones identified in this experiment cannot be ruled out.

To expand on the original analysis by Keep Britain Tidy (2014), further analysis was conducted on the number of dog fouls over time, both prior to and during the watching eyes intervention. The results showed a marginal significant difference between counts over time for the target sites and a significant difference between counts over time for the displacement sites. However, post hoc pairwise comparisons failed to find any significant differences in the number of dog fouls between counts. This is indicative of habituation over time and as discussed in the introduction, corroborates the anecdotal evidence from a participating LPC. However, due to the lack of statistical power, this is not certain.

A major limitation to this analysis was that the sample was underpowered (due to only being able to obtain five out of the 17 LPC's data). Even if significant results were obtained, having an underpowered sample reduces the likelihood that a statistically significant result reflects a true effect (Button et al., 2013) as the sampling distribution of studies with low power are often much wider resulting in a higher variance and a larger chance of having a large effect size (Smith, 2013). In other words, using an underpowered sample means that the result should be interpreted with caution (Field & Hole, 2003).

**General Discussion**

There were several issues raised in the critical reflections which questioned both the internal and external validity of Keep Britain Tidy's "We're Watching You" campaign. These issues could have been mitigated by the use of control locations alongside the target and displacement sites, and the use of a control poster that did not include an image of eyes. However, where RCTs cannot be used (e.g., due to time, cost or ethical constraints), a quasi-experimental pretest-posttest design (as used in this social experiment) is a widely used alternative in behavioural research (Dimitrov & Rumrill, 2003). Keep Britain Tidy did attempt to account for some of the possible limitations identified in the critical reflections (e.g., by using the average rather than the total number of dog fouls) and they also sought to minimise the impact of variability between sites by including a large number of test sites across a wide geographic range (e.g., 272 sites across 17 locations).

What is not clear is why there seems to be a watching-eyes effect in a naturalistic setting when the effect is increasingly difficult to replicate in a laboratory setting. Perhaps if any watching-eyes effect does exist, it is because eye images remind individuals that there could be other people in the area who could monitor their actions and judge accordingly (Pauwels et al., 2017), and thus eye images require the presence of at least some people in the area to affect behaviour (Bateson et al., 2013). When participating in a lab-based study, participants are easily able to override any subconscious effects of watching-eyes eyes by consciously observing that there is no one around to watch their behaviour, whereas, in a naturalistic setting, this may be harder to determine.

It is not possible to ascertain though that it was the eyes rather than the poster text or logos that caused the reduction in dog fouls in this social experiment. Each version of the poster had an image of eyes, a Keep Britain Tidy logo and displayed the messages “Thoughtless dog owners, we’re watching you” and as a call to action “Bag that poo, any rubbish bin will do” and there was no control poster (without an image of eyes) which could be used as a comparison. However, as the majority of target sites (which had the display posters) saw a reduction in dog fouls across a wide range of geographic locations, it is indicative that there was something about the posters which had an impact on the number of dog fouls.

There have been multiple watching-eyes experiments (e.g., Ayal et al., 2019; Brudermann et al., 2015; Kawamura & Kusumi, 2017; Odaet al., 2015) which have suggested that it is not simply the eyes themselves that effectively encourage prosocial behaviours but it is the eyes in conjunction with social norm messaging that is effective. Social norms and the eye images themselves are a form of ‘nudging’, a concept of influencing people’s behaviour without imposing rules, bans or coercion (Benkert & Netzer, 2015). Nudges are usually small, non-obtrusive changes to the environment (Corpuz & Aranas, 2020) which are often simple and cost-effective to implement (Dear et al., 2019) as seen in this study.

A growing body of literature suggests that using watching eyes as a ‘nudge’ is not a one size fits all approach (Benkert & Netzer, 2015). There is no evidence that people can be ‘nudged’ into doing something that is not already in their behavioural repertoire (Latham,

2020) so individual characteristics matter. If a person is not prosocial by nature and genuinely does not care about the consequences of dog fouling, then an image of a pair of eyes may be unlikely to change their behaviour and the design of this intervention means that individual characteristics cannot be examined or accounted for.

Keep Britain Tidy (2014) report that the watching eyes poster approach was highly effective in reducing dog fouling across test sites and suggest that the poster which provided information regarding a social norm was the most effective. However, the differences in the number of dog fouls between posters did not reach statistical significance which means it may not have been the eyes combined with the social norm message that caused a reduction in the number of dog fouls. It is possible that when combined with other information (e.g., a call to action), the watching eyes images could have a small (but meaningful) impact on behaviour change but the design of the intervention means that it is not possible to know which aspect of the poster was successfully at influencing people's behaviour. It could have been the images of eyes that were present on all posters, the call to action or just the mere display of a poster (Dear et al., 2019).

Keep Britain Tidy (2014) did not state what their *a priori* success criteria were but reported that 75% of target sites and 56% of displacement sites saw the number of dog fouls significantly reduce by almost half. This is an impressive result but as not all sites experienced a reduction in dog fouling, when combined with the aforementioned issues, LPCs may want to consider that this campaign may not necessarily be successful in their local area.

However, in their meta-analysis, Dear et al. (2019) reported a similar success rate for the watching-eyes effect with 68.57% of field experiments finding an effect. Dear et al. (2019) found that overall, eye cues may reduce the risk of antisocial behaviour by 35% and although this effect size is small, it is meaningful. Even small reductions in criminal activities can lead to large savings of public money. As such, although it is clear that the watching-eyes effect is not the panacea that perhaps early studies suggested it was, there is evidence to suggest that it may be a cost-effective deterrent to some antisocial behaviours in some circumstances (Dear et al., 2019).

**Conclusion**

This study has provided critical reflections on the stimuli, procedure, and sample used in Keep Britain Tidy’s social experiment as well as providing a re-analysis of the statistical analyses used in the report. The critical reflection questioned the degree to which eyes themselves caused the reduction in dog fouls, or whether it was some other aspect of the posters such as the call to action or just the mere presence of a poster (Dear et al., 2019). It also highlighted some potential threats to internal and external validity which calls for caution when interpreting the results, especially for councils considering signing up to this campaign in the future.

These considerations do not negate Keep Britain Tidy's (2014) findings; the “we’re watching you campaign” reported an impressive reduction in dog fouls across the majority of sites in the campaign, but the considerations should be taken into account when using this data to inform decisions on further social interventions. Although there were some limitations identified about how generalisable the Keep Britain Tidy (2014) would be across other sites and at different times of the year, the Keep Britain Tidy (2014) study does provide strong evidence for the intervention as a whole.

The findings of the field studies in this chapter, along with the previous online studies of this thesis, do not provide conclusive evidence either supporting or opposing the notion that the presence of eyes increases prosocial behaviour. However, it has been suggested that watching eyes may be more effective in reminding people to conform to social norms rather than to display prosocial tendencies (Ayal et al., 2019). The findings of study three provided some evidence that the presence of eyes may deter people from agreeing with controversial moral dilemmas (which could represent a strong deviation from the norm) and the findings from Keep Britain Tidy (2014) experiment, indicate that perhaps when combined with other nudges such as a call to action, watching eyes images are effective in reducing the number of dog fouls. This indicated that although the watching-eyes effect may not be a one size fits all approach for impacting human behaviour, in the right context, the watching eyes may provide a simple and cost-effective way of having a small (but meaningful) impact on behaviour change (Dear et al., 2019).

# Chapter Seven: General Discussion and Conclusion

This final chapter starts by recapitulating the thesis aim and research background. It then provides a summary of the empirical chapters, highlighting the novel contributions to the literature that each study has provided and the main findings. This is followed by a general discussion of those findings, their implications, and the overall theoretical contribution to the wider literature this thesis has made. It then highlights some of the limitations of the empirical studies, before suggesting directions for future research and closing with a conclusion.

## Thesis Aim and Research Background

This thesis first aimed to explore altruism as a costly signal in an online environment. Specifically, it addressed whether the watching-eyes effect could be evoked in an online context to promote donations to charity and, if so, what caveats there were to this.

Altruism has been the subject of much interest within behavioural sciences as, unusually within the animal kingdom, humans will promote the welfare of unrelated strangers even though there may not be any direct benefit to doing so (Fehr & Fischbacher, 2003). Costly signally theory (CST) proposes that individuals participate in altruistic behaviours that are costly to themselves, in terms of donating time, money or even risking their life to save others, to signal a quality about themselves to potential social partners (Becker & Eagly, 2004; Goldberg, 1995; Stern, 1995; Zahavi, 1977, 1995). As highly social creatures, humans rely on group living to thrive and survive (van Vugt & Kameda, 2013), so any behaviour or trait that benefits the group would help establish a positive reputation and aid in attracting potential mates or allies.

Research has shown that people behave more altruistically when they think or feel someone is watching them, and that this can be induced with just the presence of images depicting eyes (e.g., Bateson et al., 2006; Haley & Fessler, 2005; Sparks & Barclay, 2015). This phenomenon is known as the ‘watching-eyes effect’ and there is a large body of literature that has shown that images of watching eyes can increase prosocial behaviour in a wide range of contexts. However, with more recent studies failing to replicate the watching-eyes effect (Northover et al., 2016), there is a growing consensus that if the effect does exist, it is nuanced. In principle, the watching-eyes effect requires nothing more than displaying an image of eyes on a poster or computer screen, so, if researchers can pinpoint how and under what conditions the watching-eyes effect operates, this could potentially provide an inexpensive and straightforward way of encouraging people to act in more prosocial ways in a wide range of contexts.

At the time of developing the research proposal for this thesis (circa 2016), the watching-eyes effect had previously been tested on charitable donations in both a laboratory (e.g., Keller & Pfattheicher, 2011) and field experiment context (e.g., Ekström, 2012) and on cooperation in a dictator game in an online environment (e.g., Raihani & Bshary, 2012), but there had been no studies into the effects of watching-eyes on online prosocial charitable donations. With the near-ubiquitous use of the internet in modern day-to-day life across the globe, this was deemed a novel and necessary area in which to test the watching-eyes effect. If the watching-eyes effect could be evoked in this context, it could pave the way for a range of low-cost but potentially high-impact online interventions from increasing charitable donations to deterring anti-social behaviour such as online cyberbullying (Dear et al., 2019).

**Summary of Empirical Chapters and the Main Findings.,**

The main aim of this thesis, to explore altruism as a costly signal in an online environment, was explored through a series of five studies written up into four empirical chapters:

- Study One: Exploring how Watching Eyes Impact Prosocial Behaviour in an Online Environment
- Study Two: A Lab-based, Eye-tracking study on Participant Attention to Watching Eyes

- Study Three: Exploring Eye Gender, Emotion, and Salience
- Study Four A: A Field-based Exploration of the Watching-eyes Effect
- Study Four B: Keeping Britain Tidy; A Closer Look.

This thesis was designed via an iterative process, with each study planned and conducted based on the findings of the previous study to enable a robust but adaptable exploration into the watching-eyes effect. This thesis has provided a rigorous exploration of altruism as a costly signal in online and field environments and has provided several novel contributions to the watching-eyes literature. An overview of each empirical study is provided below.

***Study One: Exploring how Watching Eyes Impact Prosocial Behaviour in an Online Environment.***

The aim of the first empirical study of this thesis was to explore whether the presence of eyes positively affected prosocial behaviour in an online environment. As a key point in eliciting the watching-eyes effect appears to be in making people *feel* like they are being watched (Bateson et al., 2006), it was theorised in this study that the salience of the eye images (i.e., how noticeable the eyes are) may play an important role in evoking this feeling. Therefore, study one examined whether there was a difference in prosocial behaviour when participants were exposed to either a static image of eyes (a photograph), blinking eyes (a short video of blinking eyes) or no-eyes (a university logo).

Participants (*N* = 210) were recruited from online labour crowdsourcing websites; 'Call for Participants' (CfP) and 'Amazon's Mechanical Turk' (MTurk) to complete an online survey where they were randomly assigned to one of three conditions (static, blinking or no-eyes) in an experimental, between-subjects design. There were three measures of prosocial behaviour which were compared across the three groups: 1) the percentage of their initial bonus that participants donated to the public goods pot, 2) the percentage of their final bonus that they donated to charity, and 3) the number of hours that they indicated they would be willing to volunteer their time to charity for on a monthly basis.

Analysis of the data showed no significant differences between the eye stimuli groups in the amount that participants donated to the public goods pot or to charity. However, there was a was a significant difference between conditions in the number of volunteer hours, with a marginal significant difference and a moderate effect size seen specifically between the blinking and static conditions. Although the manipulation checks indicated that there were more participants in the blinking and static groups who correctly reported seeing an eye image of eyes, these numbers were still quite low which suggests that many participants were not noticing the eyes. In addition, the manipulation checks demonstrated that being exposed to the eye images did not significantly increase participants' feelings of being watched. Further exploratory analysis was conducted to see if there were any significant differences between groups in prosocial behaviour when the analysis was restricted to those participants who reported feelings of being watched. No significant differences were found.

There were some issues identified with this study which limits the ability to make any definitive conclusions. This is due to the ceiling effects seen within the no-eyes group in the amount donated to the public goods game and also due to the low statistical power seen across all tests of this analysis.

At the time of developing the research proposal for this thesis (circa 2016), there had been no studies into the effects of watching eyes on *online* charitable donations and no studies which had tested whether increasing the salience of eyes cues by using an image of blinking eyes could increase prosocial behaviours. This first empirical study was seemingly the first to explore these potential aspects of the watching-eyes effect.

