THE CONCEPTUALISATION & MEASUREMENT OF PROSOCIALITY

by

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A thesis submitted to the University of Birmingham for the degree of

DOCTOR OF PHILOSOPHY

Centre for Human Brain Health

School of Psychology

College of Life and Environmental Sciences

University of Birmingham

January 2026

Abstract

Prosociality - attitudes and behaviours intended to benefit others - is a defining feature of human social life. While other animals exhibit limited forms of helping or cooperation, humans are an extreme outlier in both the scale and diversity of prosocial behaviours they display, including donating, cooperating, volunteering, and helping others, often at substantial personal cost or risk. These behaviours have been fundamental to the development of human societies and to our success as a species. Consequently, understanding the causes, mechanisms, and boundary conditions of prosociality is a central aim of psychological science.

This thesis examines prosociality at two levels: empirical and conceptual. At the empirical level, I present two lines of work (Chapter 2 and Chapters 3–5) examining how prosocial attitudes and behaviours vary as a function of financial status, and how they manifest in a novel group-based reward decision-making task. In Chapter 2, using a globally representative dataset, I show that higher income and greater subjective financial satisfaction are reliably associated with higher reported prosociality across a wide range of measures and cultural contexts. Chapters 3–5 describe the development of a new public-goods-style task, derived from social foraging theory, designed to capture key features of real-world dynamics, such as those elicited in Chapter 2, more closely than its predecessors. Results from three experiments probing reward-based decision-making in group contexts are presented.

In addition to the empirical work, the thesis addresses how prosociality is conceptualised and measured. This unresolved issue is critical because no single, widely accepted definition exists, key terms are often left undefined, and prosociality is frequently operationalised using single measures despite its conceptual breadth. As a result, the foundations of much prosociality research are fragile. In the general introduction, I argue for caution in treating prosociality as a unified empirical construct, and suggest that subtypes such as altruism and cooperation offer greater methodological clarity. The concluding discussion illustrates how this approach improves the validity of inferences drawn from the empirical studies.

Together, these findings provide new insights into prosocial behaviour while highlighting the importance of clear conceptualisation and rigorous measurement in psychological science.

Acknowledgements

I guess every thesis has a story behind it. This one started with a nine year-old boy who wanted a chemistry set for Christmas. Thanks Mum. For the kit, but also for tolerating the resulting purple mess, and lingering smells throughout the house. Thanks for so many things - reading to me when I was a kid, for letting me sit at the back of your O level Biology class, and more recently for the continual flow of Einstein quotes of dubious provenance. They may not have always been historically accurate, but they always made me laugh. But thanks most of all for continually reminding me that the heart of science is about asking questions. I dedicate this work to you.

To my partner, Mitch, you have encouraged and supported me from the very beginning of this change. So many Masters' degrees that they seemed to merge into one, and the subsequent PhD (including when I decamped to another country for three months) which feels like it's been going on as long as we can both remember. It's been six happy years and a great experience, and I'm so very grateful to you for it. I am so lucky to have had you by my side through all of this, pronouncing it pro-'snow-shoe'-ality just to annoy me and for regularly dragging me away from the computer to practice being a normal human. Thank you with all my heart, I'm looking forward to whatever's next.

To Jake, Holly, Daisy and Albie – thank you all for being proud of your old Dad. It has meant, and will always mean, a lot.

There are a few people I've met along the way without whom the experience wouldn't have been the same. Lily, for all your cheerful positivity and calm pragmatism during the experimental work and the G19 sessions. Andrea Pisauro for some happy study days at the Bodleian, and your wisdom and advice on both technical and academic matters. Yang Zhang for PsyBuilder help at (your) ungodly hours. Adam Baimel and Professor Gavin Davidson for sage advice when I needed reassurance and perspective. Special mention to my mentor, Renata, for your tireless and indisputably prosocial intentions in helping me through - thank you for your counsel, for being there when I needed you - and for being so fantastically Dutch about everything. Margot, merci d'être mon ami.

I would like to thank Professor Lockwood for giving me the opportunity to do this PhD - I have appreciated the advice and challenge in helping me to develop and hone my ideas over the past 4 years, Dr Jo Cutler for her statistical expertise and astounding knowledge of the inner workings of R functions, and to Dr Seongmin Park for graciously hosting my three-month placement at the Institute of Cognitive Science in Lyon. I would also particularly like to thank Professor Ian Apperly for his calm midwifery through the final stages of my write-up and submission.

I would also like to thank the workers who created the wealth to fund this research, the erstwhile politicians, legislators and administrators who put in place the mechanisms to collect and distribute it, and, ultimately, the ESRC for this studentship. I hope that my own labour has resulted in something worthy of all that went before it.

And finally, to the spirit of rock n roll, to "Eulogy" by Frank Turner, to Billy, Malcolm and Angus, and Lemmy.

"If liberty means anything at all, it means the right to tell people what they do not want to hear", George Orwell.

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Chapter 1
General introduction

1 Introduction

Prosociality - the tendency to act in ways that benefit others - has long been a central concern across disciplines, including evolutionary biology, psychology, neuroscience, economics, child development, and political science. Within psychology, the concept is typically used to describe behaviours that confer benefits on non-kin individuals (K. Jensen, 2016), encompassing acts such as helping, sharing, and cooperation. Prosocial behaviour plays a fundamental role in shaping individual well-being, social relationships, and the functioning of societies at scale. Its consequences are sufficiently wide-reaching that prosociality has recently been proposed as a public-health priority in its own right (Kubzansky et al., 2023).

This thesis considers prosociality at two levels, one empirical and the other conceptual. The main body of the thesis consists of two empirical segments. The first of these (Chapter 2) is a published paper of global survey data analysing the relationship between prosociality and wealth. The second section (Chapters 3-5) presents a series of experiments using a new paradigm which was designed and developed as part of my PhD research. The sections are related - the lab-based experiments were created to test some of the conclusions from the global survey - but each can be interpreted as a standalone piece of work. Together, they represent the empirical contribution of this thesis. Across both projects, what holds them together thematically are considerations of how prosociality is conceptualised and measured. This has been a fundamental issue across the field for the past fifty years (Pfattheicher et al., 2021; Wispé, 1972) and remains largely unresolved.

Both empirical sections are considered in light of how they conceptualise and seek to measure prosociality, and important broader conclusions can be drawn from this. The structure of the thesis reflects this construction. The general introduction is concerned with how prosociality is conceptualised and measured across a wide range of experimental techniques. Each of the two major sections (**Chapter 2**, and **Chapters 3-5**) has its own introduction covering the specific literature and theoretical basis, and its own discussion summarising the results and drawing conclusions at a chapter level. The general discussion then summarises and discusses the findings for each of the two empirical sections, but also synthesises insights at a more conceptual level regarding how prosociality is conceptualised and measured, tying back to this general introduction.

The remainder of this general introduction is dedicated to the conceptualisation and measurement of prosociality relevant to both sections. Here, I lay out a case that prosociality is not a singular concept, and we should instead focus primarily on operationalising and measuring its sub-types, such as cooperation or altruism.

1.1.1 Origins of prosociality as a scientific term

To frame subsequent discussion, it is helpful to first understand the roots of this object of study. Early usage of the term prosocial started early in the 20th century, with the first recorded mention in the Cincinnati Board of Education Annual review of 1918 describing schools as “the great prosocial medium” (Cincinnati Board of Education, 1918), and the OED recording its first usage as 1925 in an article regarding the role of employe (sic) relations in “furthering the ends of a prosocial production and wholesome, happy human relation” (Metcalf, 1925). However, academic usage did not begin in earnest until the 1970’s (*Google Books Ngram Viewer*, n.d.) most notably with Lauren Wispé’s appropriation of the phrase ‘prosocial aggression’ from developmental psychology, where the word prosocial was used as a concise substitute for “positive forms of social behaviour” (Wispé, 1972). It is interesting to note that at this point, the term had no formal definition and had already been applied in the diverse fields of child and organisational development. It is particularly worth noting the caution in interpreting the different forms of prosociality noted by Wispé:

“these different manifestations of positive social behavior should be distinguished so that they can be operationalized more precisely, and their genotypic similarities and differences clarified. As openers, we might distinguish among altruism, sympathy, cooperation, donating, and helping.” (Wispé, 1972)

Similarly, Gergen et al (1972) noted that "different personal dispositions are related to various forms of prosocial behavior in different ways. The traditional search for general correlates of prosocial behavior is viewed as short-sighted". The adoption and spread of the term was propelled through the work of Eisenberg and Batson in the 1970s onwards, who delivered seminal work on prosociality from the perspective of child development and social psychology respectively (Darley & Batson, 1973; Mussen & Eisenberg-Berg, 1977), enhanced and extended by others such as Dovidio and Penner (Dovidio et al., 2006; Penner et al., 2005, 1995b). Almost five decades later, we find that these problems persist with conceptual and operational inconsistencies in the study of prosociality "In order to improve on the conceptual ambiguity surrounding the study of prosociality, we urge researchers to provide definitions, to use operationalizations that match their definitions, and to acknowledge the diversity of prosocial behavior" (Pfattheicher et al, 2021). The issue of loose categorisation been acknowledged by others (Dovidio et al., 2006; Pearce & Amato, 1980) and over that time span, at least a dozen distinct attempts have been made to categorise prosociality, none of which have been universally adopted by its scientific community.

1.1.2 Definitions of prosociality

Having already noted some of the classification issues endemic to the study of prosociality, which form an important context, a review of existing definitions is provided here as the basis for the remainder of the thesis. At the highest level, prosociality may be described as an object of study in psychological science composed of a collection of attitudes, motivations, emotions and behaviours intended to improve the mental or physical well-being of others. We may define prosocial behaviour as a subsection of the main category of prosociality, which may also include attitudes, pre-dispositions or traits which do not directly manifest in observable behaviour. For example, it may be the case that some circumstances elicit prosocial emotions, but no prosocial behaviour, such as someone watching a news report on an overseas tragedy, but not taking any action. Hence, a definition of prosociality may include both behavioural and psychological phenomena, but it is useful to differentiate between them.

One important aspect of the definition of prosociality is that of intention. Pfattheicher et al's (2021) excellent analysis of the conceptual issues surrounding prosociality provides a useful set of exemplary definitions from the extant prosociality literature (Table 1.1). Of these, some mention intention explicitly, others are purely consequentialist, and some, such as that of Penner, define prosociality as behaviours which are determined by societal value and thus do not require the actors' intent to be benevolent, only that they comply with societal norms.

Concept and dimension	**Exemplary definition**	**Reference**
Prosocial behavior emphasizing intentions	"Prosocial behavior covers the broad range of actions intended to benefit one or more people other than oneself"	Batson and Powell (2003, p. 463)
Prosocial behavior emphasizing consequences	"Any action that benefits another"	Schroeder and Graziano (2015, p. 255)
Prosocial behavior emphasizing intentions and consequences	"Voluntary, intentional behavior that results in benefits for another"	Eisenberg and Miller (1987, p. 92)
Prosocial behavior emphasizing societal context	"Prosocial behavior means no more, and no less, than behavior that is valued by the individual's society"	Dovidio (1984, p. 364)
Prosocial behavior emphasizing societal context and consequences	"Prosocial behavior represents a broad category of acts that are defined by some significant segment of society and/or one's social group as generally beneficial to other people"	Penner et al. (2005, p. 366)
Altruistic motivation from an intentionalist perspective	"Motivation with the ultimate goal of increasing another's welfare"	Batson et al. (2010, p. 16)
Altruistic behavior from an intentionalist perspective	"Voluntary behavior intended to benefit another, which is not performed with the expectation of receiving external rewards or avoiding externally produced aversive stimuli or punishments"	Eisenberg and Miller (1987, p. 92)
Altruism from a consequentialist perspective (emphasizing economic costs and consequences)	"Costly acts that confer economic benefits on other individuals"	Fehr and Fischbacher (2003, p. 785)
Altruism from a consequentialist perspective (emphasizing evolutionary costs and consequences)	"A behavior that is costly to the actor and beneficial to the recipient or recipients. Costs and benefits are defined on the basis of the lifetime direct fitness consequences of a behavior"	West et al. (2011, p. 232)
Altruism from a societal perspective	"A moral norm (which) implies certain social expectations of helping others in different social contexts"	Bykov (2017, p. 808)

Table 1.1 | Exemplary definitions of prosocial behaviour and altruism
(reproduced with permission)

Many authors however do recognise the importance of conscious intention in their definitions, Batson and Eisenberg, but also Mikulincer and Shaver (2010) define prosocial behaviours as “voluntary behaviour enacted with the intention of benefitting others”. Jensen (2016), Wispe (1972), and Bar-Tal (1986), amongst others, are examples of discussions which place intention as central to the concept of prosocial behaviour. De Waal (2008) provides a clear summary, contrasting the consequentialist, ultimate-goal focus of evolutionary biology with the motivational, proximate-goal orientation of psychologists. The latter he terms “directed altruism”, which is characterised into three elements: altruistic impulse (spontaneous, disinterested), learned altruism (conditioned response) and intentional altruism (“based on the prediction of behavioral effects [...] we may call this intentionally altruistic altruism”).

Kopp et al’s (2024) review of comparative research across human and non-human species also emphasises intention as a fundamental characteristic of prosociality, “three key features that need to be in place for behaviour to count as prosocial: benefitting others, intentionality, and voluntariness”, a position endorsed even more strongly by Li et al (2025) who suggest that “Amending the working definition of prosociality to be centred on intentions, not outcomes, would make it more useful as a tool for comparative and developmental research.” Various studies show that different prosocial behaviours have unique motivations (Paulus, 2018), that multiple motivations can alter prosocial bias (Saulin et al., 2022) and the importance of intention in defining prosocial acts (Li et al., 2025). One final clarification is how this definition relates to specific instantiations of prosociality, such as cooperation or altruism.

There is no universally accepted taxonomy for prosociality sub-types (Appendix 1), but we may take the general principle that behaviours such as altruism or cooperation can be considered as different sub-types of prosocial behaviour. Each of these can be distinguished definitionally for example, altruism is an act that benefits others at a cost to self (Fehr & Fischbacher, 2003), whereas cooperation is defined as an explicit understanding of an act that benefits both self and other(s) (Pruitt & Kimmel, 1977).

In sum, there exists substantial theoretical and empirical support for the consideration of intention in definitions of prosociality, and the thesis treats intentionality as a theoretically significant dimension of prosocial behaviour. However, the thesis does not advance intentionality as a necessary definitional criterion — rather, it is acknowledged as one of several contested but important features of prosociality identified across the literature, the operationalisation of which presents genuine empirical challenges. Where empirical measurement of intention is not possible, the thesis focuses on observable behaviour, while recognising that the motivational basis of that behaviour must be inferred.

1.1.3 Why prosociality is important

Prosociality is something which is beneficial by definition. Those being helped, donated to, volunteered for, shared with, comforted, informed, and trusted experience the myriad benefits that these forms of prosociality convey. However, the benefits do not only accrue to the targets of the prosociality. It has been repeatedly shown in a variety of ways that prosociality is also beneficial to the actors themselves.

People who behave more prosocially are happier (Chancellor et al., 2018; Curry et al., 2018; Nelson et al., 2016; Regan et al., 2023) have better physical and mental health (Brown & Brown, 2015; Hui et al., 2020; Nakamura et al., 2024; Nichol et al., 2023) particularly in later life (Kahana et al., 2013), cope better with stress (Raposa et al., 2016), suffer less loneliness (Lanser & Eisenberger, 2023) and it is suggested even live for longer (Hilbrand et al., 2017; Nakamura et al., 2023; Post, 2005). Such are the benefits of prosociality that it has been proposed that it should be considered as a public health priority (Kubzansky et al., 2023).

If this were not reason enough to be interested in prosociality, it becomes even more compelling when one considers that it can also be seen as a conduit for action on global challenges such as climate change and the environment (Boon-Falleur et al., 2022; Klein et al., 2022; Parks et al., 2013; Van Vugt, 2009). When one considers the benefits to beneficiaries, to the actor and to society as a whole, it is understandable that some have called for it to be classified as a public health priority (Kubzansky et al., 2023).

1.1.4 Modern theoretical foundations of prosociality

During the history of research and study into prosociality, theories and hypotheses have been proposed to account for different aspects of how it manifests itself. Examples are the empathy-altruism hypothesis (Batson, 2010), social preference theory (Fehr & Schmidt, 1999), the theory of warm glow giving (Andreoni, 1990), socio-emotional selectivity theory (Carstensen, 2021), and attachment theory (Gillath et al., 2005), amongst others. Many of these theories provided important progressions in understanding in their historical context, such as distinguishing the psychological motivations for prosocial behaviour, incorporating benefits to others in valuation (utility) of personal reward, and accounting for changes in prosocial motivation with age. However, many researchers recognise that at this stage of its development, psychology as a scientific discipline still lacks a universally agreed-overarching theoretical framework, and each of these theories has relatively limited generalizability (Muthukrishna & Henrich, 2019), though this is an active field of research (Badcock et al., 2019; Friston, 2010; Kringelbach et al., 2024). Muthukrishna and Henrich (2019) described the status of current psychological theory as “largely a potpourri of disconnected empirical findings on topics that have been popular at some point in the discipline’s history, and clustered based on largely American and European folk categories”. Their dual-inheritance theory provides a framework for understanding human prosociality as a combination of genetic and cultural influences. The basis of the theory is the so-called modern synthesis, combining Darwinian selection and genetic variation (Mayr & Provine, 1981). However, ever since Darwin, the puzzle of human prosociality has always been, why would one organism sacrifice its own fitness for the benefit of another? From a biological and evolutionary perspective, a great deal can be explained by looking at this question at a population level. Nowak’s (Penner et al., 1995a) five rules explain how mechanisms of cooperation such as direct and indirect reciprocity, the importance of kin selection (Penner et al., 1995a), and group selection can create the conditions in which cooperative behaviours (and ultimately therefore genes) are selected. Further important work in this area deals with mechanisms of how kin-based instincts become extended into non-kin based prosocial behaviours in humans (Bowles & Gintis, 2011; West et al., 2007). But humans are complex creatures, and we are shaped as much by the culture we have ourselves created as we are by our genetic inheritance (Bell et al., 2009; Chudek & Henrich, 2011; Feygina & Henry, 2015; House et al., 2013). Thus, a full understanding of prosociality also critically requires an understanding that transmission is both genetic and cultural. Mathematically formalised models demonstrating how altruism can evolve and sustain in animals through a mixture of both genetic and cultural co-evolution have been clearly elucidated by Bowles and Gintis (2011) among others.

In its broadest sense then, prosociality can be understood as an adaptive suite of behaviours that enhance collective fitness by promoting cooperation. It likely originated through the fitness benefits of caring for kin, favouring genes that increase the success of related others, but has since extended, particularly in humans and other social mammals, to encompass cooperation among non-kin (Bowles & Gintis, 2011; West et al., 2007). Group-level modelling has been shown to confer competitive advantages to populations in which prosocial norms are widespread (Bowles & Gintis, 2011). In humans, these tendencies are shaped and transmitted not only genetically but also culturally (Cofnas, 2018; Feygina & Henry, 2015; Septarini et al., 2025), allowing prosocial preferences and norms to evolve and diversify far more rapidly than biological evolution alone would permit. In psychological science, we are concerned with how prosocial attitudes are formed and how they influence behaviour.

It should be clear from the previous discussion that, rather than a sharply defined univocal construct, prosociality is best thought of as a broad and descriptive term for a set of loosely related types of other-helping attitudes and behaviours. In this sense, to invoke an analogy, we may think of measuring prosociality like measuring weather. There is a common-sense view of what good and bad weather is, but weather, like prosociality, is a broad and loose term for a range of characteristics, and what is good and what is bad depends heavily on the situation. We can measure rainfall, temperature, wind speed and so on, and each of these combined gives us a sense of the weather for whatever purpose we have, but it seems obvious that *measuring* weather makes no sense. I argue it is the same for prosociality. Each sub-type may have different characteristics (Li et al., 2024; Pfattheicher et al., 2021), but we require accurate and reliable ways of measuring prosociality in all of its forms. The next section summarises the common experimental methods in measuring them and more fully considers the evidence supporting a focus on sub-types of prosociality rather than the singular concept.

2 Measuring prosociality

In keeping with the breadth of conceptualisations of prosociality, there is a commensurate breadth of ways in which it is measured. Early social psychology experiments used survey data (Gergen et al., 1972), or live experiments where behaviour was manually recorded/coded, for example, whether people stopped to help a person in need on the street (Darley & Batson, 1973; Radant, 1985). Most contemporary methods can be broadly categorised as follows:

1. Analysis of survey data, e.g. traits and self-reported attitudes and behaviours, sometimes combined with dimensionality reduction techniques like factor analysis
2. Observation and classification of actual behaviours (in lab or in the wild) with or without experimental manipulation
3. Analysis of child development trajectories
4. Observation and classification of neurocognitive data

When considering prosociality that includes a motivational criterion it is important to note that psychological motivations cannot be directly observed, only inferred, and this creates a difficulty in measuring prosociality defined in this way. Such inference can be made using neuro-imaging, a specific experimental design or a combination of the two. For example, Hein et al (2016) showed differences in the functional mapping of neural response between empathy-based and reciprocity-based altruism. In their meta-study of prosocial paradigms and associated neural responses, Rhoads et al (2021) found three distinct feature-based clusters of prosociality, which they classified as cooperation, equity and altruism. Other researchers have used experimental designs such as the 'black box' method, which works as an asocial control to quantify the extent to which people are behaving prosocially in a task (Burton-Chellew & West, 2022). However, of the methods listed above, the most relevant to this thesis are survey data (including traits) and lab-based behavioural experiments.

1.2.1 Self-reported prosocial behaviour (SR-PB)

Perhaps the most straightforward and obvious method of measuring prosocial behaviour is simply to ask people what they do or have done. Self-report behaviour is the basis of many measures of prosociality. For example, the Gallup World Poll (Chapter 2) asks people if they have done any of the following over the past 12 months: donated to charity, volunteered, or helped a stranger. Self-reported measures are open to inaccuracy and bias on the part of the responder, and this is exacerbated in the case of prosocial behaviour by its normative nature. Respondents are potentially more likely to overstate behaviours they see as morally good, and vice-versa, the so-called social desirability bias, and it is particularly difficult to correct for methodologically (Podsakoff et al., 2003). Despite this, there are many advantages to self-reported behaviour measures as they are direct, simple and quick to administer.

