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Social cues in context the interdependence between social cue senders and receivers

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Social cues in context: The interdependence between social cue senders and receivers.

By

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Summary

Most of the research exploring social communication has focused on the 'sender' perspective, examining how and why people choose to produce the cues they send to others. This thesis explores the experience of social interaction from the 'receiver' perspective. Broadly, this work examines how receivers perceive and interpret social cues and make social judgments, depending on senders' states and intentions. It relies on data from both laboratory-based experimentation and from naturalistic face-to-face interactions.

The first section of the thesis examines how changing a receiver's internal social state, i.e., manipulating feelings of social 'need,' alters the utility or subjective desirability of a social reward, specifically, a genuine smile. My experimental findings show that high states of social need enhance the utility of genuine smiles and cause the devaluation of polite smiles – important social tokens in their own right. These findings extend to the face-to-face social environment, in which I show that this social state manipulation changes behaviour, including the use of smiles, and ultimately a dyad's shared experience.

In the second part of this work, I explore how judgments receivers make about senders in one context influence their interpretation of the same senders in a new setting. I ask, for example, how the presence of different types of social cues shape receiver judgments and the extent to which these serve as useful and valid cues to future sender behaviour. Findings show that when receivers make judgments about senders in a naturalistic context, these judgments do not enhance their ability to decode senders' behaviour in a new setting. However, senders do signal some traits honestly, e.g., trustworthiness, and these traits induce biases in receiver interpretations. Specifically, senders' affective cues appear to bias receiver ratings such that expressions of positive affect induce more positive ratings for high trustworthy senders (although not for those merely rated as high trustworthy), and vice versa for low trustworthy senders.

Finally, I ask whether a sender's prior beliefs about a receiver's behaviour can influence that receiver's behaviour in a specifically predictable way. I did this in the context of three "experimenter effects" studies. The results of this work show that experimenters unwittingly serve as stimuli in the experiments that they conduct, and can elicit specific behavioural patterns in their participants.

Taken together, these findings begin to uncover some of the complexities inherent in real-world social interactions and demonstrate the interdependence between senders and receivers in terms of perceptions, motivations and actions.

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Chapter 1

General Introduction

Humans are social beings and thus social contact is an essential part of life. As such, social interactions have a profound impact on day-to-day life affecting decision-making, behaviour, and overall quality of life (Cohen, 2004). As well as being unavoidable, regular participation in relationships and social interactions fulfils a critical human 'need' (Maslow, 1962), without which, normal development cannot occur (Doise, Mugny, & Perret-Clermont, 1975). In particular, attachment theorists emphasize the importance of social relationships and suggest that from early infancy the reciprocal exchanges of social cues between babies and their mothers determine the course of relationship development throughout the lifespan (e.g., Bowlby, 1958). Thus, the rich and complicated interactions and relationships adults enjoy have their foundations in the social reciprocity of early life. These foundations influence individuals' experiences, decision-making and future behaviours (Bowlby, 1969; Honig, 2002; Howes & Ritchie, 2002), and consequently, influence those of their interaction partners as well.

Social interactions evolve on a moment-to-moment basis. In order to be sensitive to changes in the social environment, interaction partners must attend to cues conveyed in several modalities simultaneously. For example, nonverbal and paralinguistic cues may help people interpret the verbal content of an utterance (Knapp & Hall, 2009; Siegman, 1987), as might an individual's prior knowledge about, or judgments of, a speaker. Unfortunately, this complexity, which enhances the experience of social life, hinders scientific exploration. Researchers must take a reductionist approach to the study of social phenomena, focusing on single cues in isolation, rather than on the relationships between cues and the interdependence of senders and receivers. The present work aims to bridge this gap by examining multiple social cues simultaneously, while attending to the relationship between cue senders and receivers.

The face is a primary social stimulus. Faces contain considerable information, both as static stimuli, and when engaged in dynamic expressive behaviour. Theorists as early as Darwin (1872/1998) and Duchenne (1862/1990), highlighted the importance of the face in affective display. This work led researchers to seek and develop a body of evidence showing that sender displays serve expressive functions, as they are external expressions of internal emotional states (Ekman, 1972; Izard, 1977). However, faces also serve communicative functions, signalling senders' intentions, traits, attitudes, beliefs, etc. (Fridlund, 1994; Manstead, 1991). Although there is empirical evidence supporting both sides of the debate (Boucher & Carlson, 1980; Ekman & Friesen, 1971; Fischer, Manstead & Zaalberg, 2003; Kraut & Johnston, 1979) this work has tended to focus on the cues senders produce, paying less attention to how receivers interpret and use those cues (Hess, Kaplan & Scherer, 1998; Salovey & Mayer, 1990).

Importantly, both partners in a face-to-face social interaction occupy the roles of both sender and receiver of social cues. The task of conveying intended information and interpreting another's intentions switches between interaction partners in a smooth and coordinated manner. In this way, the senders and receivers of social cues are interdependent, allowing the interaction to evolve on a moment-by-moment basis. Thus, it is important not only to understand senders' signals, but also how receivers interpret, understand and respond to those signals as they switch between the roles of sender and receiver.

It is important to remember that as social interactions unfold, the only objectively observable features of an interaction are the verbal, nonverbal and paralinguistic social cues that partners exchange. However, from the perspective of both senders and receivers, these observable cues do not exist in isolation. For senders, behaviour is defined by their traits,

emotions, goals, prior beliefs about their interaction partners and the situations in which they find themselves (including information from previous interactions with a particular partner). For receivers, the ability to interpret a sender's signals is coloured by a similar set of factors. Thus, while superficially an interaction is a coordinated exchange of social cues, beneath the surface, interaction partners are engaging in far more in-depth exchanges, often with remarkable automaticity.

Predictably then, scientific investigations have found it difficult to capture the multiple layers of information exchanged while attending to the unique and interdependent aspects of senders' and receivers' social experiences. The goal of this work is to begin the process of uncovering some of these complexities. In the context of this work and the literature, a 'sender' is a signaller of information (Hall, Murphy, & Mast, 2007), referred to in other work as an actor (Ames & Johar, 2009) or a target (Agthe, Sporrle, & Maner, 2011). The cues that senders signal range from static appearance-based cues, such as attractiveness, to dynamic expressive cues, such as smiles, which are presented statically, as in a photograph, or dynamically, as in a film clip or a live interaction. The 'receiver' is the perceiver or interpreter of the social signal, (Hall et al., 2007) also referred to as the observer or judge (Carlson, Furr, & Vazire, 2010; Verplaetse, Vanneste, & Braeckman, 2007). Receivers make judgements about sender traits (Hall, Andrzejewski, Murphy, Mast, & Feinstein, 2008; Kramer, King, & Ward, 2011), states (Niedenthal, Halberstadt, Margolin, & Innes Ker, 2000), and social intentions (Todorov, 2005; 2008) based on both static and dynamic cues. Finally, I note that the expression and interpretation of cues is not a fixed process and changes in sender and receiver states and contexts lead to alterations in social exchanges. Ultimately, these exchanges can guide behaviour. It is therefore important to

consider how individuals are able to influence each other by merely presenting overt signals.

Social Signals

Research shows that people make automatic and rapid judgements of other people based solely on their appearance (Hassin & Trope, 2000; Todorov, Said, & Engell, 2008b; Willis & Todorov, 2006). These judgements range from appraisals about physical appearance, e.g., gender, attractiveness (Aharon et al., 2001; Perrett et al., 1998), to assessments of latent personality characteristics, e.g., extraversion, agreeableness (Kramer et al., 2011; Little & Perrett, 2007; Penton-Voak, Pound, Little, & Perrett, 2006) and social intentions e.g., trustworthiness (Stirrat & Perrett, 2010; Todorov, 2008). Typically, research participants make these judgments from still photos of faces, often neutrally posed. For example, many experiments rely on static images of faces that are computer generated (Oosterhof & Todorov, 2008; Todorov, Baron, & Oosterhof, 2008a) averaged photographs of many individuals (Jones, Kramer, & Ward, 2012; Penton-Voak et al., 2006), or photographs of individuals about whom the researchers know nothing (e.g., photographs from face databases; (Fernández-Dols, Carrera, Barchard, & Gacitua, 2008; Olivola & Todorov, 2010a; van't Wout & Sanfey, 2008). These methods treat senders as simple stimuli, rather than as interaction partners who possess and can communicate particular traits.

Some of the characteristics participants must judge, such as age, sex and to a degree race, are generally easy to verify from static images without prior knowledge of a sender (Johnston, Kanazawa, Kato, & Oda, 1997). However, research also suggests that senders unconsciously signal information about latent personality traits and general social intentions in their faces (Penton-Voak et al., 2006; van't Wout & Sanfey, 2008). Indeed, the literature shows that receivers make reliable judgements about sender extraversion,

conscientiousness and trustworthiness based on static images of faces with neutral expressions (Adams Jr, Nelson, & Soto, 2012; Hess, Adams, & Kleck, 2009; Penton-Voak et al., 2006; Todorov, 2008). Research using composite faces morphed or 'averaged' across multiple senders who all rate themselves as high in a particular trait, shows that receivers can discriminate between composites of senders who rate themselves as high versus low in extraversion and other traits (Kramer & Ward, 2010; Penton-Voak et al., 2006). This research supports the idea that subtle signals of extraversion are present in the face and become enhanced or accentuated when individual differences in appearance are averaged together (Jones et al., 2012; Kramer & Ward, 2010; Little & Perrett, 2007).

Research using facial composites shows that receivers can discriminate between faces that are high and low in traits such as agreeableness, extraversion, and conscientiousness, as well as physical health (Kramer & Ward, 2010; Little & Perrett, 2007; Penton-Voak et al., 2006). Researchers have also attempted to determine the specific facial features that signal these characteristics by examining, for example, internal features (i.e., eyes, nose and mouth (Kramer & Ward, 2010); width to height ratios (Stirrat & Perrett, 2010; Wong, Ormiston, & Haselhuhn, 2011) and other cues including laterality differences (Kramer & Ward, 2011; Lindell & Savill, 2010) to determine which ones receivers require to make accurate judgments of particular traits. However, the degree to which these results generalize to interpersonal settings in which such traits are important is unclear. When people make judgements about senders in real-world settings, individual differences in sender appearance might overwhelm the subtle trait related-cues that are enhanced in the process of producing composite faces. Interestingly, data using more naturalistic stimuli suggests that cues of latent traits are not as visible on individual senders' faces as this body of work implies (Olivola & Todorov, 2010a).

In addition to static appearance-based signals, senders also signal information about their states and traits in the dynamic facial expressions that they make (Ambady, Hallahan, & Rosenthal, 1995; Hall et al., 2008). For example, extraverted males gesture more whereas extraverted females tend to display more facial expressions (Riggio & Friedman, 1986). Extraversion also predicts eye-gaze behaviour. For example, highly extraverted people are likely to fixate their gaze for shorter periods (Rauthmann, Seubert, Sachse, & Furtner, 2011). In a similar vein, participants who report high levels of neuroticism may produce many fewer displays of positive emotion than their less neurotic peers (Buswell & Keltner, 1995). Altruism may also manifest in expressive behaviour such that highly altruistic individuals tend to display more smiles than less altruistic people (Oda, Naganawa, Yamauchi, Yamagata, & Matsumoto-Oda, 2009) and co-operators in an investment game may display both more positive and more negative emotional expressions than do non-co-operators (Schug, Matsumoto, Horita, Yamagishi, & Bonnet, 2010). Thus, higher frequencies of positive affective displays may signal trustworthiness (Krumhuber et al., 2007), a finding supported by the theoretical work of Boone and Buck (2003). In as much as facial expressions are the externalisation of an internal emotional state, the type of dynamic display changes dependent on the sender's current mood state (Ekman, 1993). Similarly, states of high arousal change the dynamic cues that senders display. People suffering from high social anxiety fidget more, reciprocate fewer smiles and talk more about themselves in an interaction (Heerey & Kring, 2007).

Although the current work focuses primarily on visual cues, i.e., static "unfakeable" signals and dynamic facial expressions, verbal utterances are another important social signal that allow the expression of emotion, state, or intention (Fussell, 2002). The content of verbal dialogue is an important and explicit source of information whose meaning is

modified by the accompanying paralinguistic cues (Scherer, Uno, & Rosenthal, 1972). In addition, behaviour is an important signal of sender traits and social intentions. The act of being sociable at a party is a behavioural signal of extraversion whereas returning a lost wallet is likely a good indicator of trustworthiness.

Taken together, these findings suggest that, in controlled settings, there may be features in the static neutral face that signal elements of senders' characteristics, both in terms of appearance-based characteristics and latent personality traits. In addition, senders signal information about their states, stable traits, and intentions in dynamic facial expressions, verbal language and behaviour. However, in order for a signal to perform its intended function, it is important to consider the receiver of these signals and what these displays mean to those who perceive them.

Social Judgements

The judgements receivers make about senders have important social outcomes. They influence the decisions that receivers make at both the societal level, e.g., voting preferences (Olivola & Todorov, 2010b) and criminal sentencing (Porter, ten Brinke & Gustaw, 2010), and at the more interpersonal level, e.g., the decision to trust another (Todorov, Said, & Engell, 2008b; van't Wout & Sanfey, 2008). Because these judgments are ubiquitous and appear to be automatic (Bar, Neta & Linz, 2006; Berry & Brownlow, 1989; Hassin & Trope, 2000; Olsen & Marshuetz, 2005; Willis & Todorov, 2006), they have profound effects on interpersonal life. Therefore, the question of whether they are valid readings of sender traits and whether they help receivers to predict or use other sender cues is an important one.

One way to determine whether sender cues are valid is to learn about senders' latent traits, via self-report, behavioural observation and/or ratings from close others (Murphy, Hall, & Colvin, 2003), and then to learn how receivers judge those senders. The degree to which receiver judgments are accurate depends on the degree to which those judgments differ from sender data. This is known as agreement accuracy (Carney, Colvin, & Hall, 2007) and is the most common way in which researchers measure accuracy (e.g., Hall et al., 2008; Kramer & Ward, 2011). Using this method, research suggests that receivers make accurate judgements of sender extraversion and agreeableness, (Hall et al., 2008; Jones et al., 2012; Kramer et al., 2011). However, simply knowing (or believing) that a sender is extraverted, trustworthy or agreeable is unimportant until a receiver is in a position to interact with that sender. The primary purpose of making accurate interpersonal judgments is therefore to enhance the ability to interpret or predict a person's behaviour in an interaction (Fiske & Linville, 1980).

Predictive accuracy is the degree to which a receiver's ratings of a sender's traits allow the receiver to predict that sender's behaviour (Carney et al., 2007). For example, if a receiver rates a sender as high in extraversion, the judgement might be considered accurate if the sender has a highly active social life (Borkenau & Liebler, 1995). Research indicates that ratings of senders' social intentions, e.g., trustworthiness, have some predictive accuracy (Naganawa, Yamauchi, Yamagata, Matsumoto Oda, & Oda, 2010; Oda, Naganawa, Yamauchi, Yamagata, & Matsumoto-Oda, 2009). The degree to which senders demonstrate behaviour indicative of trustworthiness can be captured in experimental settings using games in which participants decide whether to cooperate or defect or when they make and return investments with other participants (Berg, Dickhaut & McCabe, 1995). Thus, receiver ratings of photos of more and less trustworthy senders can be correlated with senders'

behaviour. This research suggests that receiver ratings predict sender behaviour in trustworthiness games (e.g., Oda, Yamagata, Yabiku, & Matsumoto Oda, 2009b; Stirrat & Perrett, 2010). Predictive accuracy is predominantly examined in the context of dynamic social cues, for example, using photographs in which senders display facial expressions such as smiles or frowns (Nowicki & Duke, 1994), or using film clips of sender behaviour from which receivers make judgements about senders' characteristics. Taken together, research assessing both agreement and predictive accuracy shows that, to some extent and, arguably more practically for predictive accuracy, senders signal valid cues of their latent traits and intentions. However, an alternative method of exploring senders' facial signals has been to focus on receiver judgements of senders (e.g., Little & Dunbar, 2008; Penton-Voak et al., 2006), the idea being that if receivers make consistent judgements of senders, this is due to the fact that senders are signalling information about themselves.

Research suggests that there are signals that individual receivers consistently recognise across senders (Brown & Moore, 2002; Hassin & Trope, 2000). However, the way that people make social judgements hints that senders may have the ability to manipulate these judgements. Research suggests that people make inferences about neutral faces that are based on the 'emotionality' of the neutral face. That is, social judgments are based on properties of facial structure that mimic emotional expressions (Adams Jr et al., 2012). For example, a face with a slightly upturned mouth is likely to look happier and therefore more agreeable (Borkenau & Liebler, 1995). Impressions of trustworthiness appear to be particularly vulnerable to such manipulations, as research shows that researchers can enhance receiver judgements of trustworthiness by digitally increasing the degree to which a neutral face mimics approach-related expressions such as smiles (Oosterhof & Todorov, 2009). Conversely, by digitally manipulating faces so that they subtly mimic angry

expressions, trustworthiness judgments decrease (Oosterhof & Todorov, 2009). Senders may easily make subtle alterations to facial appearance either by engaging facial musculature or with the use of more permanent cosmetic procedures (Neal & Chartrand, 2011). Thus, the manipulability of social signals suggests that measuring the reliability of receiver judgements is an imperfect measure of receivers' ability to interpret and understand sender signals.

Together, measures of the validity of sender signals and the reliability of receivers' judgements of senders suggest that there may indeed be both valid signals of sender traits, as well as mechanisms that receivers use to judge senders, however, both methods treat senders as simple stimuli. In live interactions the exchange of social signals is far more complex and prior beliefs, traits, affect and memory may influence the cues that senders signal and the interpretations that receivers make of senders. Therefore, it is important to consider the stability of senders' traits and state over time and how the interaction between states and traits might influence both senders and receivers.

Stability of traits/judgements/behaviours

The essentialist view of personality, the idea that personality remains the same across different situations, has received much criticism, with theorists negating the idea of "situational invariance" (Michel & Shoda, 1995) that this position presents. Indeed, disposition, or personality has been described as a pattern of behaviour (Tellegen, 1991), which is not wholly consistent (Allport, 1961). Although there is general agreement that personality depends on place, tasks and interactions (Lewis, 2001), there is evidence suggesting that there is some consistency across the lifespan in personality. Evidence using test-retest correlations and longitudinal studies, shows that the rank order stability of personality traits is consistent, i.e., if someone is highly extraverted relative to their other

traits, that is unlikely to change (Caspi & Roberts, 2001; Caspi, Roberts, & Shiner, 2005). However, given that people may tap into different aspects of their personality at different times, the research on social signalling seems to adopt the out-dated essentialist point of view without considering the fact that a sender's actual behaviour may vary tremendously from one situation to the next.

Given that evidence suggests a fair amount of variance in personality and behaviour across situations, it seems remiss to assume that sender cues are invariant and valid signals of latent traits. For example, shifts in status, which likely accompany transitions between multiple roles in daily life, may lead to changes in displays (Hall, Coats, & LeBeau, 2005). Research shows that power influences dynamic displays of affect. People who hold power in a social situation gesture more with their hands, and tend to maintain more eye contact when speaking and engage in less eye contact when listening (Dovidio, Ellyson, Keating, Heltman, & Brown, 1988; Dovidio, Brown, Heltman, Ellyson, & Keating, 1988). In addition, research shows that manipulations that alter a participant's power role cause changes in their smile displays. In one study, although people in high and low power positions displayed the same number of smiles in a 5-minute interaction, the smiles displayed by high power individuals were indicative of positive affect whereas low power participants tended to smile as a placating gesture (Hecht & LaFrance, 1998). There are likely many situations in which situational context influences sender displays, for example, spending time with family or being interviewed for a job (Howard & Ferris, 1996). Thus, the judgments receivers make about a particular sender may vary dramatically depending on social context.

If sender signals are valid cues of sender traits and behaviour, this should lead to stability in receiver judgements over time. However, research suggests that there are individual differences between senders and between receivers that moderate how well

senders can be read. These include expressivity (Kring, Smith, & Neale, 1994), along with receivers' ability to engage in social monitoring and consequently interpret sender behaviour (Ickes, Stinson, Bissonnette, & Garcia, 1990). In addition, certain traits may be easier to judge than others, partly because these traits may manifest more in observable behaviour (Gray, 2008). For example, extraverted people may indeed exhibit more sociable behaviour whereas conscientious individuals may take more care with personal appearance and grooming. Likewise, receiver judgements are also subject to bias. For example, research shows women tend to be more accurate judges of emotional expressions with the exception of anger, a trait that men judge more accurately (Hall, 1984). Thus, cultural differences in the expression and interpretation of emotion exist and can be learned (Elfenbein and Ambady, 2002, 2003). These biases however, are not limited to different cultural groups. Merely categorising people into an in-group or out-group, according to a fabricated personality type, changes receivers' judgement accuracy because people are more motivated to accurately interpret the behaviour of in-group members (Young & Hugenberg, 2010). Similarly, people in a good mood show a perceptual bias to see happy expressions whereas those in a bad mood show a bias to see sad expressions (Niedenthal et al., 2000).

Although it seems plausible that receiver characteristics might bias receiver judgments, research shows that sender displays bias the judgements that receivers make. Although less empirical work has been done in this area, evidence suggests that people rate happy expressions as higher in dominance and affiliation, angry expressions as high in dominance and low in affiliation, and fearful and sad expressions as low in dominance (Knutson, 1996). Interestingly, this experiment used static images with 'apparent movement' – edited stills that were chained together in order to create the illusion of dynamic movement. If such movements lead to biases in receiver perception, these findings

have important implications for researchers who rely on the accurate perception of facial expressions and the judgements of static and dynamic traits.

Together, these findings suggest that characteristics of the senders, such as states and traits are liable to fluctuate over time thus influencing sender displays. Likewise, changes in receiver states influence their judgements. It follows, therefore, that sender and receiver characteristics likely exert some social influence on each other via subtle changes in displays and perceptions.

Social Influence

The ultimate goal of social interaction is to fulfil the need to belong (Baumeister & Leary, 1995). Thus, having strong relationships leads to benefits for physical and mental wellbeing (Argyle, 1992). In addition to this general social need or goal, social interactions can have more specific goals, for example, an interaction which culminates in a decision about whether or not to trust another, can have specific implications for financial outcomes (Biele, Rieskamp & Gonzales, 2009). Not only does the act of making judgements about another help predict behaviour in future instances, people are also able to influence their interaction partners based solely on the social cues that they display, which include verbal, nonverbal, behavioural and appearance-based indicators of emotions, traits and social intentions (Ames & Johar, 2009; Knapp & Hall, 2009; Ng & Bradac, 1993; Parkinson, Fischer, & Manstead, 2005; Zaki, Schirmer, & Mitchell, 2011).

The process of emotional contagion is one suggested mechanism by which people may make judgements about others (Neumann & Strack, 2000; Wild, Erb, & Bartels, 2001; Wild, Erb, Eyb, Bartels, & Grodd, 2003). Interestingly, emotional contagion may also serve as a form of social influence (Barsade, 2002). Research shows that in a social interaction, participants often mirror partner displays and evidence suggests that mimicking or

reciprocating a partner's facial expression, e.g., a genuine smile, may lead receivers to experience the related emotion, i.e., happiness (Wild et al., 2003). In this way, senders and receivers may be able to unconsciously influence each other's affective states. Likewise, in the verbal domain, asking a question should elicit an appropriate response, which, in turn, may lead the conversation in a desired direction. In this way, each participant's verbal and nonverbal cues mutually influence a partner's responses over the course of an interaction (Cappella, 1981). It is this cycle of mutual influence, which allows a social interaction to evolve on a moment-to-moment basis that makes understanding sender-receiver interdependence so important.

Both senders and receivers bring many variables to an interaction (e.g., current states, traits, which may fluctuate in prominence over time, prior beliefs about specific situations, previous behavioural history, etc.), all of which shape the processes by which they send and receive cues. These subjective and hidden or latent factors in an interaction likely influence the outcome of the interaction for both parties. It is therefore important, when exploring the unfolding of social experience, to consider this interdependence between senders and receivers and their respective experiences.

Conclusions

Taken together, this research highlights the multifaceted nature of social interactions and demonstrates that, while research tends to acknowledge the theoretical implications of this complex nature, empirically, little has been done to take this complexity into consideration. Therefore, in the current work, I attempt to address some of the issues in previous work while simultaneously considering the interdependence between the senders and receivers of social cues and how this interdependence manifests in social

signalling, making social judgements and the stability of these judgements as well as considering the mutual influence between the senders and receivers of social cues.

Preface

In Chapter 2, I explore the interdependence of senders and receivers in a live interaction in which I manipulate participants' expectations of the interaction in the context of social rejection. Here, I explore how the expectation of rejection alters an individual's cue displays and also examine how this affects the experience of a naïve interaction partner. In this way, the participant functions as both a receiver of an experimental manipulation and a sender, signalling cues to another participant who functions as a naïve receiver. In Chapter 3, using the same social rejection context as Chapter 2, I focus, in a much more controlled environment, on how the social need state of the receiver alters the interpretation of social cues, specifically examining genuine and polite smiles.

To further understand the processes involved in making judgements of others, Chapter 4 explores predictive accuracy and examines how receivers' judgements of others' trustworthiness influence the ability to decode senders' behaviour in a context different from the one in which they made the judgements. In addition, I look at the stability of these judgements and whether senders' displays influence or bias judgements that receivers make.

In Chapter 5, the final empirical chapter, I explore how a sender's expectations shape receiver behaviour in the context of a set of experimenter effects studies. This work examines how an implicit social power prime and the expectations of an experimenter influence receivers' behaviour in specific ways.

Chapter 2

Interpersonal Effects of Social Rejection

Abstract

The pain of social rejection should motivate rejected individuals to engage in prosocial behaviours to repair social relations. Nonetheless, evidence also suggests that rejected people show a decline in self-regulatory ability and behave more aggressively. Here, we examine the consequences of anticipated social rejection in a naturalistic social interaction with a potential rejecter. Participants received false feedback about whether or not a partner expected to like them and then participated in a social interaction with that partner. In terms of verbal behaviour, participants anticipating rejection expressed and discussed negative affect with greater frequency but did not engage in more prosocial verbal behaviours than did participants expecting acceptance or an unpleasant non-rejection-related interaction. Interestingly, they returned fewer of their partners' polite smiles than did other participants. As a result, their partners reported reduced interaction quality and liked participants expecting rejection less. These findings suggest that the expectation of rejection may indeed cause rejection to occur, by leading to subtle alterations in social behaviours, which have a profound effect upon perceivers.

Humans are social beings and the desire to belong to a social group is a primary human motivation (Baumeister & Leary, 1995; Leary, Haupt, Strausser, & Chokel, 1998). From an evolutionary perspective, this desire is essential to survival – without social groups, it is unlikely that people would reach adulthood or successfully raise offspring (Schmidt & Cohn, 2001). Accordingly, humans are sensitive to exclusion or rejection by social group members (Pickett, Gardner, & Knowles, 2004) and experience exclusion as painful (Eisenberger, Lieberman, & Williams, 2003; Williams, 2007). This pain, so the theory suggests, motivates people to repair damaged social relations by apologising for transgressions, submitting to group norms, and generally behaving more sociably (Williams, 2007).

Actual responses to social rejection, however, are far more complex and depend on individuals' motives as well as the particular context in which they find themselves. The Multimotive Model (Richman & Leary, 2009) explains how characteristics of a rejection experience interact with people's prior experiences to shape reactions to the incident. Richman and Leary (2009) suggest that, in the aftermath of a rejection incident, three motives arise simultaneously: a motive to reconnect, a motive to retaliate in anger, and a motive to avoid future rejection. Individual interpretations of a rejection experience may alter which motive is prevalent in any given situation and how this motive informs subsequent behaviour. For example, if the cost of the rejection is perceived to be high (e.g., the relationship is highly valued and there is a good chance of relational repair), a receiver is more likely to engage in behaviours that promote reconnection. Examples of pro-social responses to rejection include reports of greater desire for social engagement (Maner, Dewall, Baumeister, & Schaller, 2007a), working harder on a group task (Williams & Sommer, 1997) and increased conformity to group norms (Williams, Cheung, & Choi, 2000).

On the other hand, if there are opportunities for alternative relationships, the rejection is perceived as unjust and the experience of rejection is chronic or pervasive, then receivers are more likely to react in avoidant and/or antisocial ways. For example, they may show a temporary decline in self-regulation (Twenge, Catanese, & Baumeister, 2002), altruistic behaviour (Twenge, Baumeister, DeWall, Ciarocco, & Bartels, 2007) and cognitive function (Baumeister, Twenge, & Nuss, 2002), along with increased aggressive behaviour (DeWall, Twenge, Gitter, & Baumeister, 2009; Leary, Twenge, & Quinlivan, 2006).

Among romantic couples, the tendency to expect and perceive social rejection is associated with increases in negative behaviour during interactions (Downey, Freitas, Michaelis, & Khouri, 1998) and with poor quality relationships (Purdie & Downey, 2000). Interestingly, people who have been exposed to a rejection experience also show heightened sensitivity to social cues (Bernstein, Young, Brown, Sacco, & Claypool, 2008; Gardner, Pickett, Jefferies, & Knowles, 2005; Pickett et al., 2004). This might be interpreted as a pro-social response, in that participants show increased attention to social information (DeWall, Maner, & Rouby, 2009) which may reduce the likelihood of future social rejection. Together, these findings imply that the experience and expectation of rejection is likely to have important interpersonal consequences.