***Study Two: A Lab-based, Eye-tracking Study on Participant Attention to Watching Eyes***

The first aim of this second empirical study was to build on study one by testing whether the presence of eye cues positively affected prosocial behaviour in an online environment when trying to specifically evoke reputational concerns. The second aim of study two was to collect data on participants' eye gaze behaviour to determine whether the participants paid attention to the image of eyes whilst completing the prosocial tasks, as the results of the first empirical study had shown that being exposed to eye images did not significantly increase the participants' feelings of being watched.

**Aim one.** Participants ($N$ = 49) completed an online survey in a lab-based setting (to allow for the collection of the eye-tracking data) which included the same three measures of prosocial behaviour as study one. In this study, they were advised that the amount they donated to the public goods pot would be made public to other participants in the game, and that their charitable donations would be made public to the charity receiving the donations. This was to address a limitation identified in the first study where participants were led to believe that their prosocial decisions were completely anonymous. However, as with study one, there were no significant differences between the groups in the amount donated to the public goods pot or in the number of volunteer hours. There was a marginal significant differences and moderate effects size seen in the amount donated to charity.

**Aim Two.** There were two eye-tracking measures used to explore how much attention the participants were paying to the stimuli: 1) the percentage of the total time the participants spent looking at the stimuli across three points of the survey (i.e., each time they were exposed to the stimuli), and 2) how quickly the participants looked at the stimuli across the same three points of the survey. The latter was used to ascertain whether blinking eyes were more salient than static eyes as theorised.

Results showed that the percentage of total time spent looking at the stimuli did not significantly differ between groups the first or third time the participants were exposed to the stimuli. However, participants in the static and blinking groups spent significantly more time looking at the stimuli compared to the no-eyes group at the point participants were playing the public goods game. There were marginal significant differences and moderate effect sizes between the blinking and no-eyes group, and the static and blinking groups. This increased attentiveness to the eye images did not result in an increased level of prosocial behaviour. Results also showed that the time to first fixation did not significantly differ between the participant groups across the survey, which suggests that the blinking eye stimuli were not more salient than the static eye stimuli.

Building on study one, this is the first study to test the salience of eyes in an online environment whilst manipulating participant reputational concerns and has also added to the small watching eyes literature which has examined gaze behaviour (e.g., Vaish et al., 2017). However, as with study one, due to issues with low statistical power, no definitive conclusions can be made on the presence or absence of watching eyes on prosocial behaviour.

***Study Three: Exploring Eye Salience, Emotion and Gender***

The main aim of this study was to expand on the first two empirical studies of this thesis, which tested whether the presence of eye cues positively affected prosocial behaviour in an online environment, by further exploring the range of eye stimuli in three areas: Gender, emotion, and salience. It was felt that, due to the growing body of evidence that shows artificial monitoring cues (i.e., eye images) do not influence human prosociality in a uniform way (Saunders et al., 2016), it would be intuitive that the type of eye stimuli that would be effective in capturing and maintaining attention (and thus affecting prosocial behaviour) could be nuanced. Research had shown that significant results of the watching-eyes effect could be dependent on a range of factors such as gender of the eye images (Bateson et al., 2006), perceived valence (e.g., kind vs unkind eyes) of eyes (Pauwels et al., 2017) and salience of the eye cues (Panagopoulos, 2014a). However, the research in each of these areas was limited and warranted further exploration.

This exploration was conducted by recruiting participants ($N$ = 176) to complete an online survey that included three measures of prosocial behaviour: 1) donations to charity, 2) moral dilemmas, and 3) total prosocial score. The main analysis revealed that the gender (male, female, and no-eyes), emotion (angry, happy, and no-eyes) and salience (static, blinking, audio, and no-eyes) of the eye images did not significantly impact the amount donated to charity or the participant's total prosocial score. However, there was some evidence in this study from the outcome of the moral dilemma scenarios, which suggested that in the presence of eyes, participants felt less able to communicate controversial ideas.

The presence of floor and ceiling effects and the low statistical power of this analysis means that as with studies one and two, no definitive conclusions about the watching-eyes effect can be drawn from this study. However, in addition to replicating and extending previous research, an additional strength of this study was that it included a novel aspect, the use of audio cues, to mimic the presence of others which aided in capturing participant attention and had not been seen in the watching eyes literature before.

***Study Four A: A field-based exploration of the watching-eyes effect, and***

The aim of study four A was to conduct an online field experiment in a naturalistic setting where participants may be less likely to be consciously aware of a prime (i.e., the eye images) by testing whether the presence of eyes on a 'real-world' charity website increased charitable donations. It had been claimed that positive results and larger effects sizes for watching eyes have predominantly been found in field studies rather than laboratory experiments (Kelsey et al., 2018; Manesi & Pollet, 2017) but to date, *online* lab experiments have found no evidence for the watching-eyes effect on donations to charity and there are no known online field experiments in which the watching-eyes effect has been tested.

This online field-based experiment took place as a collaboration between The International Federation for Athletes with Intellectual Impairments (INAS) charity and the researcher (Keli Jenner). A series of three single web pages were created to mimic the charity's real website. Each pseudo webpage had information about INAS and contained a Charities Aid Foundation (CAF) link to donate money directly to the charity. What differed between the web pages is whether the web visitors saw a static image of eyes, a short looping video of blinking eyes, or no eye images on the top of the web page. The web pages were then promoted to random Twitter users in a series of advertising campaigns to compare the amount donated to charity across the three different web pages. At the end of the Twitter campaign, despite over 33,000 web clicks, no donations were received via any of the three web pages. These results are evaluated within the discussion of the empirical chapter.

Despite the problems identified with this study, this study is the first known online field experiment that has tested the watching-eyes effect. In addition, it utilised a novel approach (i.e., the Twitter ad campaigns) to test the impact of watching eyes on donation behaviour.

***Study Four B: Keeping Britain Tidy; A Closer Look.***

As this thesis had, up to this point, had not found any conclusive evidence of the watching-eyes effect, this study aimed to re-evaluate a successful field experiment to take a closer look at the methodology, analysis, and results to assess the reliability of the researchers' claims. This is similar to the approach undertaken by Carbon and Hesslinger (2011) in their re-analysis of Bateson et al's (2006) watching eyes field experiment on contributions to an honesty box. It was decided to explore the Keep Britain Tidy (2014) "we're watching you" campaign as it was the only published public policy social experiment for which the raw data could be obtained.

In a field experiment conducted by the charity Keep Britain Tidy (2014), 17 local councils partnered up with the charity to test whether the display of a pair of eyes would be effective in reducing the number of dog fouling incidents at eight identified dog fouling hotspots. Keep Britain Tidy (2014) reported that overall, the watching eyes posters were highly effective in reducing dog fouling at the target sites without simply displacing the problem to an area nearby. As this seemingly highly successful intervention is in discord with the growing academic literature which has found little or no support for the watching-eyes effect (Northover et al., 2016), a closer inspection to assess its reliability was warranted.

This study provided critical reflections on the stimuli, procedure, and sample used in Keep Britain Tidy's (2014) social experiment, as well as providing a re-analysis of the statistical analyses used in the report. The critical reflections raised questions about the degree to which the eyes themselves caused the reduction in dog fouls or whether this was caused by some other aspect of the posters, such as the call to action. It also highlighted some potential threats to internal and external validity. As a non-academic piece of research, the Keep Britain Tidy (2014) campaign did not go through the same peer-review process that the majority of watching-eyes studies are subject to. This re-evaluation was seemingly the first to provide a methodological critical reflection of the Keep Britain Tidy (2014) campaign.

**Implications of findings**

The results of this thesis can be summarised into four main findings. First, no evidence was found that the presence of watching eye images increased prosocial behaviour in an online context. Second, no evidence was found that the images of eyes were effective in making the participants feel like they were being watched. Third, some evidence was found that that the presence of eyes may deter people from agreeing with controversial moral dilemmas (a type of anti-social behaviour). Fourth, watching-eyes field experiments may not be as successful as reported. Each of these findings is discussed in more detail below.

### *1. No Evidence that the Presence of Eyes increased Prosocial Behaviour*

Despite a rigorous approach that considered the reputational context of the experiment, various prosocial measures, and different aspects of the eye stimuli itself, this thesis found no evidence that the presence of watching eyes images can increase prosocial behaviour in an online environment. Although the results of this thesis should be interpreted with caution due to the low statistical power present in each study, these results are in line with a growing body of literature that is failing to find evidence that artificial cues of being watched increase generosity (e.g., Northover et al., 2017).

In this section of the discussion, it is suggested that with the absence of potential 'real' observers in an online environment, it is difficult to evoke the reputational concern that may be needed to trigger the watching-eyes effect. Even if this was not the case, the measures of prosocial behaviour may not have been suitable measures of the watching-eyes effect in an online setting. In contrast to studies that have suggested that the watching-eyes effect may be dependent on qualities of the eye cues, there was no indication that manipulating the salience (by utilising movement and sound), the gender, and the emotion of the eye cue impacted prosocial behaviour.

**A Potential Lack of Reputational Concern in an Online Environment.** A key component of the watching-eyes effect is reputational concern, which refers to individual differences in sensitivity to reputation. Within the context of the watching-eyes effect, this can be driven by the benefits received from engaging in altruistic acts observed by others, namely an enhanced reputation which leads to future rewards such as attracting a potential romantic partner (as highlighted in Kawamura & Kusumi, 2018). A limitation identified in study one was that it did not account for reputational concerns and had led the participants to believe that their prosocial decisions would remain completely anonymous. To address this limitation, study two built on the first by attempting to evoke reputational concerns by explicitly telling the participants that their prosocial decisions would be made public to both the 'other' participants in the public goods game and to the charity receiving the charitable donations. However, the results of study two demonstrated that there were no significant differences between groups in the amount donated to the public goods pot, or the number of volunteer hours and there was only a marginal significant difference in the amount donated to charity. In addition, the participants in study one donated significantly more money to the public goods game and charity than those in the study two.

One explanation for this could be that participants in study two may simply not have believed that they were playing against real people, and although participants were told their charitable donations would be made public to the charity, they would most likely be aware that this would not have been communicated in real-time and that there would be no immediate and obvious impact of their actions on their reputation. As the participants in study two were alone when completing the survey, if they did not believe they were being watched then it is possible that any emotional response subconsciously triggered by the eye stimuli may have been overridden by a quick and conscious realisation that the eye images were not real and that there was nobody around to watch their behaviour. The participants would have then known that there would be no repercussions (positive or otherwise) to the decisions they make, thus it would have failed to trigger any reputational concerns (Krátký et al., 2016).

Another explanation could be that the participants in study one may not have been alone when completing the survey. In the first study, participants were asked to ensure that they were completing the survey alone and with no one else in the room but there is no way of knowing for certain whether the participants were alone. It has been suggested that participants with total privacy (e.g., the participants in study two) may be immune to cues of being watched (Northover et al., 2016) because eye images may only work by reminding individuals that there are people in the area who can monitor their actions and therefore require the presence of at least some people in the area to affect behaviour. If the majority of the participants in study one did have other people in the room while they were completing their survey, this could explain why they donated significantly more money to both the public goods pot and charity than those in study two. However, even if the presence of others in the room may have led to increased donations to the public goods pot and charity in study one overall, this did not result in a watching-eyes effect on prosocial behaviour.

This suggests that, in the absence of potential 'real' observers, evoking reputational concern in an online environment may be a difficult endeavour for future researchers to elicit. These findings are consistent with other online watching-eyes studies (e.g., Raihani & Bshary, 2012; Saunders et al., 2016) which have suggested that sensitivity to eye cues may be weakened in online environments due to a perception of true anonymity; cues of being watched are less likely to trigger the reputational concerns needed for the watching eyes to influence prosocial behaviour.

**Prosocial Measures.** This thesis used multiple measures of prosocial behaviour such as donations to the public goods pot, total prosocial score (both as prosocial measures), various methods of donating to charity (as specific altruistic measures) and the number of volunteer hours per month (as a non-monetary form of altruistic behaviour).

***Public Goods Game and Total Prosocial Score.*** In empirical studies one and two, the percentage of the participants' initial bonus donated to the public goods pot was used as a single-item measure of prosocial behaviour. This measure was included as economic games have traditionally been used within the watching-eyes literature (Burnham & Hare, 2007; Haley & Fessler, 2005) and a public goods game simulates a key common feature of many human interactions (such as cooperative food production or child-rearing); whereas the collective benefits from cooperation in these interactions, each individual would be materially better off by free-riding on others (Alger, 2010). Viewing this through the lens of CST, contributing to a public good provides a good platform in which the giver can advertise their qualities (e.g., resources or generosity) as a potential interactive partner, group member or sexual mate (van Vugt & Hardy, 2009). However, results showed that there were no significant differences between groups in the amount donated to the public goods pot.

It has been suggested that economic games may not be very suitable for measuring the watching-eyes effect, as such games with low stakes often lead to the majority of participants donating (Nettle et al., 2013) as seen in the amount donated to the public goods game in study one. These potential 'ceiling effects' may overshadow potential differences across conditions (Manesi et al., 2016). In addition, a recent systematic review by Galizzi and Navarro-Martinez (2019) found that economic games (specifically one-shot games, as used in studies one and two) have poor external validity. However, donations to the public goods game was not the only measure of behaviour utilised in these studies and it provided a useful mechanism for studies one and two, where participants could earn a bonus in that they could then go on to donate to charity (the main measure of altruistic behaviour) if they so wished.

An additional limitation identified with the public good games was that as a single-item measure of prosocial behaviour, there were concerns around a potential lack of sensitivity (Bergkvist & Rossiter, 2007). Using a single-item measure means that there were fewer points of discrimination in which to detect a watching-eyes effect, which is particularly problematic in small sample sizes (Sauro, 2018). To address this limitation, in the third study, a nine-item measure of prosocial behaviour was utilised (Messick & McClintock, 1968). It was hoped that by providing more points of discrimination, it would increase the sensitivity of the measure but, despite these additional items in the prosocial measure, there were no significant differences between groups in the total prosocial scores. All groups in all conditions had a median prosocial score of nine (the maximum amount available) which could be indicative of a ceiling effect potentially masking any effect of gaze on overall task performance (Manesi et al., 2016).