There are very few studies which address the external validity of instruments purporting to measure prosociality. Most reviews focus on internal consistency or convergent validity with other measures, such as traits, with very little work directly connecting SR-PB to actual behaviour (Luengo Kanacri et al., 2021; Reig-Aleixandre et al., 2023). Where evidence does exist, it generally reveals null or weak links between the two (Awan et al., 2020; Falk et al., 2023; Galizzi & Navarro-Martinez, 2019; Gollwitzer et al., 2022; U. T. Jensen, 2020). Without mitigation therefore, it would seem unwise to use SR-PB as a measure of prosociality. However, the Global Preference Survey that provides four of the seven measures of prosociality in Chapter 2, is unusual in that the measures were developed by selecting items that were most predictive of behaviour in lab-based economic games with real money incentives (Falk et al., 2023). This unique methodology gives an additional level of confidence in these measures in drawing valid conclusions about prosocial behaviour.

1.2.2 Behavioural studies and economic games

Many behavioural studies of prosociality will use a defined action that is self-evidently prosocial. Typical examples would be donating money to others (Landry et al., 2006), helping a needy stranger in the street (Darley & Batson, 1973), or children giving away toys/coloured pencils to other children (Grusec & Redler, 1980). Similarly, there exists a genre of classic experiments whereby unwitting participants find a (planted) lost letter or wallet and whether or not they return them and/or whether they remove some of the contents (e.g. money) is recorded by researchers (Cohn et al., 2019; Holland et al., 2012). These experiments score highly on ecological validity, but only measure one form of prosocial behaviour and are limited by the fact of random sampling, whereby there is no direct control over the sample composition and little or nothing is known about the participants themselves. Lab-based experiments give more control to the experimenter, and quantifiable outcomes, such as decisions involving costs and rewards to self and others, have provided the backbone of much prosociality research. Such so-called economic games have been widely used by psychologists and economists over the past few decades to further our understanding of prosociality. Classical examples, such as the Prisoner's Dilemma or Ultimatum game, generally measure prosociality as the difference between economically optimal behaviour and what people actually choose in these games (Camerer & Thaler, 1995; Fehr & Fischbacher, 2003; Van Lange et al., 1997). A specific variant of this branch of experimental psychology, the public goods game, is the basis of the work in **Chapters 3-5,** and so these warrant a more detailed examination.

To take one example, the Dictator Game (DG) gives the subject an endowment, say $10, and they are asked to split this money between themselves and another player. There are many different variations of the game, but the canonical (and simplest) version is one in which the dictator's choice is binding, and each player simply receives the amount that the dictator decided to relinquish. Clearly then, the economically rational choice would be for the dictator to keep all the money for themselves and give none to the participant, but this is not how all humans behave. Across 616 different DG studies, giving zero is the most popular choice, with 36% of participants making this choice (Engel, 2011). However, the mean amount gifted to the other participant is 28%, and 5% of dictators give away the whole amount (Engel, 2011). These studies give clean and simple measures of prosociality which are easily operationalised. However, their simplicity can be seen as both a strength and a weakness in that they are somewhat far removed from typical everyday situations. Social dilemmas are extensions of these basic economic games, which involve interactions between two or more people.

The participants in social dilemmas typically must make a trade-off between their own welfare or reward and that of the collective. An example of this is the so-called Prisoner's Dilemma (PD), a two-player game where each player must decide whether to cooperate or defect without knowing the other person's choice (this was first framed as years in prison, hence the name). If the player cooperates and the other person defects, then the player receives a bad reward (ie. a long jail sentence) whilst their fellow player gets a good reward (ie. goes free), if on the other hand both players choose to cooperate, they both receive a medium reward (ie. a small sentence). These games become particularly interesting when they are played over multiple rounds, which also brings other elements like learning, reputation building, theory of mind and strategy. In particular, it is commonly observed that levels of cooperation (as a specific type of prosociality) decline over time in multi-round games (Bowles & Gintis, 2011; Ledyard, 1994).

Van Lange (2013) characterises social dilemmas as “situations in which a non-cooperative course of action is (at times) tempting for each individual in that it yields superior (often short-term) outcomes for self, and if all pursue this.. all are (often in the longer-term) worse off than if all had cooperated”. Many real-world scenarios can be abstracted into these kinds of game structures, for example, CO_2 emissions, arms races, and the destruction of rainforests have all been described in terms of social dilemmas (Tamura & Morita, 2024; Van Lange et al., 2013). Public goods games (PGGs) extend classic social dilemmas by introducing multi-player interdependence and group benefits or targets, making them a rich paradigm for studying collective action, prosocial behaviour and the dynamics of cooperation.

A typical PGG is constructed as follows. The player is given an initial endowment of monetary units (MU’s), which will be exchanged for real money at the end of the experiment, hence reflecting some degree of real-world consequence to the player's decisions. Over a series of turns or rounds (15-20 would represent a normal range), the player is instructed that on each round they can put some proportion of the endowment into a shared pot with other players that then will be multiplied by some factor (m) with the proceeds being shared equally amongst the total number of players (p), regardless of whether or how much they contributed to the shared pot. Provided $m < p$, then the optimal strategy for self-interest is to always put zero into the communal pot, whereas the optimal strategy for maximising group returns is to always put 100% into the pot. In reality, humans go somewhere in between, typically starting around 40-60% (Ledyard, 1994), although it declines substantially over time, a phenomenon often attributed to a decline in conditional cooperation (Fischbacher & Gächter, 2010) though this is questioned by some (Burton-Chellew et al., 2015).

1.2.3 Psychological traits

Traits are defined as “relatively enduring patterns of thoughts, feelings, and behaviors” that distinguish individuals from one another (Roberts, 2009). Several instruments are available for measuring prosocial tendencies. Typical examples are the Prosocial Personality Battery (Penner et al., 1995a), Social Value Orientation (Murphy et al., 2011), and the Prosocialness in Adults Scale (Luengo Kanacri et al., 2021). The PSB is a battery of 30 items which are a combination of direct self-report questions, e.g. “When I’m upset at someone, I usually try to ‘put myself in their shoes’ for a while” and responses to imaginary scenarios, e.g. “My decisions are usually based on what is the most fair and just way to act”. The SVO presents respondents with a series of monetary trade-offs to make between themselves and another person resulting in an assessment of whether they as predominantly altruistic (taking a personal loss for others gain), prosocial (cooperative and equitable maximisation of self and other rewards), individualistic (maximising self only) or competitive (taking a cost to self to reduce others reward). The PSA is similar to the PSB, but shorter (16 items) and contains items such as “I am pleased to help my friends/colleagues in their activities”. The PSA was preferred for the assessments made in my experimental work because it is shorter, but also because it was developed using item-response theory (IRT). IRT delivers theoretical improvements over classical test theory (CTT) on which the PSB is based, as each item is individually modelled to assess its value to the construct and estimates discrimination and threshold parameters, rather than the more basic score summing and consistency checks of CTT (Caprara et al., 2005).

These instruments are therefore prima facie measurements of prosociality. Assessing behaviours against traits is one way that a behaviour may be classified as prosocial, if it can be shown that individual differences in behaviour are correlated with that trait.

However, trait-based approaches to prosociality have important limitations. Classic work in personality psychology highlights the limited situational consistency of behaviour (Mischel, 1968) and the weak predictive power of global traits for single instances of action (Epstein, 1979). Furthermore, traits as constructs may represent largely descriptive summaries rather than mechanistic explanations of behaviour. These concerns are particularly acute for prosociality, which is highly context-dependent and sensitive to costs, norms, and beliefs about others (Penner et al., 2005). Self-report prosociality measures are also vulnerable to social desirability bias (Vesely & Klöckner, 2020), and show only modest convergence with behavioural and economic-game measures of prosocial behaviour (Böckler et al., 2016). There is no commonly agreed scientific standard for measuring prosociality, and each will represent or capture a particular facet. For example, estimates of the correlation between PSA and SVO are as low as 0.1-0.2 (Böckler et al., 2016). This, however is in contrast to previous findings across multiple studies that personality traits can reliably predict prosocial behavior (Balliet et al., 2009; Thielmann et al., 2020).

Collectively, this work suggests that prosociality traits may capture something of inter-individual differences in prosociality, but also elements of self-concept or reputational orientations, and hence relationships with behaviours should not be over-interpreted.

1.2.4 Multi-trial paradigms and computational modelling

In recent years, there has been a huge expansion of experimental tasks that use multiple trials and frameworks from non-social decision making e.g. effort discounting, learning, and foraging, to more precisely understand the mechanisms of prosocial behaviour (Lockwood et al., 2016, 2022; Mobbs et al., 2018). These have the advantage that the types of prosociality they measure are more closely related to the kind of problems the brain may have evolved to solve, such as locating food, but also crucially, invoking aspects of psychological function that are non-financial, such as learning or effort-based decision-making. Typically, whilst still lab-based, these experiments use more ecological decision-making paradigms and run over multiple (ie. up to 100 or more) trials, enabling the fitting of trial-by-trial computational models (Daw, 2011; Lockwood & Klein-Flügge, 2020). These methods are superior to simple correlational approaches as measurement over multiple trials increases power and can accommodate a variety of conditions, allowing for experimental manipulation to test causality. Allied with computational modelling, often alongside neuroimaging, researchers can test formal mathematical hypotheses of human decision-making and correlate this with neurobiological evidence of mechanisms (O'Doherty et al., 2007; Ruff & Fehr, 2014; Wilson & Collins, 2019). Such studies confirm that pro-self bias exists in domains such as learning and effort-based decision-making (Lockwood et al., 2022) but also reveal more subtle mechanisms, such as the moderating effects of empathy on learning (Lockwood et al., 2016). A particular strength of these studies is that they separate behavioural choices that are for self versus for another person, enabling a very clean quantification of prosociality.

Limitations of these approaches are that, whilst more ecological than classic economic games, they still represent a simplification and abstraction of real-world environments, so should not be interpreted as fully ecologically valid. For example, while cleanly separating self and other choices is methodologically elegant, the method still relies on an individual response devoid of social context e.g. social norms, reciprocity, or reputation. This risks measuring individual-level valuations of choices rather than genuinely interactive prosocial behaviour. Computational, multi-trial tasks often seek to measure latent parameters (e.g. learning rates, effort sensitivity, social discounting) that may be *interpreted* as prosociality-related, but which are not prosociality per se. For example, a "pro-self bias in learning" may reflect any number of other psychological factors, such as attentional differences, feedback salience, or risk sensitivity, rather than genuine motivational differences. Researcher topic interests (and potentially their biases) are coded into a model specification, which may be focused on, for example, learning or prosociality, and the model parameters will capture variance to the best extent possible, regardless of the ground truth of the model. Model specification, therefore, contains hidden degrees of researcher freedom and the concomitant risk that model parameters become incorrectly reified as psychological constructs (Palminteri et al., 2017). Relatedly, inconsistent results have been reported as the parameterisation of behavioural choices can be very sensitive to context and thus inconsistent across studies (Eckstein et al., 2022). The correlation of model parameters with neural activity, however, can mitigate many of the concerns regarding the capriciousness of modelling, grounding their results in biological reality.

Overall, these new tools provide powerful, testable and detailed accounts of how humans and other animals make other-benefitting decisions, and can be viewed as complementary to rather than replacements for broader behavioural and self-report approaches.

1.2.5 Game theory and Nash equilibria

Importantly, one of the key attributes of games such as PD and PGG is that they can be analysed and have solutions which represent optimal decisions against which human behaviour can be measured, following the foundational mathematical work on game theory of von Neumann (2007) and Nash (1951). Such concepts have been applied to psychological sciences in terms of understanding human behaviour, which is routinely biased and non-optimal (Camerer, 2003; Rabin, 2011). A key reference point in analysing closed-form (analytically tractable) games is the Nash equilibrium. This is defined as the point at which no player can improve their payoff by a unilateral change of strategy, if the strategies of the other players remain unchanged. The previously described economically rational choices in PD and PGGs are Nash equilibria for those games. The logic then proceeds that the difference between this objective point and actual behaviour is explained by some human bias, such as a psychological trait or preference. In classical experiments using economic games, the difference has been attributed to prosociality, either directly or because it has been labelled a prosocial behaviour and therefore results like this are then taken as a measure of prosociality. But is this the whole story? To establish this scientifically requires, firstly, an unambiguous definition of what is meant by prosociality, and also, crucially, the rejection of alternative hypotheses that might explain some or all the differences.

The first of these potential errors occurs from a confusion of definition when differences from optimality may be labelled as prosocial behaviour purely based on their outcome. For example, Koppel et al (2025) measured understanding of various economic games, including UG and PGGs and found that misunderstandings were common and in some cases up to 70%, clearly disrupting measurements of prosocial behaviour.

This may be justified by any purely consequentialist definition of prosociality because if one is agnostic about motivation, any behaviour which has positive outcomes for others can be legitimately classified as prosocial. However, as previously explained, because we are interested in the *psychological* aspects of prosociality, intention and motivation are fundamentally important, and consequentialist definitions are not suitable for our purposes. The second question recognises that the social preference hypothesis, namely that people make choices based at least in part by valuing benefits to others, is just one possible explanation for the difference between Nash points and actual behaviour.

One alternative explanation is that this could simply be mistakes (Andreoni, 1995; Bayer et al., 2013; Houser & Kurzban, 2002; Koppel et al., 2025), with people simply making poor choices due to misunderstandings about the game. Alternatively, it could also be that this type of prosociality is a bias which conditionally applies when mapping new action spaces – the ‘explore’ in explore/exploit dilemmas (Lloyd et al., 2025). The idea that people may make sub-optimal decisions (based on immediate rewards) when in a new environment is supported by the consistent observation that co-operation declines over time in iterated economic games, unless some form of punishment is present (Burton-Chellew et al., 2015; Gächter et al., 2008). Other evidence calls into question whether what might be classified as prosocial behaviour is at all prosocially motivated. One revealing study showed that levels of co-operation in a task were invariant to whether the participant was aware of a benefit to others in their choices i.e. the same ‘prosocial’ bias was observed in situations which had no prosocial outcome (Burton-Chellew & West, 2013) and that there was no correlation of levels of co-operation with self-reported prosocial preference. In fact, it seems that what might initially appear as sub-optimal, or even irrational, choices could be adaptive in real-world situations. For example, it has been shown that confirmation bias in learning tasks actually improves performance in noisy environments (Lefebvre et al., 2022). Similarly, the initial co-operative bias observed in PGGs could also be adaptive as a safe choice in new environments when important social reputations (Berman & Silver, 2022) are being first established with strangers. Others reviewing the use of social games in their entirety note that when looking at co-operation in an iterated Prisoners' Dilemma (PD) game, “relatively small changes in the outcome possibilities of a PD game sometimes can generate different behaviour patterns and larger changes can completely change the nature of the game” (Murnighan & Wang, 2016). Thus, a range of alternative explanations exists, other than the intention to help others, that may account for some, or all, deviations from mathematically optimal behaviour. Hence, the corpus of research on prosocial behaviour carries with it some fundamental questions about whether the difference between (any kind of) optimality and actual behaviour is a valid measure of prosociality. This issue is directly examined in **Chapter 3**. In addition to this quantitative challenge, we have the further complication of considering the different qualities of prosociality.

Whilst the Nash concept is a powerful and elegant way of analysing economic games, it is only analytically tractable for relatively simple game structures. When games become more complex, for example, when multiple players are involved over multiple rounds, the number of possible histories and actions becomes computationally demanding, and states (e.g. player recall or mental representation of other players' strategies) are unseen and unknowable, then using Nash equilibria is only possible by making simplifying assumptions. A different approach to solving such games is provided by simulating human choices many times over to find the optimal solution by trial-and-error, referred to as agent-based models.

1.2.6 Agent-based models and simulation

Agent-based models offer an alternative method for finding optimal equilibria in any kind of event-action space, even when environmental parameters are unknown, stochastic or wholly/partially observable. They operate by computer simulation of a set of discrete entities (agents) interacting with their environment according to a pre-specified set of rules. Agent-based modelling is a generalised framework or approach that is essentially based on the idea of simplifying and simulating any system based on its constituent units (Bonabeau, 2002). An example might be the modelling of a traffic flow whereby the agents are individual vehicles, and the environment is the road system. Each agent has a set of parameters, such as speed, speed variability, weight and so on, plus a set of rules to decide its next action based on observation of the state space e.g. distance to car in front, change in distance to car in front, and any other number of relevant variables that the modeller wishes to include. From this simple set of rules, one can model systems of dozens or hundreds of agents in real time to understand important elements of how the system functions and test specific hypotheses. The use and application of ABMs in social and biological sciences has increased dramatically over the past twenty years, in line with increased computational power combined with dedicated modelling software and accessible computer programming languages (Vincenot, 2018).

ABMs have been applied to prosocial behaviour (Bowles & Gintis, 2011; Crabtree et al., 2024; Izquierdo et al., 2014), including in the social foraging models that form the basis of **Chapters 3-5,** where they have been shown to successfully account for behaviours in the wild (Afshar & Giraldeau, 2014). One of the benefits of ABMs is that they reproduce emergent behaviours, whereby simple rules can produce complex outcomes (Bonabeau, 2002), which can sometimes explain counter-intuitive but empirically observed phenomena such as how costly punishment supports cooperation (Hauert et al., 2007).

However, the proliferation of ABMs and the creation of increasingly complex models have created several challenges. The ABM approach has been criticised for a lack of transparency. For example, models will often only produce interpretable results within a certain range of the model parameters, but the process of setting parameters and other aspects of ABM specification has not always been reported transparently. In **Chapter 3,** I will follow a recently-developed approach to tackling these problems, formalised by the Overview, Design concepts and Details protocol (Grimm et al., 2020).

1.2.7 Types of prosociality

Many types of prosociality have been identified over the decades of research into the topic, and the taxonomies I have identified yield 29 distinct sub-types of prosociality (Appendix 1). Jensen (2016) lists informing, comforting, sharing, helping, rescuing, adopting and teaching as prosociality sub-types and furthermore notes the difficulty of judging which is which based on experimental observation alone. Dunfield (2014) distinguishes helping, sharing and comforting by their observably different developmental onsets. The distinction is further highlighted by observing differences in neurodivergent individuals. For example, children with autism spectrum disorder generally display helping and sharing behaviours (Liebal et al., 2008), but are less likely to volunteer assistance to the appearance of distress in others (Hobson et al., 2009).

Differences in the characterisation of types of prosociality are robustly supported by experimental evidence. For example, prosociality is generally seen to increase with age, but a recent meta-study showed that this only holds for certain types of prosociality, and then only in cases of high socio-economic status (Li et al., 2024). Trust is sometimes used as a measure of prosociality, but it has been observed that trust has a negative correlation with household income, unlike other forms of prosociality, such as donating, volunteering and helping strangers, which show a positive correlation (Vanags et al., 2025).

A more systematic approach to deconstructing prosociality can be achieved using surveys combined with dimensionality reduction techniques such as factor analysis. This statistical technique is used to reduce a large set of variables to a smaller set of the most important, retaining only the most important ones and inferring an underlying structure as a result. In some domains, factor analysis has yielded single general factors such as general g in intelligence research (Deary, 2012), and the p-factor in psychopathology (Caspi et al., 2014). However, prosociality lacks a dominant latent core in the way intelligence and psychopathology do, raising questions about whether it constitutes a unitary psychological construct at all.

Psychometric analyses have shown six different prosociality subtypes: public, emotional, emergency, altruistic, anonymous, and compliance-obedience (Carlo & Randall, 2002). Subsequent replication studies (Carlo et al., 2003; Rodrigues et al., 2017) have revealed inconsistent relationships between the factors, even to the extent that some have been shown to be negatively correlated, suggesting that prosociality is not a coherent psychological construct (Reig-Aleixandre et al., 2023). In their comprehensive study of prosocial personality factors, Böckler et al correlated a variety of economic game behaviours with psychological self-reports to identify four distinct sub-types of prosociality: altruistically motivated prosocial behaviour, norm motivated prosocial behaviour, strategically motivated prosocial behaviour, and self-reported prosocial behaviour. Another study using multi-level factor analysis of global survey data identified a specific style of prosociality termed ‘ideological prosociality’ i.e. having liberal views on a range of topics, as a construct distinct from what is termed ‘inter-personal’ prosociality, defined as benefitting and being loyal to those around us (Nezlek, 2022). So the evidence for some kind of sub-type structure to prosociality seems strong, even if there is not yet a consensus.

It is also informative to consider evidence from cognitive neuroscience studies focusing on activation patterns of neural activity that are associated with prosocial behaviours. Whilst it is not possible to directly infer a particular function from an observation of neural activity in a certain location, the so-called problem of 'reverse inference' (Poldrack, 2006), it is clear that there is overlap between social and non-social processes, even if a level of social specificity is present (Lockwood et al., 2020).

For example, across diverse tasks, prosocial outcomes reliably engage canonical reward-valuation regions, consistent with the proposal that prosocial acts are processed via a shared value-computation mechanism rather than being prosocial-specific (Cutler & Campbell-Meiklejohn, 2019; Moll et al., 2006; Zaki & Mitchell, 2011). Though this position is nuanced with some processes, such as learning for self versus others, being dissociable (Lockwood et al., 2022) and whilst others may not be.

Evidence for the divergence of prosociality sub-types comes from those cognitive neuroscience studies which consistently show dissociated neural processes dealing with different aspects of prosociality. One set of researchers recognising the "diverse ways that prosociality is defined and the heterogeneity of prosocial decisions" employed a graph-based cluster analysis based on the experimental features of 43 fMRI studies on prosocial decision-making (Rhoads, Cutler, and Marsh 2020). This identified three distinct types of prosocial decision-making tasks defined as co-operative, equitable or altruistic. Analysis of the fMRI data showed distinct neural signatures for each of these types of tasks, clearly demonstrating that different neural processes are invoked when faced with these different types of prosocial decisions. Others have found neural activations based on Dunfield's previously referenced segmentation of helping, sharing and comforting (Wu and Hong 2022). This research combined evidence from rodent as well as human species, the former allowing for more precision from direct recording or optogenetic manipulation of neuronal cells. The granularity available from the rodent data in particular, highlights different neural circuits that are causally responsible for comforting and helping behaviours. Others have identified clear neural responses representing 'pure' altruism allied to psychological self-report and financial giving behaviour (Hubbard et al., 2016), though interestingly, these findings were not consistent for non-monetary giving, suggesting the type of behaviour is also important (Best & Freund, 2021).

Critical to the analysis of prosociality is the notion of intention. If another person benefits from an action that wasn't intended to benefit them, is this a prosocial action? If someone intends to benefit another person, but ends up harming them, is that a prosocial action? This point can be argued theoretically, but there is plenty of evidence that motivations can be functionally differentiated in the brain based on their intention (Paulus 2018; Saulin et al. 2021). Neural evidence shows that an identical prosocial action may be invoked by distinct motivations such as altruism or reciprocity, using distinct neural mechanisms, but resulting in behaviourally identical responses (Hein et al. 2016). This evidence is striking in that it shows that identical behaviours may mask differences at a neural level which reflect the type of prosocial condition being experienced.