Although the Multimotive model (Richman & Leary, 2009) anticipates the varied reactions people have to rejection experiences, the literature has two important limitations that the present study addresses. First, most of the existing research in which participants are induced to experience rejection has examined only rejection's intrapersonal effects. That is, an individual is the 'receiver' of rejection-related information and the findings focus on the effects of this information on the receiver's cognitive or social behaviour. For example, this work has focused on feelings of negative affect (Eisenberger et al., 2003;

Zhong & Leonardelli, 2008) problem solving ability and persistence (Baumeister et al., 2002; Twenge, Catanese, & Baumeister, 2003), ability to forego an immediate reward (Tice, Bratslavsky, & Baumeister, 2001; Twenge et al., 2002), attention to nonverbal cues (Bernstein et al., 2008; Pickett et al., 2004) and reactions to the verbal content of speeches (DeWall et al., 2009), all in non-social settings. Second, the studies that have attempted to address the more interpersonal consequences of rejection have done so in implied social contexts, rather than in face-to-face interactions. These studies have examined self-reported desire for a positive social experience (Maner et al., 2007a), willingness to donate time or money to another (Twenge et al., 2007), and the tendency to punish another person in a computer game (DeWall et al., 2009). Thus the effects of rejection on face-to-face social behaviour in an interaction with a potential rejecter remain unknown. Given that people respond differently in implied versus face-to-face social settings (Fridlund, 1991), our first question asks whether findings from implied social settings generalize to face-to-face interactions? For example, people may be less prosocial if there is no reasonable possibility of a live encounter with that person.

Assuming people change their social behaviour after a rejection, we ask a second question: What are the consequences of a rejected person's behaviour for a social partner? A commonly held hypothesis is that the experience or expectation of social rejection becomes a self-fulfilling prophecy, increasing the likelihood of further rejection (Downey et al., 1998). Although this is a plausible idea, in order for the expectation or experience of rejection to engender future rejection, it must alter social behaviour aversely, and to a great enough degree that an interaction partner notices. There is evidence that people high in rejection sensitivity, the tendency to anticipate, perceive and react to rejection, are likely to behave negatively during interaction (Downey & Feldman, 1996) and that this has

implications for relationship longevity (Downey et al., 1998). However, it may be that people who behave more negatively are simply less well-liked and therefore more likely to experience and anticipate rejection. Alternatively, people in already distressed relationships, who have more worries about relationship breakdown, may report higher levels of rejection sensitivity and behave more negatively. Thus, these studies conflate the tendency to perceive rejection and the unambiguous experience of rejection.

The present study addresses these questions by examining the effects of a social rejection manipulation on subsequent social behaviour with a prospective rejecter. In this study, we induced participants to believe that an interaction partner (who received no information about what to expect in the interaction) was not going to like them. Participants then engaged in a short interaction with the partner. We examined verbal and nonverbal behaviour for signals of positivity and negativity, and related these to a partner's ratings of interaction quality, liking of the partner, positive and negative affect and social behaviour. We anticipated that individuals expecting rejection would show behavioural evidence of discomfort and increased negative affect, relative to individuals expecting a positive interaction, and that the interaction partners of rejected participants would rate their interactions as poorer in quality and report less positive affect and less partner liking than would partners of participants expecting a positive interaction. To ensure that any differences in behaviour related to rejection anticipation, rather than the simple effects of expecting an unpleasant interaction, we included a control group who expected a poor quality interaction but not interpersonal rejection.

Method

Participants

One hundred and forty-four individuals participated in a study of "personality characteristics and first impressions." Participants were randomly assigned to same-sex dyads with the constraint that the age gap between participants be no greater than three years. There were 36 male and 36 female dyads (72 dyads in total). Participants provided written consent before participating and were paid £6 for taking part in the study, which was approved by the University's Ethics Committee. Participant characteristics appear in Table 2.1.

Procedure

Upon arrival, an experimenter escorted participants to separate rooms in which they completed the Big-Five Inventory (a 5-factor personality model measure; John & Srivastava, 1999) and several other questionnaires online. When both participants had completed the questionnaires, the experimenter entered a code into the computer that allowed participants to view a personality profile, which they thought was the partner's. In reality, all participants viewed the same profile. After viewing the profile, participants completed a questionnaire in which they rated how much they looked forward to meeting the partner (e.g., "I'm looking forward to meeting my partner," "I expect we'll have something in common"; 7-point Likert scale ranging from 1=not at all to 7=extremely). This questionnaire served to create a context within which we could provide some participants with false feedback about their partner and thereby affect expectations about the subsequent interaction.

After participants read the profile and completed the questionnaire, they saw a screen that provided one of four types of false feedback about the partner's questionnaire

response. The computer randomly assigned one participant in each dyad to receive no partner feedback. This "naïve" partner, or receiver, saw a screen that appeared to indicate a computer error ("Error. Unable to compile feedback."). The other participant in each dyad received one of the remaining three types of feedback. These were acceptance feedback ("Your partner thinks you will have something in common. Your partner thinks you will get along well. Your partner is expecting to like you."), negative control feedback ("Your partner is not feeling very sociable. Your partner has personal reasons for not looking forward to the interaction. Your partner is not looking forward to meeting anyone."), and rejection feedback ("Your partner does not think you will have anything in common. Your partner does not think you will get along. Your partner is not expecting to like you."). The negative control condition was included to induce expectations that, while negative, were not explicitly rejection-related, i.e., the feedback was not personal to the participant.

These feedback types were counterbalanced across male and female groups, but otherwise randomly assigned to dyads. Because the computer controlled assignment of feedback to participants, the experimenter was blind to both which participants received active feedback and which type of active feedback each dyad received. Immediately after seeing the feedback, participants completed a mood inventory and the experimenter escorted them to an interaction room equipped with visible video cameras.

The experimenter introduced participants, ascertained that they did not know one another and left them alone in the interaction room with the instruction to "have a conversation for a few minutes." Conversations lasted five minutes and were captured on high-definition video. After the interaction, the experimenter escorted participants back to their individual rooms where they completed a post-interaction mood inventory (see below) and questionnaire measures of interaction quality and liking of the partner. After

participants completed the questionnaires, the experimenter paid them, explained the manipulation and allowed them either to provide fully informed consent or to have their data destroyed. No participant declined consent.

Due to a camera malfunction, we were unable to record the interactions of two dyads. One additional rejection dyad participated but did not believe the feedback. These three dyads were replaced to achieve the full 72-dyad sample.

Table 2.1: Participant Characteristics

	Naïve (N=72)	Acceptance (N=24)	Rejection (N=24)	Control (N=24)	p-value
Age	22.08 (2.68)	21.92 (2.77)	22.08 (3.16)	21.17(1.69)	.51
Year in University	2.42 (1.21)	2.71 (1.55)	2.54 (0.93)	2.58(1.25)	.77
Ethnicity *					.82
Caucasian	49	18	17	16	
Asian	16	5	3	6	
Other	7	1	4	2	
RSQ	9.24 (3.33)	9.98 (4.19)	9.68 (2.98)	9.76(2.54)	.63
Post-Manipulation Affect					
Positive	29.39 (6.16)	30.54 (7.08)	26.00 (5.93)	28.09(5.73)	.06
Negative	14.65 (4.74)	14.46 (4.59)	17.67 (5.29)	17.13(6.09)	**.02**

Note: Except where noted, table reports means (standard deviations in parentheses); group differences tested with one-way ANOVA. Bold type indicates significant differences. RSQ: Rejection Sensitivity Questionnaire. Participants did not differ in rejection sensitivity therefore we did not analyse this variable further.
* Table reports frequencies. Group differences tested with chi-squared.

Questionnaires

Positive and Negative Affect Scale (PANAS; Watson, Clark, & Tellegen, 1988). Participants completed the PANAS to measure mood both immediately before and after the interaction. It contains ten items measuring positive (e.g., interested, excited) and ten items measuring negative affect (e.g., jittery, bored). Responses are scored on a five-point Likert

scale (1=very slightly or not at all; 5=extremely). In the present sample, both the positive affect and negative affect scales showed good reliability (α-values of .89 and .87 respectively).

Quality of Interaction Scale (QI, adapted from Berry & Hansen, 1996). The QI is a 9-item questionnaire in which participants rate the quality of the interaction they just experienced (e.g., "To what extent did you think the interaction was smooth and coordinated?") using an 8-point Likert scale (1=not at all; 8=very much). This questionnaire showed good internal consistency in the present sample (α=.86).

Desire for Future Interaction scale (DFI, Coyne, 1976). The DFI is a measure of liking. The DFI includes 6 items that assess how much a participant looks forward to meeting the partner again (e.g., "I would like to meet my partner again."), rated on a 6-point Likert scale (1=definitely no; 6=definitely yes) and 6 items that measure how much a participant thinks that their partner likes them (e.g., "I think my partner would like to meet me again.") measured on the same 6-point Likert scale. We calculated an internal consistency coefficient of .84 for the DFI.

Coders

Six research assistants independently coded either verbal (two coders) or nonverbal behaviour (four coders) from DVDs of the study sessions. All were naïve to study hypotheses and participants' feedback conditions. Prior to coding data, coders were trained to 95% agreement on a set of pilot interactions. To check post-training reliability, each coder independently coded a series of three interactions. Verbal behaviour coders achieved 96% agreement, and nonverbal behaviour coders achieved 92% agreement. For the remaining interactions, we examined agreement by having two coders overlap their coding for 50% of the sessions (66 sessions, both participants; reliability coefficients are reported below).

We recorded interactions at a rate of 25 frames-per-second and coders used frame counts to identify the onsets of each episode of behaviour and to link to dyad members' behaviour. The codes for each verbal behaviour (described below) were defined as mutually exclusive – that is, no verbal code could occur simultaneously with another verbal code. Nonverbal facial behaviours (described below) were also considered mutually exclusive. Coders identified which behaviour was most prominent in each frame of the interaction, including episodes of silence, rest, and "other" behaviours not of theoretical interest to the present investigation (these behaviours were coded into an "other behaviour" category and are not discussed further). Thus, each frame of each participant's interaction included a code for one verbal behaviour and one facial behaviour. As there were no gaps in coding, the offset of one behaviour was considered to have happened during the frame prior to the onset of the new behaviour (for a complete description of this coding/recording method, see Bakeman & Gottman, 1997).

Verbal Coding

We coded verbal behaviour for the presence of the following behaviours: asking the partner questions (e.g., "What do you study?"), showing empathy/support for the partner (e.g., "I know how you feel."), expressions of positive opinion (e.g., "I really enjoy the mountain walking club hikes.") and negative opinion (e.g., "This study is kind of boring."; see Appendix A for coding manual). Raters achieved acceptable levels of agreement for all categories indicated by Cohen's kappa coefficients ranging from .76 to .95 (see Bakeman & Gottman, 1997).

Nonverbal Coding

Coders examined participants' faces for polite smiles (smiles lacking involvement of the eye region), genuine smiles (smiles involving the musculature around the eye) and

frowns. Coders determined the presence of each expression using a FACES-based coding system (Kring & Sloan, 2007), see Appendix B for coding manual). Kappa coefficients for these nonverbal behaviours ranged from .81 to .95.

Data Analysis

The design included four *participant types*: participants who received rejection feedback, negative control feedback, acceptance feedback, and naïve participants. Participant types were nested within three *dyad types* (Acceptance, Rejection, Negative Control). Because individuals interacted within dyads, their experiences of the interaction were not statistically independent, creating a third source of variance. We used the ANOVA model discussed in Kenny and La Voie, (1985; Kenny, Kashy, & Cook, (2006) to control dyad-level interdependence (see also Heerey & Kring, 2007; Kenny, 1995; Kenny & Judd, 1996). Thus, each ANOVA tests both dyad- and participant-type differences (all omnibus tests of dyad- and participant-type are reported in Table 2). Post-hoc comparisons are reported in the text and used Bonferroni's correction for Type I error.

Results

Positive and Negative Affect. After receiving the false feedback and before the interaction, participants completed a mood measure which showed a main effect of affect, $F(1,139)=278.53$, $p<.001$, $\eta^2_p=.67$, such that all participants reported higher levels of positive than negative affect, and a significant Affect x Participant type interaction, $F(3,139)=5.48$ $p=.001$, $\eta^2_p=.11$. Simple effects analyses show significant differences between conditions for negative affect, $F(3,139)=3.30$, $p=.02$, $\eta^2_p=.07$, such that participants who received rejection feedback did not differ significantly from those who received negative control feedback ($p=.72$) but did differ from those who received acceptance ($p=.012$) and naïve participants ($p=.029$). In terms of positive affect, there was a trend

towards feedback-related differences, $F(1,139)=2.57$, $p=.057$, $\eta^2_p=.05$, with contrasts showing no significant differences between rejection and negative control feedback ($p=.252$) but differences between rejection and acceptance ($p=.013$) and naïve conditions ($p=.022$).

Direct Effects of Social Rejection

Verbal Behaviour. The literature suggests that the expectation of social rejection may lead to reduced sociability when interacting with the partner from whom one expects rejection. To test this idea, we examined participants' frequency of asking the partner questions and making supportive or empathic comments. There were no significant differences across groups. There were, however, differences in the frequencies with which participants expressed themselves in affect-laden terms. Frequency of positive talk did not differ across either dyad- or participant-types. However, we did see dyad-level, but not participant-type differences in negative talk (Table 2.2), such that rejection dyads had more negatively valenced conversations than did acceptance dyads ($p=.02$). There were however, no differences in frequency with which participants in negative non-rejection dyads expressed negative opinions compared to both rejection ($p=.13$) and acceptance ($p=1.0$) dyads. These findings may have related to participants' mood changes caused by the manipulation. However, there were no correlations between pre-interaction affect and frequency of positive or negative talk (p-values>.14).

Nonverbal Behaviour. Previous research suggests that frowning may serve to signal discomfort or distress (Harrigan, Harrigan, Sale, & Rosenthal, 1996; Katz et al., 1993). However, we did not see differences in frowning across dyad or participant types (see Table 2.2). Smiling is a nonverbal behaviour that has important consequences in social interactions (Cappella, 1997; Hareli & Hess, 2012). We examined differences in both

genuine and polite smiling. There were no differences in frequency of genuine or of polite smiles across dyad or participant types (see Table 2.2). Nonetheless, interaction partners' ability to coordinate their behaviours conveys important social information (Lakin & Chartrand, 2008). In particular, returning a partner's smiles has important social consequences (Cappella, 1997; Heerey & Kring, 2007). As well as examining the number of genuine and polite smiles produced in each interaction, we also examined smile reciprocity (e.g., returning a partner's genuine smile with a genuine smile of one's own) for both genuine and polite smiles. There were neither dyad- nor participant-type differences in genuine smiling reciprocity (see Table 2.2). However, rejected participants engaged in less polite smiling reciprocity than did participants who received acceptance feedback and their own interaction partners (*p*-values <.03).

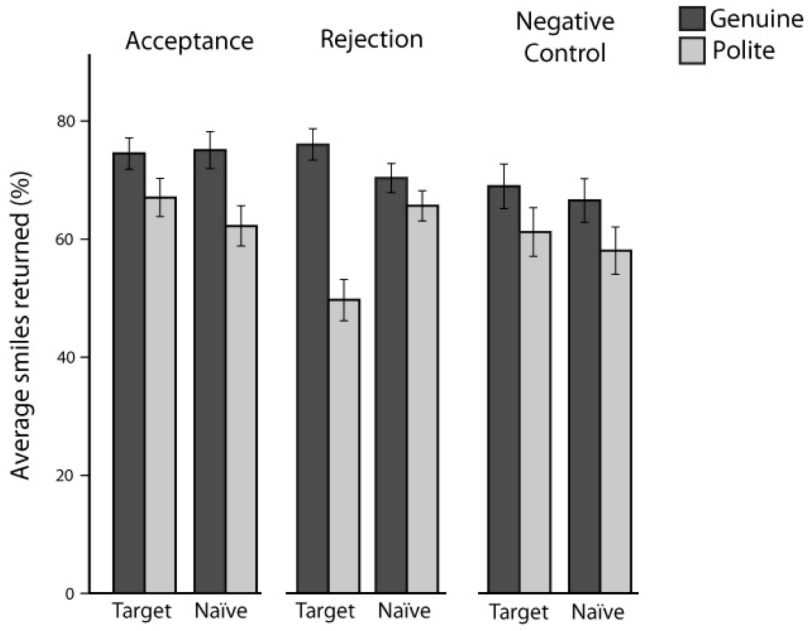

Figure 2.1. Percentage total genuine and polite smiles returned across dyads and participant types. Error bars show ±1 standard error of the mean.

Table 2.2. Results	Acceptance Dyads		Rejection Dyads		Negative Control Dyads		Dyad-Type Differences			Participant-Type Differences		
	Target (n=24)	Naïve (n=24)	Target (n=24)	Naïve (n=24)	Target (n=24)	Naïve (n=24)	F	*p*	η^2_p	F	*p*	η^2_p
Verbal Behaviour												
Asking Questions	10.29 (4.70)	11.25 (5.48)	9.75 (4.26)	10.33 (3.99)	11.33 (6.87)	9.83 (6.36)	0.43	.65	<.01	0.39	.86	.01
Empathic/supportive	2.48 (1.73)	2.65 (1.69)	3.02 (1.84)	2.91 (1.79)	2.91 (2.07)	2.62 (1.70)	0.60	.55	<.01	0.32	.90	.01
Positive Opinion	2.38 (1.72)	1.79 (1.53)	2.75 (2.29)	2.17 (1.66)	2.17 (2.12)	2.29 (1.57)	0.51	.60	<.01	0.70	.63	.03
Negative Opinion	2.25 (2.02)	2.21 (2.40)	3.54 (2.86)	4.08 (3.51)	3.04 (3.10)	2.25 (2.91)	**4.06**	**.02**	**.05**	1.88	.10	.06
Nonverbal Behaviour												
Frowns	3.64 (3.08)	7.30 (5.33)	2.90 (2.23)	4.85 (9.47)	8.73 (14.49)	6.13 (3.18)	1.10	.34	.04	0.82	.54	.07
Genuine Smiles	19.48 (15.55)	17.70 (10.84)	21.57 (14.62)	20.35 (9.65)	15.19 (11.40)	16.90 (13.81)	1.64	.20	.02	0.75	.59	.03
Polite Smiles	19.30 (18.51)	16.00 (14.67)	12.00 (7.38)	14.65 (12.99)	19.57 (18.47)	17.27 (17.08)	1.46	.24	.02	0.80	.55	.03
Reciprocity (% returned)												
Genuine Smiles	.75 (13)	.75 (.15)	.76 (.13)	.70 (.12)	.69 (.18)	.67 (.17)	2.86	.06	.04	1.54	.18	.06
Polite Smiles	.67 (.16)	.62 (.16)	.50 (.16)	.66 (.12)	.61 (.19)	.58 (.19)	2.00	.14	.03	**3.23**	**.009**	**.11**
Outcomes												
Positive Affect Change	3.17 (5.95)	2.83 (5.45)	4.67 (4.74)	0.29 (4.69)	3.39 (6.33)	0.71 (6.39)	0.34	.71	<.01	**2.13**	**.07**	**007**
Negative Affect Change	-2.58 (3.06)	-2.50 (3.46)	-5.42 (5.36)	-1.54 (2.93)	-5.00 (5.36)	-3.00 (2.67)	1.62	.20	.02	**3.88**	**.003**	**.12**
Quality of Interaction	56.71 (7.92)	55.29 (7.03)	48.62 (11.25)	50.33 (7.11)	53.63 (6.45)	53.17 (10.03)	**6.58**	**.002**	**.09**	2.75	.02	.09
Liking	26.79 (4.72)	27.13 (4.77)	22.83 (5.48)	24.04 (5.06)	23.17 (5.32)	24.67 (6.19)	**6.34**	**.002**	**.08**	2.84	.018	.09

Note: Table shows means (standard deviations in parentheses), test-statistics, p-values and effect sizes (η^2_p =partial Eta Squared). Except where noted, behaviours are reported as frequency counts. Bold type indicates significant differences.

Thus, although they generated their own polite smiles with frequencies similar to other participant types (see Table 2.2, Figure 2.1), rejected participants reciprocated their partners' polite smiles less often than did other participants. Together, these findings suggest that interacting with an ostensible rejecter alters patterns of nonverbal behaviour.

Interaction Outcomes. To understand how the expectation of rejection alters interaction outcomes, we examined participants' self-reported interaction quality, liking for their partners and positive and negative affect. We found that at the dyad level, participants in rejection dyads reported reduced interaction quality relative to those in acceptance dyads (*p*-values <.01) but there were no differences between negative non-rejection dyads and either rejection (*p*= .10) or acceptance dyads (*p*=.47). Participants in acceptance dyads reported liking their interaction partners more than those in rejection and negative non-rejection dyads (*p*-values < .02; see Table 2.2).

We also computed participants' change in positive and negative affect from pre- to post-interaction. As Figure 2.2 shows, we found significant participant-type, though not dyad-type, differences in both positive and negative affect (see Table 2).

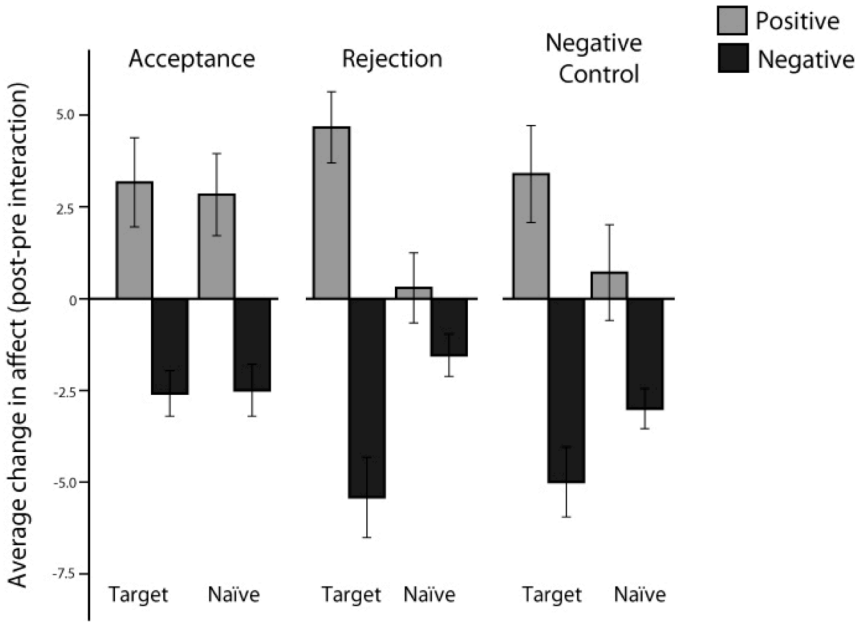

Figure 2.2. Average changes in positive and negative affect (post-interaction – pre-interaction). Error bars show ± 1 standard error of the mean.

A repeated measures 2 (Time: before, after interaction) X 2 (Affect: positive, negative) ANOVA with participant type as the between-participants variable, showed a main effect of affect ($F(1,137)=98.84$, $p < .001$, $\eta^2_p = .42$) with all participants generally reporting higher levels of positive than negative affect. There was also a significant Affect x Participant type interaction ($F(5,137)=4.37$, $p = < .001$, $\eta^2_p = .14$). Specifically, simple effects analyses showed that rejected and negative control participants reported the largest drop in negative affect from pre- to post-interaction (*p*-values <.05). This was not surprising as these participants reported higher levels of negative affect immediately prior to interacting (see Table 2.1).

More interestingly, naïve participants in rejection and negative control dyads did not show the same increase in positive affect after the interaction as their interaction partners or participants in the acceptance dyads. These results suggest that rejected and negative control participants' behaviour had significant effects on their partners' experience of the interaction.

Effects of Social Rejection on Naïve Receiver

One way of understanding group differences in interaction quality and post-interaction affect is to examine how interacting with an individual expecting rejection changes the experience of social interaction. If the conversation partners of rejected individuals experience qualitatively different and worse interactions than those of other types of participants, this would be one explanation for why rejection seems to reoccur. To answer this question, we correlated target participants' behaviour (i.e., participants receiving active rejection, acceptance and negative control feedback) with their naïve partners' ratings of interaction quality, liking, and post-interaction positive and negative affect.

Verbal Behaviour. Expressions of positive opinions did not correlate with partners' self-reported experiences of the interaction (*p*-values >=.56), however expressions of negative affect were negatively correlated with partner's post-interaction positive affect, r(72)=-.26, *p*=.03, ratings of interaction quality, r(72)=-.24, *p*=.04, and ratings of liking, r(72)=-.24, *p*=.04. Unsurprisingly, these findings suggest that the more one directs conversation toward negative topics, the less one's conversation partners enjoy it.

Non-verbal behaviour*.* Although genuine smile reciprocity did not correlate with interaction outcomes (*p*-values >.23), the frequency with which participants reciprocated polite smiles related to how much their partners actually liked them, r(68)=.33, *p*=.006, as well as how much participants thought their partners liked them, r(68)=.39, *p*=.001. Thus, polite smiles appear to be an important token in social interactions.

Interaction outcomes. Interestingly, naïve participants in rejection dyads reported significantly smaller drop in negative affect than did their rejected partners (*p*=.01). These results suggest that rejected participants' behaviour had significant effects on their partners' experience of the interaction.

Discussion

The experience of an interaction in which one expects rejection is not particularly pleasant. Participants who received this feedback differed in the frequency with which they discussed positive and negative topics. Participants who received rejection feedback engaged in more negatively-valenced conversations, e.g., "I hate my modules this semester," than did those who had received acceptance or negative control feedback. This difference occurred for both participants within rejection dyads, suggesting that the naïve partners of rejected participants also engaged more in this type of behaviour. Thus, it appears that naïve partners of rejected participants perceived these alterations in verbal

behaviour and adjusted their own behaviour accordingly. Participants who received both rejection and negative control feedback reported increased levels of negative affect after receiving the feedback so this change in verbal behaviour is unlikely to be due to increased negative affect alone. In the negative control condition, perhaps target participants felt that they needed to 'look after' or support the interaction partner and so avoided negative conversation topics whereas, participants who received rejection feedback may have felt more personally affected and responded by articulating their negative feelings. Together, these findings show that subtle verbal and nonverbal cues people send are important predictors of a social interaction's outcome and that the expectation of rejection alters these cues.

With regards to nonverbal behaviours, participants differed in their reciprocity of genuine versus polite smiles, an important social behaviour (Cappella, 1997; Fridlund, 1992; Hess & Bourgeois, 2010). Previous research by Bernstein and colleagues has found that social rejection improves people's ability to distinguish genuine from polite smiles (Bernstein et al., 2008), and enhances their preference to interact with genuinely smiling individuals (Bernstein, Sacco, & Brown, 2010). This effect has been suggested to relate to an adaptive bias toward genuinely smiling individuals who are likely to be more affiliative and thus to redress the rejection (Bernstein et al., 2010). However, we found no differences in genuine smiling or in reciprocity of genuine smiles, suggesting that interacting with a prospective rejector does not alter actual genuine smiling behaviour.

Interestingly, rejected participants in our study showed a reduction in the extent to which they displayed and returned their partners' polite but not genuine smiles. In this respect, our smiling findings are consistent with the data from Bernstein's group and may suggest a reduced sensitivity to these important social tokens in rejected participants.

Unfortunately, the fewer of these polite smiles participants returned, the less post-interaction positive affect their partners reported, which suggests that this apparent bias in smile reciprocity may ultimately damage rejected individuals' attempts at social reparation. It may be that the production of certain nonverbal cues such as smiles generates the expectation that one's action will be reciprocated (Iacoboni, 2009; Preston & Stansfield, 2008). Insofar as this is true, reduced reciprocity may lead to a decreased sense of connection during interaction, and consequently, altered interpersonal judgments (Cheng & Chartrand, 2003; Van Baaren, Holland, Kawakami, & Knippenberg, 2004). Additionally, polite smiles may be expected in an interaction as a 'rule of conduct' (Goffman, 1956). Thus decreased polite smile reciprocity may appear as a slight because, while genuine smiles are seen as automatic responses to felt emotion, polite smiles likely require more effort to produce. If an interaction partner is unwilling to make this effort, it might be interpreted as a deliberate insult.

Interestingly, there were no differences in outcome measures (interaction quality and liking) for those who received active feedback and their interaction partners. That is, rejected participants did not differ from their naïve partners in their reports of either interaction quality or liking. The same was true of the negative control participants. These results suggest that both target and naïve partners responded to alterations in their interactions and reported feeling similarly to each other.

There are two important limitations of this study. In particular, participants interacted with strangers in a "getting-to-know-you" type of interaction. Although our results suggest that social cues matter during impression formation, the behavioural alterations we observed may not be sufficient to alter perceptions or behaviour among well-known interaction partners (e.g., Downey et al., 1998). Second, we cannot rule out the

possibility that the lack of prosocial behaviour we observed was related to the study environment. Aggressive responses are commonly observed in social rejection manipulations in non-face-to-face studies (DeWall et al., 2009; Twenge & Campbell, 2003; Twenge, Baumeister, Tice, & Stucke, 2001) as well as in conflict discussions when participants know one another well (Downey et al., 1998; Purdie & Downey, 2000). Low levels of prosocial behaviour may have served as a socially appropriate and mildly aggressive response to anticipated rejection. However, in interactions with a more neutral individual (a stranger from whom one does not expect rejection) behaviour may have been significantly more prosocial.