These findings suggest that if the watching-eyes effect does exist, the public goods game may not a suitable measure of the watching-eyes effect. Cañigueral (2020) maintain that tasks like economic games are useful to measure prosocial behaviour in experiments because they have repeated trials, which facilitates reputation building between participants. However, in the absence of other real participants or in the lack of belief that they were playing against real people, both the one-shot public goods game (used in studies one and two) and the nine-item measure of prosocial behaviour (used in study three) may not have provided the participant with a reason to believe that their reputation was at stake and therefore it would have negated any watching-eyes effect.

***Donations to Charity.*** The main aim of this thesis was to explore whether the presence of watching eyes had a positive impact on altruism (as opposed to prosocial behaviour). The amount donated to charity is potentially a more reliable measure of prosocial behaviour to use when testing the watching-eyes effect (i.e., when compared to the public goods game). Although participants may donate to a public goods pot for altruistic reasons, they may also be motivated by the potential to earn a bonus for themselves, whereas there are no immediate benefits for participants to donate at least some of their bonus to charity, making any donations to charity a more reliable measure of altruism.

In studies one and two, participants were awarded a small bonus based on the decision they made in a public goods game and were then given the opportunity to donate any amount of their bonus to charity. The results showed that there were no significant differences between groups in the amount donated to charity in either study. In study three, to improve on this altruistic measure, a higher potential financial incentive was offered to the participants.

This was to address the possible limitation of the small bonus amounts awarded to participants in studies one and two. Research has shown that economic games with low stakes typically lead to a high proportion of players making a non-zero donation meaning the additional effects of eye images on giving behaviour are difficult to detect (Raihani & Bshary, 2017). However, despite increasing the financial incentive, results showed that there were no significant differences between groups in the amounts donated to charity.

It has been suggested that perhaps eye cues work by increasing the probability of donating rather than increasing the mean donation amount to charity (Nettle et al., 2013). For example, Raihani and Bshary (2012) reported that they had failed to find a watching-eyes effect but in their meta-analysis, Nettle et al. (2013) re-examined the data and found there was an increased probability of donating something in the presence of eyes. However, a more recent and more comprehensive meta-analysis by Northover et al. (2017) found no evidence that artificial cues of being watched increased generosity, either by increasing how generous individuals are or by increasing the probability individuals will show any generosity at all. The overall mean effect size was small and not significantly different from zero. In line with this meta-analysis, this thesis did not find any evidence that the presence of eye images significantly increased the amount donated to charity.

***Total Volunteer Hours.*** It has been suggested that non-monetary forms of prosocial behaviour (e.g., volunteering) may be a more reliable signal than donating to charity as, at the point of donation, donating money is relatively less effortful and time-consuming than volunteering. Therefore, in studies one and two, the number of hours that participants indicated they would be willing to volunteer their time to charity on a monthly basis was used as a prosocial measure. Results showed there were only marginal significant differences in volunteer hours between groups in study one and no significant differences between groups in study two.

On reflection, this may be because an *indication* of willingness to volunteer involved no actual cost to the participants in terms of either time or money. As there was no cost involved to the participant, the self-reported intentions to volunteer may not a reliable prosocial signal as it is too easy to fake intention (Bradley et al., 2018). In addition, self-reported prosocial intentions are sensitive to social desirability bias (Tussyadiah & Miller, 2019). Social desirability bias is the tendency to give socially desirable answers instead of answers that reflect the true feeling of the individual. Future research into this area would benefit from testing whether social desirability bias mediates any watching-eyes effect on volunteering behaviour (Miyazaki, 2017).

**Different aspects of the Eye Stimuli.** There have been surprisingly few watching eyes studies that have taken into account aspects of the eye cues themselves (Vrouwe & Balliet, 2014) but some research has shown that significant results of the watching-eyes effect could be dependent on a range of eye cue factors such as the salience of the eye cues (Panagopoulos, 2014a), gender of the eye images (Bateson et al., 2006), and perceived valence (e.g., kind vs unkind eyes) of eyes (Pauwels et al., 2017). This thesis explored the watching-eyes effect using a range of eye stimuli, manipulating the salience (by utilising movement and sound), the gender, and the emotion of the eye cue.

***Salience.*** With the near-ubiquitous use of the internet and constant exposure to advertisements, it was thought that perhaps in an online environment, people may have ample experience at ignoring task-irrelevant stimuli (Sparks & Barclay, 2013). Therefore, if the eye cues were more salient (i.e., noticeable) then they may be better at capturing and maintaining participant attention and thus allow for the watching-eyes effect to emerge. It was thought that the blinking eyes images would be more noticeable (i.e., than a static image of eyes) and thus more effective at eliciting a watching-eyes effect. However, the results of study one showed that there were no significant differences between any groups in the measures of prosocial behaviour.

In study two, this was re-tested whilst trying to specifically evoke reputational concerns and an eye-tracking device was used to determine if people were paying more attention to the blinking eye images than the static eye images or a university logo. The time to first fixation on the stimuli was used to determine if participants looked at the blinking eyes quicker than those who looked at the static eyes or university logo. Results showed that the 'time to $1^{st}$ fixation' did not significantly differ between the static, blinking and no-eyes groups, suggesting that the blinking eye stimuli were not more salient than the static eyes stimuli.

As manipulating the movement of the eyes did not result in increased levels of prosocial behaviour in either empirical study one or two, it was thought that the blinking eye images may still have not been salient enough in an online environment to have captured participant attention. Research has suggested that the effectiveness of surveillance cues (i.e., eye images) could be improved by the use of audio messages (Jansen et al., 2018). It was therefore anticipated that increasing the salience of the eyes to include an audio element (which would mimic the presence of other people) would help to evoke the watching-eyes effect on prosocial behaviour. However, results showed that there were no significant differences found between groups (static, blinking, audio, and no-eyes) in the measures of prosocial behaviour.

These results, combined with the findings from the first two empirical studies, suggest that increasing the salience of the eye cues (i.e., how noticeable they are) did not increase prosocial behaviour in an online environment. As with each study of this thesis, these results should be interpreted with caution. Due to the low statistical power present in each study, no definitive conclusions from these results can be made.

***Gender and Emotion of the Eye Cues.*** In study three, it was decided to expand the range of eye stimuli in the experiments to also include an exploration of the gender of the eye cues and the emotion expressed to explore whether specific nuances and qualities of the eyes themselves may trigger prosocial behaviour. It had been suggested from the literature that male eyes are more effective at eliciting a watching-eyes effect (e.g., Bateson et al., 2006). It has also been theorised that 'angry' eyes would have a type of policing effect (e.g., Pauwels et al., 2017) which would stop people from deviating from the social norm. The effects of gender and emotion on prosocial behaviour were explored by donations to charity and total prosocial score in a modified public goods game. No evidence was found in this study that the gender or emotion of the eye cues affected the levels of prosocial behaviour.

### *2. No Evidence that Eyes made the Participants Feel Watched*

The second main finding of this thesis was the participants who were noticing the eye images and paying enough attention to accurately recall that they saw female eyes, did not report feelings of being watched. It is thought that the belief of being watched by another person is critical in generating the watching-eyes effect. The watching-eyes effect posits that if a person experiences feelings of being watched, it triggers reputational concerns and motivates them to behave in a prosocial manner (Conty et al., 2016).

In this section of the discussion, it is suggested that as the watching-eyes effect is thought to occur on a subconscious level, the *conscious* feeling of being watched and attentiveness to eye cues may not be important. Despite the participants in study two paying more attention to the eye cues at the point of playing the public good game, this increased attention did not translate into increased prosocial behaviour. This section also discusses how the lack of evidence for the watching-eyes effect seen in this study cannot be conclusively attributed to participants becoming habituated to the eye images but it could be that in an online environment, any emotional response subconsciously triggered by the presence of the eye stimuli would be quickly overridden by a conscious realisation that the eye images are not real and there is nobody around to watch their behaviour.

**Conscious feelings of being watched.** The manipulation checks for studies one, two and three in this thesis showed that there was no association between being exposed to eye stimuli and increased feelings of being watched. However, these feelings of being watched were measured on a self-reported basis. As any changes in behaviour in response to eye images is likely to be an involuntary, subconscious response (Burnham & Hare, 2007), it is possible that *conscious* feelings of being watched are not important. Evidence has shown that visual awareness of a direct gaze is not required for the watching-eyes effect to occur (Conty et al., 2016) and that both the feelings of being watched and the reputation management mechanism it triggers could be occurring at a sub-conscious level (Matland & Murray, 2015) which may not be reflected in participants' self-reported measures of whether they felt observed.

**Attentiveness to the eye cues.** As eye-tracking equipment can detect even subconscious observations, it is an important tool to try to ascertain whether participants paid attention to the different eye stimuli during the tasks (Weggelaar-Jansen et al., 2016). An aim of study two was to determine whether the participants paid attention to the image of eyes whilst completing prosocial tasks. Results showed that at the point participants were playing the public goods game, those participants who were exposed to eye images (e.g., those in the static and blinking groups) spent significantly more time looking at their stimuli compared to the no-eyes group. However, this increased attentiveness to the eye images did not result in an increased level of prosocial behaviour. These results combined with the results from the manipulation checks indicate that participants noticed the eye images and were paying enough attention to accurately recall that they saw female eyes, but this attentiveness did not translate into increased prosocial behaviour.

**Habituation.** One prominent theory within the literature is that any effect of watching eyes on prosocial behaviour may decline when individuals are presented with eye stimulus for a prolonged period (Sparks & Barclay, 2013). This prolonged exposure leads to a decrease in responsiveness due to individuals becoming habituated to the image of eyes (Oda, 2019).

As part of the eye-tracking analysis in study two, further exploratory analysis was conducted using a series of one-way repeated measures tests which compared the percentage of total time the participants looked at the eye stimuli across the three points of the survey. A decrease in the percentage of total time looking at the stimuli across the locations (i.e., time) could be an indication of habitualisation (Sparks & Barclay, 2013). The results of this exploratory analysis showed that there was no significant decrease in the percentage of total time that participants spent looking at the eye stimuli across the three locations in the survey although a moderate (but non-significant) effect size was seen within the static group. This indicates that the lack of evidence for watching eyes seen in this study cannot be attributed to participants becoming habituated to the eye images. However, the moderate effect size seen within the static group could be indicative of an effect but due to the low statistical power in this analysis, this is not certain.

This was similarly the case in study four B, where the results indicated a marginal significant difference between dog fouling counts over time for the target sites and a significant difference between counts over time for the displacement sites. Although post hoc pairwise comparisons failed to find any significant differences in the number of dog fouls between counts, this is indicative of habituation over time but due to the lack of statistical power, no definitive conclusions can be made either way.

**False Cue Detection.** Exposing the participants to the eye stimuli on a screen means that the participants are viewing the eye stimuli more directly, with fewer distractions and for possibly longer periods of times than in other settings (e.g., if they were just walking past a poster of watching eyes). This may increase the chance that a person will recognise that the eyes stimuli are a false cue of actual human agency. As discussed earlier, any emotional response subconsciously triggered by the presence of the eye stimuli would be quickly overridden by a conscious realisation that the eye images are not real and there is nobody around to watch their behaviour. Therefore, a person would know that there would be no repercussions to their social decision-making (positive or otherwise). As such, it would fail to trigger any reputational concerns and would be unable to evoke a prosocial effect (Krátký et al., 2016).

***3. Watching eyes may deter anti-social behaviour***

Throughout this thesis, no evidence has been found for a watching-eyes effect on the measures of prosocial behaviour. However, the results of the moral dilemmas scenarios in study three provide some evidence that the presence of watching eyes may deter anti-social behaviour.

This section of the discussion explores how participants in the eyes present groups were less likely to agree with a controversial moral dilemma and perhaps this is because it represents an extreme deviation from the norm which could result in a fear of punishment or social judgment (Fehr & Gächter, 2002). These findings are in line with a recent meta-analysis that showed that there is a robust watching-eyes effect in reducing anti-social behaviour (Dear et al., 2019).

**Moral Dilemmas.** In study three, a modified version of the trolley problem, the classic philosophical moral dilemma, was used to explore the effects of watching eyes on moral judgements. Taken from Hauser et al. (2007), participants were presented with a series of moral dilemmas with each moral dilemma presenting a choice between action and inaction which could result in lives saved or lives lost. There were no significant differences between groups in the numbers of participants who agreed that scenarios one and three were morally permissible.

However, results showed that participants who were exposed to the eye images within the emotion and gender conditions were less likely to agree with the more controversial moral dilemma (e.g., the willingness to cause direct physical harm to one person to save the lives of others). Scenario two differs from scenarios one and three in two ways. First, the action to save lives involves introducing a new threat (e.g., pushing a person) rather than redirecting an existing threat (e.g., switching a train onto a side-track and into the path of a person). Secondly, the action in scenario two is personal rather than impersonal as it involves causing harm by direct physical contact rather than by an indirect means (Hauser et al., 2007). Although participants may feel like it was morally permissible to cause the death of one person to save the lives of five others, scenario two represents a more controversial option of the three options. This could suggest that the presence of watching eyes may be more effective in deterring anti-social behaviour or at least deter deviation from the norm, rather than promoting prosocial behaviour.