Thus, the evidence from behavioural, attitudinal and neural sources strongly supports the view that prosociality is not a coherent, singular psychological construct. One way to deal with this issue is to use multiple measures to test their convergent validity (Cronbach & Meehl, 1955), which is the approach I have taken in **Chapter 2** when analysing global survey data. My conclusions from the data of the producer-scrounger studies in **Chapters 3-5** instead rely on the understanding that different types of prosociality may be present in the same task. Both approaches take account of the multi-faceted nature of prosociality and thus demonstrate how this framework can be practically applied in different experimental contexts.

3 Thesis aims and structure

Broadly, this thesis is in three parts. The first part of this thesis investigates how prosocial tendencies vary at the global level, using large-scale survey data to test how financial status predicts differences in self-reported prosociality. This question is addressed in **Chapter 2**, which presents a published cross-national analysis (Vanags et al., 2025). That study established a macro-level pattern that those with higher incomes or higher subjective financial well-being tend to report higher levels of prosociality, crucially using a range of different measures to support the generalisability of the claim.

This result is supplemented by the finding that these effects are moderated by precarity, defined as lack of reliable access to food and shelter and more fully described in the introduction to **Chapter 2**. This work makes a contribution to the weight of evidence in support of a positive relationship, in an area that has previously been the subject of a great deal of debate (Balakrishnan et al., 2017; Clerke et al., 2018; Guinote et al., 2015; Korndörfer et al., 2015; Piff et al., 2010, 2012; Stamos et al., 2020). It also provides a real-world context for the experimental work that follows.

The second section, contained within **Chapters 3-5,** covers a new experimental paradigm developed for this thesis. Here, I test the extent to which prosociality can be found in a lab-based group social foraging task. The non-linear returns of this PGG variant provide a Nash point against which to compare human behaviour, adding to the relatively scarce literature on human producer-scrounger games and providing insight into prosocial behaviours in this context. **Chapter 3** explains the development and structure of the task, including results from two large online samples. In this analysis, I look at the question using various benchmarks of prosociality, investigate the influence of traits and use linear mixed models to uncover insights about behaviour from trial-by-trial data.

I then examine if prosociality was affected by carrying out the task in person, even if they were not explicitly part of the same group. Many studies show that small changes in experimental conditions, social cues in particular, can have significant effects on prosocial behaviours (Andreoni & Bernheim, 2009; Zizzo, 2010). This is the basis of **Chapter 4**.

Having established in **Chapter 2** that those with higher self-reported incomes and higher levels of subjective financial well-being were more likely to report prosocial attitudes and behaviours, the final experimental chapter, **Chapter 5**, takes this insight and attempts to reproduce this effect through a between-subjects manipulation of rich and poor status.

This work adds to the prosociality literature because research on prosocial behaviour in groups is relatively sparse (Penner et al., 2005), probably because it is more complex and more expensive than studying individuals or dyads (Tamura & Morita, 2024). The specific paradigm is drawn from models of social foraging theory, and whilst there is an extensive body of literature on the application of such games in non-human animals, very little has been done with humans directly. This series of experiments provides important insights into how prosociality manifests in such contexts.

The third and final segment of the thesis is conceptual and draws on overarching considerations across both sets of empirical results. These concern how prosociality is conceptualised and measured in humans. Such questions are somewhat philosophical by their nature, but are fundamentally important scientifically. The scaffolding for these ideas is contained in this introductory chapter, and their discussion in light of the empirical work is covered in the general discussion.

Chapter 2
Global associations of prosociality and wealth

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Paul Vanags, Jo Cutler, Fabian Kosse, Patricia L Lockwood, Greater income and financial well-being are associated with higher prosocial preferences and behaviors across 76 countries, *PNAS Nexus*, Volume 4, Issue 2, February 2025, pgae582, https://doi.org/10.1093/pnasnexus/pgae582.

Author contributions were as follows: P.V., J.C., F.K., and P.L.L. were involved in conceptualization and writing—review and editing. P.V., J.C., and P.L.L. were involved in methodology. P.V. and J.C. performed formal analysis. P.V. was involved in writing—original draft. P.V., F.K., and P.L.L. were involved in funding acquisition. J.C. and P.L.L. were involved in supervision.

This chapter addresses a significant gap in the existing literature on wealth and prosociality. Although prior work has established a broadly positive association between income and prosocial behaviour, these studies have predominantly used WEIRD samples, considered single measures of both wealth and prosociality, and have not distinguished between objective income and subjective financial well-being. The relationship between subjective financial well-being and prosociality specifically had not been examined, and the potential moderating role of precarity — the inability to meet basic needs — remained untested. The chapter contributes a pre-registered analysis of 80,337 participants across 76 countries, testing associations between two distinct measures of wealth and seven measures of prosocial preferences and behaviours, with moderation analyses for precarity and country-level factors.

1 Abstract

Prosocial preferences and behaviours – defined as those that benefit others – are essential for health, well-being, and a society that can effectively respond to global challenges. Identifying factors that may increase or decrease them is therefore critical. Wealth, in the form of income or subjective financial well-being, may be crucial in determining prosociality. In addition, individuals' experience of precarity (inability to meet basic needs), or country-specific factors could change how wealth correlates with prosociality, yet this impact is unknown. Here, we tested how self-reported household income and financial well-being were associated with seven measures of prosociality in a global, representative sample of 80,337 people across 76 countries. We show a consistent positive association between wealth and prosociality, across both measures and for both financial and non-financial prosocial preferences and behaviours. Household income was positively associated with altruism, positive reciprocity, donating money, volunteering, and helping a stranger, but negatively associated with trust. Financial well-being was positively associated with all aspects of prosociality, including trust. Individuals' experience of precarity reduced the strength of wealth associations for prosocial preferences but increased them for prosocial behaviours. Positive associations between wealth and prosociality were found around the world and across country-level wealth and cultural factors. These findings could have important implications for enhancing prosociality, critical for a healthy and adaptive society.

2 Introduction

One critical factor in determining prosociality is wealth, both how much money someone earns through income and how they subjectively feel about their financial situation. However, previous research tends to consider single aspects of wealth and single measures of prosociality. In addition, existing work has often focused on testing people who are predominantly from western, industrialised, rich, educated, and democratic countries (Henrich et al., 2010). It is critical that our understanding of preferences and behaviours is representative around the world, particularly for financial factors that vary dramatically outside the countries usually studied (Arnett, 2008). Here we examined how two aspects of wealth: self-reported household income and financial well-being, are associated with seven survey assessments of prosocial preferences and behaviours in the global population. Whilst these measures of wealth are correlated, they do not overlap completely and may show different associations with prosocial behaviour.

To date, the majority of studies that measure the association between income and prosociality find a positive relationship (Andreoni et al., 2021; Bekkers & Wiepking, 2011;

Kirkpatrick et al., 2015; Kosse & Tincani, 2020; Nettle et al., 2011; Zwirner & Raihani, 2020). Country level incomes (GDP) have also been shown to correlate with real-world prosocial behaviours such as returning lost wallets (Cohn et al., 2019). However, one study showed that lower family incomes were associated with a belief that people should donate more (Piff et al., 2010) and there is some evidence for a 'U'-shaped profile to charitable giving with lowest and highest income households donating more (James & Sharpe, 2007). As well as previous data suggesting the importance of associations between prosociality and wealth, theoretical accounts point to particular hypotheses. For example, models of inequity aversion imply a positive association between wealth and prosociality via mechanisms of income redistribution (Epper et al., 2020; Fehr & Schmidt, 1999).

Few, if any, studies have examined the association between subjective financial well-being and prosociality specifically, as these are typically bound up in measures of subjective socio-economic status (SES). SES is a broad concept generally measured as a composite of income, educational attainment and occupational status, with inconsistency between studies (Stamos et al., 2020). Several studies have suggested that, in contrast, higher subjective SES is associated with lower levels of prosocial behaviour (Amir et al., 2018; Callan et al., 2017; Cutler et al., 2021; Elbaek et al., 2021b; Piff et al., 2010, 2012). However, others have questioned these results (Balakrishnan et al., 2017; Clerke et al., 2018; Francis, 2012; Korndörfer et al., 2015; Piff & Robinson, 2017; Stamos et al., 2020).

In addition to the variety of measures used, a possible explanation for these mixed findings is that the association between wealth and prosociality is moderated by additional factors. One such factor is the experience of precarity, defined here as an inability to meet basic needs such as food and shelter. Research suggests important psychological and behavioural consequences of precarity (Ahl et al., 2024; Banerjee & Duflo, 2007; Shah et al., 2012). Theoretically, precarity may reduce prosocial behaviour (Lazarus, 2017). However, a meta-review examining precarity and prosocial behaviour showed physiological scarcity (hunger/thirst) increased prosocial behaviours, whereas financial scarcity did not (Elbaek et al., 2021a). Another study showed that precarity in childhood was associated with lower volunteering (Lettinga et al., 2020). Experience of precarity may also moderate how wealth is associated with prosociality. Precarity could increase the desire to help, due to mechanisms of empathy and shared experience, such as the empathy-altruism hypothesis (Batson, 2010). Alternatively, the association with wealth may be smaller or not evident within people who have experienced precarity if this limits their ability to help, regardless of income or financial well-being.

Here, we conducted a pre-registered (As Predicted #100,462) analysis of data from 80,337 people from 76 countries, representing 90% of the global population. We quantified the correlations of income (household income in international dollars, adjusted to each country) and financial well-being with seven measures of self-reported prosociality. Four of these measured prosocial preferences (positive reciprocity, altruism, trust, and negative reciprocity) and three captured real-world behaviours (donating money, volunteering, and helping a stranger). Crucially, these also covered financial measures, those that referenced monetary giving, and non-financial measures. We controlled for several factors that could covary with wealth or prosociality, including gender, age, cognitive ability, and physical health. We also tested the moderation of precarity, indexed by self-reported experience of inability to provide food/shelter in the previous year. Finally, we considered consistency across countries and whether country-level Gross National Income (GNI) and cultural factors moderated wealth-prosociality associations.

Based on previous empirical findings, our pre-registered hypotheses were that higher income would be associated with greater levels of prosociality (Andreoni et al., 2021; Bekkers & Wiepking, 2011; Kirkpatrick et al., 2015; Kosse & Tincani, 2020; Nettle et al., 2011; Zwirner & Raihani, 2020) whereas financial well-being would show the opposite (Amir et al., 2018; Callan et al., 2017; Cutler et al., 2021; Elbaek et al., 2021b; Piff et al., 2010, 2012). We additionally tested whether correlations varied based on the type of prosocial preference or behaviour. Specifically, we predicted that all forms of prosociality would have a positive relationship with income, but the effect size would be greater for financial measures that involved monetary amounts (donations, altruism, positive reciprocity) than non-financial forms of prosociality (volunteering, helping, trust, negative reciprocity). As preregistered, our hypotheses were as follows:

Q1a: Do income/ financial well-being predict individual prosocial preferences (Altruism, positive reciprocity, trust, negative reciprocity) at a global level?

Q1b: Do income / financial well-being predict prosocial behaviours (donated money, volunteered time, and helped a stranger) at a global level?
Q1c: Does precarity moderate any of the effects found in 1a and 1b?

We also examined country-level moderators of the association between income / financial well-being and prosocial behaviour.

Q2a: Do income / financial well-being effects on prosocial preferences depend on country-level economic and social resources? If so, is the case for all types of preference?
Q2b: Do income / financial well-being effects on prosocial behaviours depend on country-level economic and social resources? If so, is the case for all types of behaviour?

3 Method

The main measures were taken from the Global Preferences Survey (GPS) and Gallup World Poll (GWP). The GWP is a globally representative dataset covering 80,337 individuals, drawn as representative samples from 76 countries, representing 90 percent of both the world's population and global income (Gallup, Inc, n.d.). Individual countries are sampled with 1000 respondents per country through a combination of face-to-face and telephone data collection. The GPS adds specific questions regarding economic preferences and behaviours (Falk et al., 2023) to the GWP. Two further data sources were used for the analyses of economic and social moderators. Gross National Income (GNI) data was taken from the World Bank Open Data (*Data Catalog*, n.d.). The measure of importance of family ties was taken from the World Values Survey (*World Values Survey Association*, n.d.). Analysis of these secondary datasets was approved by the University of Birmingham Science, Technology, Engineering and Mathematics (STEM) ethics committee (ERN_20-1897PA) and details of the consent procedures is provided in the original manuscripts for each dataset.

2.3.1 Wealth measures

We used self-reported household income (HHI), adjusted to achieve purchasing power parity to be comparable across different countries (Gallup Inc, n.d.) and log-transformed (Appendix 3). Financial Well-being (FWB) is a 4-item index from the GWP capturing participants' feelings about their economic situation (Appendix 3).

2.3.2 Prosociality measures

The GPS measures four prosocial *preferences*: positive reciprocity, altruism, trust, and negative reciprocity, developed through independent experimental validation (Falk et al., 2023) (Appendix 3). The GPS defines positive reciprocity as the propensity to return a prosocial act (2 items), altruism as willingness to give to good causes without expecting anything in return (2 items), trust as assuming people have good intentions (1 item), and negative reciprocity as the tendency to punish unfair behaviour (3 items; Appendix 3). The GWP measures three prosocial *behaviours*; donating money, volunteering, and helping a stranger. Participants reporting whether they have done each in the past month, creating binary measures. Some missing data was imputed in the original GPS dataset following the procedures in Appendix AH from the Falk et al supplementary materials 'Online Appendix': https://doi.org/10.1093/qje/qjy013.

2.3.3 Moderators

Individual-level precarity was quantified using the GWP Food and Shelter Index. This measure was only weakly (Rea, L. M., & Parker, R. A., 1992) correlated with financial well-being (Cramers $V_{(18,\ N=66,654)}$=0.156), suggesting that they could be evaluated independently. Furthermore, 29% of those who reported experiencing precarity were high in financial well-being confirming that each financial well-being category contained people who experienced precarity. Country-level moderators were Gross National Income (GNI) from the World Bank Open Data (Data Catalog, n.d.), Family Ties from the World Values Survey (World Values Survey Association, n.d.), and Individualism-Collectivism (Minkov et al., 2017) (Appendix 3).

2.3.4 Analysis

All analyses were run using R (v4.1.0) in R Studio (v2022.12.0). As pre-registered, we tested income and financial well-being as explanatory variables in (generalised) mixed-effect models (G)LMMs of each measure of prosociality. Control variables, used in all models, were gender, health (GWP Physical Well-being Index), age, and cognitive ability (GWP self-reported maths ability). Income, financial well-being and control variables were entered as fixed and random effects grouped by country with a country-level intercept. Random effects of gender caused model convergence issues and were removed. All continuous variables were standardised and mean centred. Significance is reported at $p<0.01$ for analyses using the full sample.

To assess consistency around the world, we calculated wealth associations in each country with all the controls available for that country. We also calculated (i) the proportion of countries that had the same directional association as globally, regardless of significance and (ii) the proportion of significant associations ($p<0.05$) in each direction and expressed these as a proportion of the total number of countries. A one-tailed binomial proportion significance test assessed whether the proportions were significantly above 0.5. Analysis of individual and country-level moderators repeated each wealth-prosocial model, adding the moderating variable as a direct association and interaction with wealth. For precarity, a four-level factorial variable, interactions were assessed with an omnibus ANOVA.

4 Results

We analysed the link between wealth and prosociality in two linked, globally representative datasets, the Global Preferences Survey (GPS) and the Gallup World Poll (GWP). These cover 80,337 individuals, representative samples from 76 countries around the world. We fit (generalised) linear mixed-effects models (LMMs) for each combination of wealth measure and prosociality variable (see Methods). Each model contained a wealth measure and controls for gender, age, cognitive ability, and physical health. As in previous studies (Kosse & Tincani, 2020), household income was log-transformed, and our analysis focused on linear associations (see Table S1 for quadratic models). We report significance at $p<.01$ in all analyses using the whole dataset to (note this deviated from our pre-registered $p<.05$ to increase robustness, given the large sample).

2.4.1 Greater income and financial well-being are associated with increased prosocial preferences and behaviours all over the world

Income was positively associated with altruism (standardised β [95% confidence interval (CI)]=0.097 [0.077 0.117], $p<.001$), negative reciprocity (β=0.051 [0.027 0.075], $p<.001$), and positive reciprocity (β=0.112 [0.089 0.136], $p<.001$ (Figure 2.5 A-D, Table S2). In other words, people with higher incomes were more likely to claim giving money to good causes, and report returning prosocial behaviour towards them, whilst punishing unfair behaviour. However, contrary to our hypothesis, a negative association between income and trust showed those with higher incomes reported lower trust in others (β=-0.027 [-0.046 -0.008], $p=.005$). Income was also positively associated with all measures of reported prosocial behaviour: donating (odds ratio (OR)=1.562 [1.443 1.690], $p<.001$), volunteering (OR=1.157 [1.099 1.218], $p<.001$) and helping a stranger (OR=1.271 [1.210 1.336], $p<.001$; Figure 2.5 A-D, Table S2). Therefore, greater income was associated with increased prosociality across a broad range of preferences and behaviours, except for trust.

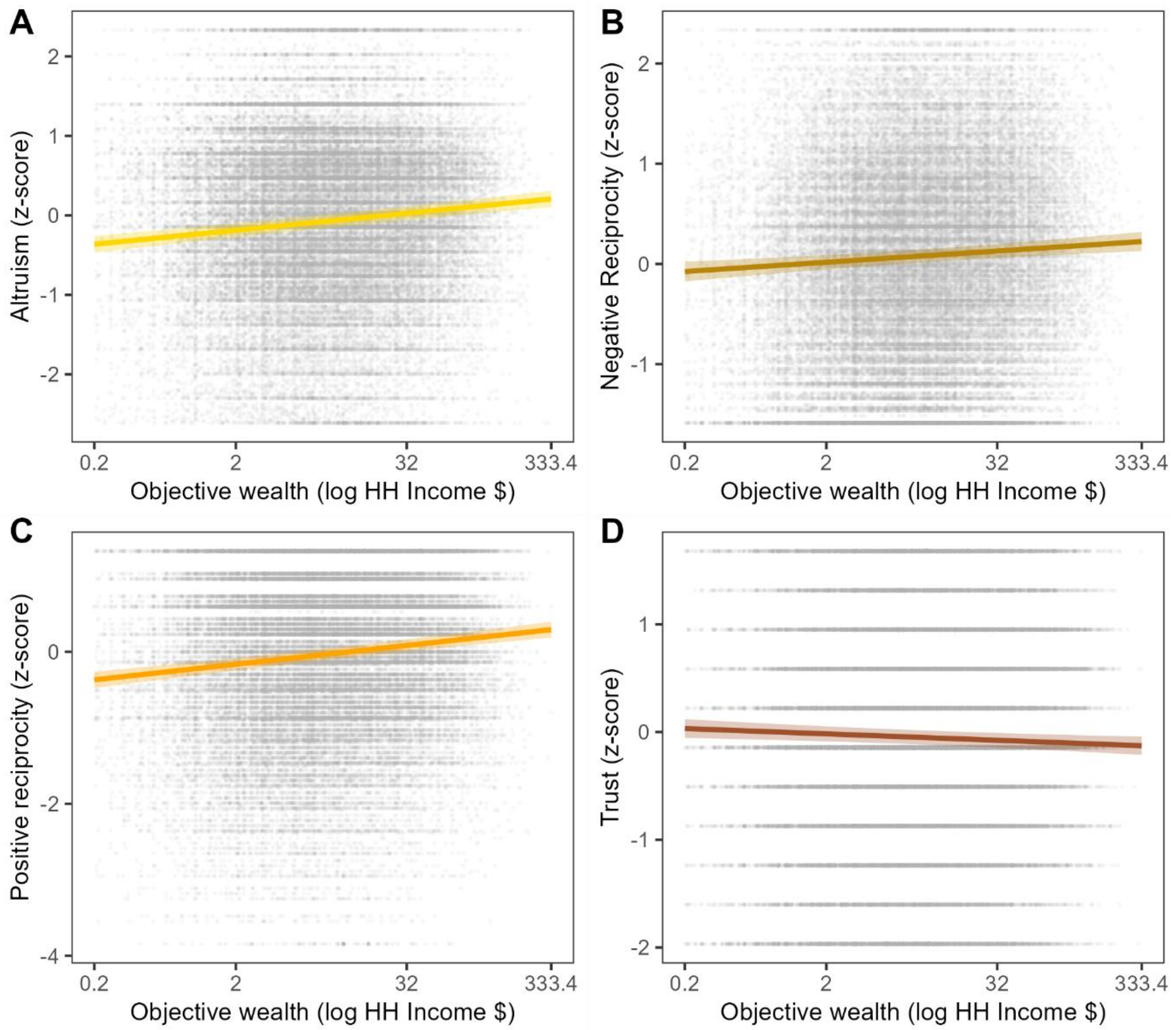

Figure 2.1 | Results of linear mixed models for prosocial preferences and objective wealth
Objective wealth showed positive associations with **(A)** altruism**, (B)** negative reciprocity and **(C)** positive reciprocity, and a negative association with **(D)** trust. Plots show predictions from linear mixed-effect models (LMMs) of prosocial preferences / behaviours, controlling for gender, age, physical health and cognitive ability. Preferences were modelled as standardised continuous variables (see Methods). Linear effects are shown here with the quadratic effects reported in supplementary materials. Plots were created by predicting response data from model fits, the shaded area representing 95% confidence interval. Plots show individual data points for each respondent, with Trust measured on a Likert scale.