Conclusions

The expectation of social rejection leads to noticeable alterations in social behaviour. These alterations are sufficient to cause similar changes in an interaction partner's behaviour and ultimately affect the quality of a social partner's perceptions of an interaction. Thus, expectations of social rejection may, in turn, lead to rejection by causing alterations to social behaviour. This finding may explain why some individuals suffer chronically from rejection and may offer insights for helping these individuals to repair damaged social relation.

Chapter 3

Changing the value of a smile: Social rejection and the subjective value of genuine and polite smiles

Abstract

Research shows that threats to social belonging lead to increased sensitivity to cues that might indicate social rejection. One possible response to the threat of social rejection is to seek social reconnection. For this reason, cues that signal affiliation, e.g., genuine smiles, may increase in subjective value when belongingness is threatened. We test this hypothesis by pitting the reward value of genuine smiles against monetary rewards and find that participants anticipating social rejection find genuine smiles significantly more valuable than do participants not anticipating rejection. In a second experiment, we show that polite smiles are also important social tokens for some participants. Interestingly however, anticipating social rejection leads to the specific devaluation of polite smiles, meaning that participants are willing to forgo monetary reward to avoid seeing these social cues. Together, these results indicate that changes in participants' feelings of social need lead to alterations in the degree to which they evaluate these social cues and make decisions based on these cues.

People perceive threats to social belongingness, such as rejection by a peer, as painful (Eisenberger et al., 2003; Eisenberger, 2012; Lieberman & Eisenberger, 2006; Macdonald & Leary, 2005). One way in which people attempt to alleviate the emotional consequences of social rejection is by seeking reconnection with others (Baumeister & Leary, 1995; Maner et al., 2007a). One mechanism that encourages reconnection is an enhanced sensitivity to cues of social affiliation, including genuine smiles (Bernstein et al., 2008), which convey positive emotion (Ekman, Davidson, & Friesen, 1990), encourage affiliation (Brown & Moore, 2002), and serve as a form of social reward (Shore & Heerey, 2011).

Because genuine smiles signal social affiliation, which is rewarding (Kendrick, 2004), they should be increasingly desirable under the threat of social rejection, when people may feel a particularly strong need for social bonds and affiliation (Leary et al., 1998). Indeed, in several studies, participants who anticipated future social rejection showed greater ability to distinguish genuine from polite smiles and a greater desire to affiliate with genuinely smiling partners (Bernstein et al., 2010). Similarly, the increased desire to (re)establish social connections among rejected individuals should enhance the value or subjective desirability of genuine smiles over polite ones.

One way to assess the subjective desirability of a smile is to measure its value in terms of another currency, such as money. The degree to which a participant is willing to forgo the chance to win money in favour of seeing a genuine smile indicates the smile's subjective value to that individual. In Economics, this is known as "utility" (Morgenstern & Von Neumann, 1953). For individuals expecting rejection, i.e., those whose social need state is high, genuine smiles should have increased utility.

Experiment 1

Here we test the hypothesis that participants in high states of social need (those expecting rejection) will value genuine smiles to a greater degree than will those in lower states of social need. To test this idea, we examined participants' responses to faces that smiled either genuinely or politely with either high or low probability. We compared participants expecting rejection in a subsequent social interaction to both those expecting a positive interaction and those expecting an unpleasant interaction in which rejection was not a factor. We expected that relative to other participants, those who anticipated social rejection would show a preference for genuinely smiling faces, even when the probability of winning money from those faces was reduced thus showing an increase in the subjective desirability of genuine smiles.

Method

Participants

Eighty-five psychology undergraduates (74% female; age: *M*=19.42, *SD*=2.24) participated in a study of "personality and first impressions" in exchange for partial course credit and a task-based monetary bonus. Participants provided written informed consent and the University ethics committee approved the study. Six additional participants were excluded from data analysis because they reported not believing the manipulated feedback.

Procedure

Participants arrived for the experiment in pairs to give the impression that they would be partners for a social interaction later in the study. In reality, they did not complete a face-to-face interaction but instead completed a computer task measuring the subjective desirability of genuine smile feedback. Upon arrival, the experimenter seated participants in individual rooms where they completed a series of questionnaires measuring aspects of

personality, along with the Positive and Negative Affect Schedule (PANAS, Watson et al., 1988) to measure mood, on a computer. After completing these measures, participants ostensibly viewed the partner's personality profile. In reality, all participants viewed the same profile.

Based upon this personality profile, participants responded to a series of statements about how much they looked forward to meeting the partner (e.g., "I think I will enjoy meeting my partner"). Participants then received false feedback about how much the partner looked forward to meeting them. The computer randomly assigned participants to one of three feedback types. These were: positive feedback ("Your partner is feeling sociable. Your partner thinks there is a good chance that the two of you will get along. Your partner is looking forward to meeting you."), social rejection feedback ("Your partner does not think the two of you will have anything in common. Your partner does not think the two of you will get along. Your partner is not looking forward to meeting you."), and negative non-rejection feedback ("Your partner is not feeling very sociable. Your partner has personal reasons for not looking forward to the interaction. Your partner is not looking forward to meeting anyone."). After viewing the feedback, participants completed a second PANAS and then a computer-based "matching pennies" game (see below), which served as the dependent measure in this experiment. Following the game, the experimenter debriefed all participants, explained that there would be no interaction, probed them for suspicion about the feedback, and gave them the opportunity to provide fully informed consent or to have their data withdrawn. No participant declined consent.

Measures

Matching Pennies Game. In matching pennies games, the participant's goal is to choose the same side of a coin as a partner. In this version, participants played the game with a set of four computerized opponents each identified by a photograph of a face (see Figure 3.1; Shore & Heerey, 2011). The task included learning and test phases.

In the learning phase, participants played one opponent at a time. The opponent

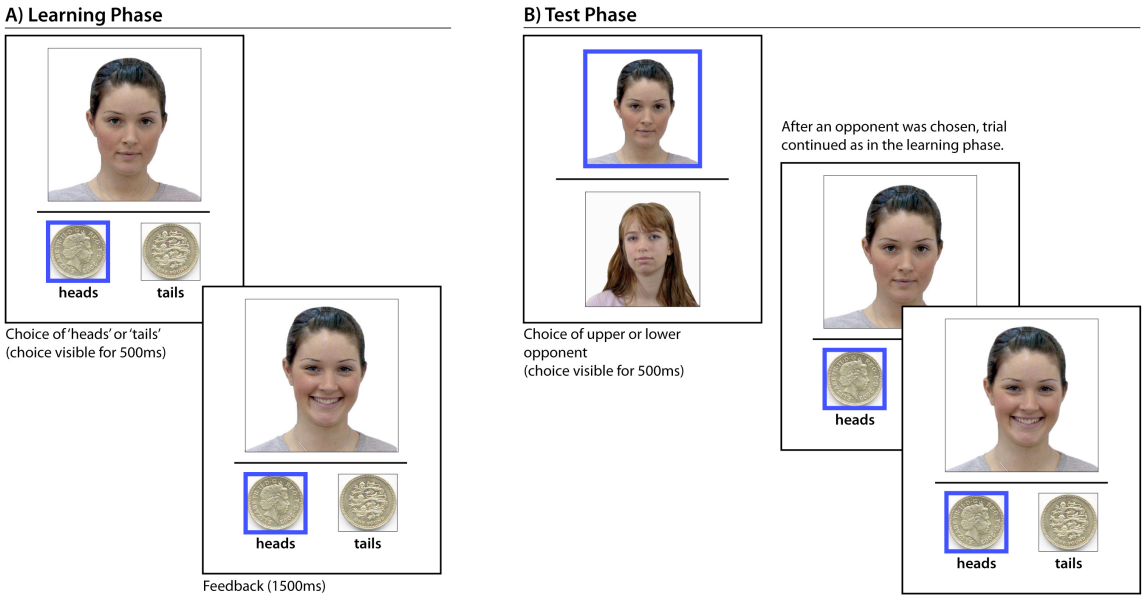

Figure 3.1. Task design. In the learning phase of the task (A), participants chose heads or tails of a coin and received social feedback. In test trials (B), participants chose the opponent they wished to play from a pair of opponents.

appeared in the centre of the screen in a neutral pose. Participants attempted to choose the same side (heads or tails) of a virtual coin as the opponent using a key press. For every match, participants won two pence; non-matches were worth 0 pence. To indicate a win, the opponent smiled either genuinely (involving zygomaticus major and orbicularis oculi) or politely (zygomaticus major only; for a complete description of the stimuli, see Shore & Heerey, 2011). On non-match trials, opponents frowned. Unbeknownst to participants, feedback did not relate to their responses. Instead, two opponents provided win feedback on 80% of trials. The remaining opponents provided win feedback on only 60% of trials. Two

opponents (one 80% and one 60%) always displayed genuine smiles on match trials whereas the others always displayed polite smiles. These contingencies remained the same across learning and test phases. Participants completed three learning blocks of 40 trials each. Opponents appeared in random order. To ensure that opponent identity effects did not influence the results, we counterbalanced opponent assignment to social and monetary feedback contingencies across participants. Half the participants saw female opponents and half saw male ones, counterbalanced by participant gender.

The test phase allowed us to determine the degree to which participants valued genuine smile feedback in monetary terms. On each trial, participants chose the opponent they wanted to play from among a pair of opponents. All six possible opponent pairings were tested 10 times each in random order (60 test trials). These choices allowed us to determine the degree to which participants were willing to trade off money in favour of seeing smiles. After participants chose an opponent, trials continued as in the learning phase.

Finally, to test participants' ability to distinguish between genuine and polite smiles, they viewed a series of still images of faces displaying either genuine or polite smiles and indicated which type of smile the face displayed using a key press. The task was programmed using the Psychophysics Toolbox (Brainard, 1997) extensions for MATLAB (version 7.5; The Mathworks), likewise for Experiment 2 below.

Data Analysis

Participants' choices in the matching pennies test phase allowed us to determine the utility of smiles, in monetary terms. We used a logistic regression model to estimate the degree to which money and smiles contributed to choice behaviour. Each participant's choice data were individually fit using the logistic response function:

$$P_{OpponentA}=\exp(\theta)/(1+\exp(\theta))$$

where $P_{OpponentA}$ is the probability of choosing the upper over the lower opponent in an opponent pair (see Figure 1B), and θ is the difference in opponents' utilities, modelled as the linear function below.

$$\theta=\beta_{Money}X_{Money} + \beta_{Smiles}X_{Smiles} + \beta_{MoneyXSmiles}X_{MoneyXSmiles}$$

In this equation, X_{Money} codes the difference between the two opponents' expected monetary values. A stimulus's expected value is the value of a win multiplied by the probability of winning (Sutton & Barto, 1998), in this case, 2 pence times either a 60% or an 80% chance of winning. Thus, X_{Money} received a value of .4 if opponent A was better than opponent B, -.4 if opponent B was better, and 0 if they were equal. X_{Smiles} was the difference in opponents' social values. If opponent A smiled genuinely and B politely, we coded this as 1; if the smile types were reversed, we coded this as -1; and if both opponents produced the same type of smile, this was coded as 0. The money x smiles interaction ($X_{MoneyXSmiles}$) was the product of X_{Money} and X_{Smiles}. The βs are the unstandardized logistic regression weightings for each model component. These were estimated using a robust, iteratively re-weighted least squares algorithm to obtain the maximum likelihood estimates for each term in the model (O'Leary, 1990).

Our smile discrimination task data were analysed using a standard signal detection framework. We recorded a hit when participants correctly identified a genuine smile; a correct rejection when they correctly identified a polite smile; a miss when a genuine smile was identified as polite; and a false alarm when a polite smile was identified as genuine. We computed d' as a measure of participants' ability to discriminate between the smiles along with a measure of the degree to which their responses were biased toward one response or the other (this measure is known as "criterion" in standard signal detection theory terms;

(Macmillan & Creelman, 2005). This allowed us to determine whether our manipulation altered participants' ability to identify smiles, as previous research suggests (Bernstein et al. 2008), or introduced bias into their responses in our discrimination task. Finally, we note that all post-hoc comparisons were Bonferroni corrected to control Type I error probability.

Results

We predicted that anticipating social rejection would lead to an increase in the utility of genuine smiles, a form of social reward. To assess the degree to which money, smiles and the money x smiles interaction contributed to choice behaviour, we performed a multivariate analysis of variance (MANOVA) with feedback condition (acceptance, social rejection and negative non-rejection) as the between-participants variable and the estimated βs from the logistic model as the dependent variables. As Figure 3.2 shows, feedback condition did not alter the degree to which money influenced choice behaviour, $F(2, 82)=0.55$; $p=.58$; $\eta^2_p = .01$. However, it did significantly alter the degree to which smiles influenced choices, $F(2, 82)=4.62$; $p=.01$; $\eta^2_p = .10$. Post-hoc analyses showed that participants anticipating rejection had significantly greater regression weightings than did participants receiving acceptance feedback ($p=.006$) and negative non-rejection feedback ($p=.01$) demonstrating that smiles had a greater influence on

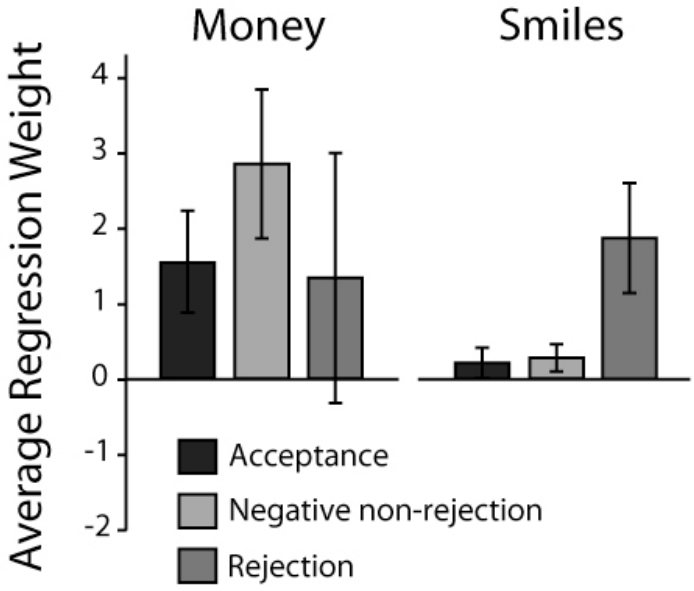

Figure 3.2. Regression weights (β s) for a difference in expected monetary reward and a difference in smiles, split by feedback condition. Error bars show ±1 standard error of the mean.

choice behaviour. The acceptance and negative non-rejection feedback groups did not differ (*p*=.82). Thus, our data indicate that anticipating rejection increases the utility of genuine smiles.

Feedback condition did not influence the money x smiles interaction, $F(2, 82)=0.26$; $p=.77$; $\eta^2_p < .01$. A single-sample t-test showed that the interaction term was not significantly different from zero, $t(84)=1.51$, $p=.13$; Cohen's d=.16, suggesting that money and smiles independently influenced decision-making in this experimental design.

Genuine smiles had significantly greater utility for participants anticipating rejection. Based on our coding of the X s in our logistic regression, the value of a smile in monetary terms can be estimated by dividing β_{Smiles} by β_{Money}. Because there were no significant differences across groups in the degree to which money influenced decisions across the groups, we used the grand average of β_{Money}, in this case 1.93, to estimate smile value. For participants not anticipating social rejection, the value of a smile was only a small fraction of a penny. For those expecting acceptance, the value of a smile was .11 pence. For those expecting an unpleasant interaction (i.e., the negative, non-rejection group), a smile was worth .14 pence. However, for participants anticipating rejection, a genuine smile was worth .97 pence. Thus, rejection anticipation led to an 8 to 9-fold increase in the utility of a smile, as expressed in monetary terms.

Interestingly, this finding cannot simply be explained by changes in positive or negative affect. We conducted a MANOVA with feedback condition as the between-participants variable and the post- minus pre-manipulation differences in negative and positive affect as dependent variables. The omnibus tests showed that feedback had a significant effect on participants' positive, $F(2, 82)=4.20$; $p=.05$; $\eta^2_p =.07$, and negative affect, $F(2, 82)=3.23$; $p=.02$; $\eta^2_p =.09$,. As illustrated in Figure 3.3, participants anticipating a positive

interaction showed no significant changes in their positive, t(27)=0.33, *p*=.75, or negative affect, t(27)=-1.51, *p*=.14. Participants in the negative non-rejection condition showed a non-significant change in positive affect, t(30)=-1.83, *p*=.08, but a significant increase in negative affect, t(30)=2.79, *p*=.009, which differed significantly from those in the acceptance condition (*p*=.04) but not the rejection condition (*p*>.99). Participants anticipating social rejection showed a significant decrease in positive affect, t(25)=-2.86, *p*=.008, as well as an increase in negative affect t(25)=-2.86, *p*=.04. Their change in mood differed significantly from those in the acceptance condition for both positive (*p*=.04) and negative affect (*p*=.04). Additionally, there were no correlations between participants' self-reported positive (r(85)=-.05, *p*=.67) or negative affect (r(85)=-.17, *p*=.12) and smile choice. These results suggest that simple changes in affect did not alter smile utility.

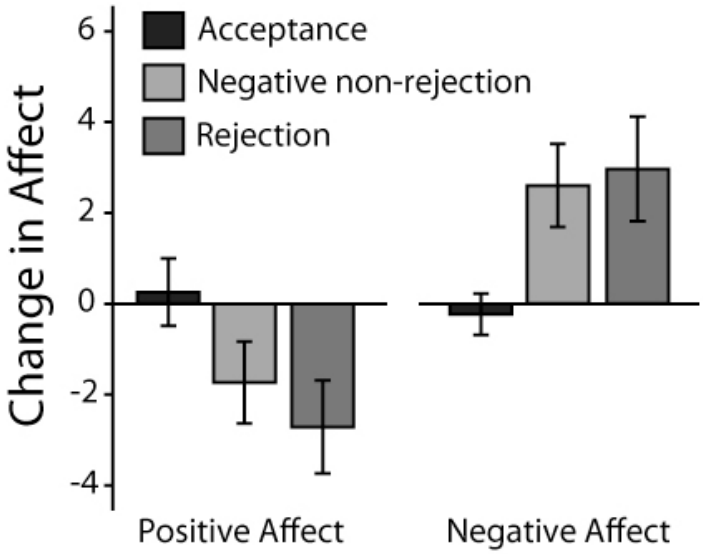

Figure 3.3. Positive and negative affect changes from pre-feedback to post-feedback, split by feedback condition. Error bars show +/- 1 standard error of the mean.

Finally, contrary to previous research (Bernstein et al., 2008), we did not find that rejection feedback altered participants' ability to discriminate genuine from polite smiles as

measured by d', nor did it affect the degree to which they showed bias in their decisions. A MANOVA with d' and our bias measure (criterion) as the dependent variables and feedback condition as the between-participants variable showed no significant smile discrimination differences for either d', $F(2, 82)=0.89$; $p=.42$; $\eta_p^2=.02$, or criterion, $F(2, 82)=0.51$; $p=.60$; $\eta_p^2=.01$.

Discussion

Results showed that the utility of genuinely smiling faces was greater for participants who were expecting rejection, compared to those expecting acceptance. This finding is even more surprising, as participants were fully aware that the faces were simply stimuli in a computer task and were not potential interaction partners. Thus, the stimuli provided no possibility of social reconnection to alleviate the increase in social need that the feedback elicited. Nonetheless, the social cues these faces displayed profoundly influenced participants' decisions. This novel result clearly demonstrates social-state-dependent modulation of smile utility.

Interestingly, the present results appear to diverge from how participants in high social need states behaved in face-to-face interaction. Specifically, we have shown that during interactions with a potential rejecter (see Chapter 2) participants do not increase the frequency with which they return genuine smiles, as the present results would predict. Rather they decrease the frequency with which they reciprocate their partners' polite smiles. This suggests that along with (or instead of) increasing the value of genuine smiles, anticipating social rejection may cause polite smiles to become devalued. We test this idea in Experiment 2.

Experiment 2

To determine whether the high social need state caused by rejection anticipation independently changes the utility of genuine and polite smiles, we altered the Experiment 1 task to allow the comparison of genuine and polite smiles to a neutral baseline. We also included two additional conditions that allowed us to explicitly evaluate the degree to which mood alone (without the element of a prospective interaction) influenced smile utility. We predicted that expecting social rejection would enhance the utility of a genuine smile and devalue that of a polite smile, relative to a no-smile baseline condition. We also predicted that the mood alone conditions would not affect smile utility.

Method

Participants

Ninety-nine psychology undergraduates (68% female; age: *M*=20.92, *SD*=5.61) participated in a study of "personality and first impressions," as in Experiment 1, in exchange for partial course credit and a task-based monetary bonus. Participants provided written informed consent and the University Ethics Committee approved the study. Three additional participants were excluded because they did not believe the false feedback (1 negative non-rejection, 2 rejection).

Procedure

The procedure was similar to that in Experiment 1. Participants arrived in pairs to give the appearance that they would participate together but in reality participated independently. All participants completed a series of personality questionnaires as well as a PANAS, which included six additional items measuring feelings of rejection (rejected, slighted, misunderstood, respected, supported, and liked; the last three items were reverse scored). The additional items showed good reliability (Cronbach's α=.68). Participants

understood that after the questionnaires the computer would randomly assign them to either participate in a short conversation with their experimental partner or to a time-filler reaction time task instead of the interaction.

Participants assigned to the interaction conditions followed the same procedure as Experiment 1. The computer randomly allocated them to receive rejection (n=18), acceptance (n=19) or negative non-rejection feedback (n=19), ostensibly from their interaction partners. Participants assigned to the reaction time task were told that they would complete a task in which they would respond to targets as quickly as possible. This reaction-time task was designed as a mood manipulation to induce either positive or negative mood. Importantly, this task did not have an explicitly social component. This allowed us to determine the degree to which changes in mood alone influenced smile utility. After all participants had either received false feedback, or had completed the mood-induction task, they completed a second PANAS, including the six rejection-related words. Participants then completed a modified version of the matching pennies game in Experiment 1. After participants had completed the game, the experimenter debriefed all participants, probed them for suspicion about the false feedback or the mood induction task, and gave them the opportunity to provide fully informed consent. No participant declined consent.

Mood Induction Task. Participants were instructed to respond as quickly as possible to a target [+], visible on the screen for 150ms. They were told that the task was difficult and that they would receive feedback after each trial. Performing well or poorly on a difficult task leads to increases in positive or negative affect, respectively (see Nummenmaa & Niemi (2004) for a review). Unbeknownst to participants, the computer manipulated the feedback they received to induce positive or negative moods. In the positive mood condition (n=21),

participants received “Good,” feedback after ~90% of trials followed by the end-of-block feedback “Your performance was excellent.” On the remaining trials, they were told that their performance was “Too slow.” In the negative mood condition (n=22), participants received positive feedback on ~60% of trials (frequency determined based on pilot testing). Feedback on the remaining trials informed participants that their responses were “Too slow,” followed by the feedback “Your performance was adequate,” at the end of each block. Participants completed three blocks of 100 trials.

Stimuli were presented at variable inter-stimulus intervals (ISI), randomly selected from a normal distribution with a mean of 1000ms and a standard deviation of 300ms. To hold positive/negative feedback rates at the required proportions, the computer adapted the time limit for coding a response as ‘on time’ on a trial-by-trial basis. This meant that it allowed more leisurely responses for participants in the positive condition and coded performance more stringently for participants in the negative condition.

Matching Pennies Game. In order to compare genuine and polite smiles to neutral feedback, we created a modified version of the game in Experiment 1. Instead of four computerised opponents, participants played six opponents. The two additional opponents displayed neutral expressions throughout and indicated wins or losses with text “You did not win,” or “You won,” superimposed across the neutral face. These two neutral opponents rewarded with the same probabilities (one 60% and one 80%) as the genuinely and politely smiling faces in the task. Thus, there were three faces that provided rewards on 60% of trials and three that provided rewards on 80% of trials. Within each of these reward-probability levels, one face smiled genuinely, one politely and one remained neutral. The learning phase of the task included four blocks of 30 trials each. Opponent identities were counterbalanced over monetary and social feedback conditions to control for identity

effects. Half the participants viewed male opponents and half viewed female opponents, counterbalanced across participant gender.

As in Experiment 1, participants chose which opponent they wished to play from a pair of opponents on each test-phase trial. All 15 possible opponent pairings were tested 8 times each in random order (120 test trials). Finally, they completed a smile discrimination task in which they viewed a series of still images of genuinely and politely smiling faces and indicated which type of smile each face displayed.

Data Analysis

In the test phase of this task, participants' choices reflect a preference for that opponent. As participants chose from every possible pairing, we were able to decouple the influence of reward probability from the influence of feedback type, i.e., genuine smiles, polite smiles, and neutral expressions. To determine whether participants preferred high-value or low-value opponents generally, we computed the proportion of trials on which they chose the high-value opponent when both opponents provided the same type of social feedback and different monetary values. Similarly, we computed the proportion of trials on which they chose genuinely smiling, politely smiling or neutral opponents when both opponents had the same monetary value but different social values. These averages are equivalent to main effects. We also examined participants' preferences for high-value opponents when opponents differed on both monetary and social value. This allowed us to examine the extent to which participants were willing to forgo monetary reinforcement in favour of different types of social feedback. To determine whether experimental condition (positive mood, negative mood, acceptance, negative control or rejection) influenced choice behaviour, we conducted MANOVAs with choice type as the dependent variable and experimental condition as the independent variable.

Results

To determine whether participants learned the reward contingencies, we analysed trials when smile types were the same. Participants chose the high-value opponent 64% of the time and significantly more often than chance, $t(98)=8.13$, $p<.001$, $d=0.82$. There were no condition type differences, $F(4,94)=1.64$, $p=.17$, $\eta^2_p=.07$, suggesting that participants understood the monetary values of the faces. Figure 3.4A shows these results.

With respect to social feedback, on trials in which reward probability was the same (e.g., the genuinely smiling 80% opponent versus the politely smiling 80% opponent), participants selected the genuinely smiling opponent significantly more often than chance both when the other opponent displayed a polite smile, $t(98)=6.67$, $p<.001$, $d=0.67$, and when the opponent displayed no expression, $t(98)=8.04$, $p<.001$, $d=0.81$. When participants chose between politely smiling and neutral opponents, they chose polite smiles significantly more than chance, $t(98)=2.53$, $p=.01$, $d=0.25$. This suggests that although the reward value of genuine smiles is well established (Shore & Heerey, 2011), polite smiles are also desirable social stimuli, albeit to a lesser degree than are genuine smiles. Interestingly, experimental condition did not influence participants' preference for genuine smiles over either polite smiles, $F(4,94)=0.46$, $p=.77$, $\eta^2_p=.02$, or over neutral feedback, $F(4,94)=0.78$, $\eta^2_p=0.54$. However, it did alter participants' preference for neutral feedback versus polite smiles, $F(4,94)=3.63$, $p=.009$, $\eta^2_p=.13$ (Figure 3.4B-D).

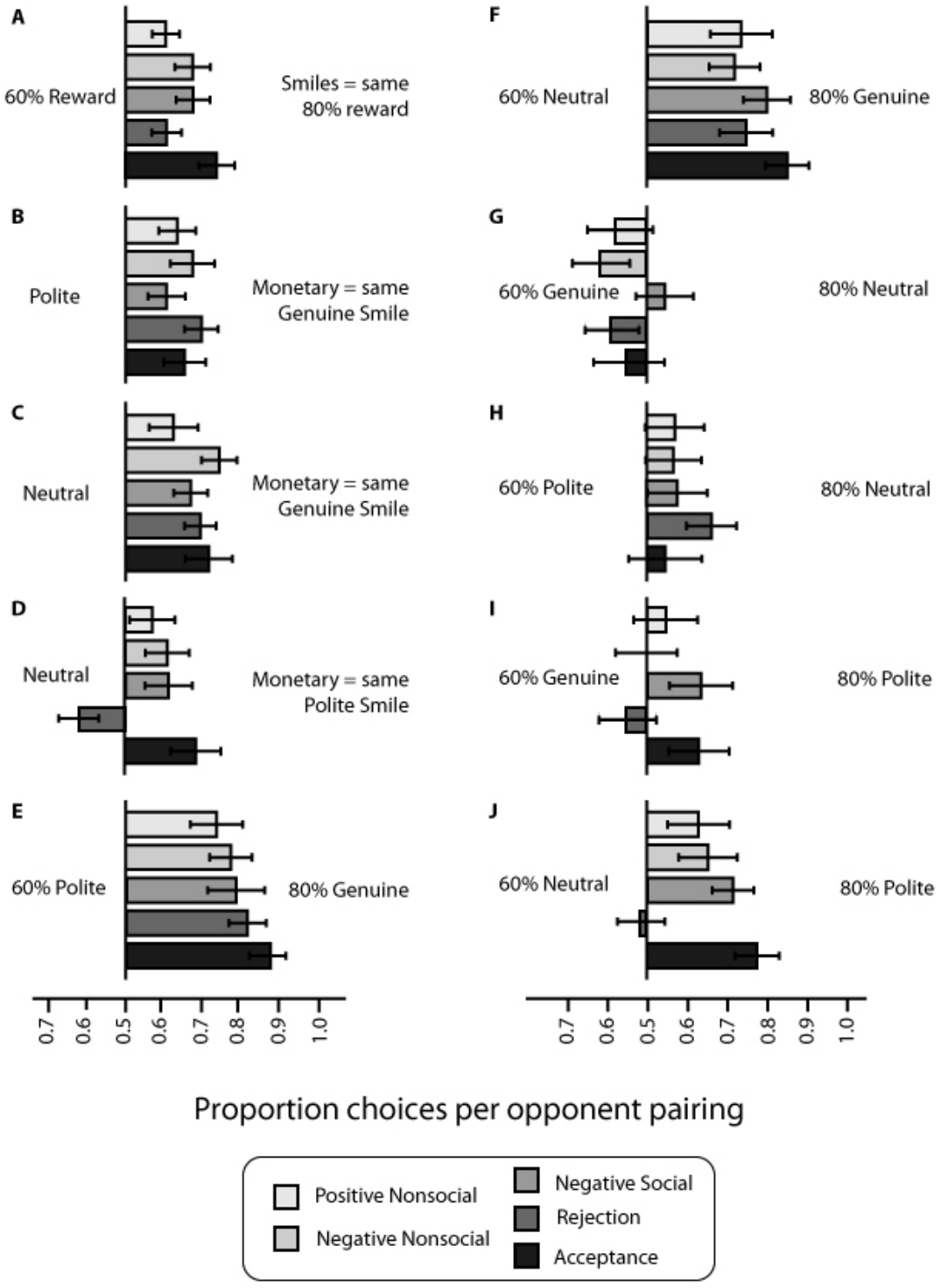

Figure 3.4. Proportion of opponent choices for all pairings Midline represents chance. A) Proportion opponent choice when social value constant. B-D). Proportion opponent choice when monetary value constant. E-J). Proportion opponent choice when monetary and social values differ. Error bars show ±1 standard error of the mean.