However, some caution should be taken with this interpretation as it is not possible to be certain that the participants in this study would have known which was the 'correct' moral judgement or what was the norm in these scenarios. This means it is also unclear how people will judge the outcomes of these moral dilemmas in the presence of watching eyes. As reputational concerns are the leading interpretation for the watching-eyes effect (Pauwels et al. 2017), then it stands to reason that the presence of eyes cannot moderate effect if there is no effect of reputation on moral judgements.

**Impact of reputation.** While there is a growing consensus that there is little evidence for a robust watching-eyes effect on generosity (e.g., on donations to an economic game or charity), the findings from study three were in line with a recent meta-analysis showing that there is a robust watching-eyes effect in reducing anti-social behaviour (Dear et al., 2019).

Dear et al. (2019) maintain that the inconsistent results in the watching eyes literature may be due to the varying effects of prosocial behaviour on reputation and that eye cues may be more effective in reducing antisocial behaviour as antisocial behaviour may be more consistent in damaging reputation than prosocial behaviour is in enhancing it. Research has shown that a bad reputation can have explicit costs as people are willing to pay to impose costs upon those who act in an anti-social manner (e.g., those who do not pitch in with their fair share in cooperative situations) and will seemingly punish even if they receive no benefit from the punishee's future cooperative behaviour. The presence of punishment makes it costly to refuse to help or act in explicitly anti-social ways (Fehr & Gächter, 2002).

It has been suggested that in the context of the watching-eyes effect, the eye images may not necessarily increase prosocial behaviour or decrease anti-social behaviour *per se* but rather encourage people to comply with social norms (Ayal et al., 2019). Cooperation between humans relies on social norms or beliefs that define how individuals should behave in certain situations and it is thought that social norm adherence is modulated by awareness of being watched (e.g., the watching-eyes effect) and the expectation of deviation resulting in consequences such as rewards or punishments (Ikuse et al., 2018). Pauwels et al. (2017) posit that eye gaze (particularly of unkind eyes) provides a type of policing effect: it serves as a reminder to the observer that their actions may result in punishment or a decreased ability to attract a mate or allies. This compels individuals to conform to prosocial norms or risk the anger or disappointment of their interaction partner. The findings of study three showed that the participants in the eyes present groups were less likely to agree with a controversial moral dilemma (e.g., the willingness to cause direct physical harm to one person to save the lives of others), perhaps due to the more extreme deviation from the norm (representing an anti-social behaviour) which could result in a fear of punishment or social judgment (Dear et al., 2019).

***4. Field Experiments may not be as Successful as they Seem***

It has been suggested that the watching-eyes effect may produce a more robust effect in the field rather than in laboratory experiments (Kelsey et al., 2018; Manesi & Pollet, 2017). Although recent lab-based studies have challenged the earlier findings of the watching-eyes effect on prosocial behaviour, there are many reportedly successful field experiments (Dear et al., 2019). In particular, the watching eyes study on deterring bicycle theft conducted by Nettle et al., (2012) is one of the most influential peer-reviewed research papers on the watching-eyes effect in UK public policy (Dear et al., 2019), and it has paved the way for a multitude of applied interventions in the UK aimed at deterring a range of anti-social behaviours. The use of 'watching eyes' is so well established in UK public policy that the UK Government's (2017) National Anti-Littering Strategy recommends the use of 'watching eyes' interventions to reduce littering (Dear et al., 2019). It is not clear why there is this discord in the success of eliciting the watching-eyes effect between lab-based and field experiments. This discord was explored in further detail in the last empirical chapter of this thesis.

First, an online field experiment was conducted in collaboration with an international charity to try and increase online charitable donations. However, despite over 33,000 visitors to the web pages, unfortunately, not a single donation was received. This result is discussed in detail in the individual empirical chapter (Chapter six: Study Four A) and there were some major limitations identified in this study which will be discussed in the next section (see 'limitations').

Second, due to the inconclusive findings of this thesis and the apparent discord within the watching eyes literature between field and lab-based studies, the last empirical study also re-evaluated a reportedly successful watching eyes field experiment. The study provided critical reflections on the stimuli, procedure, and sample used in Keep Britain Tidy's (2014) anti-dog-fouling poster campaign as well as providing a re-analysis of the statistical analyses used in the report.

Keep Britain Tidy (2014) reported an impressive reduction in dog fouls across the majority of sites in the campaign. However, each version of the poster used in the campaign had an image of eyes, a Keep Britain Tidy logo and displayed the messages “Thoughtless dog owners, we’re watching you” and as a call to action “Bag that poo, any rubbish bin will do”. As there was no control poster (i.e., without an image of eyes) that could be used as a comparison, it was not possible to ascertain (either way) that it was the eyes rather than the poster text or logos that caused the reduction in dog fouls in this social experiment or even just the mere display of a poster (or a combination of all of the components) (Dear et al., 2019). However, this critical reflection is on just one field experiment and does not mean that the success of the Keep Britain Tidy (2014) social experiment or other applied interventions is not due to images of watching eyes. It does mean though that caution should be taken when determining whether an applied watching-eyes intervention has been successful particularly if the success has been attributed to the eye stimuli itself rather than some other aspect of the intervention specifically due to the eye stimuli itself.

***Summary of Findings***

In a series of four empirical studies which explored reputational context, various measure of prosocial behaviour, and specific aspects of the eye stimuli themselves, this thesis found no conclusive evidence that the presence of watching eyes can increase prosocial behaviour in an online environment. The results also indicate that neither the presence nor salience of the eye cues impacted whether participants reported feelings of being watched (a proposed key component of the watching eyes). Eye-tracking analysis showed that blinking eyes were no more effective at capturing participants' attention than the static or control images, and also showed that increased attention to eye stimuli overall did not result in increased prosocial behaviours. However, in line with a recent meta-analysis (Dear et al., 2019), there was some evidence that the presence of eyes may deter people from anti-social behaviour (i.e., agreeing with a controversial social norm). This is perhaps because the eye cues are more successful at reminding people to not deviate from a social norm (for fear of punishment) than they are at specifically encouraging prosocial behaviour. It has been claimed that field studies yield more positive results and larger effect sizes for watching eyes than laboratory experiments, but this was not corroborated by the findings of the last empirical chapter. However, in the last empirical study of this thesis, it could not be ruled out that it was the presence of watching eyes in the Keep Britain Tidy (2014) social experiment that deterred the anti-social behaviour (i.e., the dog fouls).

**Limitations**

There were some key limitations identified in this thesis, namely small sample sizes (in studies one, two, and three), the measure of non-monetary prosocial behaviour (i.e., number of volunteer hours), and the lack of pilot study for the watching-eyes online field experiment in study four A.

***Low statistical power***

A limitation of this thesis was that there was low statistical power across all studies in this thesis. The consequences of an underpowered sample include an increased probability of a type II error (where the null hypothesis is erroneously not rejected), overestimates of effect size and low reproducibility of results (Button et al., 2013). Overall, this means that these samples had a lower probability of being able to detect a watching-eyes effect and an increased probability of a type II error which means that the results should be interpreted with caution (Rotella, 2019).

The 90% confidence intervals for each effect size were calculated with a view to compare it with the confidence intervals for the smallest sample of interest (SESOI) which was set at a small-medium effect size (please general methodology chapter). However, the confidence interval for each effect size fell outside of the boundaries for the confidence interval for the SESOI. This means that it cannot be determined that the comparison groups were statistically equivalent. Therefore, no definitive conclusion can be made either way about whether the presence of watching eyes impacts prosocial behaviour. With the growing consensus that any effect of watching eyes on human behaviour is weak (Shinohara & Yamamoto, 2018), if a watching-eyes effect does exist, the samples used in these studies may not have been powerful enough to confidently detect it.

***Non-monetary prosocial measure***

Another limitation was that the experimental design could have benefitted from a different measure of non-monetary prosocial behaviour. As previously discussed, it has been suggested that non-monetary forms of prosocial behaviour (e.g., volunteering) may be a more reliable altruistic signal than donating to charity as, at the point of donation, donating money is relatively less effortful and time-consuming than volunteering. However, on reflection, it is likely that an *indication* of willingness to volunteer is not a reliable prosocial signal as there was no actual cost to the participants in terms of either time or money and it is too easy to fake intention (Bradley et al., 2018). In addition, self-reported prosocial intentions are sensitive to social desirability bias (Tussyadiah & Miller, 2019). Alternative non-monetary measures of prosocial behaviour could include social value orientation (SVO). A stable trait that reflects an intrinsic prosocial willingness (Pauwels et al., 2017) and provides a more fine-grained, continuous measure of prosociality (Millet & Aydinli, 2019). However, it was felt that overall, this thesis utilised a wide range of prosocial measures to explore the watching-eyes effect so any additional measure would be superfluous. In future watching eyes studies, it may be beneficial to consider an alternative measure of non-monetary prosocial behaviour such as SVO or it may be more effective to measure actual impact on behaviour rather than self-reported intentions.

***Pilot studies***

A major limitation in study three and study four A, was the lack of a pilot study conducted especially considering both of these studies used novel approaches not seen within the watching-eyes literature before. Study three was the first known experiment to include audio cues to try to increase the salience of the eye cues and help in evoking the watching-eyes effect. Study four A was the first known online field experiment and in addition was utilising a novel recruitment approach (i.e., the Twitter ad campaigns).

**Study Three.** As study three was the first known watching-eyes experiment to include audio cues, this means that there were no previous studies on which to base the audio material. Without a pilot study, it is not possible to know whether the audio cue was suitable for the experiment or needed to be tweaked in some way. For example, it may have been possible that the audio message was played too often to the participant which may have resulted in them finding the audio cue annoying. If this was the case, perhaps the participant turned off the sound to the survey negating any impact it may have had on increasing the salience of the eye cues. If future studies were to explore audio cues further, they would benefit from developing and testing the audio material beforehand.

**Study Four A.** Despite over 33,000 visitors to the web pages in study four A, not a single donation was received. The large volume of web page visitors for such a small budget campaign in a relatively short amount of time was indicative of potential click fraud. Click fraud occurs when either a real person or bot imitates a legitimate user on the site, clicking on the ad without having any interest in meaningfully engaging with the ad (or in the case of the bot, without having the ability to). An abnormally large click-through rate for advertising campaigns is seen as a key indicator of the influence of bots which can result in companies wasting large amounts of money based on fraudulent Twitter views (Auty, 2018).

A pilot study was not considered for this study as the experiment was purposefully designed to mimic the existing charity website and any changes were quite minimal to specifically test the effect of the presence of eyes on donation behaviour. In hindsight, the issues experienced in this experiment highlight the importance of conducting a pilot study. An advantage of conducting a pilot study is that it may have given a warning regarding some of the practical problems in the research approach (e.g., the potential threat of Twitter bots). A pilot study would have allowed a shorter, less costly way of testing the effectiveness of the research approach and an opportunity to make improvements (van Teijlingen, Edwin & Hundley, 2010).

**Future Directions**

The findings from this thesis are in line with a large body of literature which suggests that if the watching-eyes effect does exist, it is nuanced and images of watching eyes do not influence human behaviour in a uniform way (Saunders et al., 2016). There are several avenues identified in this thesis that would aid in exploring the nuances of the watching-eyes effect in more detail.

First of all, it seems that, if the watching-eyes effect does exist, it may be too weak to translate into an online environment. This is perhaps because people are aware that in an online environment, no one is watching so there would be no repercussions to their social decision-making (positive or otherwise) or their reputation. A potential avenue for future research would be to explore how reputational concerns could be evoked in an online environment.

Second, it has been posited in this thesis that, with the near-ubiquitous use of the internet (especially social media) in 2021, participants may be accustomed to being exposed to eye images on an almost constant basis and therefore, could be well-versed at ignoring what they may consider to be task-irrelevant stimuli. It would be interesting to test whether the watching-eyes effect could be evoked in an online setting on people who may not be as used to using the internet on such a constant basis (e.g., the elderly). If so, this would suggest that an online environment would not be the most appropriate setting in which to apply the watching-eyes effect and efforts should be focused elsewhere.

Third, as Dear et al. (2019) maintain, in an experimental setting, it is difficult to see how participants cannot be aware that their actions, even if indirectly and/or anonymously, would not be the subject of scrutiny by the researcher which could potentially mask any effect of watching eyes on human behaviour. Therefore, future watching-eyes studies should be explored in more ecologically valid and/or through carefully designed field studies. This would have the added benefit of identifying potential interventions which could have a meaningful effect in 'real-world' terms (e.g., by reducing crime and saving public money).

**Conclusion**

This thesis has provided an in-depth exploration of altruism as a costly signal in online and field environments and has provided several novel contributions to the watching-eyes literature. This thesis aimed to explore whether the watching-eyes effect could be evoked in an online context to promote donations to charity. This was achieved by a series of five empirical studies, designed via an iterative process, with each study planned and conducted based on the findings of the previous study to enable as robust an exploration into the watching-eyes effect as possible. Due to persistent issues with low statistical power across the studies, no conclusive evidence was found that the presence of watching-eye images affected online prosocial behaviour, but there was some evidence that the presence of eyes may deter people from anti-social behaviour. Although no definitive conclusions can be drawn from the results of this thesis due to the low statistical power, it is clear from the indicative findings of this thesis and from recently published studies that the watching-eyes effect is not a panacea for impacting human behaviour that perhaps early studies suggested it was. Within the literature overall, there is evidence that suggests that in the right context, such as in deterring anti-social behaviour, the watching eyes could provide a simple and cost-effective way of having a small (but meaningful) impact on behaviour change. However, adding to the ever-growing list of nuances for the watching-eyes effect, it does not seem that the watching-eyes effect can be easily elicited in an online environment.