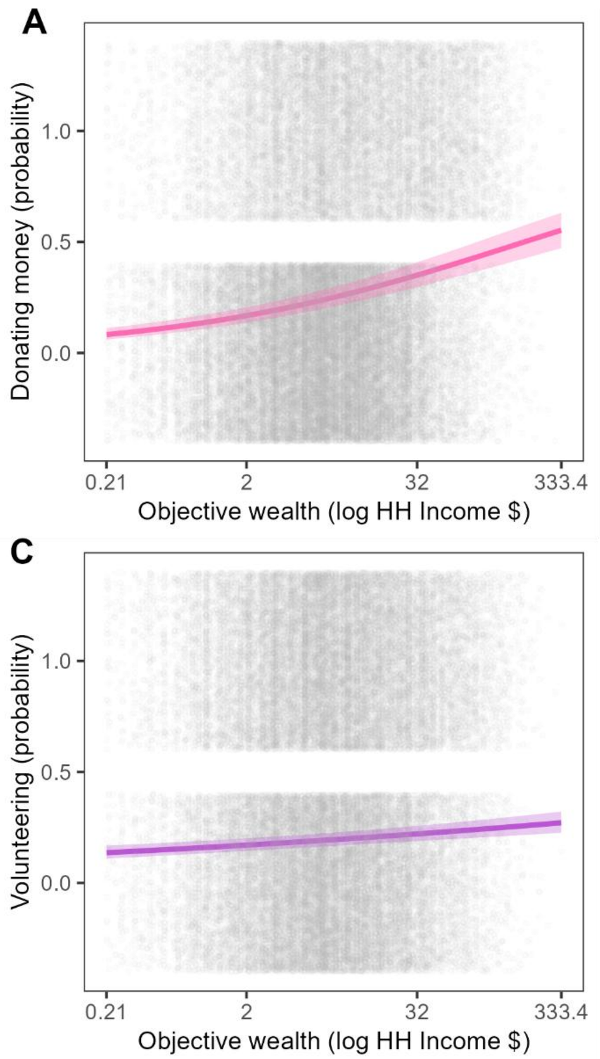

Figure 2.2 | Results of linear mixed models for prosocial behaviours and objective wealth
Binary behavioural responses (0 for a negative response, 1 for positive) were modelled with generalised LMMs (see Methods). Plots show individual data points jittered vertically around 0 or 1 for each respondent. Objective wealth showed positive associations with **(A)** donating**, (B)** helping a stranger and **(C)** volunteering.

To examine whether the negative association between income and trust was related to inclusion of country-level random effects, not present in previous studies, we re-ran our analysis using a simple linear regression model without random effects. Results showed a positive association between income and trust (β=0.038 [0.029 0.046], *p*<.001) when country-level influences were not accounted for. This suggests that failing to account for differences between countries in average levels of trust and how wealth is associated with trust can obscure or reverse the negative relationship between wealth and trust at an individual level.

Next, we examined financial well-being. Contrary to our pre-registered hypotheses, financial well-being was positively associated with increased prosociality on all measures. Individuals who subjectively reported greater financial well-being were more prosocial, for both preferences (altruism: $\beta=0.085$ [0.072 0.098], $p<.001$; trust: $\beta=0.033$ [0.018 0.049], $p<.001$; negative reciprocity: $\beta=0.025$ [0.009 0.041], $p=.002$; positive reciprocity: $\beta=0.065$ [0.050 0.081], $p<.001$) and behaviours (donating: OR=1.384 [1.334 1.436], $p<.001$; volunteering: OR=1.221[1.177 1.267], $p<.001$; helping a stranger: OR=1.200 [1.153 1.250], $p<.001$). Therefore, both higher incomes and financial well-being were associated with increased prosocial behaviours and most preferences. Interestingly, income and financial well-being showed opposing associations with trust. People with higher incomes reported being less willing to trust others whereas those who reported higher financial well-being showed greater levels of trust.

2.4.2 Wealth has stronger positive associations with financial prosociality than non-financial measures

Having observed significant wealth associations with all measures of prosocial preferences and behaviours, we next compared the size of these associations between measures. We hypothesised that wealth would be more strongly associated with financial measures involving monetary amounts than non-financial prosociality. We compared the absolute size of regression coefficients within each set of models using z-scores based on pooled variance (Paternoster et al., 1998). The association with income was significantly larger for the financial preferences altruism (A) and positive reciprocity (P) than non-financial trust (T) and negative reciprocity (N) ($Z_{A\text{-}T}=6.89$, $p<.001$; $Z_{P\text{-}T}=6.76$, $p<.001$; $Z_{A\text{-}N}=4.07$, $p<.001$; $Z_{P\text{-}N}=4.53$, $p<.001$; Table S3 for all comparisons). Similarly, financial well-being had significantly larger correlations with altruism and positive reciprocity compared to negative reciprocity and trust ($Z_{A\text{-}T}=6.90$, $p<.001$; $Z_{P\text{-}T}=4.93$, $p<.001$; $Z_{A\text{-}N}=8.26$, $p<.001$; $Z_{P\text{-}N}=6.48$, $p<.001$).

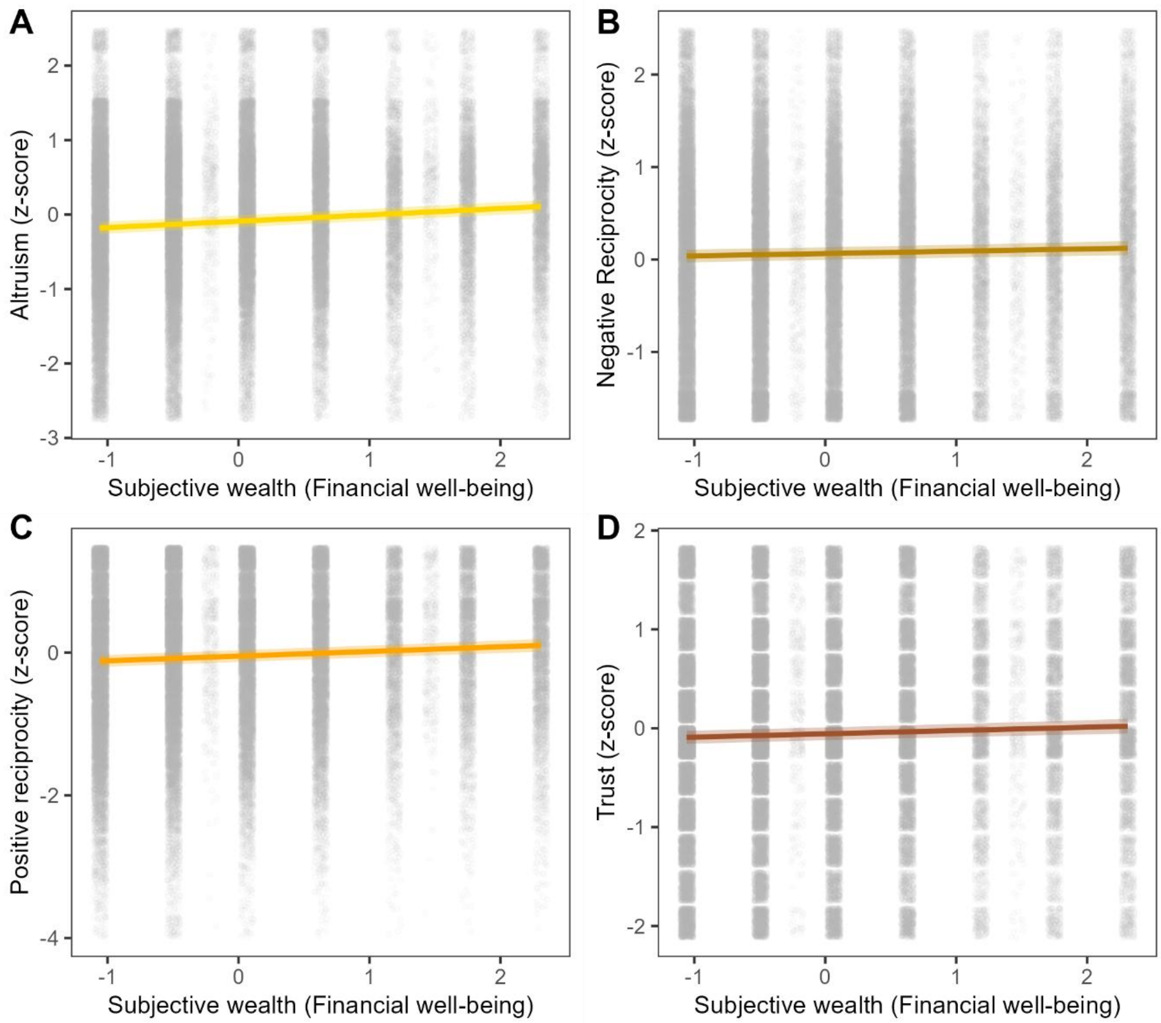

Figure 2.3 | Results of linear mixed models for prosocial preferences and subjective wealth
Plots show individual data points for each respondent jittered horizontally. Subjective wealth was calculated using responses from four questions scoring gave a 7-iterm numeric categorical scale (see Supplementary Methods). Individuals with incomplete questionnaire responses yielded slightly different outcome scores accounting for the lighter interstitial bars. Objective wealth showed positive associations with **(A)** altruism**, (B)** negative reciprocity, **(C)** positive reciprocity, and **(D)** trust.

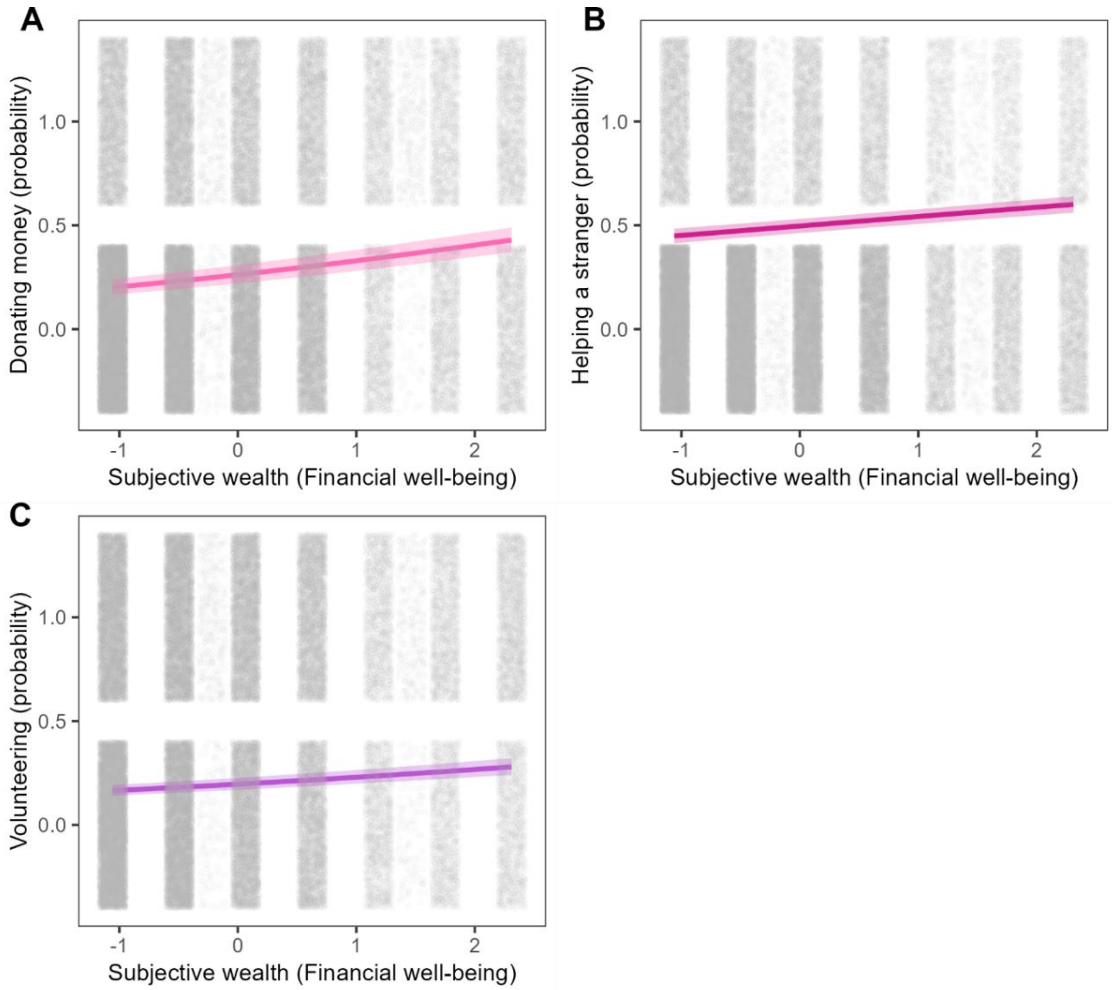

Figure 2.4 | Results of linear mixed models for prosocial behaviours and subjective wealth
Binary behavioural responses (0 for a negative response, 1 for positive) were modelled with generalised LMMs (see Methods). Plots show individual data points jittered vertically around 0 or 1 for each respondent. Subjective wealth was calculated using responses from four questions scoring gave a 7-iterm numeric categorical scale (see Supplementary Methods). Objective wealth showed positive associations with **(A)** donating**, (B)** helping a stranger and **(C)** volunteering.

For the associations of wealth on the (binary) prosocial behaviours we compared financial donating to non-financial helping and volunteering, again using z-scores capturing the difference between odds ratios. As hypothesized, income had a larger correlation with donating than helping and volunteering ($Z_{D\text{-}V}$=3.24, p<.001; $Z_{D\text{-}H}$=2.11, p=.017). Financial well-being also showed the largest correlation with donating, compared to helping and volunteering ($Z_{D\text{-}H}$=2.75, p=.003; $Z_{D\text{-}V}$=2.38, p=.009). To summarise, all measures displayed significant associations with income and financial well-being, and as predicted, the most positive associations of wealth were on financial measures, altruism, positive reciprocity and donating.

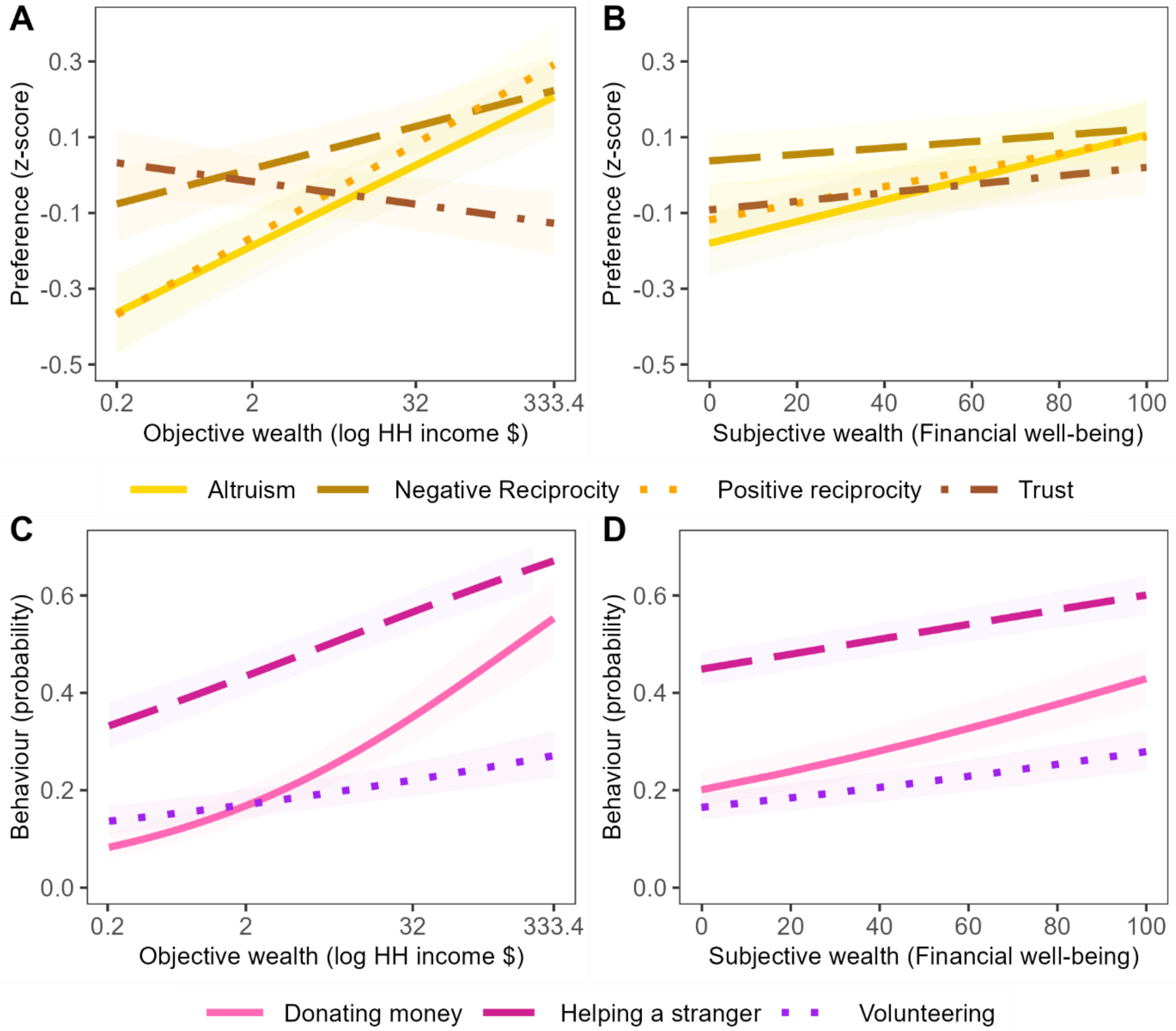

Figure 2.5 | Greater wealth is associated with higher prosociality around the world(A) Income showed positive associations with altruism, positive reciprocity and negative reciprocity, and a negative association with trust, **(B)** Financial well-being was positively associated with all preferences, **(C)** Income was positively associated with all behaviours with donating being particularly strong, **(D)** Financial well-being was positively associated with all behaviours. Plots show predictions from linear mixed-effect models (LMMs) of prosocial preferences / behaviours, controlling for gender, age, physical health and cognitive ability. Preferences were modelled as standardised continuous variables and binary behaviours were modelled with generalised LMMs (see Methods). Linear models are shown here with the quadratic models reported in Appendix *2*. Plots were created by predicting response data from model fits, the shaded area representing 95% confidence interval.

2.4.3 Precarity moderates associations between wealth and prosociality

Next, we examined how precarity moderated the significant associations between wealth and prosociality. Here, we assessed precarity as inability to access food and shelter in the past 12 months (GWP Food & Shelter Index, Appendix 2). Summing participants' binary responses to each question (food, shelter) created four levels (no precarity, food precarity, shelter precarity, or both). Precarity is distinct from financial well-being as precarity questions did not ask how satisfied participants were with their situation, only whether they could meet these basic needs, and the two measures were only weakly correlated (see Methods).

We found that the experience of precarity reduced the positive association between income and altruism (X^2=15.1, p=.002) and the positive association between financial well-being and positive reciprocity (X^2=31.5, p<.001). Those that had experienced precarity had a less pronounced increase in these prosocial preferences with wealth than those that hadn't (Figure 2.6 A-B; Table S4). Interestingly, the moderation on behaviours acted in the opposite direction (Figure 2.6 C-F). Precarity increased the positive associations of financial well-being on donating and volunteering (X^2=19.0, p<.001; X^2=15.4, p=.002), and the positive associations of both income and financial well-being on helping a stranger (X^2=21.2, p<.001; X^2=11.8, p=.008).

It is also worth noting that precarity showed significant main effects on prosociality (Table S5). These associations with precarity differed based on whether the type of prosociality was financial or non-financial. Interestingly, across both subjective and income, the experience of precarity increased prosociality for the non-financial measures of trust, negative reciprocity, volunteering and helping a stranger. For the financial measures: altruism, donating money, and positive reciprocity, experiencing precarity was associated with less prosociality.

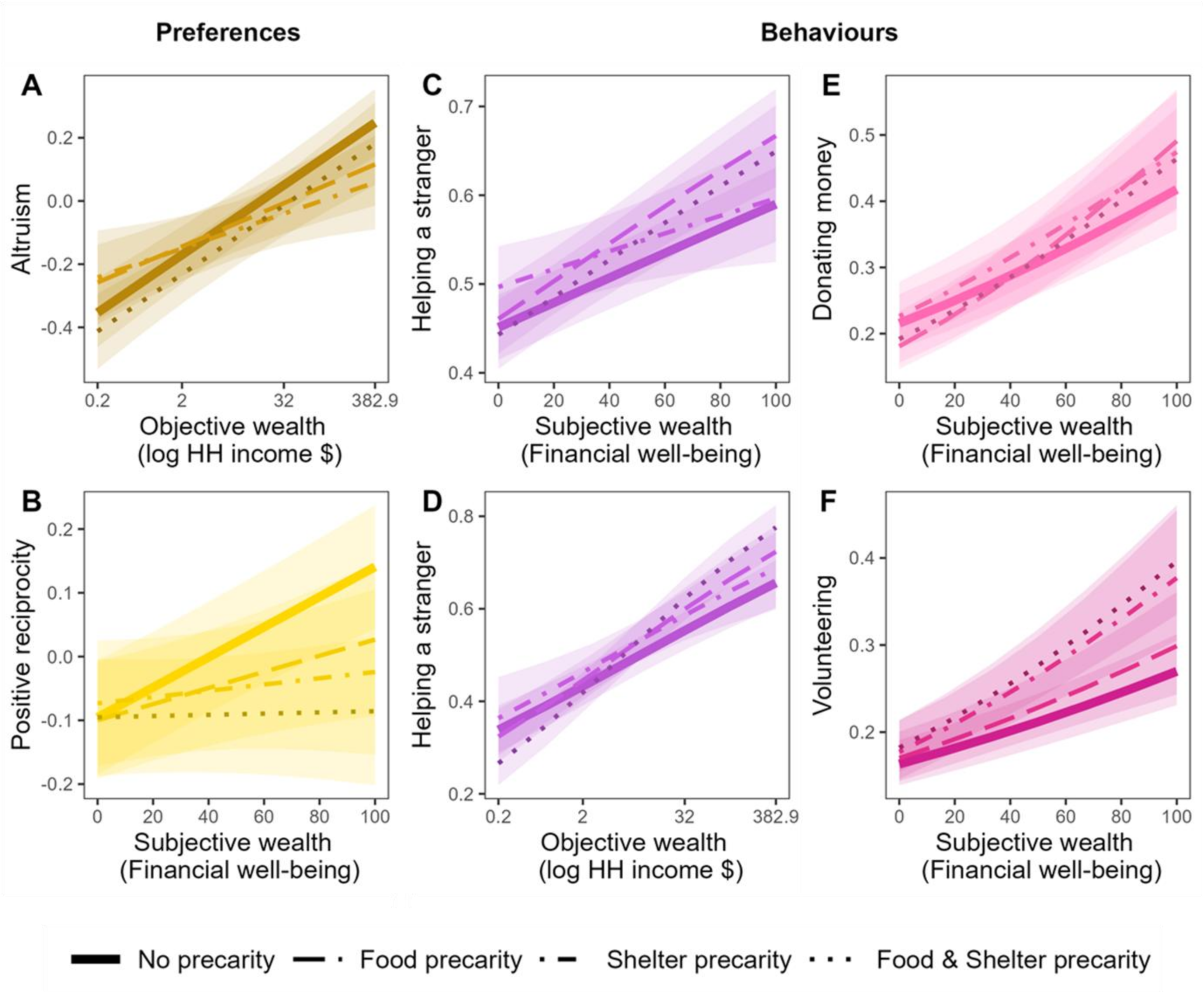

Figure 2.6 | Experience of precarity reduced associations between wealth and prosocial preferences but enhanced associations with prosocial behaviours
Models tested an interaction effect between wealth and a four-level precarity factor (no precarity, food precarity, shelter precarity, and both), with the significant interactions plotted above. The two significant interactions for preferences **(A)** income-altruism, and **(B)** Financial well-being and positive reciprocity, were both negative, meaning experience of precarity decreased the strength of the wealth-prosociality association. **(C)** In contrast, the positive interaction between precarity and the income - helping a stranger relationship meant the strongest association with wealth was for those with experience of precarity. All three interactions between financial well-being and precarity with **(D)** helping a stranger **(E)** donating and **(F)** volunteering were also positive.