When faced with a choice between a politely smiling opponent and a neutral one, participants anticipating rejection showed a distinct preference for the neutral face over the politely smiling one relative to participants in all other conditions (p-values<.02) and they chose the neutral opponent significantly more often than chance, t(18)=2.34, p=.03, d= 0.30. These results suggest that in addition to enhancing the value of genuine smiles, social rejection leads to the devaluation of polite smiles.

On choices in which both monetary and social feedback differed, we examined participants' preferences for the high monetary-value choice in the sample as a whole. As Table 3.1 shows (see also Figure 3.4E-J), in most cases, participants made the optimal choice, preferring the opponent that provided the financial gains with the highest frequency. However, participants were indifferent when choosing between a low-monetary value, genuinely smiling face and both a high-monetary neutral face and a high monetary-value politely smiling face. This result replicates previous research (Shore & Heerey, 2011) and suggests that genuine smiles enhanced the utility of the faces on which they appeared.

Table 3.1.

Differences in proportion choices across conditions when monetary and social values differ

Monetary Value 60%	Monetary Value 80%			
Social Value	Social Value	t	*p*	Effect Size
Polite	Genuine	10.89	<.001	1.03
Neutral	Genuine	9.03	<.001	0.84
Genuine	Neutral	-1.78	.08	-0.16
Polite	Neutral	2.42	.018	0.25
Neutral	Polite	4.77	<.001	0.49
Genuine	Polite	1.31	.19	0.15

Note. t-tests are one-sample t-tests examining differences from chance (df=98 for all tests). Effect sizes measured with Cohen's d.

MANOVA results showed that experimental condition did not influence these preferences with the exception of choices between a high monetary value politely smiling face and a low-monetary-value neutral one, $F(4,94)=2.58$, $p=.04$, $\eta^2_p=.10$. Interestingly, participants anticipating rejection were indifferent between the faces in this choice pairing, $t(18)=-0.33$, $p=.75$, $d=0.08$, whereas all other participants preferred the high-monetary-value politely smiling face at greater-than-chance levels, (*p*-values<.02).

To determine how the experimental manipulations changed participants' moods, we calculated the differences between pre- and post-manipulation scores on the PANAS as well as the extra rejection-related items. Figure 3.5 shows these results.

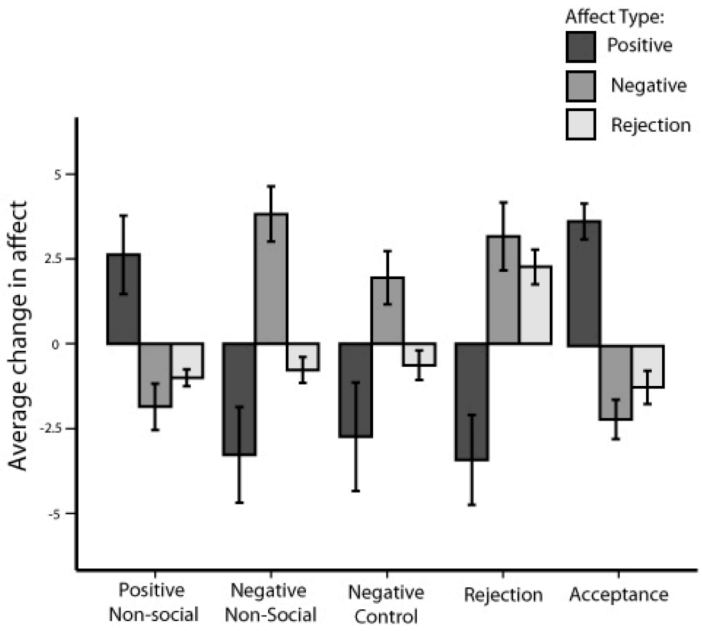

Figure 3.5. Mean changes in affect from post-pre manipulation. Error bars show ±1 standard error of the mean.

A MANOVA showed that experimental condition induced mood changes on all three measures (*p*-values<.001). Post-hoc analyses showed that participants experiencing the acceptance and positive mood induction conditions showed a significant rise in positive affect relative to the other conditions (Table 3.2 shows exact statistics). Participants in the negative

mood induction, the negative control condition and the rejection conditions experienced a significant increase in negative affect relative to the positive mood and acceptance conditions. Only participants expecting rejection showed a significant rise in the degree to which they felt rejected. These results suggest that the experience of anticipating rejection uniquely alters the utility of social feedback, over and above the simple effects of negative mood.

Table 3.2. Mean difference in Positive Affect (PA), Negative Affect (NA) and feelings of Rejection (Rej) for groups based on difference between post-pre manipulations.

	Negative Mood Induction			Negative Social			Acceptance			Rejection		
Condition	PA	NA	Rej	PA	NA	Rej	PA	NA	Rej	PA	NA	Rej
Positive Mood Induction	**5.89 (1.74)**	**5.68 (1.07)**	0.23 (0.56)	**5.36 (1.80)**	**-3.80 (1.11)**	0.37 (0.59)	-1.05 (1.83)	0.31 (1.13)	0.22 (0.59)	**6.04 (1.80)**	**-5.02 (1.11)**	**-3.26 (0.59)**
Negative Mood Induction	-	-	-	-0.54 (1.78)	1.87 (1.10)	-0.14 (0.58)	**-6.94 (1.81)**	**5.98 (1.12)**	0.45 (0.59)	0.15 (1.78)	0.66 (1.10)	**-3.04 (0.58)**
Negative Social	-	-	-				**6.40 (1.87)**	**-4.11 (1.15)**	-0.59 (0.61)	0.68 (1.85)	-1.21 (1.14)	**-2.89 (0.60)**
Acceptance	-	-	-	-	-	-	-	-	-	**7.09 (1.87)**	**-5.32 (1.15)**	**-3.49 (0.61)**

Note. Numbers represent mean differences with standard deviations in parentheses. Bold font indicates p<.05.

Finally, we used the same signal detection theory measures of sensitivity (d') and bias (criterion) as in Experiment 1 to examine participants' ability to discriminate between smile types. Consistent with Experiment 1, but contrary to previous research (Bernstein et al., 2008; 2010), we found no differences in smile discriminability across the conditions for d', $F(4,93)=0.60$, $p=.66$, $\eta^2_p=.03$. Moreover, experimental condition did not appear to bias participants to name one type of smile over another, $F(4,93)=1.83$, $p=.13$, $\eta^2_p=.07$. Overall, participants' bias to label one smile-type over another did not differ from zero, $t(97)=-1.25$, $p=.22$.

Discussion

Data from this study show two important results. First, polite smiles are important social tokens. Although they were not as subjectively desirable as genuine smiles, participants preferred polite smile feedback to feedback from neutral faces. Second, results show that the experience of anticipating social rejection shapes the utility of social feedback, relative to both the expectation of a negative interaction and to the simple effects of negative feedback. Specifically, results indicate that rather than enhancing the value of a genuine smile as previous research suggests it should (Bernstein et al., 2010; Leary et al., 1998) rejection anticipation devalues polite smiles, making them worth subjectively less than their value to other participants.

General Discussion

Together, these results highlight two important facets of social cue utility in social interactions. First, in addition to the inherent value of genuine smiles, polite smiles also hold value suggesting that they are important social tokens. Second, altering a receivers' state of social need does not increase the utility of genuine smiles as previous results would suggest (Bernstein et al., 2010; Kendrick, 2004; Leary et al., 1998), but instead decreases the value of polite smiles. Indeed, the results from Experiment 1, which showed substantially enhanced genuine smile utility, did so only in the context of the comparison between genuine and polite smiles. Thus, rather than enhancing the value of genuine smiles, rejection anticipation appears instead to reduce the value of polite smiles. This result appears to match participants' behaviour in a face-to-face social interaction. Specifically, participants anticipating rejection show reduced reciprocity of polite smiles, but do not alter genuine smile reciprocity (see Chapter 2). Because polite smiles are important social tokens to participants who are not anticipating rejection, reciprocity failures may be perceived as

social slights, thereby influencing social outcomes for the 'rejected' interaction partner.

Importantly, the general affective changes associated with anticipating social rejection cannot explain our findings. Participants in the negative non-rejection condition, who anticipated a less than usually pleasant interaction, reported similar increases in negative affect as participants anticipating rejection but did not show altered smile utility. Furthermore, participants in the negative non-social condition showed similar affective changes as the negative non-rejection participants but no smile utility differences. This implies that a specific increase in social-need state, such as that evoked by rejection anticipation, is required in order to decrease the value of a polite smile.

Interestingly, and contrary to previous findings (Bernstein et al., 2008; 2010), being in a state of social need did not alter the degree to which participants were able to distinguish genuine from polite smiles, nor did it increase their tendency to report seeing genuine smiles. One reason for our failure to replicate prior results may relate to stimulus differences. In particular, we used still photographs rather than video stimuli. This is because we found differences in the onset and offset motion profiles for genuine and polite smiles in our own video-recorded smiles, making the genuine smile videos substantially longer than those depicting polite smiles. Moreover, previous data suggest that genuine and polite smiles can be discriminated based on their motion profiles alone (Hess & Kleck, 1990), which might bias results in favour of participants in rejection conditions because of their heightened sensitivity to social cues (Pickett et al., 2004).

There are two key limitations to the present findings. First, there are many ways to manipulate social need (e.g., Lieberman & Eisenberger, 2006; Tice et al., 2001; Twenge et al., 2001; Williams et al., 2000). Although research shows that they produce similar results (Richman & Leary, 2009), it is possible that one would see differences in our task depending

on the manipulation used. However, based on participants' responses in our debriefing interview and their self-reported affect on the PANAS, we are relatively confident that they genuinely believed the feedback and had similar experiences of rejection as previous studies report. Second, we note that all our participants were healthy university undergraduates. It is unclear whether these findings would replicate in populations with chronically high states of social need as may be present in clinical disorders such as social anxiety (Heerey & Kring, 2007).

Here, we demonstrate evidence of state-dependent fluctuations in the degree to which people subjectively value social rewards. We show that in states of social need, but not other states, the subjective desirability of polite smiles is substantially reduced. Indeed, participants expecting social rejection were willing to forgo money in order to avoid seeing a polite smile (Experiment 1) and preferred feedback from neutral faces to that from politely smiling ones (Experiment 2). Thus, the degree to which a social reward is rewarding and the degree to which it influences behaviour depends on a receiver's social state. Broadly, these results support the idea that it is the utility of social stimuli, rather than their affective values that shape their social impact. Importantly, this finding corroborates results from a live interaction study which suggest that these changes in value have broad implications for behaviour and social outcomes in face-to-face interactions.

Chapter 4

Expressions of emotion: The function and flexibility of receiver ratings

Abstract

Although people are admonished to “never judge a book by its cover”, research shows that they do. People make judgements about others’ states, intentions, and traits from appearance-based cues. Although these judgments are consistent across raters, the degree to which they reflect actual sender characteristics is unclear. Here, we explore judgment accuracy and whether judgements are useful predictors of sender behaviour in a different social setting from that in which the judgements were made. We also explore the ease with which senders can manipulate the judgements others make about them. In Experiment 1, participants rated sender trustworthiness based on nonverbal behaviour during a naturalistic conversation. We then examined how these ratings guided participants’ ability to decode the same senders’ affective experience in a new setting. We found that receiver judgements did not predict decoding accuracy but that senders’ actual trustworthiness did. In Experiment 2, we show that over and above the effects of physiognomic judgments of trustworthiness, senders’ affective displays (both verbal and nonverbal) influence receivers. Taken together, these findings suggest that physiognomic judgments, based on so-called “unfakeable” facial characteristics, can be easily manipulated by senders – meaning that judgments made on this basis may be poor indicators of senders’ actual behaviour because they are based on invalid cues.

Few contest the idea that people judge others based on their physical appearance. Indeed, such ability is thought to enhance social decision-making capacity. According to this argument, if people can detect, for example, whether someone is likely to be conscientious or trustworthy, they can use this information to avoid “cheaters” and affiliate with “co-operators” (Cosmides & Tooby, 2000). In addition, trait judgments can enhance the quality and likely outcomes of interactions by enhancing people’s ability to predict others’ behaviour (Fiske & Linville, 1980)

Consistent with these ideas, a growing body of literature shows that people make rapid (Willis & Todorov, 2006) and reliable judgements about others based on how they look (e.g., Hall et al., 2008; Penton-Voak et al., 2006; Perrett et al., 1998). These impressions range from obvious gender, ethnicity and age judgments (Hess et al., 2009), to more subtle judgments about traits such as extraversion and trustworthiness (Penton-Voak et al., 2006; Todorov, 2008). Not only do people make consistent judgements about others, but they also use these evaluations to make behavioural decisions. For example, when playing an investment game with faces that were consistently rated as high or low in trustworthiness, participants invested more with opponents who were perceived as more trustworthy (van't Wout & Sanfey, 2008).

In addition to trustworthiness, people reliably ascribe many of the Big Five personality traits (e.g., extraversion, neuroticism) to static and dynamic faces (Ambady et al., 1995; Hall et al., 2008; Kramer et al., 2011; Penton-Voak et al., 2006). Given that trait ratings are consistent across judges, it seems likely that there are features of static and dynamic faces that people reliably use as cues of particular traits. However, although these findings are fairly robust, the strength of these cues as valid signals of traits is less extensively documented.

Researchers often find that people's judgments of others' personality correlate with those targets' own personality ratings (Jones et al., 2012; Kramer et al., 2011; Little & Perrett, 2007). However, some traits, such as extraversion and conscientiousness, appear to be more strongly correlated than others (Little & Perrett; Penton-Voak et al., 2006). Although some traits appear to be easily perceived, there are several factors that influence accuracy. Exposure time is one of these (Carney et al., 2007). Research shows that people accurately perceive negative affect, extraversion, conscientiousness and intelligence from video clips of only 5 seconds in length. However, accuracy for neuroticism, openness and agreeableness increases with exposure time (Carney et al., 2007). Moreover, accuracy of many personality traits increases when people rate those with whom they have close relationships (Funder & Colvin, 1988). Indeed, evidence suggests a linear relationship between relationship closeness and the accuracy with which people rate their friends and family (Biesanz, West, & Millevoi, 2007). These results suggest that for at least some traits, the quality of the relationship between rater and target is an important variable.

However, there is a small set of traits that are likely to be more important when individuals have no relationship. Trustworthiness is one of these (Boone & Buck, 2003; Cosmides & Tooby, 2000). Based on static facial features, research suggests that facial width-to-height ratio is a valid cue to trustworthiness among males (Stirrat & Perrett, 2010). Similarly, because females are more trustworthy than males (Buchan, Croson, & Solnick, 2008), people use the valid cue of target gender, when asked to predict trustworthiness (Belot, Bhaskar, & van de Ven, 2010). Moreover, research shows that people can distinguish altruists from non-altruists based on the finding that altruists tend to display more smiles (Oda et al., 2009b). This finding suggests that latent characteristics may give rise to externally observable behaviours (Gifford, 1991). For example, in a faith game, in which

participants invested with an opponent but did not learn whether their investment was fairly repaid, participants invested differently with altruists versus non-altruists (Oda, et al., 2009a). Faces rated as high in trustworthiness also attract higher investment amounts (Rezlescu, Duchaine, Olivola, & Chater, 2012). Such results suggest that people base decisions upon facial cues and characteristics.

Interestingly, smiling appears to be a reliable, as well as a valid cue of trustworthiness. Faces that are rated as more trustworthy tend to be faces that mimic positive expressions, e.g., happiness (Todorov, 2008). So robust is this finding that researchers have been able to model computer-generated faces that differ in perceived trustworthiness along a spectrum such that faces high in trustworthiness subtly mimic emotional expressions that promote approach behaviours, i.e., happiness; whereas, faces low in trustworthiness contain subtle signals of avoidance-related expressions, e.g., anger, even though these faces are all neutrally posed (Todorov, Baron, & Oosterhof, 2008a). Thus, faces that subtly indicate expressive displays may influence social judgments.

Taken together, three key findings emerge from this body of research. First, it appears that external signals, e.g., appearance- and behaviour-based cues, reflect latent traits such as trustworthiness or extraversion (Gifford, 1991; Oda, et al., 2009b; Penton-Voak et al., 2006; Shevlin, Walker, Davies, Banyard, & Lewis, 2003). Second, receivers can accurately perceive these traits using observable cues (Carney et al., 2007; Hall et al., 2008; Kramer et al., 2011). Finally, the ability to judge others' characteristics is important because it enhances the ability to predict their behaviour, and as a result, one's own social outcomes (Fiske & Linville, 1980). This third principle has received relatively less empirical attention in the literature. However, it is likely to be the most critical in terms of guiding real-world social decision-making. Here, we focus on trustworthiness, as this trait is judged both

rapidly and automatically (Todorov, et al., 2008a; Willis & Todorov, 2006) and appears to be a primary dimension in social classification (Cosmides & Tooby, 2000). Specifically, we ask whether initial impressions of trustworthiness, based on short video clips of nonverbal behaviour, enhance the ability to decode social cues in a second setting.

Experiment 1

We sought to differentiate the effects of receivers' perceptions of trustworthiness and senders' actual trustworthiness (estimated from behaviour in an investment game) on the ability to decode senders' affective displays. We hypothesised that senders' trustworthiness would predict receivers' trustworthiness ratings and that both trustworthiness estimates would influence accuracy in determining whether a target (sender) experienced positive or negative affect. Specifically, we predicted that participants would be more accurate at detecting positive emotion among senders rated as higher in trustworthiness and more accurate at detecting negative emotion among senders rated as lower in trustworthiness. In addition, we predicted that receivers would reliably rate sender trustworthiness.

Method

Participants

Fifty-one undergraduate psychology students (25 female, age: *M*= 22.65 years, *SD*= 3.40) participated in exchange for partial course credit. The University Ethics Committee approved all study procedures. Participants provided written informed consent before participating (likewise for all data collection procedures reported below).

Procedure

Participants (receivers) began the task by rating a set of 10 senders on a number of interpersonal characteristics. Receivers viewed either male or female senders

(counterbalanced across receiver gender). To rate each sender, receivers viewed a 20-second video clip (without sound) of a naturalistic casual conversation between the sender and another person who was not visible in the video (see Stimuli below). After viewing each clip, they rated the sender's trustworthiness on a 7-point Likert scale (*1 = Not at all; 7 = Extremely*).

Because senders and receivers both attended the same university and most were in psychology, we asked receivers to report how well they knew each sender on a categorical scale that assessed the amount of contact they had with each sender. If a receiver reported ever having spoken with a sender we removed that sender from the receivers' stimulus set prior to ratings to ensure that prior knowledge did not bias judgment. Based on this procedure, receivers rated an average of 9.02 senders (*SD*=1.72).

Evidence suggests that personality traits give rise to subtle but perceptible behavioural cues, especially regarding the experience of emotion (Rauthmann et al., 2011; Riggio, 1990). To examine this idea, we asked receivers to view video clips of senders as they experienced positive and negative affect and to guess, after each clip, which type of affect the sender experienced. They viewed four clips (two positive and two negative) from each sender in pseudo-random order such that receivers did not see consecutive clips from any sender.

Stimuli

Twenty Caucasian psychology students (10 female, age: *M*=20.6 years, *SD*=2.52) were recruited via advertisement to participate in a study designed to create the stimuli used in Experiment 1. In exchange for participating, they received a small payment (£6).

To capture short (20-second) clips of each sender's naturalistic nonverbal behaviour, we filmed participants while they engaged in a short conversation with an experimenter

who was not visible in the video. The experimenter began this conversation with the prompt: “How did you get interested in psychology?” All participants’ responses to this prompt were longer than 20 seconds. Therefore, to ensure that any nonverbal behaviour differences across senders were not due to differences in the conversational topic, we used video from the first 20 seconds of participants’ responses to this prompt, clipped to show senders’ heads and shoulders only. To remove bias associated with verbal content or prosody, we stripped videos of their soundtracks prior to presenting them.

To collect senders’ naturalistic displays of emotion, we asked senders to watch a series of short film clips (~90 seconds each) that included six clips depicting amusing content (e.g., comedy sketches, bloopers and outtakes, etc.), and six clips showing disgusting content (e.g., tooth extractions, medical operations, etc.) The film clips were taken from videos posted to YouTube (a list of film URLs appears in Appendix C; films were pre-rated for emotional content by an independent set of raters). Senders viewed the clips in blocks of six (three positive and three negative in random order). After watching each film clip, senders reported how they had felt while watching the clip using a series of 12 emotion probes (e.g., happy, amused, angry, afraid, etc.). They rated each clip for each emotion on using 7-point Likert scale (*1=Not at all; 7=Extremely*).

Finally, senders completed a behavioural measure of trustworthiness by playing the role of ‘trustee’ in a series of one-shot investor-trustee games with 10 individuals who, unbeknownst to participants, were computer generated (Berg, Dickhaut & McCabe, 1995). One measure of ‘trustworthiness’ in investor-trustee games is the amount of money a trustee returns to participants. More trustworthy trustees reciprocate investor trust by returning a more equitable division of the proceeds, meaning that investors profit on their

investments (Berg, Dickhaut, & McCabe, 1995). Therefore, senders' average proportion returned in the task served as a measure of trustworthiness.

Sender videos used in the emotion-judgement procedure were selected to ensure that all senders reported experiencing a high level (Likert ratings of 6 or more) of the target emotion(s), e.g., amusement and happiness for positive clips, and low levels (Likert ratings of 2 or less) of all other emotions. We also excluded films in which senders touched or covered any part of their faces or produced visible hand gestures. There were two negative and three positive films that met these criteria for all senders. We therefore selected both negative and the two most positively rated (on average) positive clips to serve as stimulus films. Therefore, the final set of sender videos consisted of four clips from each sender watching the same two positive and two negative film clips. This ensured that receivers' emotion guesses were not influenced by differences in senders' film content.

Receivers watched 10-second clips from each sender's films. These were clipped such that receivers viewed sender reactions to the 5 seconds immediately preceding and the 5 seconds immediately following, the most evocative moment of each film (as rated by an independent set of judges). Although senders viewed films with their original soundtracks, in order to ensure that these soundtracks did not bias receivers, we stripped the films of their soundtracks during the editing process.

Data Analysis

To assess reliability of receivers' ratings of trustworthiness, we treated senders as if they were items in a questionnaire with trustworthiness as the scale (e.g., Adams Jr et al., 2012; Borkenau & Liebler, 1995). This allowed us to calculate the similarity with which receivers rated senders using Cronbach's α. As receivers viewed clips from either male or female senders but not from both, we calculated Cronbach's coefficients separately for each

sex (10 females, 10 males). To offset issues caused by missing data (receivers did not rate senders whom they knew), missing data points were replaced with the average rating for that sender for this analysis only.

To determine whether receivers' estimates of sender trustworthiness (i.e., ratings) and estimates of sender's actual trustworthiness (i.e., senders' trustworthiness, based on behaviour in the investor-trustee task) influenced receivers' ability to read sender expression, we estimated a multi-level model using the Mixed Models procedure in SPSS, Version 19. Accuracy of judging the emotion display served as the outcome variable and emotion type served as an independent, dummy-coded (0=negative, 1=positive) fixed factor. As all receivers saw multiple senders, receiver ratings (Level 1) were correlated for each set of senders, and therefore nested within senders (Level 2), which we treated as a random effect as the senders in the present study are only a subset of all possible senders. Receiver estimates of trustworthiness and senders' trustworthiness, estimated from behaviour, served as predictors in the model. We also included the interactions between emotion type and sender/receiver trustworthiness estimates, because we predicted that trustworthiness estimates would interact with emotion type. We allowed both the slope and intercept for emotion to vary, as we predicted that emotion type would have differential effects on accuracy. We used a maximum likelihood estimation method and an independence model covariance structure, which makes the assumption that random effects are independent.

Finally, to assess accuracy of receiver ratings, we used a multilevel hierarchical model (Ames, Kammrath, Suppes, & Bolger, 2010; Sell et al., 2010; 2008) to determine whether senders' estimates of trustworthiness (Level 2), predicted receivers' estimates of trustworthiness (Level 1).

Results

Before using receiver ratings to predict accuracy for senders' emotion film clips, we examined the degree to which receiver ratings were consistent for each sender. Cronbach's α analyses showed that receivers achieved good consistency across both male and female senders on ratings of trustworthiness (Males: α = .75, Females: α = .67). This analysis suggests that trustworthiness ratings based on short naturalistic conversation clips are highly reliable.

Because we predicted that the degree to which someone appears trustworthy relates to subtle facial cues that resemble positive (approach) or negative (avoid) emotion (Oosterhof & Todorov, 2009), we anticipated that the cues senders generated when viewing positive and negative films would interact with perceived trustworthiness. Specifically, we predicted that accuracy for positive clips would be enhanced for senders rated as higher in

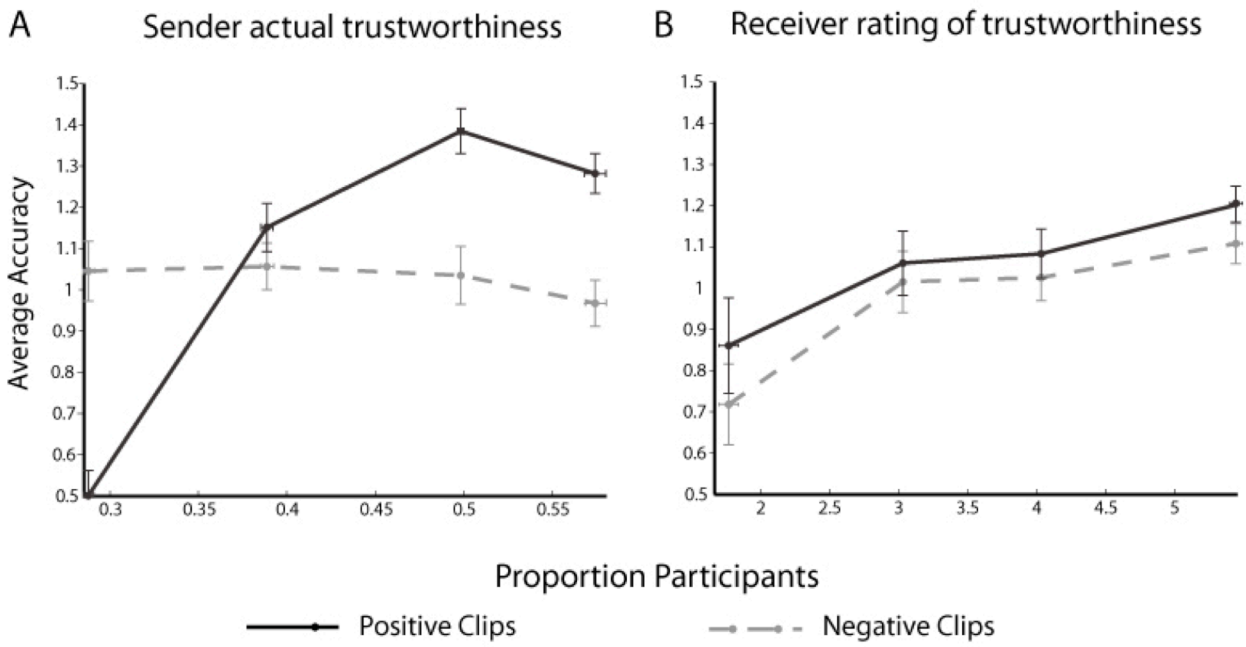

Figure 4.1. Average accuracy of emotion guesses from positive and negative clips. A). Accuracy of emotion guesses with sender actual trustworthiness as predictor of accuracy. B). Accuracy of emotion guesses with receiver ratings of sender trustworthiness as predictors. Error bars show ±1 standard error of the mean.

trustworthiness and that lower trustworthiness ratings would enhance accuracy for negative clips. Over and above the general tendency for receivers to be more accurate in detecting positive rather than negative emotion (exact results for all analyses appear in Table 4.1), we found a significant interaction between trustworthiness and emotion such that receivers were indeed more accurate at guessing positive clips for senders higher in trustworthiness (See Figure 4.1). However, contrary to prediction, results showed that it was senders' actual trustworthiness (based on behaviour), rather than receivers' trustworthiness estimates, that significantly predicted accuracy.