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**APPENDIX A – Example of Study One Questionnaire**

**Making your mind up: Individual differences in decision-making behaviour**

**Who is organising this study?**
This research is being conducted by PhD candidate Keli Jenner at Canterbury Christ Church University, UK and supervised by Dr Wendy Iredale.

**What are the aims of the study?**
This study aims to analyse individual differences in people's decision–making behaviour.

You will be invited to participate in a group task which will involve a few basic mathematical questions and then you will be asked to provide some general information about yourself.

Your participation is completely voluntary and you have the right to withdraw at any time without giving a reason. Your information will be kept strictly confidential and any identifiable information will be kept separately from your data.

**Who can take part?**
Anyone over the age of 18 years old.

**Who cannot take part?**
Those under the age of 18 years old.

**What happens to the information I provide?**
Participation in this study guarantees confidentiality of the information you provide. No one apart from the researcher and research supervisor will have any access to the information you provide. Your name and any other identifying information will be stored separately from your data in a securely locked filing cabinet. Questionnaires will be stored in a securely locked room for as long as is required by the Data Protection Act, and then they will be destroyed by our confidential shredding service. The data collected for this study will be used for a student project. Once the data is analysed a report of the findings may be submitted for publication. Only broad trends will be reported and it will not be possible to identify any individuals. A summary of the results will be available from the researcher on request.

**Contact for further information:**
If you require any further information or have any queries about this study please contact the researcher:

Keli Jenner
keli.jenner@canterbury.ac.uk

Or the research supervisor:
Dr Wendy Iredale
wendy.iredale1@canterbury.ac.uk

If you wish to withdraw your data from this study, please contact the Psychology Department:

psyadmin@canterbury.ac.uk. If you have been given a participant code you need to cite this. You do not have to give a reason for your withdrawal.

If you have any serious concerns about the ethical conduct of this study, please inform the Chair of the Psychology Research Ethics Panel (via the Psychology Department Office) in writing, providing a detailed account of your concern.

Canterbury Christ Church University

---

Please tick the below boxes to show that you understand and agree with each statement. You will not be able to proceed to the study without doing so.

☐ I confirm that I have read and understand the information sheet for the above study and have the opportunity to ask questions. (1)

☐ I understand that my participation is voluntary and that I am free to withdraw at any time, without giving any reason, without my medical care or legal rights being affected. (2)

☐ I understand that any personal information that I provide to the researchers will be kept strictly confidential (3)

☐ I agree to take part in the above study. (4)

---

Please enter your email address:

(Please note that without a valid email address you cannot receive any compensation for taking part in this study)

________________________________________________

Please state your age (in years)

______________________________________________

What is your gender?

- ◯ Male
- ◯ Female
- ◯ Not listed (please specify) ______________________________
- ◯ Prefer not to say

What is your ethnic origin?

______________________________________________

What country do you currently live in?

______________________________________________

Canterbury Christ Church University


You have been automatically matched up with two other survey participants (don't worry, this is completely anonymous!)

Everyone in the group will, in a moment (on the next page), have to decide how much of their £1.50 they will donate to the group fund and how much they will keep for themselves.

Whatever is donated to the group fund is **doubled** then **equally divided back** to all players. How much you donate and how much you keep for yourself will affect how much money everyone else receives back from the group fund, see examples below:

**Example 1:**

- Participant A - donates £1.50 and keeps 0 pence
- Participant B - donates £1.50 and keeps 0 pence
- Participant C - donates £1.50 pence and keeps 0 pence

If all participants donated £1.50 and keep 0 pence for themselves, then £4.50 is donated to the group fund.

This is then doubled to £9 and is equally divided back to all participants.

Each participant gets £3 each. Although all participants did not keep anything to themselves, they ended up with £3 each in their private fund.

**Example 2:**

- Participant A - donates 50 pence and keeps £1 for themselves
- Participant B - donates 0 pence and keeps £1.50 for themselves
- Participant C - donates £1.30 and keeps 20 pence for themselves

The participants have allocated a total of £1.80 to the group fund.

This is then doubled to £3.60 and is equally divided back to all participants.

Each participant gets £1.20 each regardless of how much they donated. Although participant C gave most money to the group fund, because participant A and participant B did not give much, participant C lost money, whereas participants A and B got more money.

---

Please now select the below statement that you agree with.

○ I have understood the rules to the group task

○ I have not understood the rules to the group task

---

You will now answer a few questions about the example to check your understanding of the rules. Please read and answer the following questions carefully. Remember the rules are: the total donated to the group fund is doubled and then divided equally back to all participants.

---

Each participant has £1. If all three participants (this includes yourself) donated 10 pence to the public fund how much would they keep for themselves?

- ○ 10 pence
- ○ 30 pence
- ○ 90 pence

---

If all three participants (this includes yourself) donated 10 pence to the public fund and then the public fund is doubled, how much would there be in the public fund?

- ○ 30 pence
- ○ 60 pence
- ○ £1.80

---

How much of the public fund would all three participants each receive back?

- ○ 20 pence
- ○ 40 pence
- ○ 60 pence

---

How much would each participant have in total (including the original amount they kept for themselves)?

- £1.10
- £1.20
- £1.30

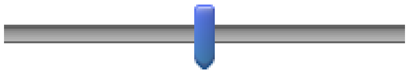
---

You are Participant C.

Please state below (in pence) how much of your £1.50 you will you donate to the group fund.

You can give anything from 0 pence to £1.50.

0 10 20 30 40 50 60 70 80 90 100110120130140150

| | |
|---|---|
| Donated to Group Fund |  |

The following amounts were donated to the group:

Participant A: 100 pence (£1)
Participant B: 50 pence
Participant C: ${Q17/TotalSum} pence

Total amount donated to group fund: $e{ q://QID41/TotalSum + 150} pence

Each participant will receive a total of: $e{ ((q://QID41/TotalSum + 150) * 2) / 3} pence

Congratulations you have earned a bonus of: $e{ 150 - q://QID41/TotalSum + ((q://QID41/TotalSum + 150) * 2) / 3} pence

This will be round to the nearest whole pence and paid into your PayPal account within 72 hours.

As part of National Volunteer Week (107 June), we are collecting information on people's willingness to participate in charitable activities.

Imagine you have been approached by a well-known and registered charity who urgently require volunteers to assist with their work.

Below is a list of typical volunteer activities that are required by charity groups. Please indicate which activities you would be willing to help with (you can tick as many as you like or none at all) and please state how many hours a month (in total) you would be willing to help.

| | Please tick which activity or activities you'd be willing to help with | How many hours per mont would you be willing to volunteer? |
|---|---|---|
| Help organise a fundraising event | ☐ | |
| Collecting donations for charity | ☐ | |
| Providing care for the elderly | ☐ | |
| Providing care for the physically disabled | ☐ | |
| Providing care for the homeless | ☐ | |
| Providing care for mentally handicapped children | ☐ | |

In support of National Volunteer Week, we are also giving you the opportunity to donate some of your bonus earnings from this survey in order to support charities.

If you would like to donate some of your bonus, please indicate the amount in pence below. Please note that this is completely voluntary.

__________________________________________________________

You will now be asked a few questions regarding the group task - please answer as many questions as possible.

How did you feel when you were making your decisions during the group task?

________________________________________________________________

Did you feel like you were being observed at all whilst completing the survey? If so, please explain why.

________________________________________________________________

Did you see an image of eyes during the task?

○ Yes

○ No

○ Not certain

○ N/A

If so, what was the gender of the eyes?

- Male
- Female
- Not certain
- N/A

Q29 What emotion were the eyes displaying?

________________________________________________________________

How attractive were the eyes?

Not Applicable

| | 1 | 2 | 3 | 4 | 4 | 5 | 6 | 7 |
|---|---|---|---|---|---|---|---|---|
| Not all | | | | | | | | |

How did you take part in the survey?

- Desktop computer
- Smart Phone
- Tablet

Briefly describe your environment and whether anybody else around you when you completed the survey? e.g. A family member in the same room

______________________________________________________________________

How do you think you will spend the money you have earned today from the survey?

______________________________________________________________________

What is your highest educational qualification?

- ○ None
- ○ Undergraduate degree
- ○ Postgraduate degree
- ○ Doctorate
- ○ Other (please specify) ____________________________________________
- ○ Compulsory schooling (please specify what age you left education)

____________________________________________

What is your sexual orientation?

- ◯ Bi-Sexual
- ◯ Heterosexual
- ◯ Homosexual
- ◯ Other (please specify)
- ◯ Prefer not to say

Q36 What is your religion or spiritual belief?

________________________________________________________________

Q37 How religious do you consider yourself to be?

| | Not at all | | | | Very | | | |
|---|---|---|---|---|---|---|---|---|
| | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 |
| 1 | | | | | | | | |

What is your household annual income?

- Less than £10,000
- £10,000 - £19,999
- £20,000 - £29,999
- £30,000 - £39,999
- £40,000 - £49,999
- £50,000+

What is your monthly disposable income?

- Less than £100
- £100 - £199
- £200 - £299
- £300 - £399
- £400 - £499
- £500+

How charitable do you consider yourself to be?

| Not at all | | | | | | Very | |
|---|---|---|---|---|---|---|---|
| 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 |

Do you consider yourself to have influence over others?

Not at all Very

0 1 2 3 4 5 6 7

Do you consider yourself to be easily influenced by others?

Not at all Very

0 1 2 3 4 5 6 7

How many dependent children do you have?

- ◯ 0
- ◯ 1
- ◯ 2
- ◯ 3
- ◯ 4+

---

Are you currently in a romantic relationship?

- ◯ Yes
- ◯ No

---

If yes, how long have you been with your current partner? (Years/months)

_______________________________________________________________

---

How sexually attractive do you think you are to the opposite sex?

| | Not at all | | | | | | Very | |
|---|---|---|---|---|---|---|---|---|
| | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 |

**Eyes on the Screen Effect:**

**Exploring the effect of Eye Images on Prosocial Behaviour**

Thank you for your time and participation in this study. I would now like to provide you with some further information about the purpose of this study and what I expect to find. This study was to investigate the effect of images of eyes on prosocial behaviour. In recent years, there have been multiple studies demonstrating an 'eyes on the screen' effect (e.g. Haley and Fessler, 2005) but there have been disagreements between studies in the level and effectiveness of this. This study aimed to further explore the conditions in which the 'eyes on the screen' may be effective. If you indicated that you would like to donate some of your bonus earnings to charity, this will be added to the total amount donated by survey participants after the survey has finished running. Donations will be made to UNICEF - an organisation working for children in danger. If you would **NOT** like would like us to donate to the stated charity, please tick the below box and we will credit your PayPal account with full payment within 72 hours.

If you have any queries about this research or would like to ask any further questions, please contact the researcher or research supervisor using the contact details below.

If you wish to withdraw your data from this study, please contact the Psychology Department email: psyadmin@canterbury.ac.uk. If you have been given a participant code you need to cite this. You do not have to give a reason for your withdrawal.

Once again, we would like to thank you for your valuable contribution to this research. Your participation is greatly appreciated.

Yours sincerely,

Keli Jenner
keli.jenner@canterbury.ac.uk

Research Supervisor contact details:
Dr Wendy Iredale
wendy.iredale1@canterbury.ac.uk

If you have any serious concerns about the ethical conduct of this study, please inform the Chair of the Psychology Research Ethics Panel (via the Psychology Department Office) in writing, providing a detailed account of your concern.

**APPENDIX B – Example of Study Two Questionnaire**

**Making your mind up: Individual differences in decision-making behaviour**

**Who is organising this study?**
This research is being conducted by PhD candidate Keli Jenner at Canterbury Christ Church University, UK and supervised by Dr Wendy Iredale.

**What are the aims of the study?**
This study aims to analyse individual differences in people's decision–making behaviour by tracking participant's eye movements.

You will be invited to participate in a group task which will involve a few basic mathematical questions and then you will be asked to provide some general information about yourself.

Your participation is completely voluntary and you have the right to withdraw at any time without giving a reason. Your information will be kept strictly confidential and any identifiable information will be kept separately from your data.

**Who can take part?**
Anyone over the age of 18 years old.

**Who cannot take part?**
Those under the age of 18 years old.

**What happens to the information I provide?**
Participation in this study guarantees confidentiality of the information you provide. No one apart from the researcher and research supervisor will have any access to the information you provide. Your name and any other identifying information will be stored separately from your data in a securely locked filing cabinet. Questionnaires will be stored in a securely locked room for as long as is required by the Data Protection Act, and then they will be destroyed by our confidential shredding service. The data collected for this study will be used for a student project. Once the data is analysed a report of the findings may be submitted for publication. Only broad trends will be reported and it will not be possible to identify any individuals. A summary of the results will be available from the researcher on request.

**Contact for further information:**
If you require any further information or have any queries about this study please contact the researcher:

Keli Jenner
keli.jenner@canterbury.ac.uk

Or the research supervisor:
Dr Wendy Iredale
wendy.iredale1@canterbury.ac.uk

If you wish to withdraw your data from this study, please contact the School of Psychology, Politics & Sociology at: psychology@canterbury.ac.uk. If you have been given a participant code you need to cite this. You do not have to give a reason for your withdrawal.

If you have any serious concerns about the ethical conduct of this study, please contact the panel of the Faculty of Social and Applied Sciences in writing, providing a detailed account of your concern.

Canterbury Christ Church University

---

Please tick the below boxes to show that you understand and agree with each statement. You will not be able to proceed to the study without doing so.

☐ I confirm that I have read and understood the information sheet for the above study and have had the opportunity to ask questions.

☐ I understand that my participation is voluntary and that I am free to withdraw at any time, without giving any reason, without my medical care or legal rights being affected.

☐ I understand that any personal information that I provide to the researchers will be kept strictly confidential.