2.4.4 Associations between wealth and prosociality are largely consistent across countries

Having demonstrated that increased wealth is associated with increased prosociality, we next examined consistency of associations across countries. Previous studies have generally focused on 'WEIRD' (Western, Educated, Industrialized, Rich and Democratic) samples (Amir et al., 2018; Côté et al., 2015; Henrich et al., 2010; Holland et al., 2012; James & Sharpe, 2007; Matsuba et al., 2007; Schmukle et al., 2019), and therefore it is unclear whether associations between wealth and prosociality vary around the world. We fitted (G)LMs with fixed effects of wealth and the control variables, to the data for each country separately (see Methods) and extracted the standardised effect sizes, plotted on global maps, regardless of significance (Figure 2.7 - Figure 2.10). We also calculated two simple statistics, the proportion of countries matching the direction of the global wealth correlation (positive or negative), and the proportion of countries that showed statistically significant effects (p<.05), either with or contrary to the global pattern (Table S6). As a measure of global consistency, we ran two separate one-tailed binomial proportion tests (H0: proportion=0.5): one testing whether the proportion of countries showing a positive wealth association was significantly greater than 0.5, and one testing whether the proportion showing a negative association was significantly greater than 0.5.

Associations with both income and financial well-being were highly consistent around the world. A positive correlation with wealth was found in the majority of countries for positive reciprocity (income=91%, p<.001; FWB=82%, p<.001), altruism (income=88%, p<.001; FWB=94%, p<.001), negative reciprocity (income=70%, p<.001; FWB=65%, p=.01), donating money (income=91%, p< .001; FWB=100%, p<.001), volunteering (income=79%, p< .001; FWB=97%, p<.001), and helping a stranger (income=84%, p< .001; FWB=83%, p<.001). Interestingly trust showed a more even split with 57% of countries showing a negative correlation of income (p=.200), as in the overall model, but 43% of countries showing a positive correlation (p=.900), and neither proportion was significantly different from 50%. When modelled against financial well-being, trust showed a consistent positive pattern (74%, p<.001).

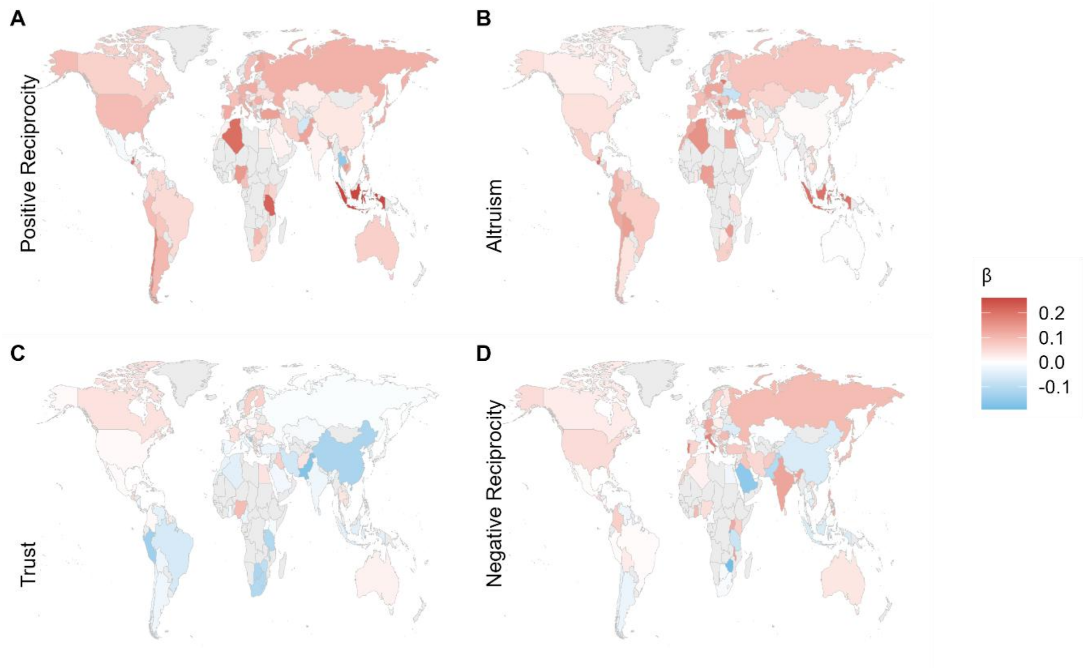

Figure 2.7 | Associations of income with prosocial preferences across the globe
Income was positively associated with **(A)** positive reciprocity in 69/76 (91%, binomial proportion test comparing to 50% p<.001) countries, **(B)** altruism in 67/76 (88%, p<.001) countries, **(C)** trust in 33/76 (43%, p=.900) of countries with 43/76 (57%, p=.200) showing negative relationships, and neither proportion being statistically significant, **(D)** negative reciprocity in 53/76 (70%, p<.001) countries. β values are standardised regression coefficients. Income was measured by self-reported household income, adjusted to achieve purchasing power parity for legitimate comparison between countries (see Methods).

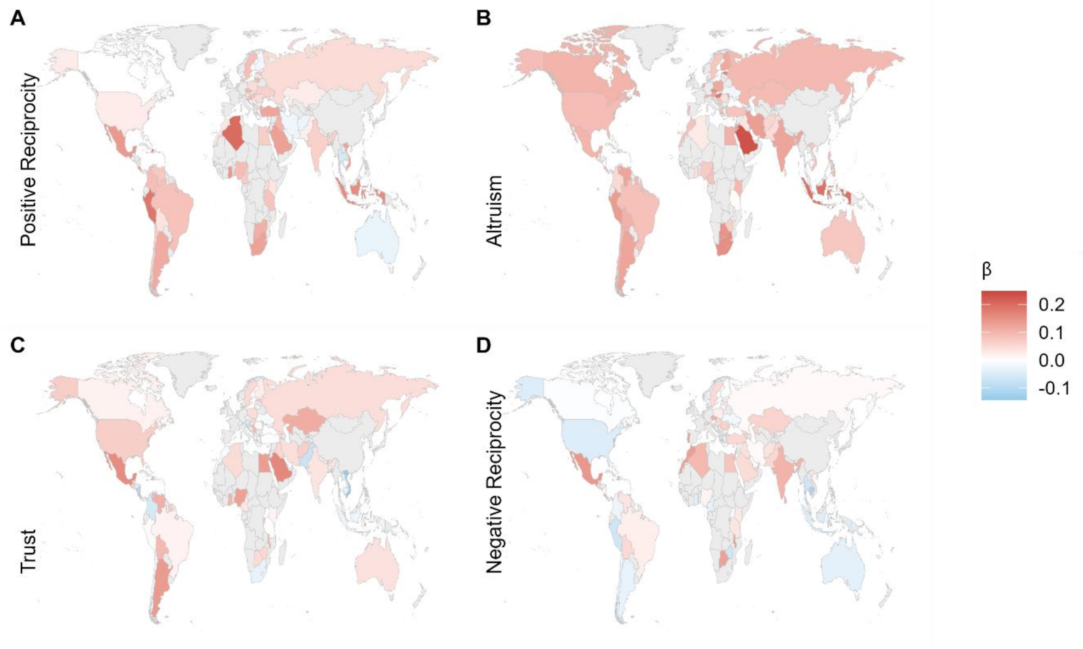

Figure 2.8 | Associations of financial well-being with prosocial preferences across the globe
Financial well-being was positively associated with **(A)** positive reciprocity in 48/68 (71%, p<.001) countries, **(B)** altruism in 55/68 (81%, p<.001) countries, **(C)** trust in 50/68 countries (74%, p<.001), and **(D)** negative reciprocity in 44/68 countries (65%, p=.01). β values are standardised regression coefficients. Financial well-being was measured by a 4-item scale capturing participants' perceptions of their personal economic situation (see Methods).

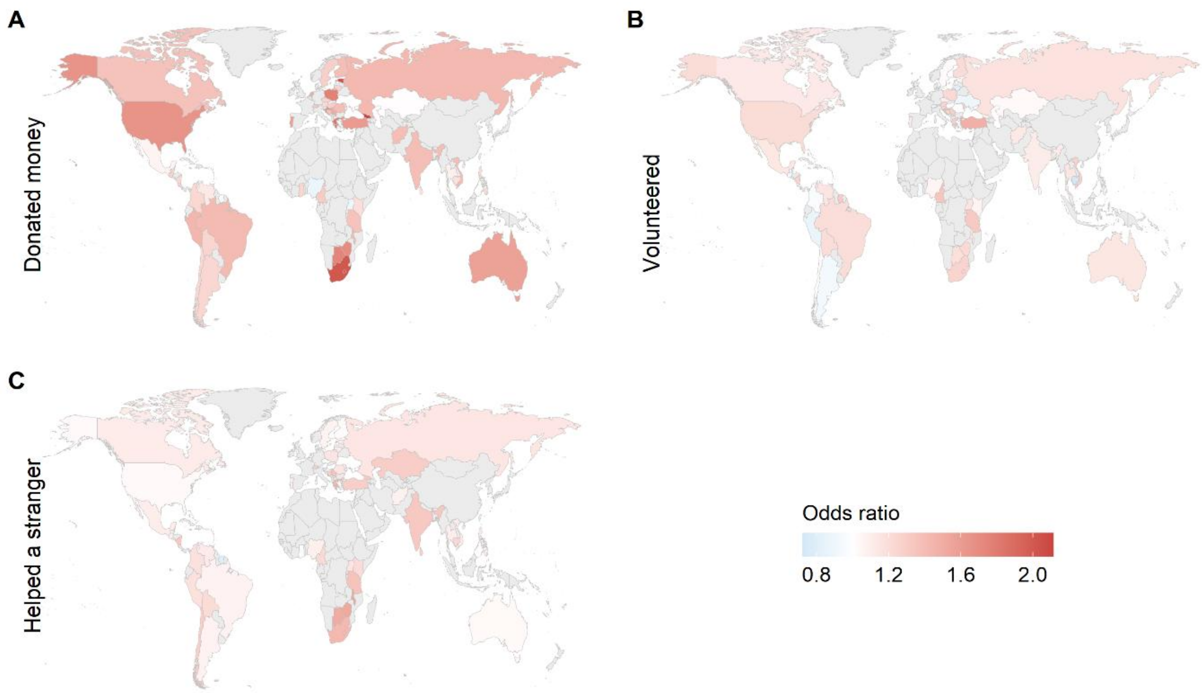

Figure 2.9 | Associations of income with prosocial behaviours across the globe
Income had a positive association with **(A)** donating in 52/57 (91%, p<.001) countries, **(B)** volunteering in 45/57 countries (79%, p<.001), and **(C)** helping a stranger in 48/57 countries (84%, p<.001). Odds ratios>1 (red) mean the behaviour is more likely with increased income representing a positive association, and vice versa for OR<1 (blue).

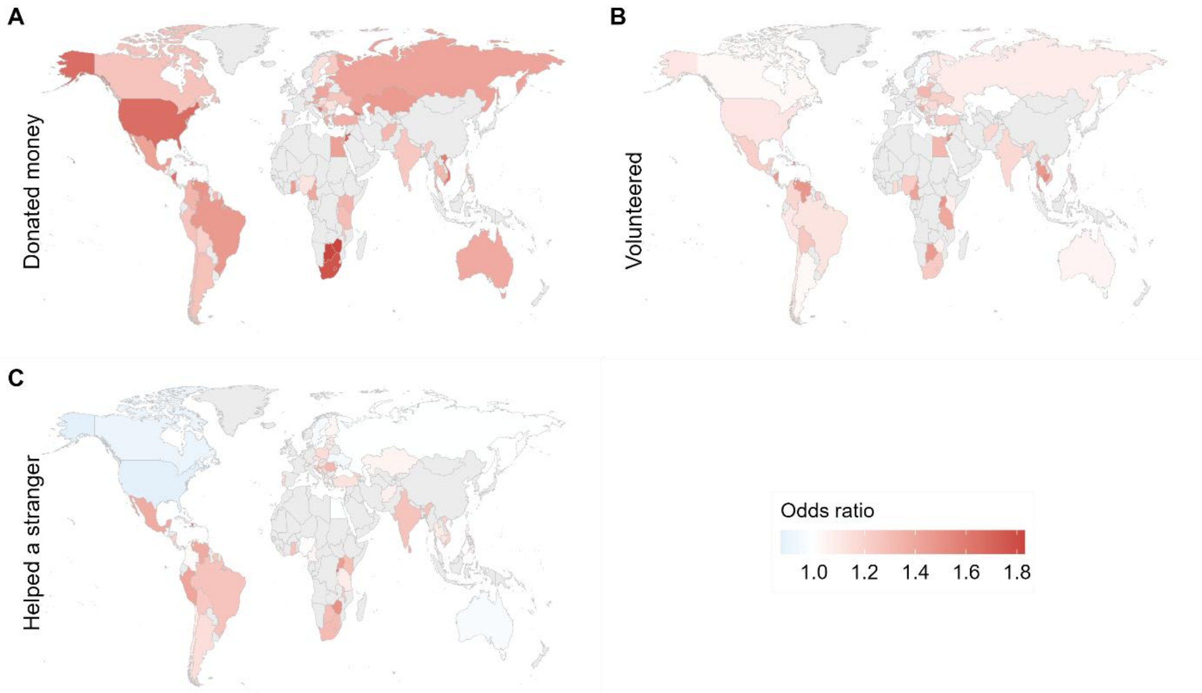

Figure 2.10 | Associations of financial well-being on prosocial behaviours across the globe
Financial well-being had positive relationships with **(A)** donating in every country measured 59/59 (100%, p<.001), **(B)** volunteering in 57/59 (97%, p<.001) countries, and **(C)** helping a stranger in 49/59 countries (83%, p<.001). Odds ratios>1 (red) mean the behaviour is more likely with increased income representing a positive association, and vice versa for OR<1 (blue).

2.4.5 Moderation of associations by country-level gross national income and cultural dimensions

While wealth was positively associated with prosociality across most countries, our final analysis examined three potential country-level moderators – per capita Gross National Income (GNI) (pre-registered), the strength of family relationships (pre-registered), and cultural individualism-collectivism (exploratory). This analysis measured if our correlations were robust to country-level factors serving as an additional control analysis for global consistency of relationships. Family Ties measures the importance of intra-family relationships, and is correlated with social and economic outcomes including trust, labour market participation, stress and well-being (Alesina & Giuliano, 2014). We also explored the moderating influence of individualism-collectivism (Minkov et al., 2017) as prosocial norms can vary by culture (Henrich et al., 2005). Correlations with the other country-level moderators showed stronger Family Ties was associated with collectivism [$r_{(25)}$=-.50, p=.007] and higher GNI with individualism [$r_{(41)}$=.76, p<.001].

GNI did not significantly moderate any wealth associations with prosocial preferences (Table S7). However, GNI did significantly moderate financial well-being associations with the behaviours: donating (OR=0.94 [0.90, 0.98], p=.005), volunteering (OR=0.94 [0.91, 0.98], p=.001), and helping a stranger (OR=0.94 [0.90, 0.98], p=.001). To understand these interactions, we split the data into quartiles. Countries with the highest GNI had smaller wealth associations, due to high levels of prosociality among the relatively less wealthy people in the richest countries (Figure 2.11 A-C). That the association between wealth and prosociality remained positive in all country-level income bands, further supports remarkable consistency around the world.

For Family Ties, there was no significant moderation on any of the prosocial preferences or behaviours (Table S9). As with GNI, there were no significant interactions between individualism-collectivism and wealth with preferences (Table S8). In contrast, in more collectivist cultures the positive relationship with financial well-being was significantly stronger for helping a stranger (OR=0.94 [0.90, 0.98], p<.001) and volunteering (0.95 [0.91, 0.98], p<.001; Fig. 7D-E), but it remained consistently positive in individualist cultures. Overall, although country-level factors moderated wealth-prosociality associations, associations were consistently positive.

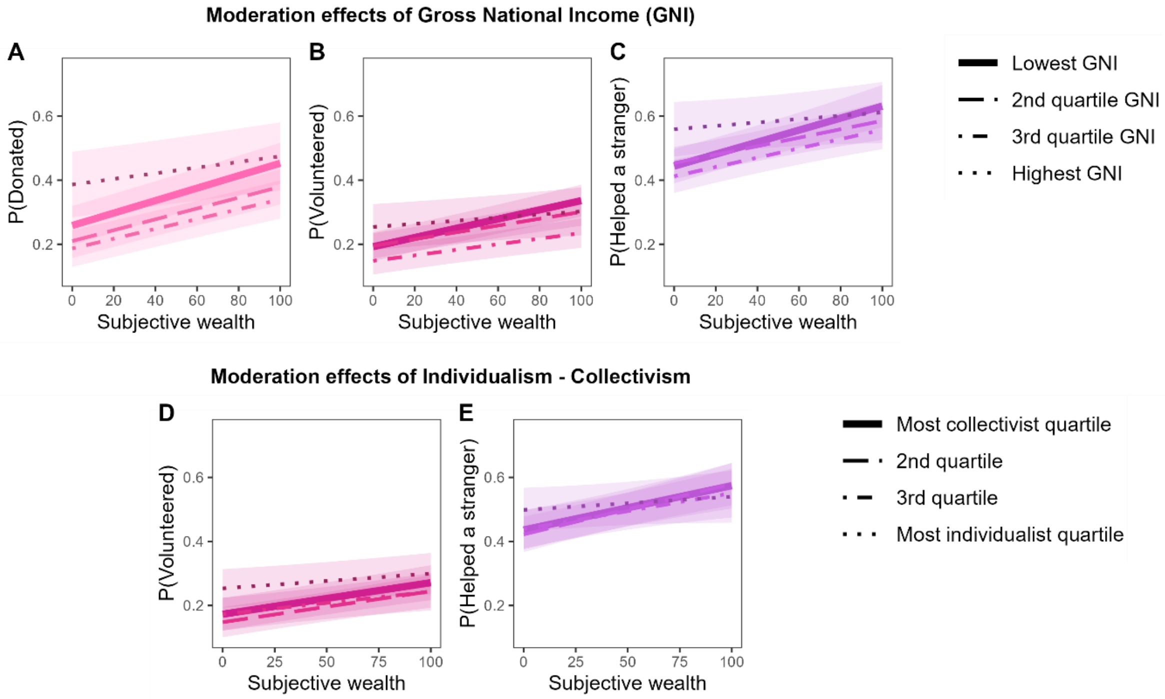

Figure 2.11 | Moderation of GNI and Individualism-Collectivism on the wealth-prosociality association by quartile
Plots show model-based probabilities of behaviour with subjective wealth. GNI moderated the association of financial well-being with **(A)** donating, **(B)** volunteering, and **(C)** helping a stranger. Positive wealth correlations were smaller in the richest countries but remained consistently positive in all quartiles. Individualism-Collectivism moderated **(D)** volunteering and **(E)** helping a stranger. The association between wealth and prosociality was smaller in more individualistic countries, but again remained positive across quartiles.

5 Discussion

Prosociality is essential for the well-being of individuals, communities, and society as a whole (Brown & Brown, 2015; Hui et al., 2020; Kosse & Tincani, 2020). We found that wealth, which we employ as a general term representing both self-reported income and subjective financial well-being, was positively associated with most measures of self-reported prosociality, consistently across the world. Income was positively associated with altruism, positive reciprocity, negative reciprocity (a tendency to punish, important to maintain co-operative behaviour), donating money, volunteering, and helping a stranger, although negatively associated with trust. Financial well-being was positively associated with all measures, including trust. Moderation analyses demonstrated that experience of precarity, country-level per capita GNI, and individualism-collectivism, explained variance in associations yet all the wealth-prosocial relationships remained positive.

Our findings significantly extend previous research suggesting a positive association between income and prosociality (Andreoni et al., 2021; Falk et al., 2021; Kosse & Tincani, 2020; Nettle et al., 2011; Wiepking & Bekkers, 2012; Zwirner & Raihani, 2020). We showed that these associations were present across a wide range of financial and non-financial measures of prosociality. Furthermore, we demonstrate high consistency across the world. We also showed moderation by per capita GNI and individualism-collectivism. The association of wealth and prosociality was weaker in countries that were the most individualistic and had the highest GNI, but remained consistently positive.

In contrast to household income, we hypothesised that financial well-being would be negatively associated with prosocial preferences and behaviours (Cutler et al., 2021; Elbaek et al., 2021b; Guinote et al., 2015; Piff et al., 2010, 2012). Instead we found that all prosocial preferences and behaviours were positively associated with financial well-being, and these results were also consistent globally. There are several possible reasons for these contrasting findings. Firstly, the results of Piff et al have been cast into doubt based on their methodology and failed replications (Balakrishnan et al., 2017; Clerke et al., 2018; Francis, 2012; Greitemeyer, 2023; Jung et al., 2023; Korndörfer et al., 2015; Stamos et al., 2020). The size and representativeness of our sample, including hard-to-find groups, may also be a factor when previous studies have used WEIRD samples. Even within studies that recruit from non-WEIRD countries, reliance on online recruitment and testing can bias samples in terms of education and internet access (Ghai et al., 2023).

Secondly, previous studies have often used measures of subjective socio-economic status (SES), a self-report regarding how well one thinks one is faring compared to others in their country based on financial status, education and occupation (Adler et al., 2000). This measure may be defined differently in different countries contributing to mixed findings (Stamos et al., 2020). People who feel greater financial well-being report higher prosociality. Associations between income and financial well-being were modest in our study, supporting measuring these constructs have somewhat independent relationships with prosociality.

In the context of positive associations of both subjective and income on most prosocial measures, the negative association of income and trust is notable. Research has shown that country-level GNI and trust are positively correlated globally (Morrone, 2009; Steijn & Lancee, 2011), and in individual countries (Ananyev & Guriev, 2019). Two aspects of our findings on trust are worth considering. Firstly, we showed the importance of including country level random effects when assessing associations between wealth and trust. Without these, there was a positive global correlation between trust and income, but without them the relationship was negative.