Table 4.1

Predictors of receiver accuracy at reading positive and negative emotion from video clips

	Regression Coefficient γ (*p*)
Trustworthiness	
Emotion	**1.25 (.03)**
Receiver Estimate (rating)	-0.001 (.95)
Sender Estimate (behaviour)	**2.23 (.007)**
Emotion x Receiver Estimate	0.01 (.78)
Emotion x Sender Estimate	**-3.05 (.01)**

Note: bold typeface indicates a significant contribution to receiver accuracy.

These findings show that sender trustworthiness, rather than receivers' trustworthiness estimates, predict receivers' ability to determine whether senders are experiencing positive emotion. This suggests that receivers were either more sensitive to positive emotion displays for high-trustworthy senders or that they developed a response bias for these senders. To determine whether "accuracy" differed because of increased sensitivity or increased bias, we coded correct identifications of positive clips as 'hits' and

coded 'false alarms' when receivers guessed that a negative emotion clip was positive. We then used a standard signal detection model (Macmillan & Creelman, 2005) to calculate a sensitivity measure (d') and a bias measure (criterion) for receiver responses to each sender. We performed a median split on sender trustworthiness (based on behaviour) to allow comparison between high and low trustworthy senders. As Figure 4.2 shows, receivers' sensitivity (d') did not differ for high and low-trustworthy senders, t(18)=-1.18, *p*=.25, *d*=-0.53, nor did it differ from chance for either type of sender (High-trust: t(9)=1.67, *p*=.13, *d*=0.52; Low-trust: t(9)=-.30, *p*=.77, *d*=-0.09).

Interestingly, receivers' bias (criterion) did differ between high and low trustworthy senders, t(18)=2.27, *p*=.04 *d*=1.02, such that receivers were biased to guess that high trustworthy senders were experiencing positive emotion. Receiver bias only significantly differed from zero for high trustworthy senders (High-trust: t(9)=-2.42, *p*=.04, *d*=-0.77; Low Trust: t(9)=1.10, *p*=.34, *d*=0.32).

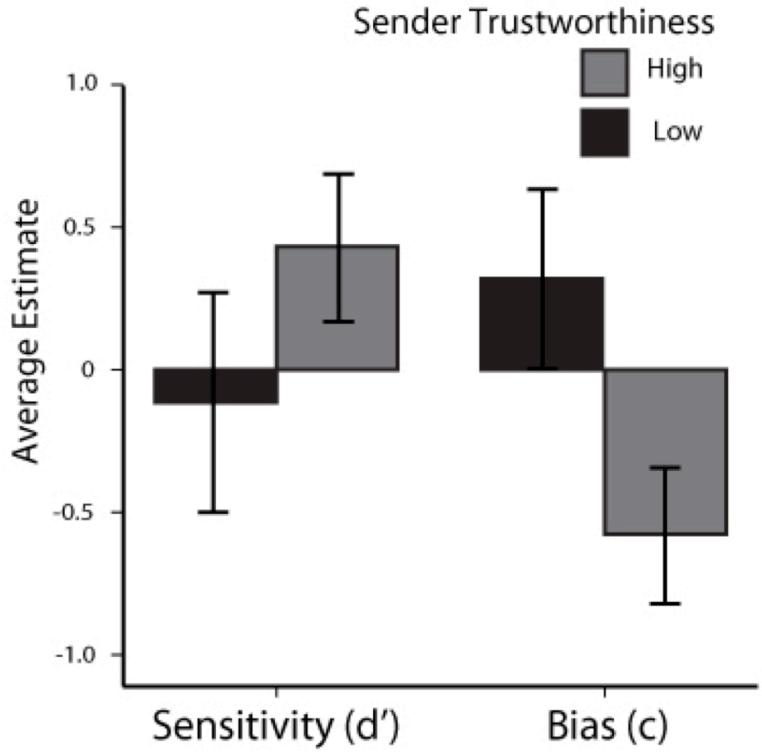

Figure 4.2. Estimates of sensitivity and bias for senders high and low in trustworthiness. Error bars show ±1 standard error of the mean.

Smiles are clear signals of positive emotion (Ekman, 1993; Ekman et al., 1990) and previous research suggests that highly altruistic people smile more (Oda et al., 2009b). Thus, one explanation for why participants were biased in their positive emotion guesses is that the highly trustworthy senders smiled more. To examine this idea, we tallied the number of smiles each sender displayed in all four film-clips. Based on smile codes in the Facial Expression Coding System (Kring & Sloan, 2007), two raters independently counted smiles. Although high-trustworthy senders tended to display more smiles during the emotion clips, this effect failed to reach the threshold for statistical significance, t(18)=1.41, *p*=.18 (high trustworthy: *M*=3.40, *SD*=1.84; low trustworthy: *M*=2.30, *SD*=1.64). This suggests that although high trustworthy senders did not necessarily smile more, they did provide some signal that biased receivers' interpretations of the clips.

Interestingly, smiling did appear to enhance perceptions of trustworthiness in the casual conversations. A similar analysis of sender behaviour from the casual conversation clips showed that although high (*M*=2.40, *SD*=2.68) and low trustworthy (*M*=2.00, *SD*=2.11) senders did not differ in smile frequency in these clips, t(18)=0.37, *p*=.72, *d*=0.17, senders who smiled more were viewed as more trustworthy (high-frequency smilers: *M*=4.49, *SD*=.39; low-frequency smilers: *M*=3.88, *SD*=.63). We performed a median split on the total number of smiles each sender produced and then calculated the average trustworthiness rating for each sender. High-frequency smilers were rated as significantly more trustworthy than were low-frequency smilers, t(18)=2.60, p=.02. There was no correlation between actual sender trustworthiness and smile frequency in the casual conversation clips, r(20)=.08, p=.73, nor was there any correlation between smile frequency in the casual conversations and smile frequency in the emotion clips, r(20)=.31, p=.19. These data show both that smile frequency varies considerably across contexts and that receivers use smile

frequency as one indicator of a sender's potential trustworthiness, even though in this sample, smiles did not appear to be a valid indicator of a sender's true trustworthiness.

These results suggest that receivers' trustworthiness judgments may lack validity. To test the degree to which receivers' estimates agreed with specific senders' actual trustworthiness, we applied a hierarchical linear model (Morrison, Gralewski, Campbell, & Penton-Voak, 2007; Sell et al., 2008; 2010) with receiver data (Level 1) nested within sender data (Level 2). To ensure that the betas in the model were comparable, we standardized receiver ratings and our estimates of senders' actual trustworthiness prior to analysis. The receiver estimate served as the outcome variable, and the sender estimate served as the predictor. Senders' estimated trustworthiness did not significantly predict receiver ratings of trustworthiness, b=0.12, S.E.=0.10, 95%CI -0.09, 0.32, p=.25. These findings suggest that trustworthiness judgments do not appear to correspond with actual sender trustworthiness.

Discussion

Although receivers reliably rate sender trustworthiness based on dynamic clips of senders' non-verbal behaviour, these ratings do not appear to be valid reflections of senders' actual trustworthiness. Furthermore, these judgements do not help receivers decode senders' nonverbal displays in other contexts, as previous literature suggests they should (Carney et al., 2007). In fact, only senders' trait trustworthiness estimates predicted accuracy scores in the emotion judgment task. Interestingly, a bias to guess that high-trustworthy senders viewed positive films appeared to cause this increase in "accuracy," even though high-trustworthy senders did not smile more in the clips. Moreover, from the receiver perspective, smiles appeared to be a clear indicator of sender trustworthiness such that receivers overestimated the trustworthiness of high smile-frequency senders and underestimated that of low smile-frequency senders. Thus, the presence of smiles appears

to enhance or bias trustworthiness judgments, despite the fact that smiles do not appear to be a valid cue of trustworthiness. Experiment 2 provides an explicit test of this idea.

Experiment 2

Here, we test the idea that senders' nonverbal displays bias receiver judgements of trustworthiness, over and above senders' physiognomic cues. Moreover, because there are many real-world occasions in which the valence of senders' non-verbal displays conflicts with the affective content of their verbal utterances, we also ask how these mismatches influence trustworthiness judgments. We predicted that positive nonverbal displays (smiles) might lead to more positive ratings of trustworthiness, whereas negative displays (frowns) might lead to reduced trustworthiness ratings as previous research shows (Centorrino, Djemai, & Hopfensitz, 2011; Krumhuber et al., 2007; Krumhuber, Manstead, & Kappas, 2006; Mehu, Grammer, & Dunbar, 2007a; Mehu, Little, & Dunbar, 2007b). However, we expected that when senders' nonverbal displays were incongruent with their verbal content, they would be rated as less trustworthy. As in previous research, we used still images of neutrally posed senders to collect baseline judgments and compared these to both photos and videos of those same senders displaying positive and negative expressions, and in the case of videos, congruent and incongruent verbal content. We also asked receivers to rate senders on two additional traits: attractiveness and dominance; both important dimensions of interpersonal perception (Gutierres, Kenrick, & Partch, 1999) that also relate to trustworthiness (Oosterhof & Todorov, 2009)

Method

Participants

Six hundred and ninety-seven people responded to an online survey. Of these participants, 306 (44%) completed at least one rating, and 237 (34%) completed the entire

survey. To maximise depth of the data set, we only analysed data from participants who contributed a full dataset (N=237; Mean age: 31.51, S.D. = 9.69, 155 female, 80 male, 2 unidentified; 193 identified as white, Caucasian or European, 34 identified as non-white, African, black, Asian, east Asian, etc., and 10 chose not to disclose ethnicity; they reported originating from Africa, the Americas, Asia, Australia and Europe).

Procedure

Participants (receivers) learned about the online study, hosted at www.surveygizmo.com, via word-of-mouth, social networking sites, and online psychology experiment databases (e.g., Todorov et al., 2008a). They received no remuneration for their participation.

Receivers viewed a series of video clips and still images of eight senders and were asked to rate each sender for trustworthiness, attractiveness and dominance. Senders were presented in random order but were selected from the collection of senders such that each receiver saw a balanced number of males and females. They each saw 6 videos of senders speaking short sentences that were positive, neutral or negative in verbal content. Verbal and nonverbal behaviour were congruent in some of the clips and incongruent in others. Receivers rated six senders from videos that included each nonverbal display condition (2 positive, 2 negative and 2 neutral in nonverbal content). Receivers also rated two senders from still images (each with a different affective display, e.g., one positive and one negative). In order to avoid carry-over effects among receivers, each receiver viewed each sender only once. Senders were presented in random order. A complete description of the stimuli appears below.

After viewing each stimulus, receivers rated senders on three characteristics. They responded to the prompt “I think this person is…” followed by a pair of adjectives describing

a trait (e.g. trustworthy, honest). The adjectives assessed attractiveness, dominance, and trustworthiness. All items were rated on a 7-point Likert scale (*1=Disagree Strongly; 7= Agree Strongly*).

Stimuli

Twelve individuals (six male; age: *M*=22.91, *SD*=2.70; 4 Indian, 1 Pakistani, 1 Iranian, 1 mixed Irish and 5 white British) participated in the study as senders, in exchange for a small monetary payment (£6). Senders produced a series of twenty positive, neutral and negative statements in front of a video camera (adapted from (Russ, Gur, & Bilker, 2008) and verified for affective content by an independent sample of raters). After hearing the experimenter read each statement, senders looked at the camera and repeated the sentence in each of three affect conditions: positive (smile), neutral and negative (frown). Each sender produced each sentence in each display condition, meaning that verbal and nonverbal content conditions were fully crossed. Therefore, in some of the videos, the verbal and nonverbal content were congruent, and in others, they were incongruent. Senders also produced facial displays of positive (smiles) and negative affect (frowns), along with a no-display/neutral pose for 20 seconds each. Still images of each actor displaying positive, neutral and negative affect were clipped from these sequences based on expression and image quality. Figure 4.3 shows a typical example of each display. Videos were recorded in high definition at a rate of 25-frames/second, and subsequently digitised. The final images/films were cropped to show the head and shoulders of each sender.

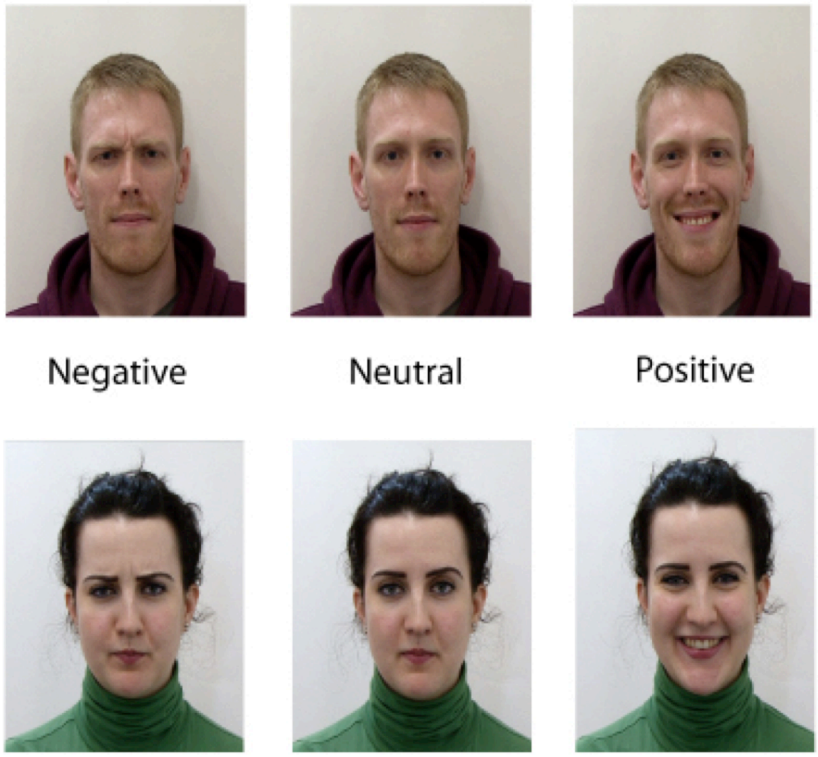

Figure 4.3. Example of static stimuli displays.

The final stimulus set included nine film clips from each sender (one positive, one neutral and one negative sentence in each display condition) along with three still images (positive, neutral and negative expressions). The three final sentences were chosen based on the length of each sentence (about 1s for all senders), the consistency of sender pronunciation, and simplicity of sentence construction (Positive: “That show makes me laugh”; Neutral: “The book is on the floor”; Negative: “I didn’t get the scholarship”). This ensured that verbal content did not vary across senders.

We collected data for each sender for 12 possible display conditions (see Figure 4.4). Each sender was rated in each condition an average of 13.17 times (S.D.=0.73).

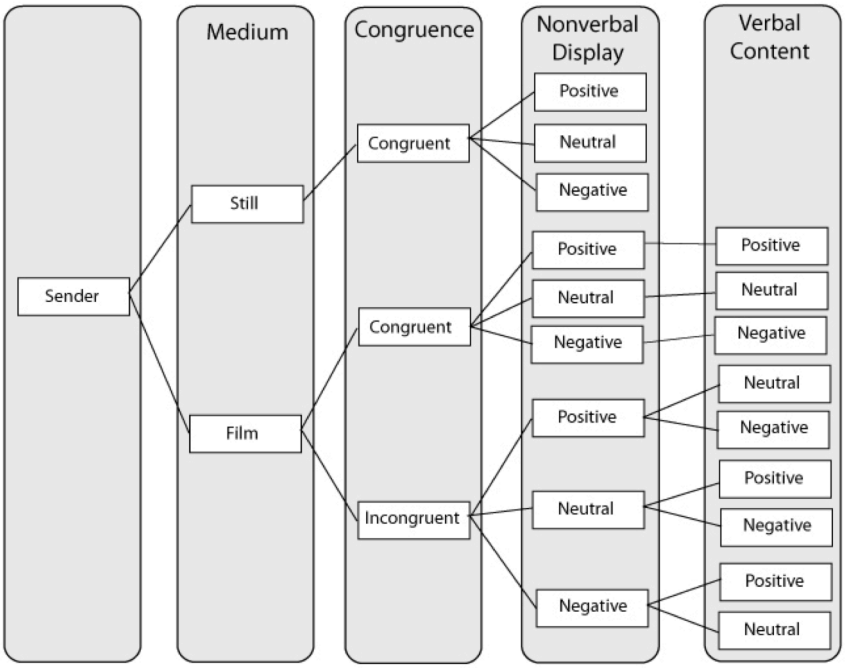

Figure 4.4. Twelve display conditions.

Data Analysis

Most studies of interpersonal judgments use still images of senders in neutral poses as stimuli (Adams Jr et al., 2012; Hess et al., 2009; Kramer et al., 2011). We therefore treated each sender's neutral still image as a "baseline" rating, which allowed us to control for basic differences in sender physiognomy. We calculated the average ratings of each sender on each trait from the neutral static image and subtracted this rating (on a trait-by-trait basis) from all other ratings of that sender. Thus, data analyses utilise mean-centred ratings. This process allowed us to examine the effects of nonverbal display, verbal/nonverbal congruence and stimulus display medium (photo, video), over and above individual differences in average perceptions of each sender and also allowed us to interpret

γ as a standardised regression coefficient (in multilevel analyses, gamma (γ) represents the Level 2 coefficients). These mean-centred ratings for attractiveness, dominance and trustworthiness served as outcome variables on senders (Level 1; random effect) nested within receivers (Level 2) in three Hierarchical Linear Models (HLM; examining trustworthiness, dominance and attractiveness ratings) using the Mixed Model procedure in SPSS Version 19. Congruence of verbal and nonverbal behaviour, nonverbal display (positive, neutral, negative) and medium of display served as predictors in the model. We also included the interactions between nonverbal display and congruence, and between nonverbal display and medium. As we did not have any Level 2 predictors in our model, the inclusion of Level 2 units was merely to statistically control for the fact that our 237 receivers each rated eight senders. We used a maximum likelihood estimation method and an independence model covariance structure, as in Experiment 1.

Results

We hypothesised that affective display, congruence of display, and medium would influence receivers' ratings of sender traits. Therefore, we included nonverbal display (negative, neutral, and positive); congruence (congruent, incongruent) and medium (film and stills) in the hierarchical linear models. As described in Table 4.2, in addition to medium and congruence, nonverbal display was a significant predictor of receivers' ratings of senders' trustworthiness, attractiveness and dominance. Senders were rated as more trustworthy and attractive but lower in dominance when they displayed smiles, and more dominant and less trustworthy and attractive when they displayed frowns (see Figure 4.5). Thus, for all three ratings, there was a main effect of nonverbal behaviour showing that expressive displays alter ratings over and above the individual differences between senders.

We anticipated that congruence might bias ratings, particularly of trustworthiness, such that when senders displayed incongruent verbal and non-verbal behaviours, they would be rated as less trustworthy than when their displays were congruent. Results, displayed in Table 4.2, support this idea, showing congruence to be a significant predictor of trustworthiness ratings, but not of ratings of dominance or attractiveness. As Figure 4.5 shows, receivers tended to rate senders higher in trustworthiness when their verbal and non-verbal displays were congruent.

Table 4.2. Summary of Hierarchical linear modelling of difference scores for receiver ratings of trustworthiness, attractiveness and dominance

Trait		Regression coefficient γ (*p*)
Trustworthiness	Nonverbal Display	**.20 (.05)**
	Verbal/Nonverbal Congruence	**.20 (.002)**
	Medium	**.24 (.001)**
	Congruence x Nonverbal display	**.21 (.01)**
	Medium x Nonverbal Display	-.13 (.17)
Attractiveness	Nonverbal Display	**.46 (<.001)**
	Verbal/Nonverbal Congruence	.10 (.13)
	Medium	.06 (.45)
	Congruence x Nonverbal display	.78 (.38)
	Medium x Nonverbal Display	-.15(.13)
Dominance	Nonverbal Display	**-.68 (<.001)**
	Verbal/Nonverbal Congruence	.03 (.68)
	Medium	**-.23 (.01)**
	Congruence x Nonverbal display	**.25 (.01**)
	Medium x Nonverbal Display	**.34 (.003)**

Note: Bold indicates significant predictors.

Previous research suggests that ratings made from photos and videos are correlated (Roberts et al., 2009). To examine this relationship, we included medium of display in the models. As Table 4.2 shows, display medium was not a significant predictor of receiver ratings of attractiveness. However, medium was a significant predictor of trustworthiness,

such that senders were rated as more trustworthy in films than in photos. Interestingly, the opposite was true of dominance ratings for which senders were rated as more dominant in photos relative to film clips.

Together with these main effects, the HLMs also revealed significant interactions between the predictors. Specifically, for ratings of trustworthiness, there was a significant interaction between nonverbal display and congruence. The planned comparisons modelled in Table 4.3 show that this significant effect was driven by the positive display conditions. In particular, a positive display coupled with positive verbal content enhanced perceptions of trustworthiness whereas for other displays, content did not alter ratings from baseline (see Figure 4.5). To further understand these interactions, we ran separate HLM analyses for each nonverbal display condition (see Table 4.3). These analyses showed that congruence significantly predicted trustworthiness ratings in the positive but not negative or neutral displays

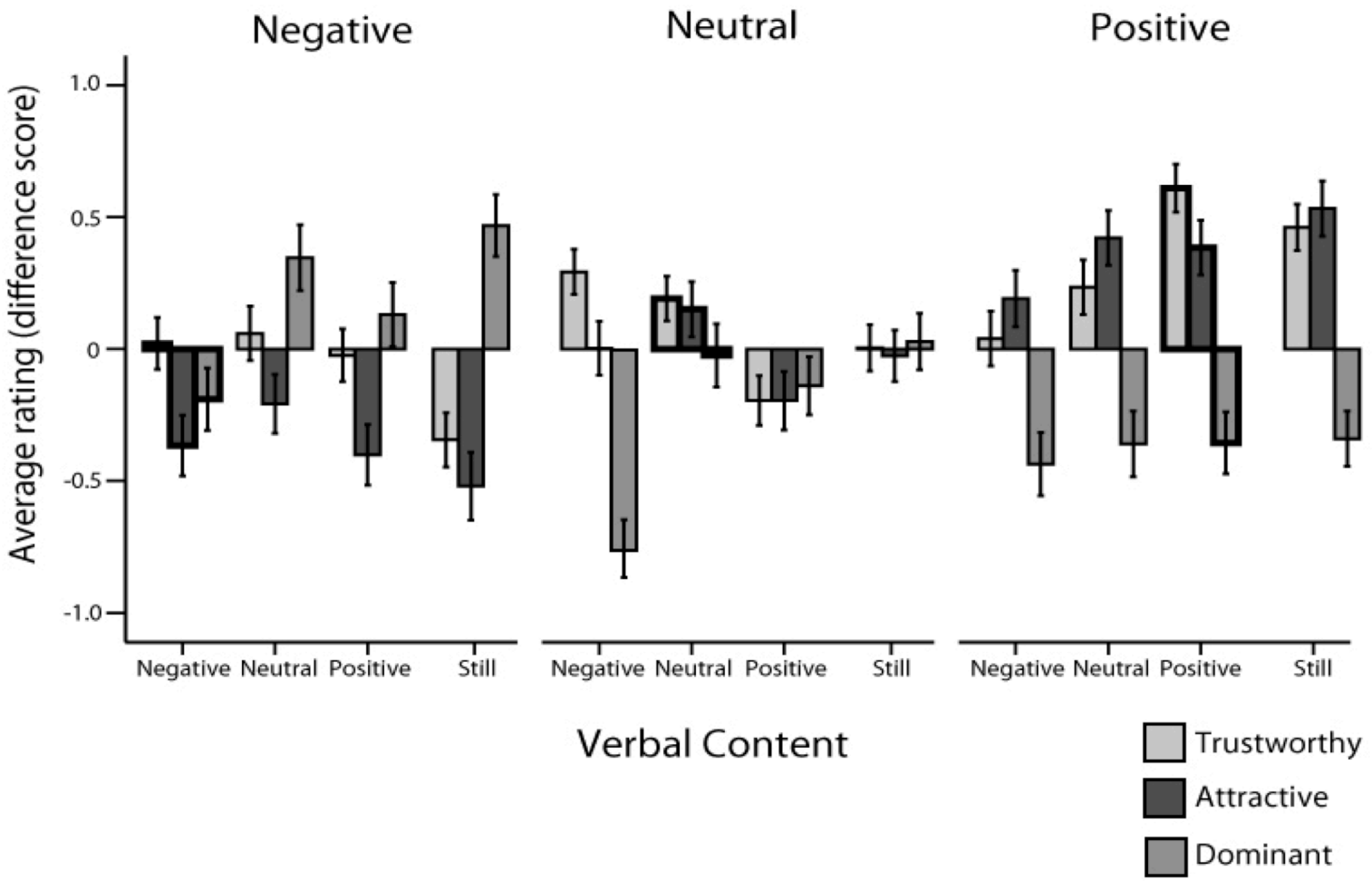

Figure 4.5. Ratings of trustworthiness, attractiveness, and dominance for all combinations of nonverbal display and verbal content. Bars with bold outline indicate congruent verbal and nonverbal displays. Errors bars show ± 1 standard error of the mean.

Ratings of dominance were also influenced by interactions between the main predictors. Namely, there was a congruence by nonverbal display interaction. Separate HLM analyses (see Table 4.3) for each non-verbal display type showed that congruence significantly predicted dominance ratings when verbal and nonverbal displays were negative and neutral. However, as Figure 4.5 shows, when nonverbal display was negative and verbal content was neutral, receivers rated senders as higher in dominance compared to baseline measure of the neutral still image. However, when the nonverbal display was neutral, the incongruent condition, in which senders uttered negatively valenced sentences, they were rated as less dominant than baseline, suggesting that interpersonal judgments are significantly more nuanced than what simple ratings from still images would suggest.

Table 4.3. Planned comparisons of nonverbal displays of receiver ratings

Trustworthiness	Nonverbal Display		Regression coefficient □ (p)
	Negative	Congruence	.004 (.97)
		Medium	**.39 (.003)**
	Neutral	Congruence	.12 (.24)
		Medium	.20 (.11)
	Positive	Congruence	**.48 (<.001)**
		Medium	.16 (.23)
Dominance			
	Negative	Congruence	**-.37 (.009)**
		Medium	**-.65 (<.001)**
	Neutral	Congruence	**.44 (.001)**
		Medium	-.05 (.74)
	Positive	Congruence	.03 (.84)
		Medium	.02 (.91)

Note: bold signals significant predictors of ratings of senders. Though medium appears to be a significant predictor of trustworthiness rating with negative displays, as there was no significant nonverbal by medium interaction in the omnibus model, we did not interpret this effect.

Discussion

Taken together, these findings suggest that, though biases differ depending on the trait being rated, they do indeed occur. For trustworthiness, the nonverbal display

influenced ratings, as did the correspondence between nonverbal and verbal display. This was particularly true when the nonverbal display was positive. Considering that trustworthiness and honesty are interrelated traits (Boone & Buck, 2003), this finding is not surprising. With respect to attractiveness, people tended to be rated as more attractive when they smiled and less attractive when they frowned, as previous results suggest. However, for dominance, the picture was complex. Static negative images appeared more dominant than dynamic clips. There was also a congruence by nonverbal display interaction, which showed that when senders presented neutral displays alongside negative verbal content, dominance ratings were reduced. This is an unusual finding, and suggests that congruence might be an important factor in perceptions of dominance. Thus, affective displays convey information about senders' immediate states (Ekman, 1993), as well as influencing the judgements that receivers make about senders' characteristics. These results suggest that, when assessing the accuracy of perceptions of others, it is important to take into account the ease with which senders may bias these perceptions.

General Discussion

A large body of research shows that people make reliable interpersonal judgments based on appearance. However, in accord with a growing body of literature (Olivola & Todorov, 2010a; Rezlescu et al., 2012), our findings suggest that these judgments may lack validity. Here, we explored whether judgements of trustworthiness, made in one context, would help receivers decode behaviour in another. Not only were receiver judgements of sender trustworthiness inaccurate, they did not enhance decoding performance. Remarkably, however, senders' actual trustworthiness predicted receivers' ability to decode sender behaviour, suggesting that trustworthy senders may signal subtle and honest cues of

their behaviour but that these are not related to the cues receivers use when making trustworthiness judgements.

One explanation for the poor validity in trustworthiness judgments may have been that receivers relied on smiles when judging trustworthiness. However, not only did this cue lack validity as an indicator of trustworthy behaviour, it caused bias in receiver judgments. This suggests that the so-called "unfakeable" facial characteristics (Rezlescu et al., 2012) by which people judge one another may be easily modified by moment-to-moment changes in nonverbal behaviour and are therefore unlikely to lead to accurate predictions of senders' future behaviour.

The present findings are contrary to previous research suggesting that people accurately perceive trustworthiness based on behaviour. However, this previous research has tended to focus on situations in which trustworthiness is a contextually salient cue, for example, when people make decisions in a trust game (e.g. Oda et al., 2009a; Oda et al., 2009b). Experiment 1 suggests that in more naturalistic circumstances, for example in casual conversation and when senders displayed affective reactions to emotional stimuli, receivers were unable to accurately rate sender trustworthiness or to use trustworthy senders' signals appropriately.