☐ I agree to take part in the above study.

---

Your participant ID is: ${e://Field/ParticipantID}

Please make a note of this as you will need to quote this code if you want to withdraw your data from this study for any reason.

---

Canterbury Christ Church University


You have been automatically matched up with two other survey participants; <**Jesse**> and <**Sam**>.

In a moment, we will play a game where everyone in the group will have to decide how much of their £1.50 they will donate to the group fund and how much they will keep for themselves. Everyone will know how much each other decides to donate to the group fund.

Whatever is donated to the group fund is **doubled** then **equally divided back** to all players. How much you donate and how much you keep for yourself will affect how much money everyone else receives back from the group fund, see examples below:

**Example 1:**

- <**Jesse**> donates £1.50 and keeps 0 pence
- <**Sam**> - donates £1.50 and keeps 0 pence
- **You** - donate £1.50 and keep 0 pence

In total, all three of you have donated £4.50 to the group fund.

This is then doubled to £9 and is equally divided back to all participants.

Each of you will receive £3 each even though you initially donated all of your money to the group fund.

**Example 2:**

- <**Jesse**> donates 50 pence and keeps £1
- <**Sam**> donates 0 pence and keeps £1.50
- **You** - donate £1.30 and keep 20 pence

In total, all three of you have allocated a total of £1.80 to the group fund.

This is then doubled to £3.60 and is equally divided back to all group members.

Each of you will get £1.20 each from the group fund meaning <**Jesse**> will receive a total amount of £2.20, <**Sam**> will receive a total of £2.70 and **you** will receive a total of £1.40. Although you gave most money to the group fund, because the others did not give as much, you lost money, whereas they got more money.

Please now select the below statement that you agree with.

○ I have understood the rules to the group task

○ I have not understood the rules to the group task

You will now answer a few questions about the example to check your understanding of the rules. Please read and answer the following questions carefully. Remember the rules are: the total donated to the group fund is doubled and then divided equally back to all participants.

Each participant has £1. If all three participants (this includes yourself) donated 10 pence to the public fund how much would they keep for themselves?

○ 10 pence

○ 30 pence

○ 90 pence

If all three participants (this includes yourself) donated 10 pence to the public fund and then the public fund is doubled, how much would there be in the public fund?

- ○ 30 pence
- ○ 60 pence
- ○ £1.80

---

How much of the public fund would all three participants each receive back?

- ○ 20 pence
- ○ 40 pence
- ○ 60 pence

---

How much would each participant have in total (including the original amount they kept for themselves)?

- ○ £1.10
- ○ £1.20
- ○ £1.30

---


Please state below (in pence) how much of your £1.50 you will you donate to the group fund.

Remember, whatever is donated to the group fund is doubled and then equally divided back to all players. The amount you each decide to donate to the group fund will be displayed to the other players at the end of the game.

You can give anything from 0 pence to 150 pence (£1.50).

0 10 20 30 40 50 60 70 80 90 100 110 120 130 140 150

| | |
|---|---|
| Donated to Group Fund | |

The following amounts were donated to the group:

<**Jesse**>: 100 pence
<**Sam**>: 50 pence
**You**: ${Q2.16/TotalSum} pence

Total amount donated to group fund: $e{ q://QID75/TotalSum + 150} pence

Each participant will receive a total of: $e{ round( ((q://QID75/TotalSum + 150 ) * 2) / 3)} pence

Q2.20 Congratulations you have earned a bonus of: $e{ round(150 - q://QID75/TotalSum + ((q://QID75/TotalSum + 150) * 2) / 3)} pence

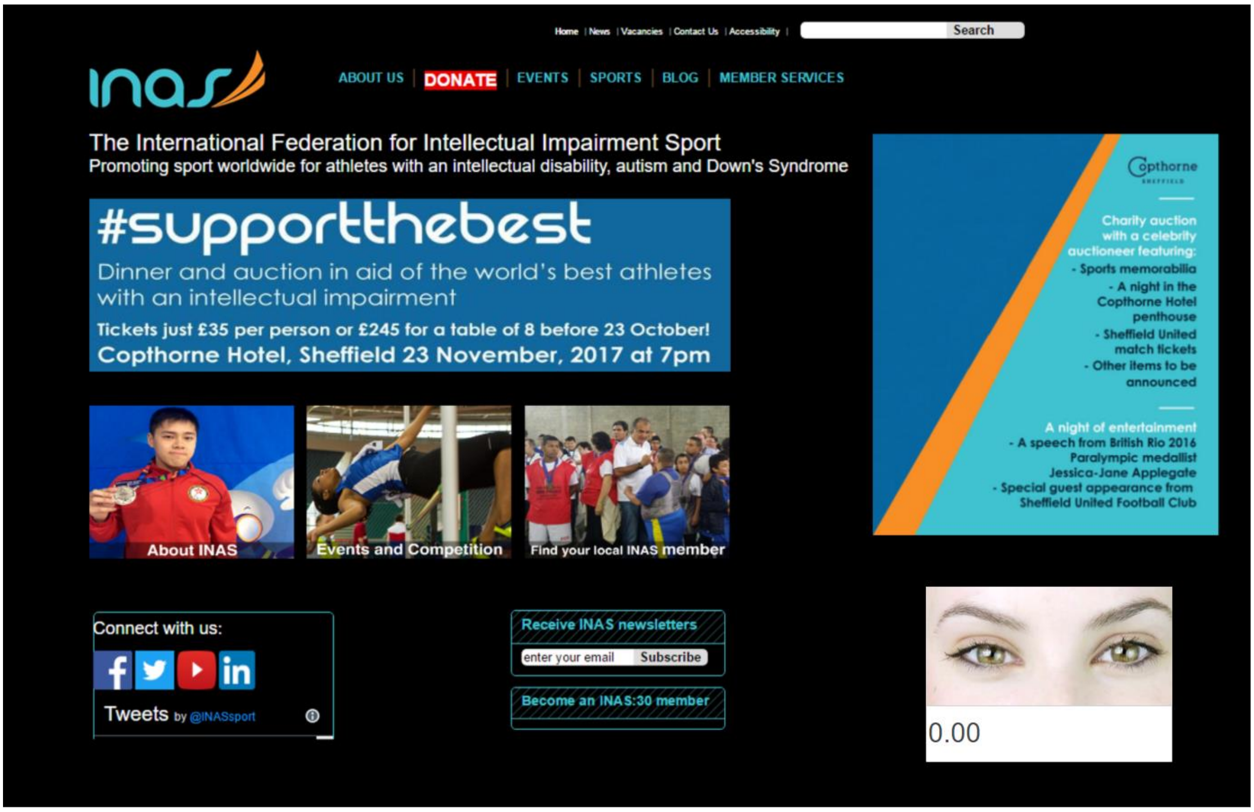
In order for us to facilitate payment, please provide a valid email address which is linked to your PayPal account. This will be paid into your account within 72 hours.

________________________________________________________________

The School of Psychology at CCCU are proud supporters of the INAS; promoting sport worldwide for athletes with an intellectual disability, autism and Down's Syndrome.

In moment, you will be taken to a INAS webpage, if you would like to donate any of your bonus that you have earnt today please indicate how much in the box on the webpage.

Any donation will be deducted from your bonus amount and your donation will be made public to INAS.


Home | News | Vacancies | Contact Us | Accessibility |
Search
ABOUT US | DONATE | EVENTS | SPORTS | BLOG | MEMBER SERVICES
The International Federation for Intellectual Impairment Sport
Promoting sport worldwide for athletes with an intellectual disability, autism and Down's Syndrome
#supportthebest
Dinner and auction in aid of the world's best athletes with an intellectual impairment
Tickets just £35 per person or £245 for a table of 8 before 23 October!
Copthorne Hotel, Sheffield 23 November, 2017 at 7pm
Copthorne
Charity auction with a celebrity auctioneer featuring:
- Sports memorabilia
- A night in the Copthorne Hotel penthouse
- Sheffield United match tickets
- Other items to be announced
A night of entertainment
- A speech from British Rio 2016 Paralympic medallist Jessica-Jane Applegate
- Special guest appearance from Sheffield United Football Club
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Tweets by @INASsport
Receive INAS newsletters
enter your email
Subscribe
Become an INAS:30 member
0.00

As part of our going work with charities, we are collecting information on people's willingness to participate in charitable activities.

Below is a list of typical volunteer activities that are required by charity groups. Please indicate which activities you would be willing to help with (you can tick as many as you like or none at all) and please state how many hours a month (in total) you would be willing to help. Please note this is just an indication of your willingness and you are not committing yourself to any volunteer activities.

| | Please tick which activity or activities you'd be willing to help with | How many hours per month would you be willing to volunteer? |
|---|---|---|
| Help organise a fundraising event | ☐ | |
| Collecting donations for charity | ☐ | |
| Providing care for the elderly | ☐ | |
| Providing care for the physically disabled | ☐ | |
| Providing care for the homeless | ☐ | |
| Providing care for mentally handicapped children | ☐ | |

---

You will now be asked a few questions regarding the group task - please answer as many questions as possible.

How did you feel when you were making your decisions during the group task?

________________________________________________________________

Did you have a sense of being watched or observed during the survey?

- ○ No
- ○ Yes

Did you have a feeling that your donations were being observed in any way?

- ○ No
- ○ Yes

How charitable do you consider yourself to be?

Not at all Very

0 1 2 3 4 5 6 7

Did you see an image of eyes during the task?

- ○ Yes
- ○ No
- ○ Not certain

If so, what was the gender of the eyes?

- ◯ Male
- ◯ Female
- ◯ Not certain
- ◯ N/A

What emotion were the eyes displaying?

______________________________________________________________

How did you take part in the survey?

- ◯ Desktop computer
- ◯ Smart Phone
- ◯ Tablet
- ◯ Laptop

What do you think this study is about? Do you think it is in line with the description at the beginning?

______________________________________________________________

Please state your age (in years)

________________________________________________________________

What is your gender?

- ○ Male
- ○ Female
- ○ Not listed (please specify) ________________________________________________
- ○ Prefer not to say

What is your ethnic origin?

________________________________________________________________

What country do you currently live in?

________________________________________________________________

Canterbury Christ Church University

**Eyes on the Screen Effect:**

**Exploring the effect of Eye Images on Prosocial Behaviour**

Thank you for your time and participation in this study. I would now like to provide you with some further information about the purpose of this study and what I expect to find.

This study was to investigate the effect of images of eyes on prosocial behaviour. In recent years, there have been multiple studies demonstrating an 'eyes on the screen' effect (e.g. Haley and Fessler, 2005) but there have been disagreements between studies in the level and effectiveness of this. This study builds on my earlier research which aimed to explore the conditions in which the 'eyes on the screen' may be effective by the addition of eye-tracking software to ascertain whether the eye images were effective. You were advised that you were playing against other participants in the public goods game when in fact there were no other people involved, these were pre-programmed responses. This was to lead each participant to believe that the decisions that they made during the task affected others which is a key component of the 'Eye on the screen' effect (Nettle et al., 2013).

If you have any queries about this research or would like to ask any further questions, please contact the researcher or research supervisor using the contact details below.

If you wish to withdraw your data from this study, please contact the Psychology Department: psychology@canterbury.ac.uk. If you have been given a participant code you need to cite this. You do not have to give a reason for your withdrawal.

Due to scientific nature, we ask you please do not discuss the contents of this survey or your answers with anybody at Canterbury Christ Church University until after the survey has closed as this may invalidate the results.

Once again, we would like to thank you for your valuable contribution to this research. Your participation is greatly appreciated.

Yours sincerely,

Keli Jenner
keli.jenner@canterbury.ac.uk

Research Supervisor contact details:
Dr Wendy Iredale

wendy.iredale1@canterbury.ac.uk

If you have any serious concerns about the ethical conduct of this study, please inform the Chair of the Psychology Research Ethics Panel (via the Psychology Department Office) in writing, providing a detailed account of your concern.

**APPENDIX C – Example of Study Three Questionnaire**

Canterbury Christ Church University

**Making your mind up: Individual differences in moral decisions**

**Participant Information**

You are invited to participate in a research study that will explore the individual differences in people's moral decisions. This study is being conducted by Keli Jenner (PhD researcher) and supervised by Dr Wendy Iredale at Canterbury Christ Church University (CCCU). Please refer to our Research Privacy Notice for more information on how we will use and store your personal data.

**Background**
This study aims to analyse individual differences in people's moral decisions.

**What will you be required to do?**
You will be presented with a series of moral dilemmas involving life-or-death situations and asked about what you would do in each situation. You will then be asked to complete a short decision-making task and then you will be asked to provide some general information about yourself for the purpose of identifying differences in decision making behaviour.

**Participant Requirements**
To participate in this research you **MUST:**

- Be over the age of 18 years old and be capable of providing informed consent to participate.
- Have access to a computer and internet connection.
- Must use either use either Internet Explorer or Microsoft Edge to take the survey (the survey does not work on other browsers).
- Have the sound on your computer turned on to an audible level (the use of headphones are recommended).

**Procedures**
Participants are asked to complete the 30-minute online survey. Please complete this survey by yourself and in one sitting (with no interruptions).

A full risk assessment has been submitted to the Ethics committee at Canterbury Christ Church University to ensure risks are kept to a minimum.

**Feedback**
Once the data is analysed, a report of the findings will be written. Only broad trends will be

reported and it will not be possible to identify any individuals. A summary of the results will be available from the researcher on request.

- Email Address
- Demographic information such as: Age, Gender, Ethnicity, Country of Residence, Sexual orientation and Income.

We have identified that the public interest in processing the personal data is:

- Research - Personal data will be processed for demographic mapping which will be utilised in the final report to present data accurately.