Secondly, additional variables may explain positive correlations between trust and wealth. Cognitive ability (in the form of literacy skills) is an important factor in the relationship between trust and income (Borgonovi & Pokropek, 2017) and we controlled for cognitive ability in our study. If other studies were not able to control for cognitive ability, they may have overestimated the positive relationship between wealth and trust, which we found to be the case for the studies previously cited (Table S10). In the case where effect sizes are small, this could make the difference between concluding a positive versus a negative association. Future studies could examine the mechanisms that drive reduced trust with increasing wealth.

It is important to highlight that the psychological phenomenon of prosociality carries a strong moral component (Burkart et al., 2018; Patrick et al., 2018; Pletti et al., 2019; Poulin, 2018). People who think and behave more prosocially may be judged more positively by others (Berman & Silver, 2022). But different responses may be nothing to do with someone's moral character, but rather an adaptive response to circumstances (Frankenhuis & Nettle, 2020). Overall, prosociality will be determined by both the desire to help but also ability to help. Our findings therefore do not necessarily relate to the personality traits or motivations of people at differing levels of wealth. Indeed, although wealth was positively associated with prosocial preferences and behaviours that were not financial, we also showed, as predicted, that wealth had a larger correlation with financial measures of prosociality – those involving monetary giving – than non-financial measures. This is not surprising as people with higher wealth may be able to give more resources.

We also showed a direct relationship of precarity with prosociality as well as moderation of wealth and prosociality associations. The relationship between precarity and wealth differed between financial and non-financial forms of prosociality. Those who had experienced precarity showed higher prosociality on non-financial measures. In contrast, our main results show that lower wealth was associated with lower prosociality, including on non-financial measures. This difference demonstrates that the experience of struggling to afford basic needs is a qualitatively different experience to the feeling of being unsatisfied with ones' standard of living. In addition, precarity also moderated wealth associations differently between prosocial preferences and behaviours. Experience of precarity amplified the increase in prosocial behaviours with higher wealth, yet reduced the increase in prosocial preferences with higher wealth. Previous research has varied in whether the prosocial measure is financial or non-financial, such as returning letters (Holland et al., 2012) whereas our study includes both to compare. Together, this pattern of results could help to explain some of the conflicting previous results on whether higher SES is associated with increased or decreased prosocial behaviour (Balakrishnan et al., 2017; Clerke et al., 2018; Korndörfer et al., 2015; Stamos et al., 2020). Measures of SES may capture or confound both precarity and wealth, which are related but distinct concepts. Future work could assess additional moderators that affect the association between wealth and prosociality. For example, whether these associations are modified by beliefs about the worthiness of the recipient (Epper et al., 2020) is an important open question.

Overall, our results provide clear evidence of the globally representative association between wealth and prosociality, across a range of financial and non-financial measures. As a cross-sectional study, it is important to recognise limitations in terms of causal inference. Whilst it is plausible that individual wealth leads to greater prosociality, it is also possible the opposite may occur, such as in the 'investment model' of volunteering (Hackl et al., 2007) or the idea of a 'prosocial premium' that benefits labour market success (Kosse & Tincani, 2020). Prosociality and wealth could also influence each other bidirectionally. Manipulating wealth experimentally in a global sample is challenging. However, there is some evidence that local direct cash transfers to poorer households may increase trust and gift-giving (Mesfin & Cecchi, 2024). Future studies could design ways to manipulate wealth experimentally to test causal links between wealth and prosociality.

It is also important to note the strengths and limitations of the measures used. There is evidence that multi-item scales breaking down income into different categories are more accurate for assessing total household income in countries such as the UK (Micklewright & Schnepf, 2010). A single-item measure of household income is used in our data for speed and global applicability. Multi-item income scales would be challenging to implement across different countries that differ in factors such as taxation, benefits and corruption, whereas household income is easily applicable and standardised across countries. Our data is also based on self-report which could be affected by demand characteristics. Future research could use experimental or observation measures of prosocial behaviour, combined with manipulations of wealth and precarity to test causality and specific hypotheses about potential mechanisms. However, we selected the prosocial preference measures as they are externally validated against behaviour in economic games (Falk et al., 2023). For prosocial behaviours, participants reported if they had engaged in the behaviour or not, creating binary measurements of behaviours that are often continuous or complex in the real world, such as charity donations.

Relatedly, two of our prosocial preference measures (altruism and positive reciprocity) involved participants imagining receiving an amount of money, which could be valued differently by individuals of differing wealth levels. Although we showed the same positive wealth-prosociality associations for income and financial well-being and with non-financial measures of prosociality, future studies could consider measures of giving in proportion to income. A further strength of our results was international consistency. This was reinforced by moderation analyses that demonstrated while some associations are significantly moderated by country-level GNI or individualism-collectivism, the wealth-prosociality associations remain positive. This is particularly important as in some countries, such as the US and UK, charity donations are tax deductible meaning it can be complicated to uncover direct wealth-prosociality associations within charity donation statistics.

In conclusion, we show that wealth is associated with higher levels of prosociality on financial and non-financial measures of prosocial preferences and behaviours. This pattern was highly consistent around the world, across countries that differ in wealth and cultural factors. In addition, experiencing precarity enhances wealth associations with prosocial behaviours but weakens how strongly wealth is associated with prosocial preferences. The clear implication is that greater levels of prosociality are enabled by increased levels of income and financial well-being, an important consideration for developing social policies.

Chapter 3
A new task for assessing prosociality in groups

The previous chapter revealed a robust global link between a range of measures of prosociality and financial status. Humans and other animals face decisions about whether to behave prosocially in everyday life, and whilst the mechanisms of such social decisions have often been studied in individuals or dyads, many of our choices take place in group contexts. This chapter presents a new paradigm for studying such choices, which also forms the basis of Chapters 4 and 5.

1 Introduction

As outlined in **Chapter 1**, prosocial behaviours are essential for well-being and societal cohesion. However, classical paradigms for testing prosocial decision-making, such as prisoner's dilemma or public goods games, often test individuals or dyads, yet many of our decisions occur in the context of social groups (Galizzi & Navarro-Martinez, 2019; Levitt & List, 2007; Penner et al., 2005). Understanding behaviour in social groups is therefore fundamental to understanding human behaviour. Furthermore, these paradigms have most commonly used payoff structures where the optimal decision is always to defect, which may not reflect the structure of group decision-making in daily life (Kameda et al. 2010). Theories from social foraging provide a more ecologically valid framework to develop experimental tasks closer to the type of decisions the brain has evolved to solve, such as foraging for food (Cisek, 2019; Gabay & Apps, 2021; Mobbs et al., 2018). These frameworks, traditionally developed to understand non-human animal behaviour, can reveal new insights into human social group decision-making.

One model in particular from the social foraging literature, so called 'producer-scrounger' (PS) models describe how groups of individuals make decisions in natural settings (Giraldeau and Caraco 2000). In these models, producing generates rewards for the whole group at a cost to the self, whereas scrounging relies on others producing to generate rewards. They employ the principle of diminishing returns, ubiquitous in nature (Kameda et al. 2010), setting up a Nash equilibrium against which prosocial behaviour can be measured. Previous producer-scrounger models have been validated experimentally in various non-human species (Afshar & Giraldeau, 2014; Dubois & Richard-Dionne, 2020; Morand-Ferron & Giraldeau, 2010) but there is relatively little experimental work using PS games with human subjects, which I will now briefly review.

Early work using the PS framework in human subjects demonstrated that stable equilibria were established as predicted by the models, and that the level of production would be higher when the cost of doing so was lower (Kameda & Nakanishi, 2002). Another study showed that when a risk factor is introduced, the risk of losing rewards increased production (Kameda & Tamura, 2007). Interestingly, a level of behavioural plasticity has been observed in PS games, indicating that to a large extent individuals will adopt different roles based on the dynamics of the group, i.e. that the situational specifics are a dominant factor in behaviour (Kim et al., 2019). PS game structures have most often been used to study social learning in humans (Kameda & Nakanishi, 2002, 2003; Kim et al., 2019; Mesoudi, 2008; Toelch et al., 2009), but only one study has ever used PS games to study prosocial behaviour (Kameda et al. 2010). This study concluded that humans made choices which were significantly more prosocial than were optimal based on selfish reward, in line with the classic economic games discussed in the introduction. However, the decision-making structure in this task meant that net payoffs were conflated with revealing unknown information, which has been shown to have inherent value even if it does not lead directly to reward (Bennett et al., 2016; Brydevall et al., 2018; Rodriguez Cabrero et al., 2019).

The PS framework is particularly suitable for translation into iterated human economic games, since it models an ongoing tension between individual incentives and collective outcomes. Producing when the total other participant choices are below the Nash equilibrium point represents an option that generates reward for the group as well as a greater net return for self, whereas producing when other participant choices are above the NE generates reward for the group but at a cost to personal reward. The former may be classified as a cooperative behaviour, and the latter as an altruistic one. As it conflates reward for self and others, cooperation should be regarded as a weaker form of prosocial behaviour compared to altruism, with some even arguing that cooperating below a population average should not be classified as cooperation at all (Schäffer et al., 2025). Thus, the game payoff matrix sets up a social dilemma and yields a benchmark for a mathematically optimal number of producers per trial based on individual reward (Vickery et al. 1991; Giraldeau and Caraco 2000). In many standard economic games designed to measure prosocial decision-making, including linear payoff public goods games, the optimal strategy is always to defect, but this is not the observed behaviour in humans or certain other animals, and some level of non-zero behaviour is always seen. With non-linear payoffs, the Nash equilibrium gives a solution where the optimal strategy is non-zero. The benefit of this is that it sets up a benchmark which is not confounded with measurement artefacts (Kameda et al. 2010). For example, the 'always defect' strategy cannot be reproduced empirically with human respondents even when heavily favoured by the experimental design (Kümmerli et al. 2010), and thus it is clear that measuring from any zero-point is fundamentally flawed. The equilibrium point created by the diminishing returns solves this problem. The equilibrium frequencies for producer-scrounger games then depend on the specific parameters of each model, such as the finder's share of resources, group size, and the costs of production (Giraldeau and Caraco 2000).

Unlike classic one-shot games such as the Dictator or Ultimatum Game, PS interactions unfold across repeated trials, allowing participants to learn from outcomes and adapt strategies. Another important benefit of producer-scrounger games is that the dynamic equilibrium ensures that cooperation is sustained over time without the decline commonly seen in traditional zero-based PGGs (Burton-Chellew & West, 2021; Chaudhuri, 2011; Ledyard, 1994). The present experiment uses a 100-trial, live, online PS game with six participants per group. Each trial requires a binary choice to produce or scrounge. Producing supports the group because there is (a metabolic) cost and risk in searching for food in order to create benefits that others may exploit. Scroungers conserve effort but depend on others' investments. In this version of the game, the choice to produce incurs a personal cost i.e. scrounging is always less costly to the individual than producing, but if everyone scrounges, everyone has their reward (equivalent to real money) taken away.

To test whether participants were sensitive to this non-linear payoff matrix and Nash equilibria, my experimental design incorporates a within-subjects manipulation: a low-cost and a high-cost production environment. Theoretically, increasing the cost of production shifts the equilibrium toward greater scrounging, and if agents are sensitive to this, it should reduce the expected proportion of producers (Afshar & Giraldeau, 2014). This manipulation, therefore, provides a way to test game-theoretic predictions of prosocial behaviour. In assessing the extent of prosocial behaviour, any significant relationship with known prosocially correlated traits such as empathy, group affiliation and psychopathy would count as positive evidence that it is indeed prosociality as a directed choice (de Waal, 2008).

Social preference theory suggests that prosocial behaviours are a product of concern for the welfare of others, driven by proximal psychological traits (van Dijk & De Dreu, 2021). I chose four such traits which would be expected to systematically predict prosociality based on their face validity or previous validation against behaviour in economic games. The Prosocialness Scale in Adults (PSA) is a validated, cross-culturally generalisable measure of everyday prosocial tendencies. In the absence of direct PSA validations in economic-game tests, convergent work shows that similar prosocial personality measures are predictive of behaviour (Fang et al., 2022; Haesevoets et al., 2022; Thielmann et al., 2020) so I expected a positive association between PSA and production in our task.

Similarly, empathy, in particular affective empathy, has been consistently linked to prosocial behaviour across paradigms (Herne et al., 2022; Pavey et al., 2012). Measuring empathy via the QCAE (Reniers et al., 2011) therefore provides an empirically grounded predictor of producing behaviour in the PS game.

In contrast, psychopathy has been shown to be negatively associated with prosocial behaviours (Wertag and Bratko 2019). The short-form self-report psychopathy scale (SRPSF) measures psychopathic traits across four facets: Interpersonal, Affective, Lifestyle, and Antisocial. In the PS task, individuals higher in psychopathic traits could be expected to be more inclined to exploit producers by adopting persistent scrounging strategies.

Group affiliation and identification also play a key role in cooperative behaviour, specifically promoting group cooperation in social dilemmas such as this (Balliet et al., 2014; Van Vugt & De Cremer, 1999). In the present design, group identification is measured using the group-level self-definition and self-investment model of Leach et al (2008) and was expected to predict greater willingness to produce, particularly in the high-cost condition where producing benefits the group at greater personal expense.

Finally, building on my insights from **Chapter 2**, socioeconomic factors such as financial well-being, income, and subjective socioeconomic status (SES) may also shape cooperative tendencies. The evidence here is mixed. Some social psychology studies suggested that lower SES individuals behave more prosocially (Guinote et al., 2015; Piff et al., 2010, 2012), but more recent large-scale analyses indicate that higher-SES individuals may display equal or greater prosociality depending on context (Andreoni et al., 2021; Korndörfer et al., 2015; Vanags et al., 2025). Importantly, perceptions of financial scarcity can reduce attentional capacity and empathy, temporarily dampening prosocial responses (Shah, Mullainathan & Shafir, 2012). Including both objective and subjective measures of financial status therefore allow a test of how economic circumstances relate to cooperation in dynamic group settings.

In addition to social preference theory, previous research based on classical economic games has consistently suggested that humans behave in ways that generally benefit others compared to pure self-interest (Mengel, 2018; Tisserand, 2014). Here, we tested whether this was the case in a more ecological, group-based decision-making framework based on social foraging theory. The following were preregistered for study one (As Predicted #168138):

(1) there will be more producers (and therefore higher levels of prosocial behaviour) than predicted by the Nash equilibrium
(2) levels of prosocial behaviour will be dependent on the cost of production

For study one we also pre-registered that we would build computational models of behaviour to examine the key factors affecting prosocial decision-making, and that we would compare overall levels of production to two other benchmarks: group maximal returns and the prediction of an agent-based model.

And for study two (As Predicted #200528):

(1) People will choose the produce option more when the costs of doing so are lower.
(2) On average people will produce more than the Nash equilibrium suggests is optimal, in both high-cost and low-cost conditions
(3) The effect of producing above Nash will be larger in the high-cost condition than in the low-cost condition
(4) There will be individual differences in behavioural strategies

Whilst not preregistered as specific hypotheses, with regard to hypothesis (4) we expected that producing at an individual level should be positively predicted by prosociality, empathy, and group affiliation, and negatively predicted by psychopathy, and that income, financial well-being and SES would moderate cooperative tendencies, with lower perceived security potentially reducing prosocial producing.

In study one, we also preregistered that we would compare the overall proportion of trials to produce vs. scrounge in high- and low-cost conditions to two alternative benchmarks: maximal group return, and the production rate predicted by an agent-based model. By using multiple benchmarks, we could more robustly assess whether participants' behaviour was caused by deliberate prosociality or some other mechanism, such as performance errors or experimental design artefacts. This question has been a topic of some debate based on previous empirical evidence, and some researchers have suggested that behaviours which superficially resemble prosociality may not be so (Burton-Chellew et al., 2016; Burton-Chellew & West, 2013, 2021). At the time of preregistration, the primary aim was to test prosocial behaviour simply based on the benchmark of the pure Nash solution, but subsequent theoretical reasoning (discussed more fully in Methods and Discussion), suggested that these and other benchmarks should be considered in confirming the presence or extent of prosociality. As a result, all aggregate results are compared to five different benchmarks; three variants of the Nash equilibrium (pure, mixed and prospect-theory weighted) alongside the ABM and group optimality benchmarks pre-registered in study one.

By combining formal foraging models with individual-differences psychology, and game-theoretic benchmarks, this experiment provides a novel test of how stable equilibria emerge in human groups, and which factors promote or hinder prosocial behaviour in dynamic collective dilemmas.

2 Task development

3.2.1 Purpose

The first step in my investigation was to develop an experimental task which would serve as a valid instrument to measure human prosocial decision-making. The aim was to do this in a way that was built on previous insights into the study of prosociality. Measurements of prosocial behaviour in groups are under-represented in the literature (Penner et al., 2005) and it is argued that designing experimental environments for use in the laboratory that mimic real-world survival problems leads to better, more grounded empirical findings (Mobbs et al., 2018). Hence, I chose to create a group-based task using a representation of a real-world behavioural problem that animal brains would have evolved to solve. I consulted social foraging literature to find a suitable paradigm, and selected producer-scrounger games as a suitable model (Barnard and Sibly 1981). These are based on kleptoparasitism, that is, the practice in animals foraging for food by a combination of spending time and effort locating food in an environment, along with parasitically exploiting those that are successful in doing so. Abstracted and simplified, such animal behaviour can be formally described by mathematical models, which can explain and predict empirically observed behaviours in the wild (Giraldeau and Caraco 2000).

3.2.2 Design goals and constraints

Having decided to use the producer-scrounger models as the foundation of the task, I then set about adapting this for a suitable prosocial decision-making paradigm. The models specified by Giraldeau and Caraco are generalised to a wide range of scenarios, for example, scrounging may be a cost or a benefit, rewards may be stochastic or deterministic, and a finders' advantage may apply or not. Thus, I needed to design a version of the general model which would create an iterated group-based social dilemma suitable for testing prosocial decision-making. To do this, it was important to simplify the model structure, which was originally designed to account for real-world behaviour and validation against empirical animal behaviour data. For the purposes of a human decision-making laboratory experiment, I needed to minimise sources of noise and complexity, hence I chose to use a deterministic reward structure with fixed clump size. The trial-by-trial nature of the task meant that any stochasticity in clump finding rate was eliminated as every trial effectively delivered a single clump for each producer. It was important to retain the diminishing return feature of the models as this established the equilibrium point from which we took one of our key measures (producing versus the Nash equilibrium), and also that the reward when there were no producers was equal to zero.

For this task, I needed to create an environment with a dichotomous trial-by-trial choice which would reflect the willingness of the participant to benefit the group versus benefiting themselves. Thus, the payoff structure was designed such that the prosocial choice was costly to the individual and as a result, scrounging had a lower metabolic cost. All rewards generated by the producers were shared evenly between all group members (i.e. finder's advantage parameter set to zero), regardless of whether they had produced or scrounged. In this way, the game closely followed the structure of a traditional public goods game.

An informative dataset would contain a breadth of participant responses from very selfish to very group-oriented, as well as the possibility of different groups and individual strategies. The task itself would need to take no more than one hour in order to fit a reasonable time-frame for online participation, minimising drop-outs (Arechar, Gächter, and Molleman 2018), and be able to be run both online and in-person.

3.2.3 Developing the formal game

I chose to run the game with six players to give sufficient event space to give a quasi-continuous measure of group choice. Smaller group numbers, such as three or four, could be subject to small variance, with the middle options being disproportionately frequently chosen, and therefore not recording sufficient variance to be informative. This need was balanced against practical requirements to be able to run online experiments – larger group sizes being proportionately more vulnerable to participant drop-outs and web server bandwidth issues.

The payoff functions in producer-scrounger games are particularly important and differ from those of classical economic public good games (PGGs). In PGGs, the payoffs are almost always linear, offering the player some simple multiple of their investment into the public pot e.g. doubling or trebling. Provided the number of players is greater than the multiple of the investment, the optimal strategy is to always defect. One of the key features of producer-scrounger games is that they utilise a more naturalistic payoff function based on diminishing returns. Such payoffs are ubiquitous in natural systems, from foraging to sentinel behaviours (Giraldeau & Caraco, 2000). Diminishing returns such as these set up natural cross-over points in the relative payoffs and thus establish equilibria that can be calculated.

The payoff function was defined by the equation (1), where t is the trial number, r_t is the net reward (payoff), N_t is the number of producers, $c_{(a,t)}$ is the cost of action a on trial t, and γ is a power term controlling the curvature of the diminishing returns rate.

$$r_t = 200 * (1 - (1/(N_t \wedge \gamma) + 2))/6 - c_{(a,\,t)} \qquad (1)$$

This was designed such that the returns for each person per trial (berries representing food in the game narrative) were easy to understand and compare (Table 3.1) varying between -18 and +20. The high/low-cost distinction throughout this thesis refers to the net cost of the prosocial behaviour of interest — producing. This is achieved mathematically by varying the cost of scrounging in the payoff matrix, with the 'winter' condition imposing a higher metabolic cost for scrounging than 'summer'. Although total task costs therefore differ between conditions, all references to cost hereafter describe the net cost of producing specifically. Stochasticity, a common element of PS models, was intentionally left out of the experimental design, with all costs and rewards being consistent and determined such that participants could clearly understand the choices they were making on each trial.

Number of producers	Total resources available	Total resources per person	Producer metabolic requirement	Scrounger metabolic requirement	P payoff	S payoff	Group total payoff
			HIGH-COST (summer)				
0	0.0	0.0	20	8	-	-8.0	-48.0
1	100.0	16.7	20	8	-3.3	8.7	40.2
2	133.3	22.2	20	8	2.2	14.2	61.2
3	150.0	25.0	20	8	5.0	17.0	66.0
4	160.0	26.7	20	8	6.7	18.7	64.2
5	166.7	27.8	20	8	7.8	19.8	58.8
6	171.4	28.6	20	8	8.6	-	51.6
			LOW-COST (winter)				
0	0.0	0.0	20	18	-	-18.0	-108.0
1	100.0	16.7	20	18	-3.3	-1.3	-9.8
2	133.3	22.2	20	18	2.2	4.2	21.2
3	150.0	25.0	20	18	5.0	7.0	36.0
4	160.0	26.7	20	18	6.7	8.7	44.2
5	166.7	27.8	20	18	7.8	9.8	48.8
6	171.4	28.6	20	18	8.6	-	51.6

Table 3.1 | Payoff matrices for high- and low-cost conditions in the producer-scrounger task

The chosen solution concept for the game was the Nash equilibrium, widely used in analysing such economic games, mathematically equivalent to the evolutionarily stable solutions described by Giraldeau and Caraco (2000), and used in the previous studies of producer-scrounger dynamics in humans (Kameda et al. 2010).