The findings from Experiment 2 showed that one reason for the reduced validity of trustworthiness ratings was that receivers relied too heavily on nonverbal behaviour in making their ratings. Indeed, as research shows, people rate computer-generated faces that subtly mimic approach-related emotional expressions, e.g., happiness (Todorov, et al., 2008a) as trustworthy and those that subtly mimic avoidance-related expressions, e.g., anger, as more dominant. Our results show that not only does this finding generalise to real, rather than computerized faces, but that these expressions bias receivers' ratings of

trustworthiness, dominance and even attractiveness. Together these findings suggest that receivers' perceptions of sender traits are heavily dependent on emotional expression and that the perception of trustworthiness may just be a general propensity to think someone looks 'nice'. This means that interpersonal judgments, such as impressions of trustworthiness, are highly prone to manipulation and that it is possible to 'fake' the appearance of trustworthiness as long as cues expressed in both verbal and nonverbal channels are congruent and positive.

In the laboratory setting, participants tend to make judgements of others based on well-controlled, neutral static images (Adams Jr et al., 2012; Jones et al., 2012; Oosterhof & Todorov, 2008; Todorov, et al., 2008a). However, in the social world, 'senders' are much more complex multi-dimensional stimuli. Thus making social decisions based on appearance is a 'noisier' process in the real social world. Data from Experiment 2 show that some of this 'noise' is the product of both the specific nonverbal display and the verbal content that shapes its context. Thus, in more naturalistic environments, the combination of cues and the context in which they occur might significantly influence the accuracy of people's interpersonal judgements.

There are, however, limitations to this study. In Experiment 1, although receivers did not show an increased ability to discriminate between positive and negative affect for high and low trustworthy senders, they did show a bias to rate high trustworthy senders as displaying more positive affect. However, contrary to expectations, and indeed what previous research suggests (e.g., Oda et al., 2009b), coding of sender behaviour did not show the expected increase in the number of smiles displayed. Research of static images of trustworthy faces suggests that there are properties of the static face that mimic approach-related expressions, e.g., smiles (Oosterhof & Todorov, 2009) so although we do not see a

behavioural difference, our findings support the idea that trustworthiness may be linked to positivity.

A second limitation relates to generalizability. Although we used naturalistic stimuli, the experimental setting is controlled. In a social interaction, the interdependence between the senders and receivers of cues, and the unfolding of the interaction may lead judgements to be biased in different ways. However, this current method does allow for exploration of potential mechanisms to explain biased judgement, which occurs in live interaction.

Conclusions

An underlying assumption of previous research on social judgements is that these judgements are useful in allowing receivers to understand and predict sender behaviour (Carney et al., 2007; Fiske & Linville, 1980). However, the present findings suggest the opposite. We find that under naturalistic conditions, reflective of how receivers might make and use social judgments in real-world interactions, participants are able to make reliable but not valid judgments of sender trustworthiness and that changes in senders' verbal and nonverbal displays bias receivers' judgments. Together, these findings suggest that not only are such judgments poor indicators of people's actual behaviour, but also that the cues that lead to judgments of high trustworthiness may be easily faked by shrewd senders and therefore may serve to expose receivers to potential social harm, rather than protecting them as researchers suggest.

Chapter 5

Experimenter beliefs influence task outcomes in social priming tasks

Abstract

Research shows that during social interactions, one individual's expectations about another can shape that person's behaviour. For example, when people hold stereotyped beliefs about others in mind, those individuals' behaviour may unintentionally conform to those stereotypes. Here, we examine whether such beliefs affect behaviour in specifically predictable ways. In the context of three "experimenter effects" studies, we asked whether experimenters could influence participant behaviour, simply by expecting a particular behaviour to occur. Using an implicit prime to activate the concept of social power, we show that when an experimenter has a belief about whether a participant will experience either a high- or a low-power priming condition, the participant's behaviour on an experimental task reflects the condition the experimenter thinks is active. This happens regardless of which condition the participant actually experiences during the priming procedure. In Experiment 1, we show that when an experimenter believes participants will receive a high-power prime participants show better abstract-thinking capacity on a subsequent task. In Experiment 2, participants took more risks when experimenters thought they were in a high-power condition. In a third experiment, this time using a double-blind design, participants failed to show a well-established power-related increase in executive cognition, despite showing differences in the degree to which they felt powerful. Findings from these studies show that people's expectations can influence others' behaviour in specifically predictable ways and suggest that people's judgments, stereotypes and beliefs have important consequences for shaping social behaviour and the outcomes of social interactions.

One important factor that determines the nature and quality of a social interaction is the set of beliefs each individual brings to that interaction. These "priors" shape both the behaviours people produce (Herr, 1986) and the ways in which they interpret their social partners' actions (Hamilton, Sherman, & Ruvolo, 1990; Herr, Sherman, & Fazio, 1983). Indeed, research on effects such as the "self-fulfilling prophecy" (Merton, 1948) suggests that over time, people not only deduce these priors but conform to them. However, most recent research in this area has focused on how the beliefs participants hold about specific groups shape behaviour towards members of those groups, e.g., boys versus girls; racial in-group versus out-group members (Bodenhausen & Wyer, 1985; Wheeler & Petty, 2001); and whether members of those groups tend to conform to social stereotypes (Kray, Thompson, & Galinsky, 2001; Steele & Aronson, 1995). Here, we take an individualistic approach to this question and ask whether a prior, held by one member of a dyad about the other's behaviour, can generate a specifically predicted pattern of behaviour from the other dyad member. We do this by manipulating an experimenter's belief in the context of a social psychology experiment involving an implicit prime.

The literature on experimenter expectancy or bias effects is relatively contentious (Rosenthal, 1994) and appears to have concluded that at least some of the studies designed to show experimenter effects are statistically or methodologically unsound (Barber & Silver, 1968; Barber et al., 1979). Although research methods coursework in psychology commonly teaches that experimenters *can* affect participants' behaviour (e.g., Hogg & Vaughan, 2008), few recent studies in the implicit priming literature report any attempt to create a double-blind experimental environment (Bargh & Williams, 2006; Bargh, Chen, & Burrows, 1996; Bargh, Gollwitzer, Lee-Chai, Barndollar, & Trötschel, 2001; Galinsky, Magee, Inesi, & Gruenfeld, 2006; Lammers, Galinsky, Gordijn, & Otten, 2008; but see Doyen, Klein, Pichon,

& Cleeremans, 2012; Pashler, Coburn, & Harris, 2012). In many implicit priming studies, an experimenter asks participants to do a task (e.g., prompted writing exercise, scrambled sentence task) that, from the participants' view, is unrelated to the measure of interest but nonetheless generates a behavioural effect on that measure. If experimenters are not blind to task conditions, their beliefs about study outcomes might subtly influence participants' behaviour and therefore actual study outcomes.

We chose to examine this experimenter effect with a priming task designed to elicit differences in social power, defined in the literature as the ability to access, control and distribute available resources within a group (Keltner, Gruenfeld, & Anderson, 2003). There were two primary reasons for choosing social power over other concepts. First, social hierarchies are ubiquitous in interpersonal settings and cues about social status are therefore likely to be salient and adhered to (e.g., Sidanius & Pratto, 1999; Sidanius, Pratto, & Bobo, 1994). Moreover, experimenter-participant dyads embody this type of power relationship between individuals (Federman, Hannah, & Rodriguez, 2003). Second, the effects of primed social power have been widely researched and are quite reliable (Galinsky, Gruenfeld, & Magee, 2003; Galinsky, Magee, & Gruenfeld, 2008; Gruenfeld, Inesi, Magee, & Galinsky, 2008; Guinote, 2008; Magee, Galinsky, & Gruenfeld, 2007; Weick & Guinote, 2010). Therefore, we were able to develop specific hypotheses about the outcomes of social power manipulations across a number of different behavioural domains.

In the cognitive domain, experimentally inducing feelings of high, relative to low social power, leads to better executive cognition, including more flexible attention, planning, reasoning, abstract thinking, working memory, and cognitive control (Keltner et al., 2003; Overbeck & Park, 2006). They are also better able to inhibit the influences of distractors when responding to targets (Willis, Rodríguez-Bailón, & Lupiáñez, 2011). Finally,

they engage in more abstract thinking (Smith & Trope, 2006), solve problems more efficiently (Fan & Gruenfeld, 1998), and rely on automatic and first-hand experience to a greater degree when making decisions (Weick & Guinote, 2008; Fiske, 1993).

In the domain of emotion and motivation, high-power primed participants engage in more approach-related behaviours than do low-power primed participants (Keltner et al., 2003), particularly when power is perceived as legitimate (Lammers et al., 2008). For example, they show increased sensitivity to rewards, more risk taking behaviours, increased optimism, and reduced negative affect (Anderson & Galinsky, 2006; John & Srivastava, 1999; Keltner et al., 2003; Maner, Gailliot, Butz, & Peruche, 2007b). Interestingly, research in naturally occurring hierarchies shows similar results. In corporate environments, managers tend to take more risks and show greater reward sensitivity than do their employees (Ronay & Hippel, 2010).

Researchers use three primary techniques for experimentally evoking power differences in the laboratory. The least frequently used of these methods involves explicit assignment to roles with implied power differences, e.g., boss and employee, for a role-play interaction (Hall, Murphy & Mast, 2006). Although this type of prime tends to be quite strongly evocative of social status differences, the nature of the role assignment means that participants are not blind to task condition. More commonly, researchers prime concepts of power implicitly, for example, by asking participants to write about a time in which they either held a position of power over another or were subordinate (Lammers, Galinsky, Gordijn, & Otten, 2012; Willis et al., 2011). Another implicit prime involves the use of scrambled sentence tasks, in which participants produce grammatically correct sentences using words associated with high or low social power (Galinsky et al., 2003). These primes are implicit because participants are usually unable to articulate the concept associated with

the prime and researchers exclude data from participants who show such awareness. Many researchers treat both types of implicit priming tasks interchangeably (Guinote, Weick, & Cai, 2012; Inesi, Gruenfeld, & Galinsky, 2012; Lammers & Stapel, 2010).

Over a set of three experiments, we use priming tasks designed to activate participants' feelings of high or low social power while independently manipulating experimenter knowledge about participants' conditions and therefore about expected results. Broadly, we anticipated that experimenter beliefs would shape participants' behaviour, over and above the effects of the priming. Thus, we predicted enhanced effects in conditions in which participants' priming condition matched experimenter belief.

Experiment 1

In this experiment, we examine how an implicit power prime and an experimenter's beliefs about prime condition affect participants' abstract thinking ability. The measure of interest was a word categorization task, as reported in Experiment 1 of Smith and Trope (2006). In the task, participants classify the degree to which each of a set of exemplars fit a category label. The exemplars relate strongly, moderately or weakly to the category label. Smith and Trope (2006) reported that participants receiving a high-power prime categorized "weak" category exemplars more strongly than did participants receiving a low-power prime. We used a scrambled-sentence priming task (Galinsky et al., 2003) in which participants produced sentences from word lists that included words associated with high or low power. We chose this priming task because it is easy for a computer to administer and therefore easy to manipulate without experimenter knowledge.

Method

Participants

One hundred and sixteen psychology undergraduates participated individually in a study about 'cognition and mood' in exchange for partial course credit. We excluded four participants' data, based on poor English fluency (they all needed the aid of a dictionary during the word categorization task) and an additional participant's data because he indicated suspicions about the power prime. The final sample therefore included data from 111 participants (77 female, 34 male, age: *M*=21.64, *SD*=4.44). They gave written consent before participating and were fully debriefed upon study completion. The University's ethics committee approved all procedures (likewise for Experiments 2 and 3 below).

Procedure

Working from a script (see Appendix D), an experimenter greeted each participant, gave a general overview of the study procedures, obtained consent and basic demographic information, and answered participants' questions. After this procedure, a computer administered the entire experimental protocol, including questionnaires and specific task instructions, meaning that the experimenter had no further contact with participants until the debriefing phase of the study. To measure any differences in mood across the conditions, participants completed the Positive and Negative Affect Schedule (PANAS; Watson et al., 1988) measured using a 100-point visual analogue scale, as a baseline measure of positive and negative affect. To measure feelings of power, we embedded seven power-related words into the PANAS at random points (subordinate, powerless, dependent, unimportant, dominant, self-assured, influential; the first four of these were reverse-scored).

After the PANAS, participants completed a computerized version of the same 16-item scrambled-sentence priming task reported in Smith and Trope (2006; Experiment 2). On each trial, they made grammatically correct sentences by using a mouse to select and organize four out of five randomly ordered words. In the high power condition, eight of the sentences included high power-related words ("dominates," "commands," etc.) and in the low power condition, eight of the sentences contained words associated with low power ("subordinate," "obeys," etc.). A second PANAS, including the additional power items followed the priming task.

Participants then completed an English-language version of the word categorisation task (see Smith & Trope, 2006); note that Smith and Trope's task is a Dutch translation of the tasks reported in Isen & Daubman (1984) using the word norms reported in Rosch (1975). We used the same categories as Smith and Trope (vehicles, furniture, and clothing). The categories were presented in random order. On each trial, participants saw the category name at the top of the screen with an exemplar from the category below it. They rated how well they thought the exemplar fit into the category using the following 10-point scale (0: *this item does not belong in this category*, 4: *does not belong to the category, but is very similar to members of that category*; 5: *does belong to the category, but is not a very good example of it*; 9: *this item is definitely in this category*). Participants responded "as quickly and accurately as possible" and saw a total of eighteen exemplars in each category, six of which were weak exemplars (e.g., "feet" is a weak exemplar of a vehicle), six moderate (e.g., "helicopter") and six strong (e.g., "car"). The first item from a category was always a strong exemplar and the remaining items appeared in random order.

After the categorization task, participants answered several questions about the experimenter on a 7-point Likert scale (*1=not at all; 7= extremely*). Participants responded

to the prompt “To what extent do you think the experimenter is:” and rated the experimenter on the following adjectives: attractive, outgoing, friendly, in control, competent, and trustworthy. A Cronbach’s alpha analysis showed that participants rated experimenters with a high degree of reliability (α=.78). All tasks were programmed and presented using E-prime (Psychology Software Tools, Inc.) on computers running Windows XP (likewise for Experiments 2 and 3).

Experimenters

Two psychology students (one 23-year-old female, one 25-year-old male) served as experimenters in the study. The experimenters conducted the study in the context of an MSc research project. To ensure that they understood the task and expected findings they read and discussed a series of papers from the power priming and cognition literature, including the Smith and Trope (2006) report, in a series of journal-club style discussions. They believed the project aimed to extend Smith and Trope’s findings by examining participants’ reaction times on the categorization task (unreported in the original) and changing the priming task from the prompted writing task in the original report to the computerized version of the scrambled sentences task. Notably, based on the journal club discussions, the experimenters were sceptical about whether they would exactly replicate Smith and Trope’s finding on categorization differences, as all the words in each category were exemplars from that category and should therefore be classified as such. However, they did believe that participants receiving the high-power prime would classify weak exemplars more quickly.

Each experimenter independently collected data from a large sample of participants (Experimenter 1: n=60, Experimenter 2: n=56). Working from a list that ostensibly assigned participant ID numbers to power-prime conditions, experimenters started the computer program before each participant arrived. After entering the participant’s ID, they typed “H”

for high or "L" for low power at the start of the task. They believed that this procedure caused the computer to administer either the high- or low-power prime. Unbeknownst to experimenters, we manipulated the actual prime condition independently from their belief. Only half of the participants completed the priming condition to which the experimenter "assigned" them. Thus, half the participants the experimenters believed to be high-power actually completed the low-power prime. The opposite was true of half the participants that experimenters thought they had assigned to the low-power condition.

Throughout the data collection phase of the experiment, experimenters remained blind to the fact that we had manipulated the prime condition independently from their belief. Experimenters were also unaware that participants had been asked to rate them. Therefore, they only had knowledge about the condition they believed participants to have completed as well as the results expected based on that belief. We fully debriefed both experimenters at the completion of the data collection phase of the project. Neither reported any suspicion about this manipulation.

Results

To examine differences in categorization behaviour, we calculated the average ratings for weak, moderate and strong exemplars, along with the average time participants required to make their judgments. These data served as the dependent variables in mixed-model ANOVAs with exemplar type (weak, moderate, strong) as the within-participants variable and experimenter belief (high, low) and prime condition (high, low) as independent variables. As expected, there was a main effect of exemplar strength such that as category prototypicality increased, so did average rating, $F(2,214)=155.25$, $p<.001$, $\eta^2_p=.59$. However, in this analysis, there were no main effects of prime condition, $F(1,107)=.08$, $p=.77$, $\eta^2_p<.01$, or experimenter belief, $F(1,107)=.83$, $p=.37$, $\eta^2_p<.01$. The Experimenter belief x Prime

condition interaction just failed to reach statistical significance, $F(1,107)=3.58$, $p=.06$, $\eta^2_p =.06$. Figure 5.1A shows these effects.

Interestingly, we did find differences in the speed with which participants made their ratings. As predicted, participants responded faster to strong exemplars, $F(2,214)=48.74$, $p<.001$, $\eta^2_p =.31$, than to moderate and weak exemplars (p-values<.001; Figure 5.1B; note that all post-hoc tests reported in this paper are Bonferroni-corrected). Although we did not find an effect of prime condition, $F(1,107)=.04$, $p=.84$, $\eta^2_p <.01$, we did find an effect of experimenter belief, $F(1,107)=4.58$, $p=.04$, $\eta^2_p =.04$. The interaction was not significant, $F(1,107)=2.45$, $p=.12$, $\eta^2_p =.02$. Thus, when experimenters believed that participants were in the low-power, relative to the high-power condition, participants responded more slowly to the weak and moderate category exemplars (p-values<.04) although they responded to the strong exemplars with equal speed ($p=.20$).

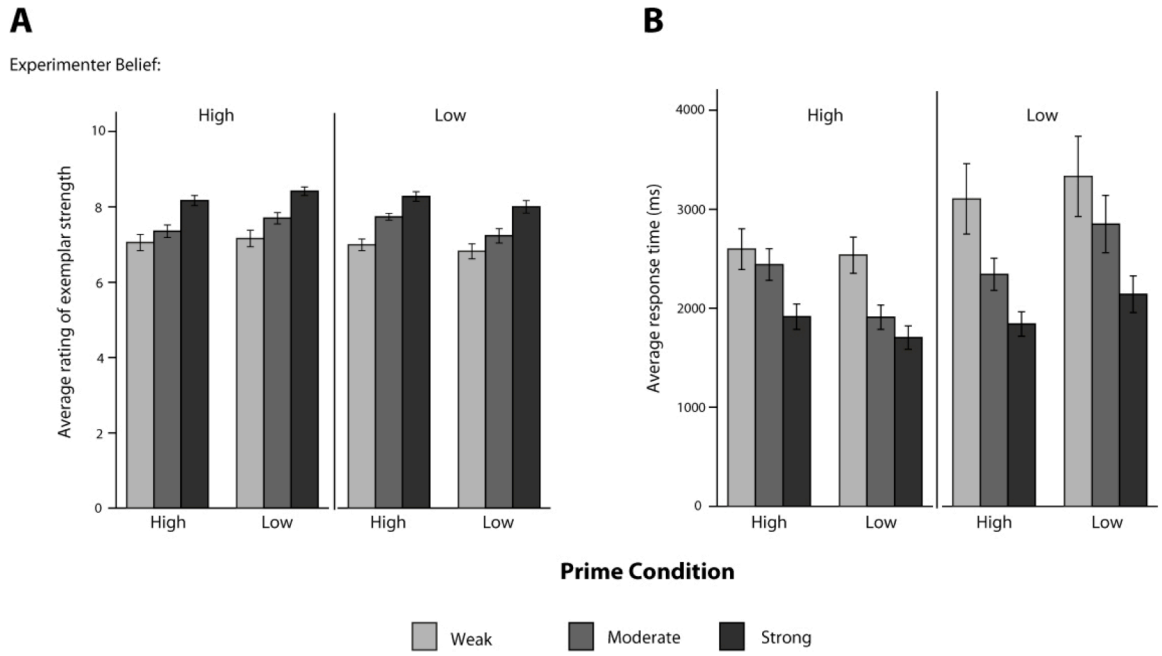

Figure 5.1. Task performance in the categorisation task. A) Average rating of exemplar strength, weak, moderate, strong, by experimenter belief and power prime. B). Average time taken to categorise exemplars by experimenter belief and power prime. Error bars show ±1 standard error of the mean.

Experimenters' beliefs about participant condition affected categorization speed in exactly the way the experimenters anticipated. To examine this effect more closely, we used a MANOVA model to analyse participants' perceptions of the experimenters to determine whether there were differences depending on the experimenter's belief about prime condition. The set of six characteristics on which participants rated the experimenters served as the dependent variables and experimenter belief as the independent variable. Results showed that only ratings of experimenter friendliness depended on experimenter belief, $F(1,107)=10.26$, $p=.002$, $\eta^2_p=.09$, such that when the experimenter believed them to be low in power, participants thought the experimenter had been more friendly (Low: $M=5.93$, $SD=1.02$; High: $M=5.19$, $SD=1.40$). None of the other judgements differed significantly across the high- and low-power groups (p-values>.12). Interestingly, Experimenter 1 reported in debriefing that she had felt slightly guilty when assigning participants to the low power condition, as they would not get the "boost" high-power

participants seem to enjoy on cognitive tasks. However, she did not believe this had affected her behaviour. Experimenter 2 did not believe his behaviour had differed across conditions.

To ensure that differences in affective experience could not explain the findings, we used a MANOVA model to examine self-reported positive and negative affect, along with the additional power-related items at the start of the task. Experimenter belief served as the independent variable in this analysis. Results showed that there were no experimenter-belief related differences for positive affect, $F(1,109)=.03$, $p=.86$, $\eta^2_p<.01$, negative affect, $F(1,109)=.43$, $p=.52$, $\eta^2_p<.01$, or for self-reported feelings of power at the start of the task, $F(1,109)=.06$, $p=.81$, $\eta^2_p<.01$. A MANCOVA model examining the effects of experimenter belief and prime condition on post-prime positive affect, negative affect and perceptions of power (controlling for baseline ratings) showed similar results. Neither experimenter belief nor prime condition appeared to influence positive affect, negative affect or feelings of power after the prime condition (F-values <1.50; *p*-values >.22) Thus, neither the experimenters' beliefs nor the prime condition affected participants' moods, including feelings of power, during the task.

Discussion

Results of this study showed that the combination of the experimenter's knowledge of participants' experimental condition and the experimenter's expectation about the results, mattered more in determining study outcome than did participants' actual primed condition. Moreover, according to participant ratings, experimenters who believed that they were in the high- versus low power condition differed in only one characteristic: friendliness. Thus, these findings suggest that the experimenter is perhaps a more powerful stimulus than many researchers, ourselves included, might care to imagine, despite the fact

that neither of the experimenters believed their knowledge had affected their behaviour. More importantly, these data hint that subtly revealed social expectations can nudge others' behaviour in quite precise ways.

To ensure that this finding was neither a simple artefact of the cognitive task we used nor unique to this specific pair of experimenters, we designed a second study using the same independent variables. In order to extend the results, we used a different experimental task, this time in the domain of affect/motivation-related decision-making.

Experiment 2

In theory, individuals higher in power are more reward sensitive and have a higher propensity to take calculated risks to obtain available rewards (Keltner et al., 2003). Although less work has addressed risk-taking differences based on primed power relative to work examining cognitive performance, results appear to show that high-power primed participants take more risks than do low-power primed participants (Anderson & Galinsky, 2006; Ronay & Hippel, 2010). We therefore ask whether experimenter belief and/or prime condition influence participants' propensity to engage in risk-taking behaviour.

Method

Participants

One hundred and ten undergraduate psychology students (66 female, 44 male; age: M=21.18, SD=3.71) participated in the study in exchange for partial course credit and a small performance-based monetary incentive.

Procedure

Participants completed the same scrambled-sentence priming procedure as in Experiment 1. They also completed the same PANAS (including additional power-related items) immediately before and after the priming task.

To assess risk taking behaviour, participants completed the “hot” version of the Columbia Card Task (CCT, see Figner, Mackinlay, Wilkening, & Weber, 2009). The CCT is a sequential risk-taking task, in which participants make a series of selections from a field of cards (Figure 5.2). Each field contains mostly “gain” cards (yellow happy face), for which they earn points, and several “loss” cards (green unhappy face) that lead to punishment if uncovered. Participants click on cards, one-at-a-time, to reveal outcomes. If the click reveals a gain card, participants earn points and may choose another card. If it reveals a punishment card, the trial immediately ends and the loss is deducted from the total points earned on that trial. As long as no loss card has been revealed, participants may stop a trial at any time (even if they have not selected any cards). Because each successive card choice increases the ratio of loss:gain cards, each click is more risky than the previous.

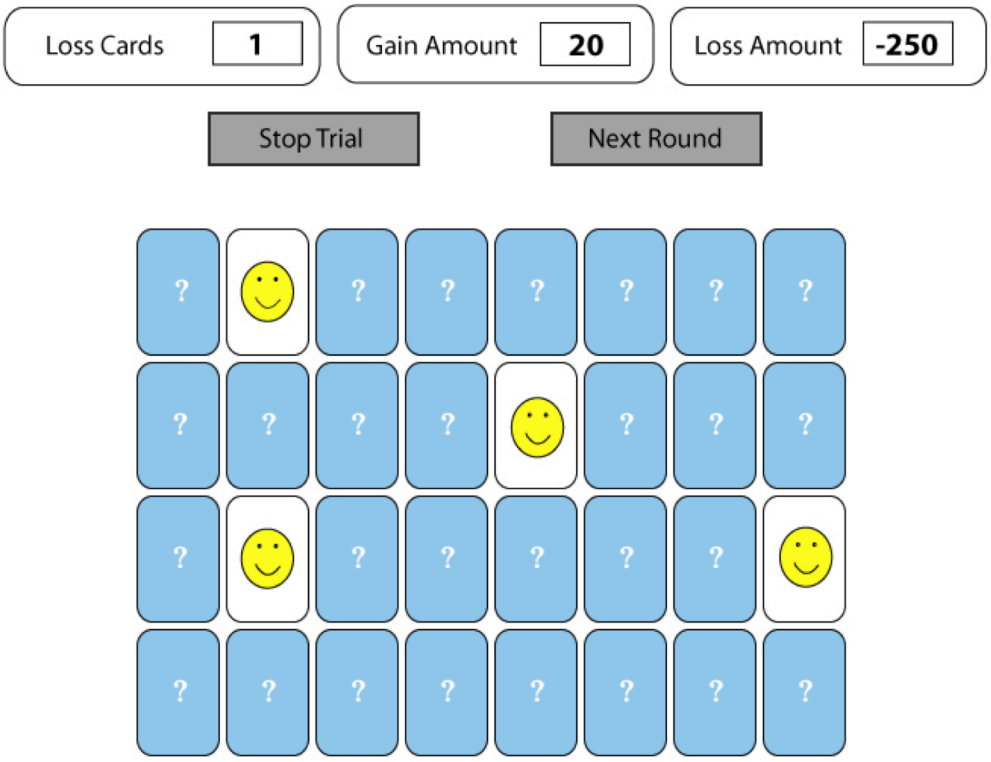

Figure 5.2. Screenshot of the Columbia Card Task.

At the start of each trial, all 32 cards are face down in a 4x8 grid arrangement. At the top of the screen, participants can see how many loss cards there are in the deck (1, 2 or 3), the number of points per loss (−250, −500 or −750) and the number of points per win (10, 20 or 30). At the end of each trial, regardless of whether the participant has elected to stop the trial or has revealed a loss card, the computer reveals the final score for that trial, along with the positions of all remaining facedown cards in the deck. In our version of the task, the three parameters (number of loss cards, gain amount and loss amount) were crossed in a factorial design, such that each participant completed one trial under each possible set of conditions (27 total trials). Participants received a small cash bonus (~£3) if the total of their points on a random selection of 3 trials was greater than zero.

Given that loss cards are randomly distributed to positions in each array, a participants' first click occasionally ends in a loss. To ensure that random errors in card

selection do not account for differences in voluntary risk-taking, we calculated the average number of cards participants selected on trials in which they voluntarily chose to stop. This adjusted average is a better measure of risk-taking behaviour than the simple average of selections per deck (Figner et al., 2009; Lejuez, Read, & Kahler, 2002). After the CCT, participants completed the same experimenter rating procedure as they did in Experiment 1.

Experimenters

The experimenter procedure and training was the same as in Experiment 1, with the exception of the specific instructions for the CCT. Two experimenters (one 24-year-old female, one 26-year-old male) individually tested a set of participants (Experimenter 1: n=60, Experimenter 2: n=50) as part of a psychology MSc research project. Experimenters were familiar with the power and risk-taking literature and therefore knew the study hypotheses and expected findings. As with Experiment 1, experimenters entered participant ID and power condition ("H" for high or "L" for low power) into the testing computers before they greeted, consented and instructed participants using a script (see Appendix D).

Results

To examine whether the experimenter's belief and the participant's experienced power prime altered risk-taking behaviour, we performed a 2x2 ANOVA with the adjusted average number of cards selected as the dependent variable and experimenter belief (high, low) and prime condition (high, low) as the independent variables. As shown in Figure 5.3, there was a main effect of experimenter belief on risk taking, $F(1,103)=10.12$, $p=.002$, $\eta^2_p=.09$, such that participants took more risks when experimenters believed them to be in the high power, relative to the low power condition. There was no main effect of the actual priming condition participants completed, $F(1,103)=2.53$, $p=.12$, $\eta^2_p=.02$, nor was the experimenter

belief x prime condition interaction significant, $F(1,103)=2.78$, $p=.10$, $\eta^2_p=.03$. Thus, only the experimenter's belief had a statistically significant effect on risk-taking behaviour.