Personal data will be used:

- Email addresses will be used to contact the winner of the prize draw only. This information will be kept separately from the rest of the data.
- Demographic information will be processed for demographic mapping which will be utilised in the final report to present broad trends in decision-making behaviour.

Data can only be accessed by, or shared with:

- Data can only be accessed by the researcher and supervisor, the examiners of the thesis, and any ethics auditors. Personal data will be held until May 31st 2021 after the researcher's thesis submission and then securely destroyed.
- Data will not be transferred outside of the UK or the European Economic Area (EEA)

The identified period for the retention of personal data for this project:

- Personal data will be kept until May 31st 2021 to enable the researcher to analyse the results and report on broad trends.

- After this period email addresses will be deleted and personal data will be amalgamated into the rest of the research data.

- The research data will be stored via the University's secure online cloud storage which will be backed up and password protected for a period of 10 years, after which the research data will be deleted.

If you would like to obtain further information related to how your personal data is processed for this project please contact: Keli Jenner (PhD researcher) at: keli.jenner@canterbury.ac.uk.

You can read further information regarding how the University processes your personal data for research purposes at the following link: Research Privacy Notice - https://www.canterbury.ac.uk/university-solicitors-office/data-protection/privacy-notices/privacy-notices.aspx

**Dissemination of results**

Once the data is analysed a report of the findings may be disseminated in any of the following methods: PhD thesis, journal article(s), chapter in a book, conference paper and conference poster. The PhD thesis will be published in the CCCU library and will be accessible by external users.

**Process for withdrawing consent to participate**

You are free to withdraw your consent to participate in this research project at any time until the May 31st 2021 without having to give a reason. To do this please contact Keli Jenner (PhD researcher) at: keli.jenner@canterbury.ac.uk and quote your participant code (which will be provided in a moment). Please note, it will not be possible to identify your data and withdraw it without this participation code.

You may read further information on your rights relating to your personal data at the following link: Research Privacy Notice - https://www.canterbury.ac.uk/university-solicitors-office/data-protection/privacy-notices/privacy-notices.aspx

**Any questions?**
Please contact the research supervisor, Dr Wendy Iredale on 01227 923894 or at: wendy.iredale@canterbury.ac.uk School of Psychology, Politics and Sociology. Canterbury Christ Church University, North Holmes Road, Canterbury, Kent, CT1 1QU.

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Please tick the below boxes to show that you understand and agree with each statement. You will not be able to proceed to the study without doing so.

- ☐ I confirm that I have read and understand the participant information for the above project.
- ☐ I understand that any personal information that I provide to the researchers will be kept strictly confidential and in line with the University Research Privacy Notice.
- ☐ I understand that my participation is voluntary and that I am free to withdraw my participation at any time up until May 31st 2021, without giving a reason.
- ☐ I agree to take part in the above study.
- ☐ I can confirm that I meet **ALL** of the participant requirements (please see above)

---

Before you proceed to the survey, please complete the captcha below.


---

Your participant ID is: ${e://Field/PID}

Please make a note of this as you will need to quote this code if you want to withdraw your data from this study for any reason.

You are free to withdraw your participation at any time up until May 31st 2021. Please contact Keli Jenner (PhD researcher) at: keli.jenner@canterbury.ac.uk and quote your participant code. Please note, it will not be possible to identify your data and withdraw it without this participation code. You do not have to give a reason for your withdrawal.

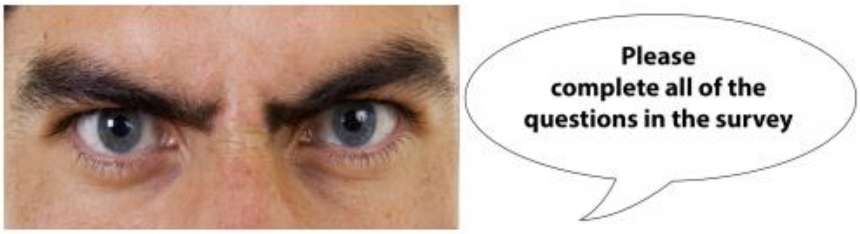
---


**Thank you!**

To thank you for your participation in this study, we would like to give you the opportunity to win £40 of Love2Shop vouchers.

To be in with a chance of winning, please provide your valid address below. The winner will be chosen at random by May 31st 2021 and only the winner will be contacted via email with confirmation. Email addresses are kept separate from your questionnaire data and the information you provide in the questionnaire is completely anonymous.

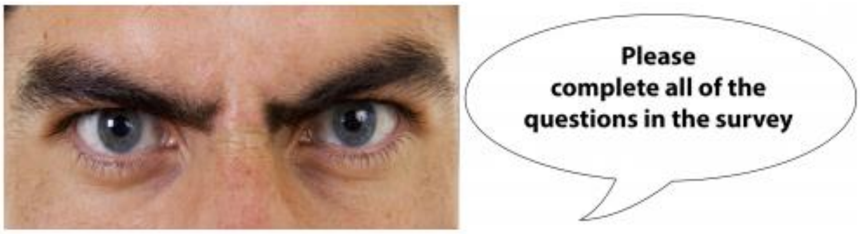
____________________________________________________________




Here in the School of Psychology at CCCU, we are raising funds to help the on-going support of Australian bushfire-affected communities by donating to [WWF's Australian Wildlife and Nature Recovery Fund].

In the event of your name being chosen in the draw, if you would like to donate any amount of your vouchers to this cause then please indicate the amount below.

____________________________________________________________

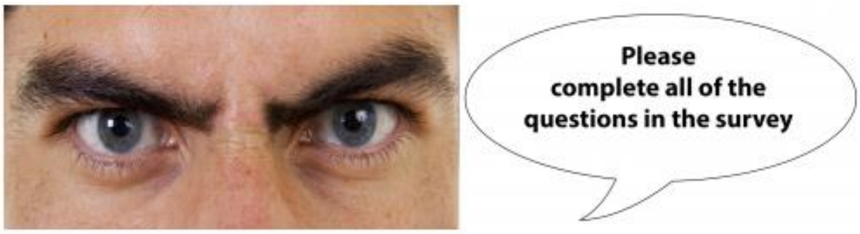



If you are selected as the winner, the amount you have indicated will be donated directly to the charity and any remaining amount will be rounded to the nearest whole pound and given to you in vouchers. Names are chosen at random and only the winner will be contacted via email with confirmation. Email addresses are kept separate from the questionnaire. The information you provide in the questionnaire is anonymous.

__________________________________________________________________________

For information purposes only (we will not collect any donations from you) - How much would you be willing to donate to this charity even if you did **not** win any vouchers?

____________________________________________________________




You are now going to be presented with five scenarios.

Please read through each scenario carefully and answer the associated question.

Please complete the task without interruption and answer the questions based solely on the information you are provided in each scenario.

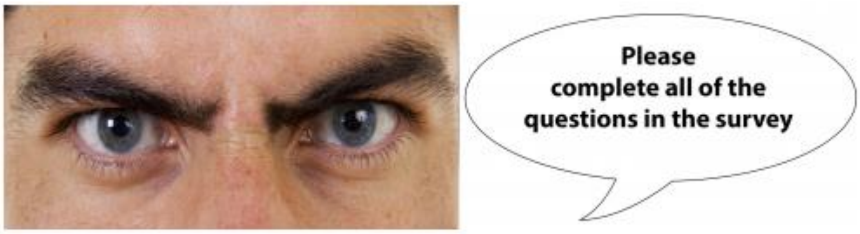
---

**Scenario:** Imagine you are a passenger on a train whose driver has just shouted that the train's brakes have failed, and who then fainted of the shock. On the track ahead are five people; the banks are so steep that they will not be able to get off the track in time. The track has a sidetrack leading off to the right, and you can turn the train onto it. Unfortunately, there is one person on the right-hand track. You can turn the train, killing the one; or you can refrain from turning the train, letting the five die.

**Q:** Is it morally permissible for you to switch the train to the side track?

- No
- Yes

---




**Scenario:** You are on a footbridge over the train tracks. You know trains and can see that the one approaching the bridge is out of control. On the track under the bridge there are five people; the banks are so steep that they will not be able to get off the track in time. You know that the only way to stop an out-of-control train is to drop a very heavy weight into its path. But the only available, sufficiently heavy weight is a large man wearing a backpack, also watching the train from the footbridge. You can shove the man with the backpack onto the track in the path of the train, killing him; or you can refrain from doing this, letting the five die.

**Q:** Is it morally permissible for you to shove the man?

- No
- Yes


**Scenario:** You are taking your daily walk near the train tracks when you notice that the train that is approaching is out of control. You see what has happened: the driver of the train saw five men walking across the tracks and slammed on the brakes, but the brakes failed and they will not be able to get off the tracks in time. Fortunately, you are standing next to a switch, which you can throw, that will temporarily turn the train onto a sidetrack. There is a heavy object on the sidetrack. If the train hits the object, the object will slow the train down, thereby giving the men time to escape. Unfortunately, the heavy object is a man, standing on the sidetrack with his back turned. You can throw the switch, preventing the train from killing the men, but killing the man. Or you can refrain from doing this, letting the five die.

**Q:** Is it morally permissible for you to throw the switch?

- No
- Yes


**Scenario:** Imagine you are in charge of a patient who is dying. All this patient needs in order for his good health to be restored is a small dose of drug X. Fortunately, you happen to have an unlimited amount of this drug X. and you can save your patient if you administer the necessary dosage at once.

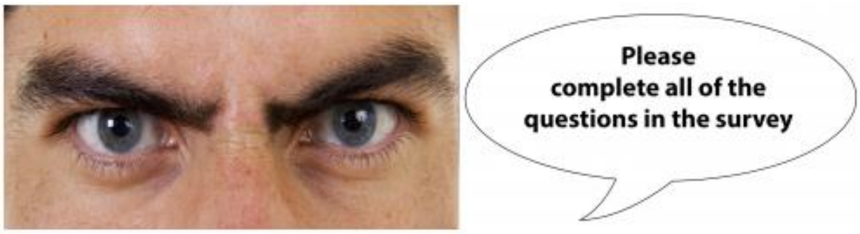
**Q:** Is it morally permissible for you to give your patient the drug?

- No
- Yes


**Scenario:** Imagine you are driving a train when the brakes fail. Ahead of you, five people are working on the track with their backs turned. They cannot see or hear the train approaching. Fortunately, you can switch the train to a sidetrack, which is completely clear, if you act immediately. If you switch the train to the sidetrack, you will save the five people working on the track. If you do not switch the train, the train will run over the five people.

**Q:** Is it morally permissible for you to switch this train to the sidetrack?

- No
- Yes




In this task, imagine that you have been randomly paired with another person, whom we will refer to as **the other.** This other person is someone you do not know and will remain mutually anonymous. All of your choices would be completely confidential.

You will be making a series of decisions about allocating resources between you and this other person. For each of the following questions, please indicate the distribution you prefer most by **selecting the button below the payoff allocations**. You can only make one selection for each question. Your decisions will yield money for both yourself and the other person. In the example below, a person has chosen to distribute the payoff so that he/she receives £50, while the anonymous other person receives £40.

**Example:**


There are no right or wrong answers, this is all about personal preferences. After you have made your decision, **select the resulting distribution of money by clicking on button below your choice**. As you can see, your choices will influence both the amount of money you receive as well as the amount of money the other receives.


| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| You Receive | 85 | 85 | 85 | 85 | 85 | 85 | 85 | 85 | 85 |
| Other Receives | 85 | 76 | 68 | 59 | 50 | 41 | 33 | 24 | 15 |
| • | • | • | • | • | • | • | • | • | • |


| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| You Receive | 85 | 87 | 89 | 91 | 93 | 94 | 96 | 98 | 100 |
| Other Receives | 15 | 19 | 24 | 28 | 33 | 37 | 41 | 46 | 50 |
| • | • | • | • | • | • | • | • | • | • |

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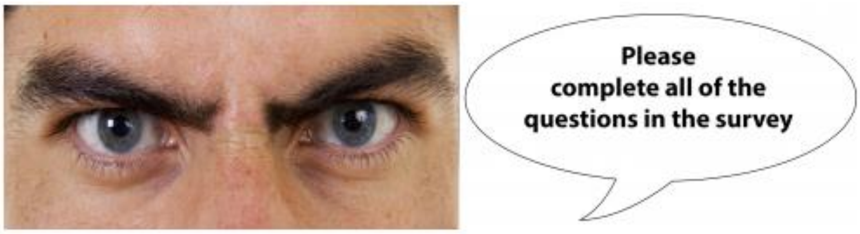


| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| You Receive | 50 | 54 | 59 | 63 | 68 | 72 | 76 | 81 | 85 |
| Other Receives | 100 | 98 | 96 | 94 | 93 | 91 | 89 | 87 | 85 |
| • | • | • | • | • | • | • | • | • | • |

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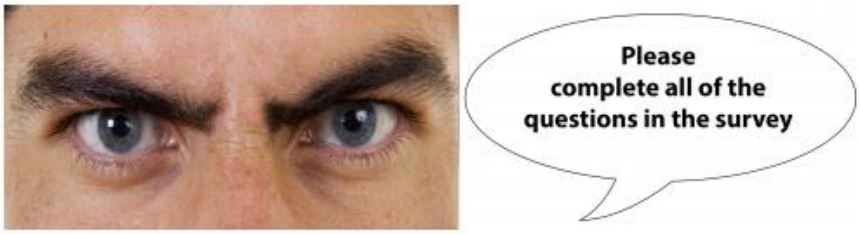


| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| You Receive | 50 | 54 | 59 | 63 | 68 | 72 | 76 | 81 | 85 |
| Other Receives | 100 | 89 | 79 | 68 | 58 | 47 | 36 | 26 | 15 |
| • | • | • | • | • | • | • | • | • | • |

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Please complete all of the questions in the survey

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| You Receive | 100 | 94 | 88 | 81 | 75 | 69 | 63 | 56 | 50 |
| Other Receives | 50 | 56 | 63 | 69 | 75 | 81 | 88 | 94 | 100 |
| • | • | • | • | • | • | • | • | • | • |

Canterbury Christ Church University

Please complete all of the questions in the survey

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| You Receive | 100 | 98 | 96 | 94 | 93 | 91 | 89 | 87 | 85 |
| Other Receives | 50 | 54 | 59 | 63 | 68 | 72 | 76 | 81 | 85 |
| • | • | • | • | • | • | • | • | • | • |

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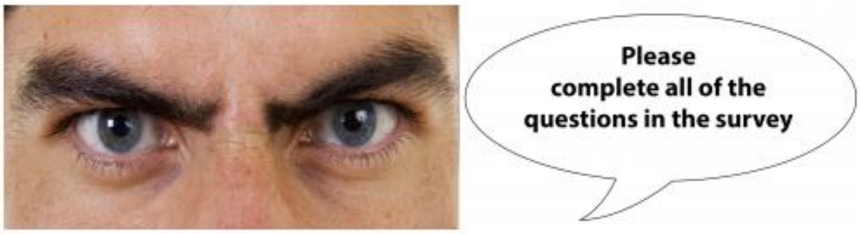


Imagine that you have to distribute a sum of money between yourself and another person, whom we simply refer to as **the other**. You will never knowingly meet or communicate with this person, nor will s/he ever knowingly meet or communicate with you. In this decision task, both you and the other will be making choices by selecting either option A, B, or C. Your own choice will produce points for yourself and the other person. Similarly, the other's choices will produce points for him/her and for you. Therefore, the total number of points you receive depends on his/her choices and your choices as well.