3.2.4 Agent-based model (ABM) as a design tool

I developed an ABM to explore the parameter space of the task, as this had been previously shown to accurately replicate a wide range of experimental results and predictions (Afshar & Giraldeau, 2014). Agents represented players with actions to produce or scrounge. The ABM was developed to translate theoretical models of prosocial decision-making into a tractable experimental task for human participants. Its purpose was to explore the parameter space of the underlying game and thus select task parameters that would produce valid and informative human data (i.e., avoiding floor/ceiling effects, ensuring variance in behaviour).

Each simulation ran for 500 rounds with simultaneous action choice per round. Agents updated propensities using a SoftMax reinforcement learning rule with inverse-temperature β and learning rate α. I varied group size (n=2–6) and payoff multipliers across scenarios. The outputs recorded were equilibrium strategy frequencies, payoff variance, and convergence diagnostics. Results identified behaviourally feasible parameter ranges to guide the experimental piloting.

The key elements of the ABM are recorded using a summary Overview, Design, Details (ODD) format (Grimm et al., 2020).

Entities, State Variables, and Scales	
Agents	Each agent represented a player in the game, making repeated choices from a binary action space of produce or scrounge.
State variables	None
Environment	The environment was defined by the group choice each round and the payoff function linking individual and group choices to outcomes. Fixed variables were patch discovery rate, patch size, required resources (base), cost of scrounging, diminishing returns power
Scales	Simulations proceeded in discrete rounds, corresponding to one trial in the human task. Each simulation run consisted of 500 rounds, with outcomes averaged over 30 independent runs per parameter combination.
Process Overview and Scheduling	
	At each round: 1. All agents simultaneously chose an action according to their decision rule. 2. Payoffs were computed based on the task payoff function. 3. Agents updated their strategy propensities according to their learning rule (Rescorla-Wagner reinforcement learning). 4.The process repeated until the pre-specified number of rounds was completed.
Design concepts	
Learning	Agents updated propensities based on reward values from previous decision using basic Rescorla–Wagner reinforcement learning rule as described in submodels section
Objectives	Agents sought to maximise expected payoff; group results emerged from aggregate behaviour.
Observation	Agents observed only their own payoffs.
Stochasticity	Choice rules included noise via a SoftMax, and initial strategy propensities were randomised with equal probability All rewards were distributed equally between all agents, regardless of agent action choice
Initialisation	
	Simulations were initialised with random strategy propensities with equal probability of produce/scrounge. Task payoff parameters were specified at the start of each run and held constant within that run.
Input data	
	The model did not use empirical input data, although payoff parameters were selected to produce tractable numbers that would be easy for humans to process. All parameters were derived from theoretical payoff functions,

	which were systematically varied across simulations to explore the design space.
Submodels	
	Payoff function: $$r_t = 200 * (1 - (1/(N_t \wedge \gamma) +2))/6 - c_{(a, t)}$$ t is trial number, r_t is net reward (payoff), N_t is number of producers, , $c_{(a,t)}$ is the cost of action a, γ is a power term controlling diminishing returns rate Choice function: $$P_{(a, t+1)} = \exp(\beta * Q_{(a, t)}) / \Sigma_t \exp(\beta * Q_{(a, t)})$$ P(a, t+1) is probability of choice on next trial, β is inverse temperature parameter and Q(a, t) is learned reward value of the choice Learning rule: $$Q_{t+1(a)}=Q_{t(a)}+\alpha \cdot [r_t - Q_{t(a)}]$$ α is the learning rate parameter (0-1)
Outputs	
	The model recorded equilibrium points of strategies, and variance of those choices across parameter regimes. These outputs were used to identify regions of the parameter space that produced both tractable equilibria and human-feasible behavioural variation. These parameter ranges then informed the final task specification piloted with human participants.

Table 3.2 | ODD summary of agent-based model for task design

	Typical values	Symbol	Description
Number of rounds	100	i	Number of rounds of each simulation
Group size	5 – 50	G	
Environmental discovery rate	5 – 20	λ	Mean number of patches found per producer per round
Patch size	10 (fixed)	S	
Metabolic requirement	10 – 100	R	Amount of food required per agent per round for survival
Cost of scrounging	-5 - +10	C	Cost (or benefit) to base metabolic requirement
Learning rate	0 – 1	α	How quickly an agent integrates new information into value estimates for each choice
Inverse temperature	0.01 – 0.5	β	Stochasticity (lower values = more randomness of choice)
Diminishing return power parameter	0.2 - 4	γ	Determines how ‘steep’ diminishing returns are (higher values = greater effect of diminishing returns)

Table 3.3 | Parameter ranges tested to establish model coherence and viability

Once the model had been created, I underwent a process of tuning the parameters to ensure the model produced appropriate results representing the natural system it is designed to mimic and avoiding those that were obviously artefacts of parameter choice and/or model design (Thiele, Kurth, and Grimm 2014). These resulted in typical parameter ranges shown (Table 3.3), such that any combination within these ranges would yield qualitatively reasonable results that successfully differentiate between behaviours.

To validate the model, multiple trial runs were conducted across a range of parameter values (Table 3.4). Each simulation was run 30 times using 500 timesteps as per Afshar and Giraldeau (2014). I defined convergence operationally as the point at which an EWMA-smoothed series remained within a tolerance band (mean ± 2SD, computed from the last 20% of trials in the series stationary tail) for at least 50 consecutive trials. This band-based criterion provides a simple quantitative equivalent of the visual judgement that the series has stabilised. Mean, mode and standard deviation results were inspected to confirm that the model behaved as expected given the parameters. To validate the model, I replicated the relevant comparisons of model predictions versus other models and experimental results, as given by Afshar and Giraldeau (2014, Table 3.4). The results (Appendix 4) confirmed that the ABM correctly replicated the expected effects and was therefore a valid instrument for modelling producer-scrounger games.

Parameter	Effect	Values tested	Predicted by model?
Group size	Larger = more scroungers	5, 8	Yes
Metabolic requirement	No significant effect	10, 20, 30	Yes
Cost of scrounging	Higher = fewer scroungers	-5, 5	Yes
Discovery rate	Higher = fewer scroungers	10, 20	Yes

Table 3.4 | Parameter tuning ranges and key results.
The model produced expected behaviours based on systematically varying each parameter. Values for other parameters were fixed as follows; α= 0.3, β= 0.2, γ = 1.

As part of the tuning process, I ran multiple versions of the RL model across these parameter ranges. One key repeating phenomenon when sampling the choice space was that if choices to scrounge were punished with a negative reward (because of a zero-producer trial), this would immediately make scrounging seem very unattractive versus producing and therefore very unlikely to be chosen again, even with a Softmax choice function. As a result, it was not uncommon to see the model sticking to produce as a choice, in a way that was not as noisy as one would expect from a human participant.

3.2.5 Piloting with human participants

The task was coded using the Lioness platform (Giamattei et al. 2020) a web-based UI led interface for multi-player economic games. Core functionality was supplemented with bespoke JavaScript/HTML/CSS to achieve the desired functionality. The game was then uploaded to a third-party server, where it was extensively load tested using bots to simulate multi-user traffic. Having used the ABM to develop and test the paradigm, I then piloted the game with groups of six human participants recruited through the Prolific platform, measuring key response variables and monitoring system performance such as drop-out rate, web application failures, and response times. Key behavioural variables, such as mean and variance in choices to produce and their individual differences were also tracked. During the piloting, the decision was taken to include a high- and low-cost within-subjects manipulation to capture greater variety in participants' responses for modelling purposes. This resulted in the game that was then employed for the experiments in **Chapters 3 and 4**.

3 Method

3.3.1 Study 1 sample

A total of 38 groups of 6 participants (N = 228) were recruited from the ‘Prolific’ online platform. As pre-registered (https://aspredicted.org/5yc2r.pdf) the recruited sample were UK/US residents, 18-35 years old, fluent in English, balanced male/female, and with no self-reported ongoing mental health conditions. This sample size was planned to detect a minimum correlation of *r*=.12 between choices and self-reported individual differences, and due to funding constraints. Experiments were run in batches between 7th March and 11th April 2024.

Fourteen participants were excluded because they had less than the requisite 70% of valid trials, giving final sample of 214 people (97 self-reported male, 105 female, M (SD) age = 29.37 (4.20); self-reported ethnicity 135 White (European/US), 30 Asian (Chinese/Indian subcontinent) and 24 Black (African, American, Caribbean), 19 students, and 163 were in employment).

3.3.2 Study 2 sample

A total of 47 groups of 6 participants of the same sample criteria were recruited once again using the 'Prolific' platform. Experiments were run in batches between 4th November and 4th December 2024.

For group-level analysis, seven groups were excluded because in total more than 30% of responses were simulated. Across all groups forty-eight individuals were excluded due to having less than 70% valid trials, thus the final sample after exclusions consisted of 234 people (119 self-reported male, 112 female, M (SD) age = 27.95 (4.82); self-reported ethnicity 122 White (European/US), 25 Asian (Chinese/Indian subcontinent) and 72 Black (African, American, Caribbean), 60 students, and 187 were in employment).

Both studies were approved by the University of Birmingham Science, Technology, Engineering and Mathematics Ethical Review Committee, approval number: ERN_20-1897AP10. Participants were presented with information about the study prior to giving their consent to participate. Players were reimbursed at an hourly rate of £6 / hour minimum, with an incentive bonus of up to £3 depending on the amount of in-game reward they collected in the task.

3.3.3 Recruitment

I employed a two-step recruitment process, beginning with a pre-recruitment stage, whereby participants were invited to join a multi-player online game at a specific time of day, usually within the following hour. Recruits from this stage were then invited to the main task at the allotted time via a white-listing procedure. Based on piloting and testing in small numbers of participants, I pre-recruited approximately 50% more players than were needed for each experimental batch to ensure a sample of 6. Participants were allocated to begin the main task on a first-come, first-served basis. They went through a self-paced instruction phase, followed by three comprehension check questions and four practice trials to familiarise themselves with the task. All comprehension check questions had to be answered correctly to proceed, and they were reminded of the instructions if they failed until comprehension could be confirmed. Participants were then held in an online lobby to be matched with the other members of the group with a counter showing how many more participants were needed. Once there were six participants in the lobby, they were formed into a group, and the main experiment began. This process enabled quick matching of groups so that participants were not waiting for long periods. Due to server limitations, we restricted the number of simultaneous groups playing the game to a maximum of six at a time. As we always recruited more players than we needed, those who showed up for the experiment but did not get matched into a group had their online session terminated and were given a payment of £1. This approach also had the advantage that the most engaged participants were the ones who completed the main task.

3.3.4 Task Design

After being matched, participants completed a real-time online group decision-making game (Figure 3.1a). The task was a type of producer-scrounger game presented as a group-based foraging task. Participants were instructed that they were stranded on a desert island with five other people. To survive, they needed to collect berries. Each day (trial), they had to make a choice whether to ‘search’ for berries to be shared amongst the group (produce), or ‘stay’ at the camp (scrounge) and benefit from the berries that other members of the group collected. I used these descriptors for participants because the word scrounge has negative connotations in the English language and I did not want to prejudice the choice. Henceforth, I will generally use the terms produce and scrounge as these are consistent with the scientific literature, unless referring to specific aspects of the participant experience. All players received an equal share of any berries found, regardless of whether they searched or stayed. However, players were told that each day they would incur a metabolic cost (Giraldeau and Caraco 2000), requiring a certain number of berries to survive that depended on their decision to produce or scrounge.

Players were told that they would begin the game with fifty berries to avoid concerns about running out of berries and not having enough to survive. Producing always carried a higher metabolic cost than scrounging. However, the net reward for any trial also depended on the number of participants choosing to search, based on equation 2 (Figure 3.1b).

$$R = 200 * \frac{\left(1 - \frac{1}{N+2}\right)}{G} \qquad (2)$$

R = Total group reward (berries) for the trial

N = Number of players who chose to produce

G = Group size (6)

Crucially, participants completed the task in two different environments. These environments were created to measure our pre-registered hypotheses surrounding how behaviour is affected by the costs of helping. In summer, producing for the group has a higher cost i.e. more berries consumed on that day, whereas in the winter, producing had a lower cost (Figure 3.1). In line with our gamification, participants were told that as winter was colder, ‘staying’ at the camp required a higher base metabolic cost and thus used more berries than in summer, reducing the net cost of producing and therefore encouraging them to choose to produce. In contrast, in summer, they could stay at the camp more easily and benefit from other people collecting berries. Therefore, summer was a high-cost condition and winter a low-cost condition for our behaviour of interest.

Participants completed 97 trials and 3 attention checks in study 1 and 100 trials and 3 attention checks in study 2. In both studies, there were two alternating blocks of each condition that were counterbalanced for order effects. In study 1, participants had 4 seconds to make a choice, and in study 2 we shortened this to 3.5 seconds based on maximum reaction times from study 1.

To ensure the viability of the online groups, trials were simulated by the computer with even probability (produce or scrounge) if a participant did not respond within 4 seconds for study 1 and 3.5s in study 2. Participants’ experience of the task was unaffected by the presence of missed trials or disconnection from other players, as the group results were presented in the normal way during the task, regardless of whether they were real or simulated. However, any participant missing a trial was informed that the berries from that trial would not count towards their bonus, and they would lose 10 berries, to incentivise responding on time.

The experiment was developed on Lioness (Giamattei et al. 2020), a free web-based platform for interactive online experiments and tailored to our needs using additional scripting (HMTL/CSS/JavaScript). The resulting dynamic web application was hosted on a third-party server provided by Ionos Cloud Ltd and extensively tested with increasing numbers of simultaneous mock participants using bots to ensure robustness.

Following the online game, participants were redirected to a series of questionnaires on the Qualtrics platform to probe their understanding of and feelings about the task itself as well as social and affective traits, in line with our pre-registration.

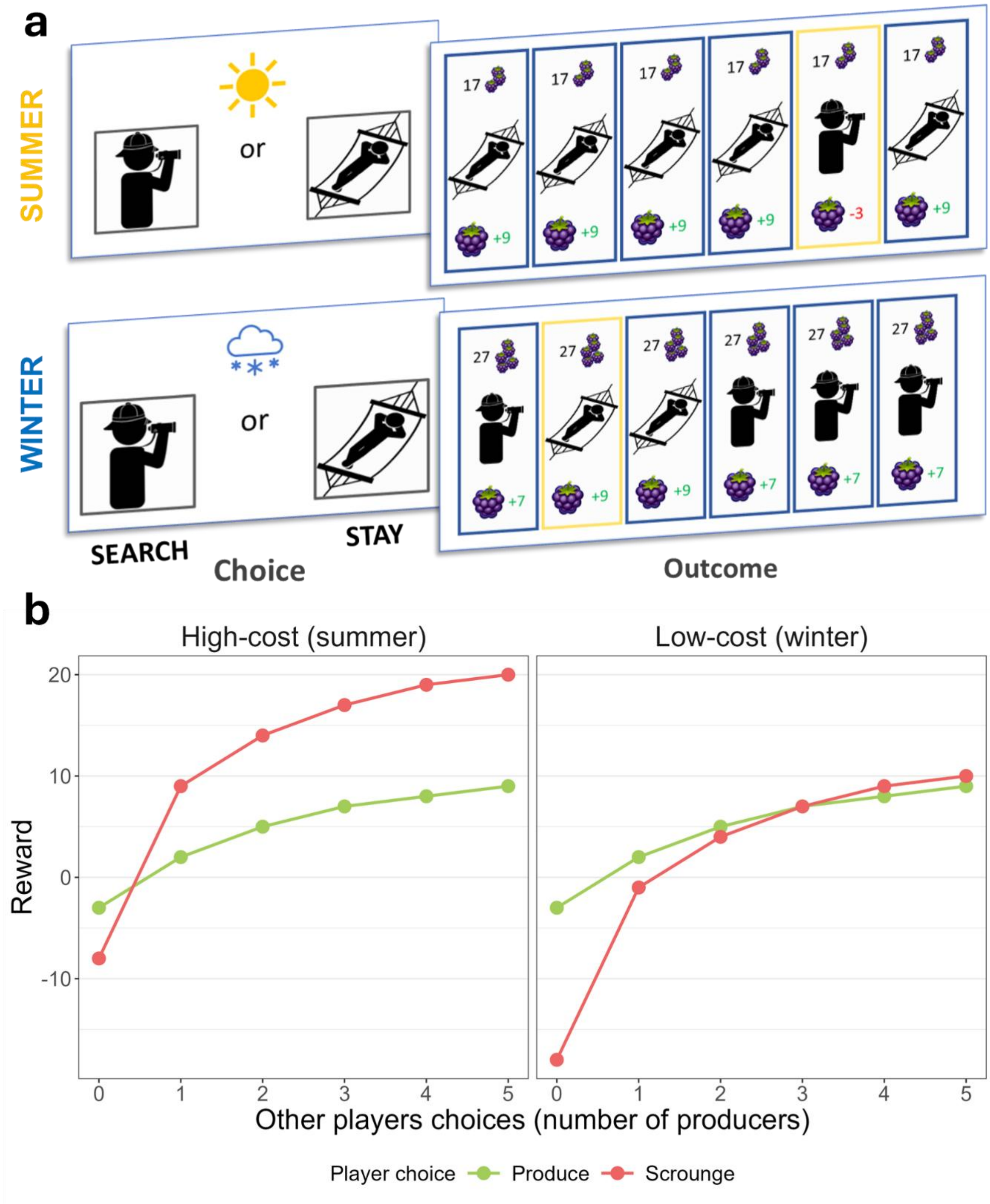

Figure 3.1 | Task and diminishing returns payoffs
a Participants made a choice on each trial whether to search for berries for the group (produce) or stay at the camp (scrounge) and benefit from other's collected berries. Participants made these decisions in blocks of summer and winter conditions, where producing was more or less costly respectively. Each choice was presented for 4 seconds (study 1) or 3.5 seconds (study 2). If no choice was made the participant was given a reminder not to miss trials and their choice for that round was simulated by computer with even probability. Results (total berries and net profit per person for each trial) were shown in a random configuration, with the participant's results highlighted to them in yellow. **b** Net payoffs for each choice per condition showing diminishing returns with greater numbers of producers, showing the relatively higher cost of producing versus scrounging in summer.

3.3.5 Nash Equilibria

The different costs of producing and scrounging, combined with the diminishing returns from producing, as described by the social foraging theory (Giraldeau and Caraco 2000), set up two payoff matrices, each with a different Nash equilibrium (Table 3.5).

Number of producers	Total resources available	Total resources per person	Producer metabolic requirement	Scrounger metabolic requirement	P payoff	S payoff	Group total payoff
			HIGH-COST				
0	0.0	0.0	20	8	-	-8.0	-48.0
1	100.0	16.7	20	8	-3.3	8.7	40.2
2	133.3	22.2	20	8	2.2	14.2	61.2
3	150.0	25.0	20	8	5.0	17.0	66.0
4	160.0	26.7	20	8	6.7	18.7	64.2
5	166.7	27.8	20	8	7.8	19.8	58.8
6	171.4	28.6	20	8	8.6	-	51.6
			LOW-COST				
0	0.0	0.0	20	18	-	-18.0	-108.0
1	100.0	16.7	20	18	-3.3	-1.3	-9.8
2	133.3	22.2	20	18	2.2	4.2	21.2
3	150.0	25.0	20	18	5.0	7.0	36.0
4	160.0	26.7	20	18	6.7	8.7	44.2
5	166.7	27.8	20	18	7.8	9.8	48.8
6	171.4	28.6	20	18	8.6	-	51.6

Table 3.5 | Payoff matrices for producer-scrounger game

A Nash equilibrium can be obtained using ‘pure’ strategies based on the payoff matrices calculated by assuming that each player consistently chooses the same option for all trials. In the high-cost condition, the pure Nash solution is one producer and five scroungers. In this case, we can see that the producers each receive a net reward of -3.3 and the scrounger +8.7. If a producer unilaterally changes their strategy from produce to scrounge, then there will be zero producers, and their reward will drop to -8.0. Alternatively, if the scrounger unilaterally chooses to change strategy and become a producer, there will then be two producers, and their reward will drop from +8.7 to +2.2. Hence, this fulfils the Nash solution requirement that, assuming all other choices remain constant, no agent may unilaterally change strategy without reducing its payoff. By the same logic, we can deduce that in the low-cost condition, the Nash solution is three producers. Thus, if a player anticipated that their group would produce less than the Nash it was in their interest i.e. they would personally get a better reward, to produce, but if they thought the group would produce at the Nash level or above, then they would benefit from scrounging. This was the original basis of our pre-registered hypotheses for both online experiments. However, note that in the low-cost condition when three other players produce the difference in reward between the choice to scrounge (+7.0) and to produce (+6.7) is very small. This had implications for the implementation of the game because in the actual experiment, the rewards were presented to participants as whole numbers of berries, rather than the precise value of the payoffs. When rounded to integers, this changes the pure strategy Nash solution in the low-cost condition. In this case, when three other participants choose to produce, there is actually no difference between the reward of producing versus scrounging in the low-cost condition, as either choice results in a payoff of seven berries. This meant that, as presented in the actual experiment, there were two possible pure Nash solutions (3 producers and 4 producers) in the high-cost condition. A better solution, therefore, is to calculate the Nash equilibrium as a mixed (probabilistic) solution. In this game solution, we look for mixed-strategy Nash equilibria, where each player cooperates with probability p.

Each player cooperates with probability p, so the number of *other* cooperators is:

$$X \sim Binomial\ (5, p)$$

The expected value of producing is a probability-weighted sum of all the possible outcomes:

$$E[\text{Produce}] = \sum_{k=0}^{5} P(X = k)\ \cdot\ \text{reward\_produce(k)}$$

Similarly The expected payoff of scrounging is:

$$E[\text{Scrounge}] = \sum_{k=0}^{5} P(X = k)\ \cdot\ \text{reward\_scrounge(k)}$$

A mixed-strategy Nash equilibrium occurs when the expected value of producing and scrounging is equal. We calculated these new equilibrium points by calculating the expected values for all probabilities between 0 and 1 in 0.001 increments and finding the first point where the difference in payoffs was less than 0.001 (Appendix 2). This yielded different Nash solutions compared to those that were pre-registered. The optimal solution in the high-cost solution was for each player to choose to produce with a probability of .098 (equivalent to ~0.6 players), and in the low-cost solution .646 (equivalent to ~3.9 players). For subsequent analyses, the mixed Nash solution is the default benchmark used, unless otherwise specified.