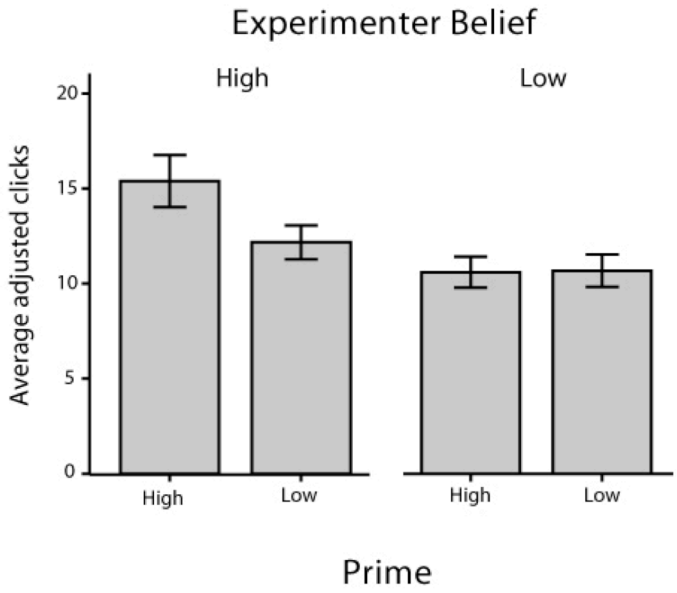

Figure 5.3. Average number of clicks in trials in which participants voluntarily chose to stop. Error bars show ±1 standard error of the mean.

To determine whether participants' ratings of experimenters differed depending on belief condition, we used a MANOVA with participant ratings of the six experimenter-characteristics as the dependent variables and experimenter belief as the independent variable. In this study, when experimenters believed participants were in the high, relative to the low power condition, participants rated them as more attractive, (High: $M=5.38$, $SD=1.06$, Low: $M=4.75$, $SD=1.44$, $F(1,109)=6.51$, $p=.012$, $\eta^2_p=.06$), and friendlier (High: $M=6.04$, $SD=1.06$, Low: $M=5.52$, $SD=1.29$, $F(1,109)=5.27$, $p=.024$, $\eta^2_p=.05$). There were no significant differences in experimenter ratings for any other traits (p-values >.08).

To determine whether there were differences in baseline affective experience depending on experimenter belief, we used a MANOVA model with the baseline measures of positive and negative affect, as well as the power related items, as the dependent variables and experimenter belief as the independent variable. As in Experiment 1, experimenter belief influenced neither participants' positive, $F(1,108)=0.40$, $p=.52$, $\eta^2_p<.01$, nor their negative affect, $F(1,108)=0.18$, $p=.68$, $\eta^2_p<.01$. However, in this study, experimenters' beliefs did have an influence on participants' perceptions of power,

F(1,108)=5.21, p=.024, η^2_p =.05. Specifically, participants felt more powerful when experimenters believed they were in the high power, relative to the low power condition at the start of the task (High: M=61.64, SD=12.56, Low: M=52.82, SD=15.63).

A MANCOVA analysis examining changes in affect and feelings of power after the prime (controlling for baseline levels) showed that although experimenter belief did not influence positive affect, F(1,103)=0.11, p=.74, η^2_p <.01, participants reported feeling slightly more negative affect after the prime if experimenters thought they were low in power, F(1,103)=4.36, p=.04, η^2_p =.04. The test for feelings of power failed to reach the threshold for statistical significance, F(1,103)=3.74, p=.06, η^2_p =.04. Actual priming condition did not significantly influence either negative, F(1,103)=3.68, p=.06, η^2_p =.03, or positive affect, F(1,103)=1.68, p=.20, η^2_p =.02. However, it did appear to influence feelings of power, F(1,103)=6.31, p=.008, η^2_p =.07. Controlling for baseline feelings of power, participants receiving the high power prime felt more powerful than did those receiving the low power prime (Figure 5.4).

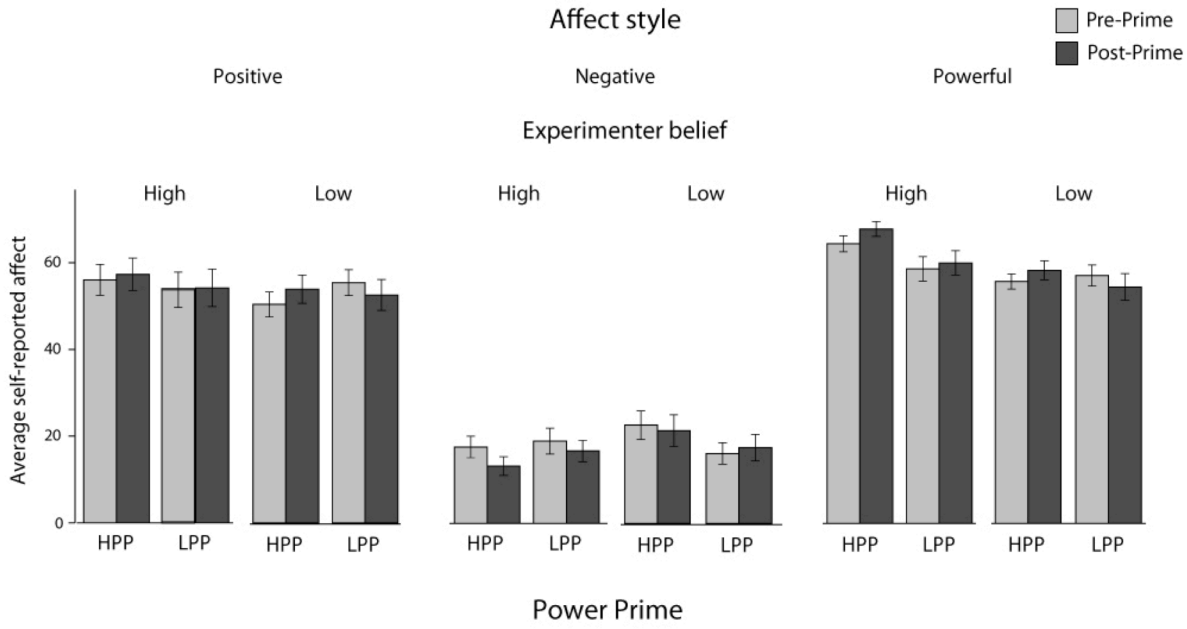

Figure 5.4. Self-reported positive and negative affect, pre- and post-prime and feelings of power across both experimenter belief and prime condition. Error bars show ±1 standard error of the mean.

Discussion

As in Experiment 1, experimenters' beliefs, rather than participants' actual primed condition, ultimately shaped participants' risk-taking behaviour. This finding extends our results from the cognitive to the affective domain and replicates the Experiment 1 effect with a new set of experimenters. This result therefore suggests that experimenters' prior beliefs play an important role in shaping participant behaviour, probably because knowledge of participant condition and expected results subtly influenced experimenters' behaviour. In support of this notion, participants rated the experimenters differently depending on experimenters' beliefs about them; even though the experimenters both reported that they did not believe their knowledge of participants' conditions had influenced their behaviour. Thus, these results highlight the extent to which one person's prior beliefs can influence another's behaviour.

Experiment 3

In both Experiments 1 and 2, the actual prime participants experienced was a scrambled sentences priming task. Based on our data, that prime did not appear to reliably manipulate feelings of power. Instead, the results appeared to relate to the beliefs experimenters held about task outcomes. Here, we extend those findings by asking whether a prime that does influence feelings of power is capable of shaping behaviour on a cognitive task in a double-blind design. The measure of interest was a "flanker" task (Eriksen & Eriksen, 1974) and we predicted that if the power manipulation enhanced participants' ability to attend to targets and ignore distractors as research suggests (e.g., Galinsky et al., 2003; Guinote, 2007; Hogg & Vaughan, 2008), participants in the high power, relative to the low power condition, would show faster and more accurate responses in a condition with incongruent targets and distractors. However, if the experimenter's belief is the important

driver of behaviour, we predicted no power-related differences in flanker responses because, in this case, the experimenter remained blind to participant condition.

Method

Participants

One hundred and fourteen undergraduate psychology students (88 female, age: *M*=20.42, *SD*=3.85) participated in a study about "personality and cognition" in exchange for partial course credits and a small monetary bonus. We excluded one participant's data (high power condition) due to a computer failure that caused data loss on a substantial portion of trials.

Procedure

Participants provided consent and were instructed in pairs to give the appearance that they were working together in the task. Before receiving instructions for the power-priming task – a game in which participants received high-power, low-power or neutral role assignments, they completed the PANAS as a baseline measure of positive and negative affect. In this case, we used a manipulation check based on the game and therefore did not include power-related words in the PANAS. Participants were then shown to adjacent rooms for the experimental procedure.

To induce power differences in this experiment, we developed a game in which a computer randomly assigned participants to one of two power-related roles: boss (n=37; "high power") or employee (n=38; "low power"). For comparison, and to decrease the likelihood that the experimenter would be able to guess participant condition, a second set of participants was assigned to a cooperative (n=38; "control") condition. They then completed the game, in which they believed they were working with the partner to earn points. They expected to receive a cash bonus based on their earnings. Pilot testing

suggested that the role assignments altered feelings of power in both the high and low power participants.

The game was a fast-paced task in which participants responded to coloured squares, appearing to either the left or the right of a fixation cross. Participants made a key press when they saw a target (blue square) on the left. They simultaneously responded to a different target (grey square) whenever it appeared on the right. Participants earned points for each target they detected within 500ms. The game included two, 3-minute blocks of trials, separated by a break.

Although all participants completed the same task, the instructions differed depending on assigned roles. All participants understood that detecting and responding to targets on the left side of the screen was the primary task. Bosses were told that, as an added responsibility of their role, they should also detect and respond to targets on the right side of the screen. Employees were told that the boss had assigned them this same duty. Participants in the cooperative condition believed they were working as a team and both partners would respond to both left and right targets. Regardless of actual performance, participants learned they had earned 4074 points at the end of the task. Bosses then assigned any number of points to their employees, retaining the remainder for themselves. To emphasise the power differential, employees were told that they had been allocated 35% of the points. In the cooperative condition, participants each received 50% of the points (at the end of the experiment, everyone received the same cash bonus). Importantly, because the computer assigned participants to conditions, the experimenter was blind to participant status during testing.

Following the power induction, participants completed a second PANAS, which served as a post-game mood check. They also completed a 4-item manipulation check to

measure their sense of fairness about the task ("To what extent do you feel like the workload division was fair?" "To what extent do you feel like the bonus money was divided fairly?"), effort expended ("To what extent did you feel like you performed the task to the best of your ability?"), and power ("To what extent did you feel that you had power or control in this situation?").

Participants then completed a flanker task (Eriksen & Eriksen, 1974), in which arrows pointing to the left or right served as targets. A pair of left- or rightward pointing arrows flanked each target and served as distractors (see Figure 5.5). Participants produced a left or right button press to indicate the direction in which the target pointed. Trials began with a central fixation cross for 500ms, followed by a target arrow (50% pointed left) surrounded by distractor arrows pointing in either the same (congruent; 50% of trials) or the opposite direction (incongruent). The target/flanker display remained visible for 650ms before being replaced by a blank screen. Participants then saw feedback about whether they were correct (1500ms). There was a 1000ms inter-trial interval. Participants completed three blocks of 60 randomly ordered trials.

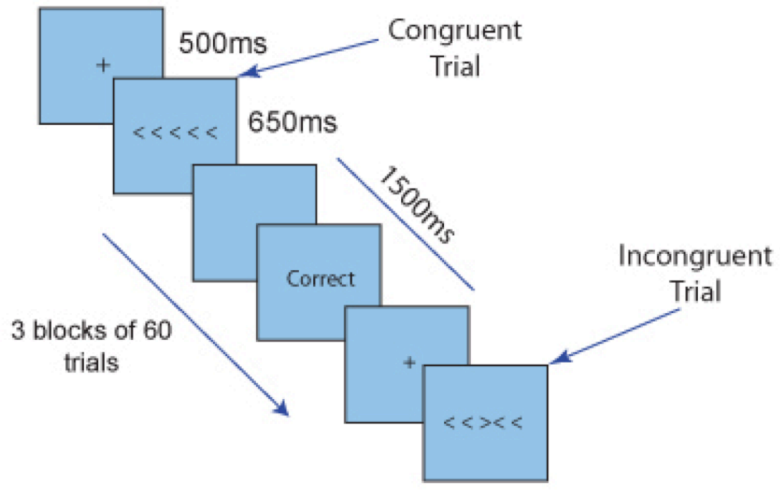

Figure 5.5. Trial timeline on the flanker task.

At the end of experimental session, the experimenter probed participants for suspicion about the power manipulation, and fully debriefed them. Once the computer portion of the task began, the experimenter had no further contact with participants until the debriefing phase of the study. This experiment involved a single female experimenter.

Results

Manipulation Check. There was a strong correlation between the two fairness items (r(108)=.47, p <.001; Cronbach's alpha =.62), we therefore computed an average score for perceived fairness. A MANOVA, with effort, fairness and power as dependent variables and power condition as the independent variable showed significant power-related differences in effort exerted, F(2,106)=3.17, p=.05, η^2_p=.06, fairness, F(2,106)=28.04, p <.001, η^2_p=.35, and power, F(2,106)=18.87, p<.001, η^2_p=.26. As Figure 5.6 shows, participants in the low power (employee) condition reported exerting significantly more effort than did control (cooperative) participants (p=.04) who did not differ from high power (boss) participants (p=.13). Participants in the high power and control conditions reported thinking that the task was fairer than did those in the low power condition (p-values<.001). High power participants also reported feeling more power than both control and low power participants (p-values<.001) although control and low power participants did not significantly differ from each other (p=.08). These results suggest that the power manipulation effectively induced feelings of high and low power.

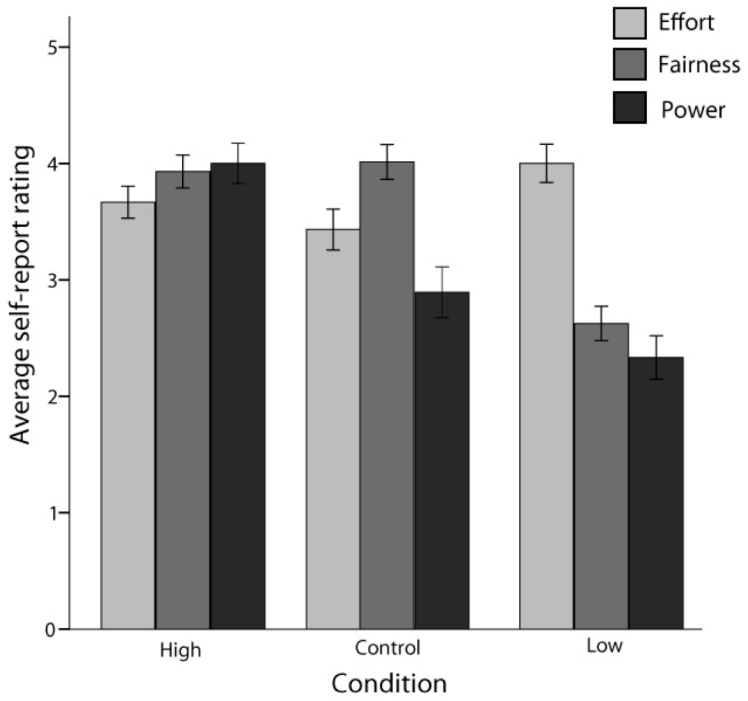

Figure 5.6. Average self-reported rating of effort expended, fairness of the task, and feeling of power across the task conditions. Error bars show ±1 standard error of the mean.

Flanker Task. To determine whether the power manipulation affected performance in the flanker task, we examined the proportion of error trials and average reaction times in the congruent and incongruent conditions. To calculate average reaction times for correct congruent and incongruent trials, we excluded trials in which participants made an error as well as the following trial. Based on this procedure, we excluded an average of 4% (SD=.05) of trials per participant in the congruent condition and 23% (SD=.20) in the incongruent trials. We examined accuracy and reaction time data in mixed-model ANOVAs with trial type (congruent, incongruent) as the within-participants variable and power condition as the between-participants variable.

As expected, there was a main effect for trial type, $F(1,110)=171.02$, $p<.001$, $\eta^2_p=.61$, such that all participants were more accurate on congruent than incongruent trials (Figure 5.7A). However, contrary to prediction, there was no main effect of power condition, $F(2,110)=0.26$, $p=.77$, $\eta^2_p=.01$, and no interaction between trial type and power condition,

$F(2,110)=1.79, p=.17, \eta^2_p=.03$. The reaction-time data followed a similar pattern (Figure 5.7B).

Participants responded more quickly on congruent versus incongruent trials, $F(1,110)=696.20, p<.001, \eta^2_p=.86$. However, there was neither a main effect of power condition, $F(2,110)=1.31, p=.27, \eta^2_p=.02$, nor an interaction, $F(2,110)=0.74, p=.48, \eta^2_p=.01$. Thus, despite evidence that the manipulation successfully altered the degree to which participants felt powerful, it did not alter their ability to perform the task using a double-blind design.

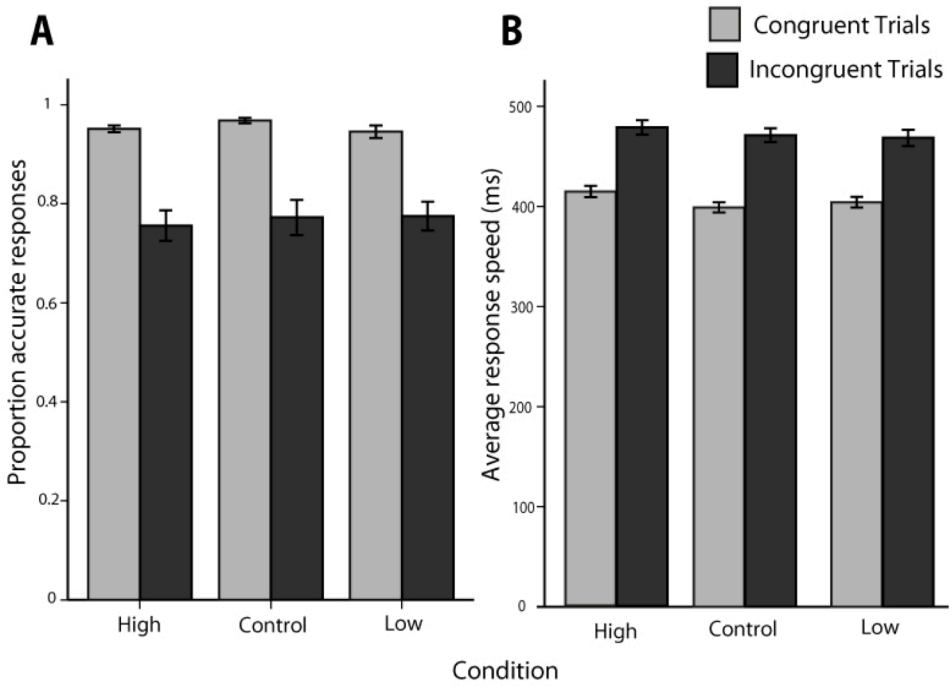

Figure 5.7. Performance measures on the flanker task. A) Average accuracy across participant condition. B). Reaction time by participant condition. Error bars show ±1 standard error of the mean.

Positive and Negative Affect. We used a MANCOVA to determine whether assigned power roles influenced participants' moods. The dependent variables were post-manipulation positive and negative affect. Pre-manipulation positive and negative affect served as covariates. Power condition (high, low, control) was the independent variable. After

controlling for pre-existing affect, results showed no power-related differences in positive affect, $F(1,108)=.99$, $p=.38$, $\eta^2_p=.02$, although all participants' positive affect increased from pre- to post-task (p-values <.05). However, we did find significant power-related differences in negative affect, $F(1,108)=4.69$, $p=.01$, $\eta^2_p=.08$. As Figure 5.8 shows, participants in the high-power condition reported a significant decrease in negative affect ($p<.001$) compared to those in the control ($p=.09$) and low power ($p=.96$) conditions. Thus, all participants, regardless of condition showed increased positive affect but after the manipulation, only the high-power participants experienced a decline in negative affect.

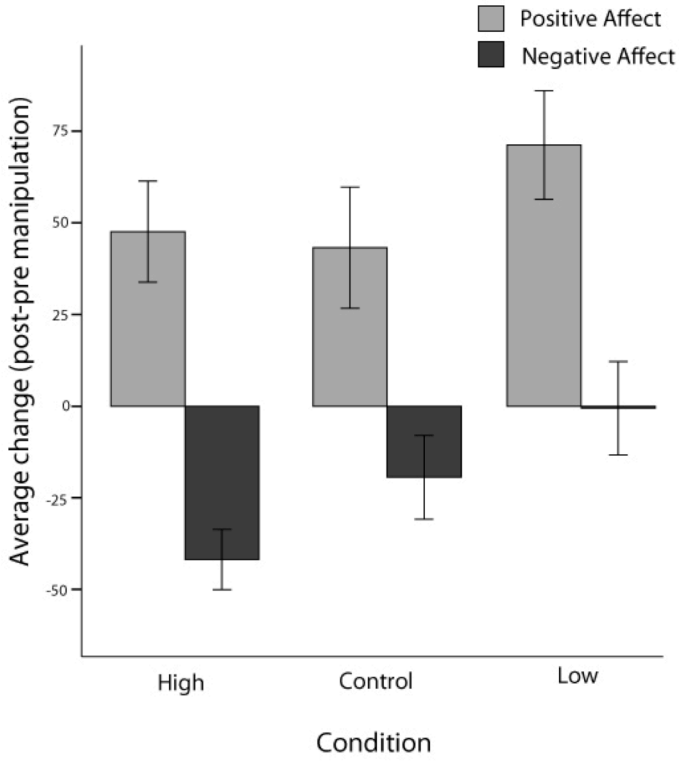

Figure 5.8. Changes in positive and negative affect from pre- to post power prime. Error bars show ±1 standard error of the mean.

Discussion

Despite a large body of work finding power-related differences in executive function tasks (Doyen et al., 2012; Galinsky et al., 2006; Guinote, 2007; Smith, Jostmann, Galinsky, &

van Dijk, 2008) our findings were not consistent with predictions derived from this literature in the double-blind study design. Interestingly, data from our manipulation check suggest that assigning participants to 'boss' versus 'employee' roles in the game led to robust differences in feelings of power, suggesting that the manipulation successfully primed the concept of power. These data suggest that the experimenter's expectations about participant performance can indeed help to guide behaviour. In the absence of any experimenter knowledge about participants' conditions, we did not find that participants showed the predicted effects on this task.

General Discussion

In Experiments 1 and 2, we have shown that an experimenter's prior belief about participant performance shaped the ways in which those participants performed. This occurred across four experimenters and in different performance domains. In Experiment 3, in which the experimenter was familiar with experimental predictions but not with participants' conditions, there were no such behavioural differences. These results suggest both that people's prior beliefs manifest as subtle differences in interpersonal behaviour and that their interaction partners, at some level, recognize and respond to them.

What were the experimenters doing differently when they expected participants to receive a high- or low-power prime? According to experimenters, they followed their scripts and treated all participants identically. According to participants, experimenters did behave differently depending on the condition that experimenters believed they ran. In Experiment 1, participants reported that experimenters were friendlier when the experimenters thought they were in the low-power condition. Perhaps, as one of those experimenters reported, they were friendlier because they felt uncomfortable allocating participants to the low-power condition and thereby denying them the cognitive benefits the power priming

literature so reliably shows. The opposite was true in Experiment 2. In this case, when experimenters believed they had assigned participants to the high-power condition, those participants viewed them as both friendlier and more attractive. Interestingly, participants in Experiment 2 reported feeling more powerful at the outset of the experiment when the experimenter believed them to be in the high-power condition. No such effect occurred in Experiment 1. Thus, despite differences in participants' experiences across the tasks, participants conformed to experimenter expectations in both experiments. These results suggest that subtle behaviours that participants collectively rated as friendliness might influence participant experience in quite specific ways.

The present results suggest that the implicit power prime we used in Experiments 1 and 2 did not cause differences in participants' behaviour on the dependent variables. Interestingly, in Experiment 1, it did not appear to activate concepts related to power as previous research suggests (e.g., Galinsky et al., 2003; Lammers, Stoker, & Stapel, 2009; Pashler et al., 2012; Smith et al., 2008). Our measure of feelings of power (included in the PANAS) did not change significantly from pre- to post-prime. In Experiment 2, the power-prime did appear to alter participants' perceptions of power in the predicted fashion. Participants receiving the high-power prime felt slightly more powerful than did those receiving the low power prime. However, in Experiment 2, participants also felt greater levels of power if the experimenter thought they were in the high-power condition, prior to the prime. The more active prime in Experiment 3, which did appear to have altered participants' feelings of power, failed to cause the predicted cognitive change. Taken together, these results suggest that effects commonly attributed to scrambled sentence style priming tasks (e.g., increased executive cognition, increased risk-taking) might instead have been caused by the unconscious "priming" of participant behaviour by experimenters.

Together, our findings suggest that one person's behaviour in a social interaction may depend strongly on interaction partner beliefs. One mechanism explaining this effect is that people's expectations may shape both their own social behaviour and how they interpret and respond to others. Interaction partners may use these behavioural cues to infer and conform to another's expectations or they may automatically respond to those cues, thereby allowing themselves to be "nudged" toward a particular behaviour or outcome (Keltner et al., 2003; Miller & Turnbull, 1986). At a societal level, this result has important implications for understanding how self-fulfilling prophesies arise. For example, teachers may inadvertently favour their male students in mathematics classes and their female students in English, thereby leading to gender differences in literacy and numeracy (Campbell & Collaer, 2009). At the individual level, people's beliefs may reinforce particular behaviour patterns within families or between couples (Federman et al., 2003; Noller, 1980), leading to both positive and negative outcomes. Thus, these results suggest that understanding the interdependence between social partners' beliefs and behaviours may be a critical element in solving some of the interpersonal difficulties that characterize social relationships.

Despite its broad implications, this study has several limitations. First, because we were exploring experimenter effects using findings common in the literature, we were unable to manipulate experimenters' prior beliefs (e.g., inducing experimenters to believe that a high-power prime would impair abstract-thinking ability). Second, social power is a very specific domain and, our findings may have been magnified due to the pre-existing power relationship between experimenters and participants, although similar results have been shown with less experimentally-salient paradigms (Doyen et al., 2012; Galinsky et al., 2003; 2008; Gruenfeld et al., 2008; Guinote, 2008; Magee et al., 2007; Pashler et al., 2012;

Weick & Guinote, 2010). Finally, we are unable to explicitly examine which specific behaviours might change with experimenter expectations. Thus, although participants' ratings differed depending on experimenter beliefs, we had no way of measuring or recording experimenters' actual behaviour without alerting them to the manipulation. Future research might explore how alterations in prior beliefs might induce subtle changes in individuals' social behaviour.

Conclusions

This set of experiments has two important implications. First, it suggests that in order to ensure the integrity and meaningfulness of research outputs, authors should carefully consider the potential for experimenter effects to influence results during the study design process. Indeed, it seems remiss that they are not always explicitly considered or reported in the published literature. Second, these findings also suggest that individuals' beliefs about their interaction partners or about the outcomes of their interactions have the power to influence interpersonal processes at a broader level. Thus, individuals' beliefs, stereotypes, or expectations may determine both the quality and nature of their interactions in ways that help to predetermine their outcomes.

Chapter 6

General Discussion

The primary findings from this body of work suggest two important ideas. First, the findings show that the relationship between sender and receiver is a complex one. What senders signal depends on biases in receivers' interpretations of social cues. The ways in which receivers act on the cues they receive depends on the quality of their interactions with senders, the stability of sender behaviour, and the beliefs senders hold. Thus, the simple conclusions that many laboratory studies make about senders' signals and receivers' judgments are unlikely to generalize to real-world social behaviour, an idea that recent research has begun to support (Mitchell, 2012). Second, this work emphasizes the necessity of considering the interdependence between senders and receivers in understanding both signals and judgments. In real-world social interactions, signalling and interpretation of social cues does not happen in isolation, nor are the roles of sender and receiver fixed. This means that underlying the apparent ease and skill with which humans navigate their social environments is a complex exchange of social cues informed by each participant's traits, mood, prior beliefs and memories, simultaneously interacting with the moment-by-moment unfolding of the interaction. Together, the present research has examined the ways in which interaction partners signal and interpret social cues, while considering the interdependence between cue senders and receivers with the aim of understanding the mutual influence of senders on receivers and vice versa.

Together, this work shows that the signalling and interpretation of social cues changes with individuals' expectations. For example, anticipating social rejection changes both behaviour and cue interpretation. In the receiver role, rejection anticipation alters value computations on received social cues. In the role of the sender, anticipating rejection changes behaviour, which in turn affects an interaction partner's behaviour and experiences, with the additional consequence of increasing the likelihood that the

anticipated rejection will occur. The fact that each cue sender is also a receiver in face-to-face interactions means that these shifts affect both social roles, and thus have important consequences for all individuals involved in an interaction. At a broader level, these results have important implications for understanding the growing body of research that examines social signalling and behaviour. Specifically, they suggest that research that fails to account for both sender and receiver perspectives may generalize poorly to real-world social environments.