**Example:**

| | A | B | C |
|---|---|---|---|
| You Receive | 500 | 500 | 550 |
| Other Receives | 400 | 500 | 300 |
| | ○ | ○ | ◉ |

In this example, if you choose C you would receive 550 points and Other 300 points. At the same time, Other is also choosing between A, B, and C. If s/he chooses A, s/he receives 500 and you receive 400. So the total number of points that you receive and that Other receives is determined by your own choice in combination with that of Other.


Please select A, B, or C depending on which column you prefer the most.

| | A | B | C |
|---|---|---|---|
| You Receive | 480 | 540 | 480 |
| Other Receives | 80 | 280 | 480 |
| • | • | • | • |


Please select A, B, or C depending on which column you prefer the most.

| | A | B | C |
|---|---|---|---|
| You Receive | 560 | 500 | 500 |
| Other Receives | 300 | 500 | 100 |
| • | • | • | • |


Please select A, B, or C depending on which column you prefer the most.

| | A | B | C |
|---|---|---|---|
| You Receive | 520 | 520 | 580 |
| Other Receives | 520 | 120 | 320 |
| • | • | • | • |


Please select A, B, or C depending on which column you prefer the most.

| | A | B | C |
|---|---|---|---|
| You Receive | 500 | 560 | 490 |
| Other Receives | 100 | 300 | 490 |
| • | • | • | • |


Please select A, B, or C depending on which column you prefer the most.

| | **A** | **B** | **C** |
|---|---|---|---|
| You Receive | 560 | 500 | 490 |
| Other Receives | 300 | 500 | 90 |
| • | • | • | • |


Please select A, B, or C depending on which column you prefer the most.

| | **A** | **B** | **C** |
|---|---|---|---|
| You Receive | 500 | 500 | 570 |
| Other Receives | 500 | 100 | 300 |
| • | • | • | • |

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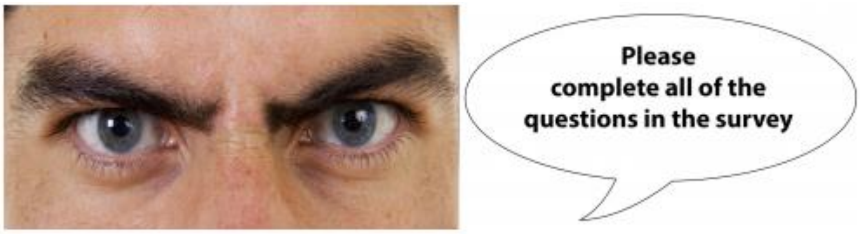


Please select A, B, or C depending on which column you prefer the most.

| | A | B | C |
|---|---|---|---|
| You Receive | 510 | 560 | 510 |
| Other Receives | 510 | 300 | 110 |
| • | • | • | • |

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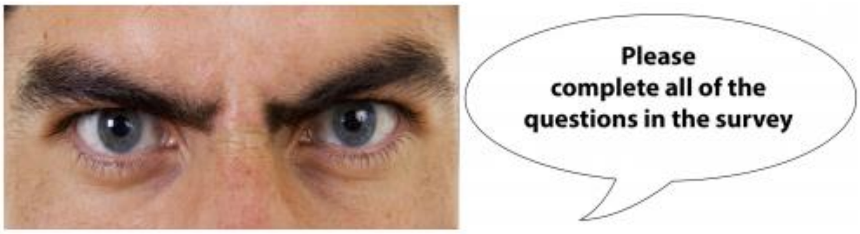


Please select A, B, or C depending on which column you prefer the most.

| | A | B | C |
|---|---|---|---|
| You Receive | 550 | 500 | 500 |
| Other Receives | 300 | 100 | 500 |
| • | • | • | • |

Canterbury Christ Church University

**Please complete all of the questions in the survey**

Please select A, B, or C depending on which column you prefer the most.

| | A | B | C |
|---|---|---|---|
| You Receive | 480 | 490 | 540 |
| \| | \| | \| | \| |
| \| | \| | \| | \| |
| Other Receives | 100 | 490 | 300 |
| • | • | • | • |

---

Did you get the sense that either you or your answers were being watched at all?

- No
- Yes

---

Do you remember seeing an image of eyes at all during this questionnaire?

- No
- Yes

---

If yes, what emotion do you think the eyes were portraying?

__________________________________________________________

---

How strong is the emotion being expressed by the eyes?

| Not at all 1 | 2 | 3 | 4 | 5 | 6 | Very 7 |
|---|---|---|---|---|---|---|
| ○ | ○ | ○ | ○ | ○ | ○ | ○ |

How attractive do you think the eyes were?

| Not at all 1 | 2 | 3 | 4 | 5 | 6 | Very 7 |
|---|---|---|---|---|---|---|
| ○ | ○ | ○ | ○ | ○ | ○ | ○ |

Which internet browser did you use to take this survey?

- Internet Explorer
- Chrome
- Microsoft Edge
- Safari
- Other (please specify) ________________________________________________

Were there any other people in the same room as you when you were taking this survey?

- No
- Not Sure
- Yes ________________________________________________

Please state your age (in years)

________________________________________________________________

What is your gender?

- Male
- Female
- Not listed (please specify) ____________________________________________________
- Prefer not to say

What is your ethnic origin?

______________________________________________________________________

What country do you currently live in?

______________________________________________________________________

What is your sexual orientation?

- Bi-Sexual
- Heterosexual
- Homosexual
- Other (please specify) ____________________________________________________
- Prefer not to say

What is your household annual income?

- Less than £10,000
- £10,000 - £19,000
- £20,000 - £29,000
- £30,000 - £39,000
- £40,000 - £49,000
- £50,000+

What is your monthly disposable income?

- Less than £100
- £100 - £199
- £200 - £299
- £300 - £399
- £400 - £499
- £500+

In the future, we will be running similar studies to this one. However, it is unlikely that we will be able to offer any opportunities to win vouchers.

If you would be willing to help us by participating in these future studies, please indicate by selecting the box below.

- Yes
- No

Please click on the button below to confirm that you have read this statement and have completed the survey (please make sure you still click through to the last page to get your course credit).

Canterbury Christ Church University

**Eyes on the Screen Effect:**
**Exploring the effect of Eye Images on Prosocial Behaviour**

Thank you for your time and participation in this study. I would now like to provide you with some further information about the purpose of this study and what I expect to find. This study was to investigate the effect of images of eyes on prosocial behaviour (i.e. moral decisions). In recent years, there have been multiple studies demonstrating an 'eyes on the screen' effect (e.g. Haley and Fessler, 2005) but there have been disagreements between studies in the effectiveness of this. This study aimed to further explore the conditions in which the 'eyes on the screen' may be effective.

Please note that if you have chosen to enter the prize draw and your name is chosen, the amount you have indicated on this form will be donated directly to the charity and any remaining amount will be rounded up and given to you in vouchers. Names are chosen at random and only the winner will be contacted via email with confirmation.

You were asked about your willingness to participate in future studies as part of a prosocial measure. You will **NOT** be contacted about participating any future studies.

You will not be contacted about the results of this project but if you have any queries about this research or would like to ask any further questions, please contact the researcher or research supervisor using the contact details below. Alternatively, it is expected that the results will be written up for publication in journal articles.

If you wish to withdraw your data from this study, you have until 31st May 2021 to do so. To do this please contact Keli Jenner (PhD researcher) at: keli.jenner@canterbury.ac.uk and quote your participant code. Please note, it will not be possible to identify your data and withdraw it without this participation code. You do not have to give a reason for your withdrawal.

Once again, we would like to thank you for your valuable contribution to this research. Your participation is greatly appreciated.

Yours sincerely,

Keli Jenner
keli.jenner@canterbury.ac.uk

Research Supervisor contact details:
Dr Wendy Iredale
wendy.iredale@canterbury.ac.uk

01227 923894

If you have any serious concerns about the ethical conduct of this study, please inform the Chair of the Psychology Research Ethics Panel (via the Psychology Department Office) in writing, providing a detailed account of your concern.

## APPENDIX D – Study Two Supplementary Analysis

**Hypothesis 4.** There will be a significant difference between conditions in the total time (ms) spent looking at the AOI at location point location one (at the top of the public goods game instructions), location two (at the point where participants were playing the public goods game), and location three (at the point of donating to charity).

A series of Kruskal-Wallis H tests were conducted to explore whether there were any significant differences between conditions in the total time (ms) that participants spent looking at the AOI at locations one (total time ranged from 0 to 5.23 ms), two (total time ranged from 0 to 7.16 ms), and three (total time ranged from 0 to 5.43 ms).

The total time at location one (the instructions page) did not differ significantly between the static (*Mdn* = 0.49, IQR = 0.12 – 1.99), blinking (*Mdn* = 0.95, IQR = 0.69 – 1.71) and no-eyes (*Mdn* = 0.15, IQR = 0.00 – 0.77) groups, $H(2) = 5.659$, $p = .059$, $\varepsilon^2 = 0.12$, 90% CI [0.04, 1.00]. However, those in the static (*Mdn* = 1.29, IQR = 0.37 – 1.75) and blinking (*Mdn* = 0.50, IQR = 0.24 – 2.58) groups spent significantly more time looking at the AOIs at location two (during the public goods game) than those in the no-eyes (*Mdn* = 0.05, IQR = 0.00 – 0.52) groups, $H(2) = 11.591$, $p = .003$. Post hoc Mann-Whitney U test revealed that the total time was significantly different between the no-eyes and static groups, $p = .005$, $r = -0.54$, 90% CI [-0.63, -0.43] and the no-eyes and blinking groups, $p = .023$, $r = -0.49$, 90% CI [-0.59, -0.38] but not between the static and blinking groups, $p = 1.000$, $r = 0.15$, 90% CI [0.01, 0.28]. Nevertheless, the total time at location three (at the point of donating to charity) did not differ significantly between the static (*Mdn* = 0.75, IQR = 0.01 – 1.41), blinking (*Mdn* = 0.75, IQR = 0.00 – 1.23) and no-eyes (*Mdn* = 1.23, IQR = 0.37 – 2.52) groups, $H(2) = 1.651$, $p = .438$, $\varepsilon^2 = 0.04$, 90% CI [0.01, 1.00].

**Testing habituation to the eye images.** Non-parametric Friedman tests were conducted to assess whether there were any significant differences between the static and blinking groups in the total time (ms) spent observing the corresponding AOI across location one (at the top of the public goods game instructions), location two (at the point where participants were playing the public goods game), and location three (at the point of donating to charity).

The total time that participants in the static eyes group spent looking at the AOI did not significantly change between location one (*Mdn* = 0.49, IQR = 0.12 – 1.99) and location two (*Mdn* = 1.29, IQR = 0.37 – 1.75), $r$ = - 0.23, 90% CI [-0.45, 0.02], location one and location three (*Mdn* = 0.75, IQR = 0.01 – 1.41), $r$ = - 0.02, 90% CI [-0.27, 0.23], or location two and location three, $r$ = - 0.07, 90% CI [-0.31, 0.18], $\chi^2$ (2, $N$ = 14) = 1.48, $p$ = .492.

The total time that participants in the blinking group spent looking at the AOI also did not significantly change between location one (*Mdn* = 0.95, IQR = 0.01 – 1.41) and location two (*Mdn* = 0.50, IQR = 0.24 – 2.58), $r$ = -0.13, 90% CI [-0.37, 0.12], location one and location three (*Mdn* = 0.75, IQR = 0.00 – 1.23), $r$ = -0.13, 90% CI [-0.37, 0.12], or location two and location three, $r$ = -0.03, 90% CI [-0.28, 0.22], $\chi^2$ (2, $N$ = 15) = 4.133, $p$ = .127 (see Figure Appendix.1).

**Figure Appendix.1**

*Total time (ms) participants spent looking at the eye stimuli at each location*