Previous research shows that experiencing social rejection leads to increased sensitivity to social cues that signal affiliation, specifically genuine smiles, (Bernstein et al., 2010; Bernstein et al., 2008; Pickett et al., 2004) as a mechanism that promotes reconnection, however, this research does so in the context of implied, as opposed to face-to-face social interaction. In Chapter 2 I explored how the anticipation of a painful social experience (Eisenberger et al., 2003), alters behaviour in a live interaction and how these shifts in behaviour impact the experience of a naïve interaction partner. These data show that anticipating social rejection leads to changes in behaviour, which, in turn, affects the experience of a naïve interaction partner who has no expectations about the interaction.

A sender's expectations about a social interaction influence the mood of a naïve interaction partner, which manifests as changes in both self-reported affect as well as behaviour within the interaction. Both participants anticipating rejection and their naïve partners engaged in more discussions of negatively-valenced topics, suggesting that the negative feelings induced by the expectation of an unpleasant interaction influenced the experience of the naïve participant by causing the interaction to focus on negative topics. In fact, after the social interaction, participants anticipating both a rejection and negative but non-rejection-related interaction experienced increases in positive affect. However, their

naïve interaction partners experienced no such increase in positive affect. Importantly, this diverges from the experience of naïve partners of participants expecting a positive interaction, who did report an increase in post-interaction positive affect. It is likely that participants in social interactions expect to feel more positive at an interaction's conclusion. Their failure to do so may have caused them to interpret their interactions more negatively. Thus, despite naïve participants having no preconceived ideas about the interaction, their interaction partner's behaviours influenced their own moods and altered their enjoyment of the interaction.

A pleasant interaction can be partially characterised by the degree to which it feels smooth and coordinated, which manifests as consistent (Heerey & Velani, 2010) and predictable behaviour (Chang, Doll, van't Wout, & Frank, 2010), as well as the coordinated reciprocity of appropriate cues (Lakin & Chartrand, 2003; Stel & Vonk, 2010). Contrary to expectations, which suggest that anticipation of rejection might lead to changes in genuine smiling behaviour (Bernstein et al., 2010), I show that anticipating social rejection led to a decrease in the reciprocity of polite, but not genuine smiles. This finding diverges from research that shows that anticipating social rejection leads to an increased sensitivity to genuine smiles (Bernstein et al., 2008), suggesting that this mechanism may not generalise into a live interaction setting.

Interestingly, the reduction in reciprocity of polite smiles had a knock-on consequence for the naïve interaction partners of those anticipating rejection. The fewer polite smiles returned, the less positive affect interaction partners reported after the interaction. Naïve interaction partners also reported liking their 'rejected' partners less than those in acceptance and negative control conditions. Thus, the behavioural changes prompted by the anticipation of social rejection influenced both interaction partners. That

there were no differences in outcome measures (interaction quality and liking) for participants who received active and naïve feedback with dyads, suggests that both participants in the interaction noticed behavioural changes and this affected their interpretations of the interaction. In this way, the experiences of both participants in an interaction are interdependent.

To understand why the failure to return polite smiles so strongly affected interaction outcomes, I extract participants anticipating rejection from the face-to-face interaction environment and explore their experiences as receivers. In Chapter 3, Experiment 1, I show that anticipating social rejection leads to an increase in the value of genuine smiles relative to polite smiles. Although this finding is consistent with the literature (Bernstein et al., 2010; Gardner et al., 2005; Pickett et al., 2004), it does not clearly explain participants' behaviour in the live interaction (Chapter 2). Therefore, in Experiment 2, I examine the comparison of genuine and polite smiles to neutral expressions. In addition, to ensure that simple changes in positive and negative affect did not influence the findings, I include two additional conditions in which participants' experience increases in positive or negative affect that are unrelated to social context. This design shows that anticipating rejection leads to the systematic devaluation of polite smiles over and above the influence of mood. These results suggest that one mechanism for understanding the change in social behaviour caused by senders anticipating rejection is that they alter their valuation of important social cues as receivers. Thus, this finding both highlights the interdependence of the sender and receiver perspectives and demonstrates that changes in individuals' states dramatically influence the social environment.

Even without the manipulation of specific expectations about an interaction, people make judgements about others' states (Hess & Kleck, 1990; 1994; Hess, Blairy, & Kleck,

1997), traits (Ambady, Hallahan, & Rosenthal, 1995; Hall, et al., 2008; Penton-Voak et al., 2006) and social intentions (Stirrat & Perrett, 2010; Todorov, 2008). Such judgements have significant social consequences (Blair, Judd, & Chapleau, 2004; Olivola & Todorov, 2010; Steele & Aronson, 1995). In Chapter 4, I explore the predictive validity of these judgments, as well as the ease with which sender behaviour might bias these judgments. The tendency in the literature has been, to some extent, to confound the reliability and validity of social judgements (Brown & Moore, 2002). Whereas in some instances judgements of sender traits have been compared to self- or other reports of sender traits (e.g., Jones, Kramer, & Ward, 2012; Little & Perrett, 2007; Penton-Voak et al., 2006), or used to predict future behaviour (Oda et al., 2009a; Oda et al., 2009b), researchers have also made the assumption that if receivers make consistent judgements of a particular sender, there must be something that the sender is signalling (Brown & Moore, 2002). Although these first two ideas measure validity of receiver judgements, the third, which comprises the majority of research in this area, merely examines reliability and is therefore relevant only to receivers and not to senders.

In Chapter 4, Experiment 1, I show that receivers do indeed rate sender trustworthiness reliably; however their judgements do not show predictive accuracy, suggesting that these judgments may not be useful to receivers. That is, receivers' trustworthiness judgements do not enhance their ability to accurately decode sender behaviour in another context, even though they do use cues from high- and low-trustworthy senders differently. Because research finds that receiver ratings of trustworthiness relate to the degree to which faces mimic smiles or frowns (Todorov, 2008), trustworthiness ratings may simply measure the degree to which a face looks approachable. The fact that trustworthiness ratings in this study related to the frequency with which senders smiled in

the casual conversation clips is evidence of this idea. However, even though high trustworthy senders likely possess some of these traits, trustworthiness is really about the degree to which one can be depended on in order to attain a goal (Simpson, 2007). Thus, being judged to appear trustworthy and the act of behaving trustworthily may differ, thereby highlighting the importance of considering both senders' and receivers' contributions to the social process.

To further explore the validity of receiver judgements of senders' latent traits, in Experiment 2 (Chapter 4), I take a slightly different approach. In much of the work that finds that receivers make reliable (Brown & Moore, 2002; Kramer & Ward, 2011; Little & Perrett, 2007) or accurate judgements of senders' traits (Ambady et al., 1995; Hall et al., 2008), researchers assume that if receivers are interpreting senders' latent traits consistently, then there must be cues to latent traits that senders signal. In fact, many such studies use composite and computer-generated faces (Jones et al., 2012; Kramer & Ward, 2011; Penton-Voak et al., 2006; Todorov et al., 2008) to show that there are features of static human face that purportedly signal specific traits. This idea implies that if senders signal valid cues to their latent traits, receivers should perceive these cues in vivo. However, in the social world, people rarely see static neutral images of faces. I therefore ask about the degree to which sender displays can bias receiver judgments. Results show that receiver judgments of senders' trustworthiness, attractiveness and dominance are susceptible to bias based simply on the sender's affective display. This suggests that, like the transitory nature of affective states, moment-to-moment changes in senders' displays bias receiver judgements. This suggests first that senders may quite easily alter the so-called 'unfakeable' (Rezlescu et al., 2012) characteristics present in their neutral faces and second, that receiver judgments depend strongly on what senders signal in different states.

Together, their inaccuracy and susceptibility to bias calls into question the function of receiver judgements. In theory, receivers make these judgements in order to predict sender behaviour, thereby allowing receivers to improve their social outcomes (Fiske & Linville, 1980; Van Kleef, De Dreu, & Manstead, 2004). However, the data from these two experiments suggests that such judgments may not be helpful in this regard. One reason for this mismatch between theory and data may be that, in a casual conversation, in which trustworthiness is irrelevant, senders simply do not signal their trustworthiness. However, they may signal their trustworthiness in situations in which trustworthiness is a salient trait (Krumhuber et al., 2007; Scharlemann, Eckel, & Kacelnik, 2001). Certainly, this fits with the theoretical position that suggests that personality traits may fluctuate across situations (Lewis, 2001). An alternative hypothesis, which may also explain why receiver judgements are susceptible to bias, might be that the purpose of judgements is merely to make decisions about whom to approach or avoid (Adams et al., 2012). Thus, specific personality characteristics might be superordinately categorised into broad approach-related traits, e.g., trustworthy, extraverted and agreeable, and avoidance-related traits e.g., neurotic or untrustworthy, and the necessity of making a particular judgement forces receivers to specify descriptors.

Given that receiver judgements are neither accurate assessments of sender traits, nor good predictors of sender behaviour, it may be that the automatic act of forming first impressions is as much about the sender as it is the receiver. Insofar as receivers base their judgements on their own prior beliefs and social intentions in addition to sender behaviour, forming first impressions is an interdependent process. Importantly, different situations likely activate contextually relevant personality characteristics in both senders and receivers (Hareli & Hess, 2012), which influence the behaviour and impressions of both. For example,

a sociable individual may be unnerved and therefore stumble at the prospect of public speaking, meaning that a receiver might judge the sender as awkward and shy. Similarly, a normally shy lecturer may appear extraverted when in front of a class, leading her students to believe she is highly extroverted. This notion of flexibility in overt behaviour poses problems for the literature on social judgments, particularly when assessing their accuracy. This is because receivers make those judgements based on sender displays that are captured at a particular moment, in the case of a photograph, or in a particular situation, in the case of a film clip. In addition, when assessing agreement accuracy, in which receiver judgements are correlated with self- or close others' reports of characteristics, it is important to remember that personality measures tend to measure behaviour over an extended period of time (John & Srivastava, 1999), rather than in a single moment or situation. Thus, predictive accuracy may be a more important social judgment measure than agreement accuracy, although my data do not suggest that the predictive accuracy of such judgments is high.

So far, I have shown the importance of considering the interdependence of senders and receivers for research into verbal and nonverbal behaviour and interaction outcomes, as well as during the process of making first impressions. In Chapter 5, I show that a sender's displays can influence receiver behaviour in very specific ways. During interactions, people respond to one another and the subtle variations in their behaviour may have important consequences for their interaction partners (Cappella, 1981). In the context of an experimenter effects study, I show that senders, in a relatively one-sided social exchange, unconsciously influence receivers' behaviour based on senders' own prior beliefs. In this study, experimenters unconsciously influenced their participant such that participants conformed their behaviour to experimenter expectations. That is, participants responded

differently to an experimental task, depending on whether the experimenters believed that they had assigned participants to a high or low power prime.

In Experiments 1 and 2 of Chapter 5, participants whom experimenters believed to be in a high power prime condition performed in accord with experimental hypotheses, by engaging in more abstract thinking and risk taking behaviours. This occurred regardless of the power prime condition participants actually experienced. Moreover, the prime itself did not appear to reliably elicit the experience of feeling high/low power (although in Experiment 2, the experimenters appeared to cause this effect). In a third experiment, in which the power prime was stronger, but the experimenter was blind to participant condition, there were no differences in participants' behaviour. Together, these studies suggest that experimenters may subtly influence participant experience and consequently, participant behaviour. Interestingly, participants' explicit judgements of their experimenters suggested that experimenters altered the degree to which they behaved in a friendly manner toward participants. This suggests that subtle changes in senders' verbal and nonverbal cues can influence receivers in very specific ways, according to sender expectations in this instance.

Traditionally, the study of social influence in psychology has been in the context of explicit influence, for example, persuasion (Cacioppo & Petty, 1985; Chaiken, Wood, & Eagly, 1996; Cialdini & Goldstein, 2004), leadership (Krause, 2004), and dominance (Anderson & Kilduff, 2008), in which one individual or group, exerts influence over another (Brown, 1988). Often, this influence is understood as coercing another to do something that he/she might not otherwise do (Cialdini, 1994). However, these data suggest that social influence also emerges in the more subtle exchange of social cues in everyday interactions. Although in this set of studies, the sender/receiver roles are relatively one-sided, as

embodied by the experimenter-participant dyad, in daily interactions this influence is likely to be mutual, as the Chapter 2 findings in this thesis demonstrate.

Implications

Together, this work has implications both for future research and for understanding real-world social interactions. Though previous work suggests that senders communicate valid signals about their states, traits and intentions (Hess, Banse, & Kappas, 1995; Penton-Voak et al., 2006; Schug, et al., 2010), this current work highlights the fact that interpreting these judgements is not straightforward. Although senders may signal valid information, it is likely that these signals are context dependent and highly associated with changes in senders' states. The degree to which a sender's cues in one context will generalise to another context is unclear and thus has important implications for research that makes claims about the social abilities of both senders and receivers (Hareli & Hess, 2012).

Broadly, this work also has implications for everyday experiences of social interactions. Research shows that clinical disorders, such as depression and social anxiety (Heerey & Kring, 2007; Keltner & Kring, 1998), have an impact on social interaction. Adopting the understanding that the unfolding of an interaction depends on both parties may have important consequences for those who experience difficult interactions. In addition, the data that suggest that anticipating social rejection has consequences for interaction partners, rather than only the individual who anticipates rejection, may explain why some people chronically experience social rejection. In addition, in situations in which social processes depend on judgements of others, such as in the courtroom, or the border control queue at an airport, it is important to understand that the signalling and interpreting of cues is an interdependent process.

The present research has worrying implications from a research perspective as well. These findings suggest that the gold standard of randomized, double-blind between-group studies is extremely important. Indeed, our results are consistent with two recent studies suggesting that experimenter effects are more important than previously believed (Doyen, Klein et al., 2012; Pashler et al., 2012). More broadly, these findings hint that researchers should make every effort to ensure that their experimenters remain unaware of experimental hypotheses until study conclusion to avoid biasing the results. At the very least, peer reviewers should demand that researchers report the steps they have taken to ensure the integrity of their results.

The findings presented here also raise an important philosophical question. To what extent are a sender's signals about the sender, and to what extent are they about the receiver? In a photograph or a short video clip of behaviour, the only observable cues receivers can receive are in the sender's appearance and the behaviours that the sender produces. Therefore, the sender's states, traits and intentions can only be understood based on the inferences a receiver makes. This means that receivers' states, traits, intentions and interaction history can influence the process. To some degree then, a sender's cues and a receiver's interpretations of these cues may be decoupled such that it is clear to what extent judgements made refer specifically to sender behaviour or are informed by receivers assumptions based on their own prior beliefs. However, the nature of the social world means that sender and receiver roles are inextricably intertwined. As researchers then, it is important that we understand the constraints of our methodologies and recognise the sources of the data that we interpret.

Limitations

Like all experimental research, this work has a number of limitations. First, although all care has been taken to make this work as generalizable as possible, the very nature of experimental research necessitates a level of control that is not present outside of the laboratory setting. To balance ecological and internal validity, I have presented a study, (Chapter 2) in a somewhat uncontrolled environment, face-to-face interactions. However, to examine specific mechanisms that affect the results, I have also included well-controlled laboratory experiments.

In live interactions, people use a wide range of cues and display a number of verbal and nonverbal cues simultaneously. Therefore, a second limitation is that I have focused on quite specific social cues, with an emphasis on genuine and polite smiles. There are likely other important cues that senders and receivers display and interpret which have profound impacts on the outcomes of interactions. However, smiles are iconic and frequently displayed in natural social environments (Hess & Bourgeois, 2010). Thus, I have focused on understanding their roles and values. However, similar mechanisms may govern the use of other social cues, such as frowns, paralinguistic cues, etc. Moreover, it is also likely that verbal and nonverbal cues interact with each other to influence receiver judgements. Although I show that the congruence of verbal and nonverbal cues has some influence on receiver judgements, it is more difficult to explore this in naturalistic settings due to the 'noisiness' of these interactions.

Finally, due to the diverse and complex nature of the social interaction and this work as a whole, I have presented data collected in several contexts. Though it is difficult to generalise the more specific findings to other contexts, for example, one can only speculate whether other states, such as depression or social anxiety, affect smile utility, it is clear that

the importance of understanding the interdependence between senders and receivers of social signals generalises to many social contexts.

Future Directions

Together the findings of this research raise an interesting set of questions for future exploration. First, to further understand the extent to which individuals are able to exert social influence, as presented in Chapter 5, one might explore senders' ability to influence receivers in such specific ways when the relationship between senders and receivers is not fundamentally defined by power, as in the experimenter–participant dyad. In a cooperative situation, might one person's expectations lead to behaviour changes that ultimately influence specific non-social behaviours in an interaction partner?

Second, although I discuss the inherent complexity of social interaction, the interactions to which I refer have all been dyadic, i.e., between two people both serving as senders and receivers of social cues. It might be interesting, therefore to examine interactions between small groups of three or more individuals. The added complexity of navigating an interaction between more people may help us answer questions about how individuals exert influence over each other such that some people naturally assume leadership roles, for example. In addition, one might examine interpretations by different receivers of the same sender that occur simultaneously in order to understand individual differences in receiver interpretations. Together, such studies may help disentangle sender and receiver effects.

Conclusions

Taken together, this work highlights the interdependence between the senders and receivers of social cues along with their fundamentally important roles in the natural moment-by-moment evolution of an interaction. In addition, though research tends to

separate cue senders and receivers, in a face-to-face interaction both participants occupy both roles simultaneously and as such, their own traits, states, and intentions influence the ways in which they both signal and interpret social cues. Importantly, social cues cannot be reduced to a one sided process. In order to understand how humans interact, it is crucial to acknowledge that both roles are equally important and interdependent. This understanding has broad implications for those who struggle with social interactions, for those in positions of power and those who lack it. It also has important implications for the interpretation of psychological research more generally, highlighting that people's ability to influence or bias others' behaviour is an important consideration in the design of future research work.

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Appendices

Appendix A: Verbal coding manual – Chapter 2

Social Rejection: Interaction Coding Manual—Verbal Behaviours

Category:	Subcategory	Code	Example
Questions that:			
	change the conversation topic	101	[Any question that changes the conversation topic]
	elicit detail about a particular topic	102	“What happened?”; “How long were you in France for?”
	ask the other partner about the same topic	103	"What about you?"
	solicit feedback about self/behaviour/opinions	104	"What do you think about that?"
	seek clarification	105	"What did you say?"; "Excuse me?"; "Pardon me?"
	elicit agreement/affirmation from partner	106	"...isn't it?"; "…aren't you?"
	cannot be classified as above	199	
Self-focused talk about:			
	general information about self		
	highly superficial	201	[providing little more that a yes/no answer to partner's query]
	provides some detail	202	"I'm in psychology."; "I live in [residence hall]."; "I have 2 brothers."
	provides moderate detail	203	"I'm thinking about doing a certificate in primary education."
	self-promotion (e.g., achievements, abilities)	204	“I’m pretty good at Spanish.”
	self-denigration (e.g., downplaying abilities)	205	"I'm really awful at maths."
	excusing own behaviour/apologizing	206	“I’m sorry.”; “I didn’t really mean that.”; “I don’t mean to pry, but….”
	expressions of uncertainty (self-relevant)	207	"I don't know."; "I'm not sure."
	cannot be classified as above	299	
Partner-focused talk about:			
	general information about partner	301	"I think I've seen you at volleyball."
	complimenting the partner	302	“You must be really good at that.”; “Those are great shoes.”
	agreeing with partner	303	“I agree.”; “Of course.”
	disagreeing with partner	304	“No way.”; “I disagree.”
	offering advice to partner	305	“You should talk to the lecturer about that.”
	supporting/defending partner	306	“I know how you feel.”; “That seems pretty reasonable.”
	cannot be classified as above	399	

General talk about:			
	factual information	401	[any provision of a fact/item of knowledge unrelated to self/partner]
	the weather	402	[any comment about the weather]
	general small-talk	403	[any general small-talk]
	opinions/politics	404	[offers opinions; preferences about general topics]
	expressions of uncertainty (general)	407	"I don't know."; "I'm not sure."
	cannot be classified as above	499	

Emotion-focused talk expressing:			
	negative affect/preference related to study		
	self-relevant	501	"These situations make me kind of nervous."
	partner-relevant	502	"You look a little unhappy."
	negative affect/preference **un**related to study		
	self-relevant	503	“The lecturer makes me so bored.”; “She really annoys me.”
	partner-relevant	504	"You must have been really frustrated."; "That sounds unpleasant."
	positive affect/preference related to study		
	self-relevant	505	"I think this study is kind of fun."
	partner-relevant	506	"It was good to meet you."
	positive affect/preference **un**related to study		
	self-relevant	507	"I always like going to [club name]."
	partner-relevant	508	"So you enjoy eating at [resaurant name]."
	cannot be classified as above	599	

Exclamations/expletives:	exclaims about something/utters expletive	601	“Brilliant!”; “Really.”; "Shit."
Laughter:	laughs (either quitely or aloud)	602	[laughter]
Back-channel verbalization	verbal comment to mark engagement	603	“uh-huh.”; “mm-hmm.” ; “yeah.”; “right.”

s:			
Filler verbalizations:	utterance produced to fill an awkward silence	604	"hmmm."; "Not sure what to talk about…"

Study-related comments:			
	specific queries about study	701	"Are we supposed to be talking in English?"
	general queries about study	702	"How long will we be talking for?"
	expressions of uncertainty (study-related)	407	"I don't know."; "I'm not sure."

Foreign Language:	[speaks in foreign language]	799	

Silence:	episodes in which participant is quiet	0	[participant is not producing any verbalization]

Unclassified:	any other unclassified verbal behaviour	999	

Appendix B: Nonverbal coding manual – Chapter 2

Social Rejection: Interaction Coding Manual—Nonverbal Behaviours

Face

Code	Behaviour
100	Smile—pleasurable (in this type of smile, the muscles around the eyes contract, pulling the eye corners tighter)
110	Smile—polite (no eye muscle involvement)
112	Smirk, sneer, contempt
114	Open mouth - surprise
116	Lip purse
120	Frown (mouth turned down at corners; may or may not include brow activity)
130	Grimace (mouth open and corners pulled down; may or may not include brow activity)
132	Nose wrinkle
140	Brows raised
142	Brow scrunch
150	Brows lowered
160	Squint (eye corners tight)
162	Lip lick/bite
164	Chin pushed up
166	Lip press (lips pressed tightly together)
168	Eye-roll/close eyes
170	Neutral: either talking or listening but none of the above facial expressions
197	Yawn
198	Face not in view of camera
199	Unclassifiable facial behaviour (please note this in the notes field)

Hands

200	Hands engaged in communicative gesture (iconic gesture with clear meaning – e.g., thumbs up signal)
210	Hands engaged in non-communicative gesture (gesture without clear meaning – e.g., moving hands while talking)
220	Hands fidgeting (with or without object; hands may touch one another)
230	Face/head/hair/clothing touch
240	Hands still
250	Shakes partner's hand
298	Hands not in view of camera
299	Unclassifiable hand behaviour (please note this in the notes field)

Posture

Code	**Behaviour**
300	Facing forward (body oriented towards partner); posture **open** (shoulders back and/or arms uncrossed)
310	Facing off-center (at least 20 degrees off center); posture **open** (shoulders back and/or arms uncrossed)
320	Facing forward (body oriented towards partner); posture ***closed*** (shoulders slouched and/or arms crossed)
330	Facing off-center (at least 20 degrees off center); posture ***closed*** (shoulders slouched and/or arms crossed)
_ _ 1	Leaning on chair arm (change the last digit of the appropriate code from 0 to 1 if participant is leaning on chair)
399	Posture other than above (please note this in the notes field)

Appendix C: List of Film URLS – Chapter 3 Experiment 1

Positive clips:

Monty Python's ministry of silly walks: http://www.youtube.com/watch?v=iV2ViNJFZC8

Wedding Bloopers: http://www.youtube.com/watch?v=LBO9bY6rAsk

Negative clips:

Botfly extraction: http://www.youtube.com/watch?v=23eimVLAQ2c

Hand operation: http://www.youtube.com/watch?v=_7ihlyW7LxQ

Appendix D: Experimenter Scripts – Chapter 4 Experiment 1

Personality, Emotion, Behaviour Script

BEFORE PARTICIPANT ARRIVES:

- Prepare study information and consent forms
- Start the computer task by entering the participant ID and the participant's condition when prompted. Enter 'H' for high-power and 'L' for low-power (the ID list says which condition each participant has been assigned to).
- Collect participant from waiting lounge.

AFTER PARTICIPANT ARRIVES:

Hi, welcome to the experiment. My name is _____ and I'll be working with you for today. First, can you please read the study information sheet, which describes what you'll be doing in this experiment. Once you've done that, if you're happy to participate please sign the consent form on the next page.

I'll leave the room while you do that, but I'll be back in a couple of minutes, and then I'll explain what you need to do in the experiment itself. **[Leave the room while participants complete the form.]**

Ok, do you have any questions? **[Check that participants have signed the consent form and collect the completed form.]**

The first thing I'll ask you to do is to complete the demographics questionnaire. For the question that asks about your years of education, you can just put what year you are in University. **[Leave the room while participants complete the form.]**

[Check that participants have completed the demographics form and collect the completed form.] Now that we are about to start, can you please turn off your mobile phone? **[Wait while participant turns phone off.]** Thanks. This experiment is about how subtle changes in a person's mood alter performance on a variety of tasks. To measure this, we will ask you to complete a mood inventory on the computer. Then the computer will ask you to complete a scrambled sentences task. In this task, you will see a set of 5 words, presented in a random order. You will need to use the mouse to make a grammatically correct sentence with 4 of them. There are more instructions about this in the task itself.

After the scrambled sentence task, you will complete another mood inventory followed by a second task. This task is a word rating task where you will see a word and decide how well the word fits into a category. For example, you might be asked to decide how well the word 'dog' fits into the category 'pet.' You should try to rate each word as quickly and accurately as you can. There are more instructions in the task as well. The word rating task will be followed by a set of questionnaires on the computer. If you have questions during the task, I'll be next door so please ask if you need something.

Do you have any questions? **[Answer any questions they have. Then, press the space bar to start the first mood inventory.]** Ok. Here is the first mood inventory. Click the line at the point that indicates how much of this **[point]** feeling you are experiencing right now. **[Leave the room while participants complete the task.]**

WHEN TASK IS FINISHED:
Ok. Did you have any questions about the task? I just have a few follow up questions for you. **[Read questions from debriefing sheet and complete the debriefing information.]**
Here is a bit of information about the task for you to take with you. **[Give participants the debriefing information sheet, along with the initial participant information sheet.]** I'll make sure your SONA account is credited for participating today. Thank you for participating.

Appendix D: Experimenter Scripts – Chapter 4 Experiment 2

Personality, Emotion, Behaviour Script

BEFORE PARTICIPANT ARRIVES:

- Prepare study information and consent forms
- Start the computer task by entering the participant ID and the participant's condition when prompted. Enter 'H' for high-power and 'L' for low-power (the ID list says which condition each participant has been assigned to).
- Collect participant from waiting lounge.

AFTER PARTICIPANT ARRIVES:

Hi, welcome to the experiment. My name is _____ and I'll be working with you for today. First, can you please read the study information sheet, which describes what you'll be doing in this experiment. Once you've done that, if you're happy to participate please sign the consent form on the next page.

I'll leave the room while you do that, but I'll be back in a couple of minutes, and then I'll explain what you need to do in the experiment itself. **[Leave the room while participants complete the form.]**

Ok, do you have any questions? **[Check that participants have signed the consent form and collect the completed form.]**

The first thing I'll ask you to do is to complete the demographics questionnaire. For the question that asks about your years of education, you can just put what year you are in University. **[Leave the room while participants complete the form.]**

[Check that participants have completed the demographics form and collect the completed form.] Now that we are about to start, can you please turn off your mobile phone? **[Wait while participant turns phone off.]** Thanks. This experiment is about how subtle changes in a person's mood alter performance on a variety of tasks. To measure this, we will ask you to complete a mood inventory on the computer. Then the computer will ask you to complete a scrambled sentences task. In this task, you will see a set of 5 words, presented in a random order. You will need to use the mouse to make a grammatically correct sentence with 4 of them. There are more instructions about this in the task itself.

After the scrambled sentence task, you will complete another mood inventory followed by a second task. This task is a card game where you will turn over cards. Most cards allow you to win points – but there are cards that are punishment cards that make you lose points also. You can use the mouse to turn over as many cards as you like. You will earn a printer credit bonus based on your earnings on a random selection of trials so bear this in mind as you play. The more trials you earn points on the more bonus credit you can earn. There are instructions about this task in the computer program so please read them carefully. The card task will be followed by a set of questionnaires on the computer. If you have questions during the task, I'll be next door so please ask if you need something.

Do you have any questions? **[Answer any questions they have. Then, press the space bar to start the first mood inventory.]** Ok. Here is the first mood inventory.

Click the line at the point that indicates how much of this **[point]** feeling you are experiencing right now. **[Leave the room while participants complete the task.]**

WHEN TASK IS FINISHED:
Ok. Did you have any questions about the task? I just have a few follow up questions for you. **[Read questions from debriefing sheet and complete the debriefing information.]**
Here is a bit of information about the task for you to take with you. **[Give participants the debriefing information sheet, along with the initial participant information sheet.]** I'll make sure your SONA account is credited for participating today. **[If the computer screen does not say how many printer credits they earned, then ask them (the maximum is £4). If they earned credits, then tell them that they should see the credits reflected in their account in the next couple days.]** Thank you for participating.