3.3.6 Agent-based model benchmarks

Having established the ABM as a valid instrument, I used it to generate theoretical benchmarks for comparison with observed behaviour. Each simulation was run for 500 trials and replicated 30 times; the resulting data were averaged to form a single bundle. Learning parameters were drawn from distributions to reflect inter-individual heterogeneity: α (learning rate) ~ Beta(1.5, 1.5) and β (inverse temperature) ~ Gamma(2, 0.2). This procedure was repeated 100 times to capture variability across parameter draws. Convergence was defined based on stability within a defined threshold, namely that the exponentially weighted moving average (EWMA) of the series had to remain within a tolerance band (mean ± 2 SD, estimated from the final 20% of trials in the stationary tail) for at least 50 consecutive trials.

In the high-cost condition, 99% of bundles converged, with a post-convergence grand mean of 2.09 (SD = 0.08, 95% CI [2.07, 2.10]). In the low-cost condition, 97% converged, with a grand mean of 3.98 (SD = 0.07, 95% CI [3.97, 4.00]). These benchmarks represent the expected stable behaviour of a heterogeneous population, and provide a reference point against which human data can be evaluated.

3.3.7 Group optimality benchmark

One other useful benchmark is represented by group optimality (sometimes referred to as Pareto-optimal). This is defined as the point where no better return exists for the group as a whole, and in the case of this game, is equivalent to the point at which rewards for the whole group are maximised. It therefore provides an upper bound on what the group could achieve through perfectly coordinated, welfare-maximising behaviour.

3.3.8 Questionnaire measures

Questionnaire for Cognitive and Affective Empathy (QCAE). This scale measures individual levels of empathy (Reniers et al., 2011), a 31-item scale that measures cognitive and affective empathy as four distinct subscales. Participants are asked to rate their agreement with each item using a 4-point Likert scale. Higher scores indicate higher levels of empathy.

MacArthur Subjective SES. This measures subjective socio-economic status via a single item response to the question about where on a ladder (0-10 Likert scale) people place themselves relative to others on money, education level and having a good job (Adler et al., 2000).

Prosocialness Scale for Adults. A 16-item scale for assessing behavioural and attitudinal differences in adult prosociality (Caprara et al., 2005). For each item, participants indicate on a five-point Likert scale whether the statement was never/almost never true (1), occasionally true (2), sometimes true (3), often true (4), or almost always/always true (5).

Self-report psychopathy scale. This 29-item scale measures psychopathic tendencies (Paulhus et al., 2009). Items are scored on a 5-point Likert scale as disagree strongly (1), disagree (2), neutral (3), agree (4) and agree strongly (5). One question (involvement in gang activity) is reverse scored, and higher scores equate to higher levels of psychopathy. It has four subscales: interpersonal manipulation, callous affect, erratic lifestyle, and antisocial behaviour.

Group affiliation. This scale quantifies the extent to which participants felt connected to their group (Leach et al., 2008). Responses are measured on a seven-point Likert scale from ‘Totally disagree’ to ‘Totally agree’, across fourteen items. Higher scores show a stronger affiliation with the group.

Risk attitude. For study 2, we added the Domain-Specific Risk-Taking (DOSPERT) Scale, which assesses risk-taking behaviour across different domains, acknowledging that people's willingness to take risks varies depending on the type of situation (Blais & Weber, 2006). It covers five different types of risk: Financial Risk (e.g., investing in stocks, gambling), Health/Safety Risk (e.g., unprotected sex, drinking and driving), Recreational Risk (e.g., skydiving, bungee jumping), Ethical Risk (cheating on taxes, lying), Social Risk (e.g., disagreeing with authority, asking someone out). Each uses a 7-point scale (e.g., 1 = extremely unlikely, 7 = extremely likely). As this was an addition to the questionnaire battery from study 1, we were mindful of the total length of the questionnaire and so decided to use only the risk-taking subscale ("How likely are you to engage in this activity?") as a standalone scale, which is itself validated as an independent predictor (Blais & Weber, 2006). Higher scores on risk-taking indicate a greater willingness to engage in risky behaviour in that domain.

Gallup World Poll (GWP) measures. In addition to the psychological scales, we also included questions taken from the GWP, an annual global survey of individuals which has been running since 2005 (Gallup, Inc, n.d.). These covered objective and subjective wealth as well as a measure of precarity, namely, reliable access to food and shelter. Self-reported personal and household income was coded in nine bands, each of range 5,000 - 15,000 GBP/USD. Financial well-being, a subjective measure of the level of material wealth/comfort based on categorical responses to four questions (Table 3.6). Two questions concerning whether participants had reliable access to food or shelter over the past twelve months were recorded as a measure of precarity.

Task specific questions. A set of questions seeking to understand how participants viewed the game, their play strategies, and the extent to which they tried to influence others and were influenced by others was also included. They were all based on 10-point Likert scales (Table 3.6).

Name / question	Reference / measures
Traits and demographics	
Questionnaire of Cognitive and Affective Empathy (QCAE)	(Reniers et al., 2011)
MacArthur Subjective SES	(Adler et al., 2000)
Prosocialness Scale for Adults (PSA)	(Caprara et al., 2005)
Demographics	Age, gender, ethnicity, religiosity, income, education, and job level
Gallup World Poll	Personal and household income, financial well-being, food and shelter Index
Self-report psychopathy scale short form (SRP-SF)	(Paulhus et al., 2009)
DOSPERT Risk attitude scale*	(Weber, Blais, and Betz 2002)
Group affiliation	(Leach et al., 2008)
Task specific questions	

Extent I tried to maximise my reward	Likert 0-9 Never-Always
Extent I tried to maximise group reward	Likert 0-9 Never-Always
How did I feel when others searched	Likert 0-9 Very negative – Very positive
How concerned was I to avoid the zero berries situation	Likert 0-9 Not at all concerned – Very concerned
To what extent play game cooperatively vs competitively	Likert -5 to +5 Fully competitive – Fully cooperative
To what extent were you influenced by how others played	Likert 0-9 Not at all – Always
What was impact when others searched	Likert 0-More likely SEARCH – More likely STAY
What was impact when others stayed	Likert 0-9 More likely STAY – More likely SEARCH
To what extent did you try to influence others	Likert 0-9 Not at all – Always
What extent did you feel others were reacting to the way you played	Likert 0-9 Not at all – Always
What extent did you keep track of how others were playing	Likert 0-9 Not at all – Always

Table 3.6 | List of trait questionnaires and task-based self-report questions
(Note * only in experiment 2)

3.3.9 Data, outliers and exclusions

The task generated the following trial-by-trial data for each participant: choice, reward, cost, and reaction time. Due to a technical issue connected to how some browsers handle JavaScript, some of the participants were not subject to the specified time-out on each trial, meaning that reaction times could sometimes be much greater than this. So as not to distort results, any reaction times more than 3 SD's over the mean were excluded. In one instance, again due to a technical issue, a single player became desynchronised from their group – this player's subsequent choices were excluded from the final analysis.

Data was screened for quality at the participant and group level. Drop-outs ie. people becoming disconnected, or terminated their participation early by shutting their browser, were the main source of missing data. Any participants (13 in total in study 1) who dropped out before completing 70% of trials were excluded from all individual level analyses, but valid responses before the drop-out point were retained in group level analyses. We also applied the 70% completed trials criterion at a group level, but all groups passed this check. Three attention check trials were present in the task. As per pre-registered criteria, one participant was removed in study 1 for failing all three attention checks. Missed trials (time-outs) were recorded, and the simulated choice data were excluded from individual analyses. In total, across the whole experiment, there were 311 (1.4%) missed trials, with no single participant missing more than 10 trials (10.3%).

In study 2, 22 individuals' data were excluded from the analysis because they completed less than 70% of trials. There were 541 (2.6%) missed trials.

3.3.10 Analysis

All statistical analyses including our pre-registered hypotheses was carried out using linear mixed models from the glmmTMB (v1.1.12) package of R (v4.1.0) in R Studio (v2024.04.0). The primary hypotheses were analysed using a Wilcoxon signed rank test to account for non-normal data using the functions 'wilcox_test' for significance testing, and 'wilcox_effsize' for effect size from the 'rstatix' (v0.7.2) package. Exploratory factor analysis was carried out using 'factanal' from the 'stats' (v4.5.0) package ('varimax' rotation). Correlations were calculated to explore relationships between behavioural and questionnaire variables using the 'cor' function using the 'stats' package, supplemented by p-value estimates from 'cor.mtest' in 'corrplot' (v0.95), and we used the Benjamini-Hochberg method to correct for multiple comparisons (Benjamini and Hochberg 1995). The 'BayesFactor' package (v 0.9.12.4.7) was used to calculate Bayes factors (BF01) for non-significant comparisons, with the rscale parameter set to 0.707.

3.3.11 Modelling

Models were fitted according to the principle of using AIC as the criterion for model selection, to balance control of Type I error rate with power (Matuschek et al. 2017). Predictors and their interactions were tested sequentially as fixed effects and as interactions, and this structure was also used as random effects. To improve convergence, the estimation of intercepts and slopes of random effects assumed zero correlation between them. The winning model was defined as that which minimised AIC whilst still converging. We found that models had problems converging when the trial number within each block was included in random effects, due to a very low variance, so this was included in the final model as a fixed effect only.

Using previous trial choice to predict choice carries a risk of autocorrelation within blocks, if participants have generalised tendency to produce or scrounge. There was a significant and large correlation between choice on previous trial and choice apparent from the raw data ($r = 0.42$, $p < .001$, $d = 0.925$). To mitigate this, I structured the random effects to include subject-specific intercepts, and slopes based on previous choice so that individual variation on the extent to which this affected choice were accounted for in the model.

3.3.12 Pre-registration

Study 1 pre-registration can be found at https://aspredicted.org/5yc2r.pdf.

Study 2 pre-registration can be found at https://aspredicted.org/ws37rj.pdf.

Deviations from pre-registration

In study 1, we planned to test 35 groups (210 participants) but recruited 38 groups (228 participants). Of these groups, there was an imbalance in the number of groups that started with the summer or winter condition (17 winter, 21, summer).

The computational modelling specified in the pre-registration for both studies was not included because it did not produce interpretable or reliable results. Specifically, a large proportion of fits gave estimates at the parameter boundaries, indicating unreliability, possibly as a result of model specification issues. As such, the results are omitted here, but the decision not to report them was made before interpreting any other findings.

There were no further deviations from pre-registration in study 2.

4 Results

3.4.1 Sample one

For the pre-registered hypotheses of sample one, a Shapiro test showed that the data for player behaviour was non-normal for both high and low-cost conditions (W_{high} = 0.928, p < .001, W_{low} = 0.934, p < .001). Thus, I conducted non-parametric Wilcoxon signed rank tests for the two pre-registered hypotheses. Here, I report results for the (pre-registered) pure Nash solutions as well as other benchmarks, including mixed Nash equilibrium (see Methods) as exploratory analyses.

3.4.2 Pre-registered hypotheses: High-cost v low-cost and comparison to pure Nash optimality

First, I examined the question of whether participants chose to produce more frequently when the net cost of doing so was lower. As predicted, more people chose to search in the low-cost (W(214) = 1391, p < .001, r = 0.75), than high-cost condition (Figure 3.2a). Examining each condition separately showed that the mean player producing behaviour was higher than the pure (pre-registered) Nash equilibrium in both high- and low-cost conditions (W_{high} = 17526, p < .001, r = 0.45; W_{low} = 13836, p < .001, r = 0.22).

For this study, two further benchmarks had been pre-registered as a comparison to actual behaviour. The maximal group return for each condition was calculated from payoff matrices based on the total number of producers (Table 3.5). In the high-cost condition, the maximum group return was achieved when three people produced, and in the low-cost condition, when six produced. In both conditions, the actual mean producing was significantly lower than the point of maximum group return for each condition (W_{high} = 2922, p < .001, r = 0.64; W_{low} = 0, p < .001, r = 0.87). The other pre-registered hypothesis examined the difference of the actual mean producing level compared to the ABM prediction (see Methods) in each condition using a Wilcoxon signed rank test. In sample one, in both cases the observed mean producing was significantly lower than the ABM prediction (W_{high} = 7893, p < .001, r = 0.27; W_{low} = 8441, p < .001, r = 0.23).

3.4.3 Exploratory analyses – alternative benchmarks

Given the limitations of the pre-registered pure Nash solution, in addition to the ABM and group optimality comparisons, I investigated two further comparison points (Table 3.7). First, I compared rates of production to those predicted by the mixed Nash equilibrium. A paired Wilcoxon test showed a significant difference between mean producing versus the mixed Nash in high-cost condition compared to low-cost ($W(214) = 20970$, $p < .001$; Figure 3.2b). However, versus the pure solution, the effect in the low-cost condition was reversed, showing that players on average produced less than the optimum ($W_{high} = 19913$, $p < .001$, $r = 0.63$; $W_{low} = 8891$, $p = .004$, $r = 0.20$; Figure 3.2b).

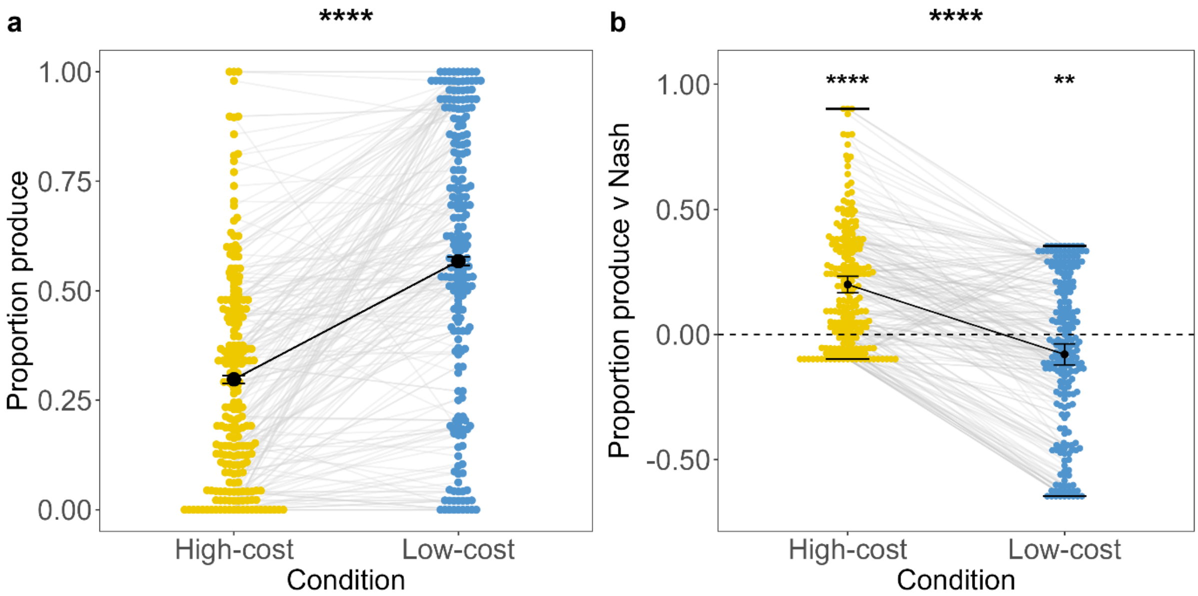

Figure 3.2 | Proportion of 'search' trials per participant – sample 1
Panel (a) shows there was significantly more searching behaviour in low-cost compared to high-cost, [$W(214) = 1355$, $p < .001$], whilst (b) demonstrates that searching behaviour was higher than the optimal Nash equilibrium in the high-cost condition but lower in the low-cost condition , [$W_{high} = 19913$, $p < .001$, $r = 0.63$; $W_{low} = 8891$, $p = .004$]. There was a significant difference in producing versus (mixed) Nash in high-cost compared to low-cost [$W(214) = 20970$, $p < .001$]. Error bars show 95% confidence intervals.

Building on this result, I explored the possibility that the difference in relative production v Nash between the two conditions could be explained by the proximity of the negative reward threshold, in line with prospect theory's concept of loss aversion (Kahneman and Tversky 1979). To determine the extent to which loss aversion might influence strategic behaviour, I re-parameterised the payoff matrix using a loss aversion coefficient (λ=1.95) from a recent meta-analysis (Brown et al., 2024). Negative payoffs were multiplied by lambda, generating a subjective payoff matrix. I then recomputed the mixed Nash equilibrium using these transformed values. This approach reflects a prospect theory based equilibrium prediction, rather than the equilibrium of the original objective payoffs. The prospect-theory equilibria were higher than the objective payoff equilibria given the increased weighting of negative rewards, but producing behaviour remained higher than the benchmark in the high-cost condition albeit with a smaller effect size (W_{high} = 18122, $p < .001$, $r = 0.50$) and lower in low cost (W_{low} = 8558 $p = .001$, $r = 0.22$) in line with the objective mixed Nash equilibria.

	Pure NE	Mixed NE	Prospect theory NE	Group maximum	RL-ABM prediction
			High cost		
Mean actual producing v benchmark	+0.131	+0.200	+0.053	-0.202	-0.050
Statistic (W)	17526	19913	18122	2992	7893
p- value	< .001	< .001	< .001	< .001	< .001
Effect size (r)	0.45	0.63	0.50	0.87	0.27
			Low-cost		
Mean actual producing v benchmark	+0.068	-0.078	-0.090	-0.432	-0.095
Statistic (W)	13836	8891	8558	0	8441
p-value	< .001	.004	.001	< .001	< .001
Effect size (r)	0.22	0.20	0.22	0.87	0.23

Table 3.7 | Summary of grand producing behaviour by condition - sample 1
Compared to each specified benchmark, the table shows the absolute difference between actual and benchmark plus the Wilcoxon signed rank test statistic, p value and effect size expressed as a correlation coefficient

3.4.4 Linear mixed models

To investigate the drivers of behaviour in more detail, I fitted binary outcome (logit) generalised linear mixed-effect models which included condition (high- vs. low-cost), previous trial choice, group outcome from previous trial, and the trial number within each block to test for time-series effects (see Methods).

The best model, based on AIC (Appendix 5), was specified as:

$$\text{choice} \sim \text{ trial number within block} +$$
$$\text{condition} * \text{previous trial group outcome} * \text{previous trial choice} +$$
$$(1 + \text{condition} * \text{previous trial group outcome} * \text{previous trial choice} \,||\, \text{player})$$

Model fit results (Appendix 6) revealed that choices to produce were strongly related to condition, with more production when the cost was low as per the pre-registered hypotheses (Odds ratio, OR = 2.20, 95% CI = [1.60, 3.03], z = 4.81, p < .001). There was also a positive interaction effect between previous choice and condition (OR = 3.55 [2.09, 6.02], z = 4.70, p < .001; Figure 3.3). The interaction effect can be clearly observed in the behavioural data (Figure 3.3), as the difference between producing and scrounging at trial (t) is much greater in the low-cost condition compared to the high-cost condition.

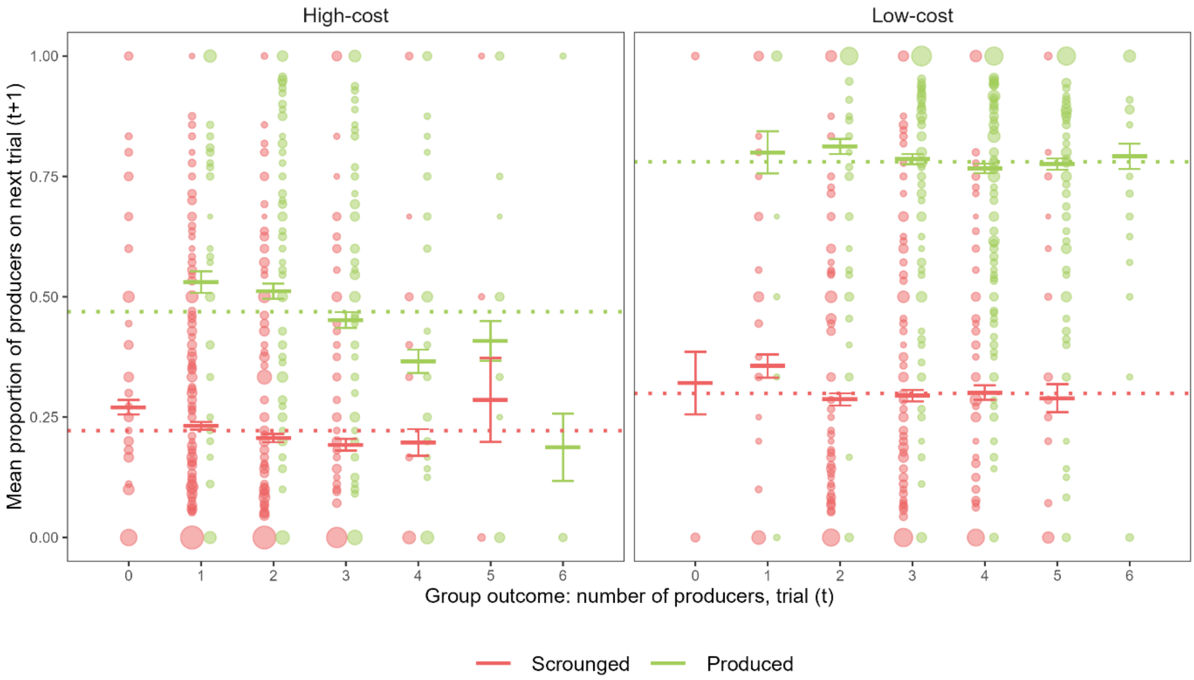

Figure 3.3 | Participant mean probability of producing on trial t + 1, depending on number of producers on trial t – sample 1
Points represent the mean of all individual choices across the relevant trials for each combination of choice on trial t and group outcome, with size proportionate to incidence in each case. Solid horizontal lines show the mean producing behaviour on trial t+1 for all instances of that particular group outcome, with bars showing standard error. Dashed lines show the grand mean behaviour on t+1 for those that produced (green) or scrounged (red) on trial t.

It was notable that in the high-cost condition, the mean producing behaviour on trials after zero producer group outcomes was higher than the mean across all outcomes for those that had scrounged (Figure 3.3). I tested for variations of each group against the overall mean for each condition-choice combination using sample-weighted linear contrasts of a fitted Anova model specific to that particular choice/condition combination e.g. high-cost/scrounge. This approach tests whether any given group significantly deviates from the overall mean level of production on the subsequent trial. The test confirmed that in the high- condition the tendency of players to produce was higher if they had just experienced a zero trial ($t = 6.20$, $p < .001$; Appendix 7).

There was also a main effect of trial within block ($OR = 0.90$, [0.87, 0.94], $z = -5.24$, $p < .001$), showing that participants became less likely to produce on successive trials within each block, although the extent of this decline was marginal ($r = -0.03$) and much less than seen in traditional zero-based PGG's where initial levels of cooperation typically drop from 50-60% down to 10-20% (Burton-Chellew & West, 2021; Chaudhuri, 2011; Ledyard, 1994).

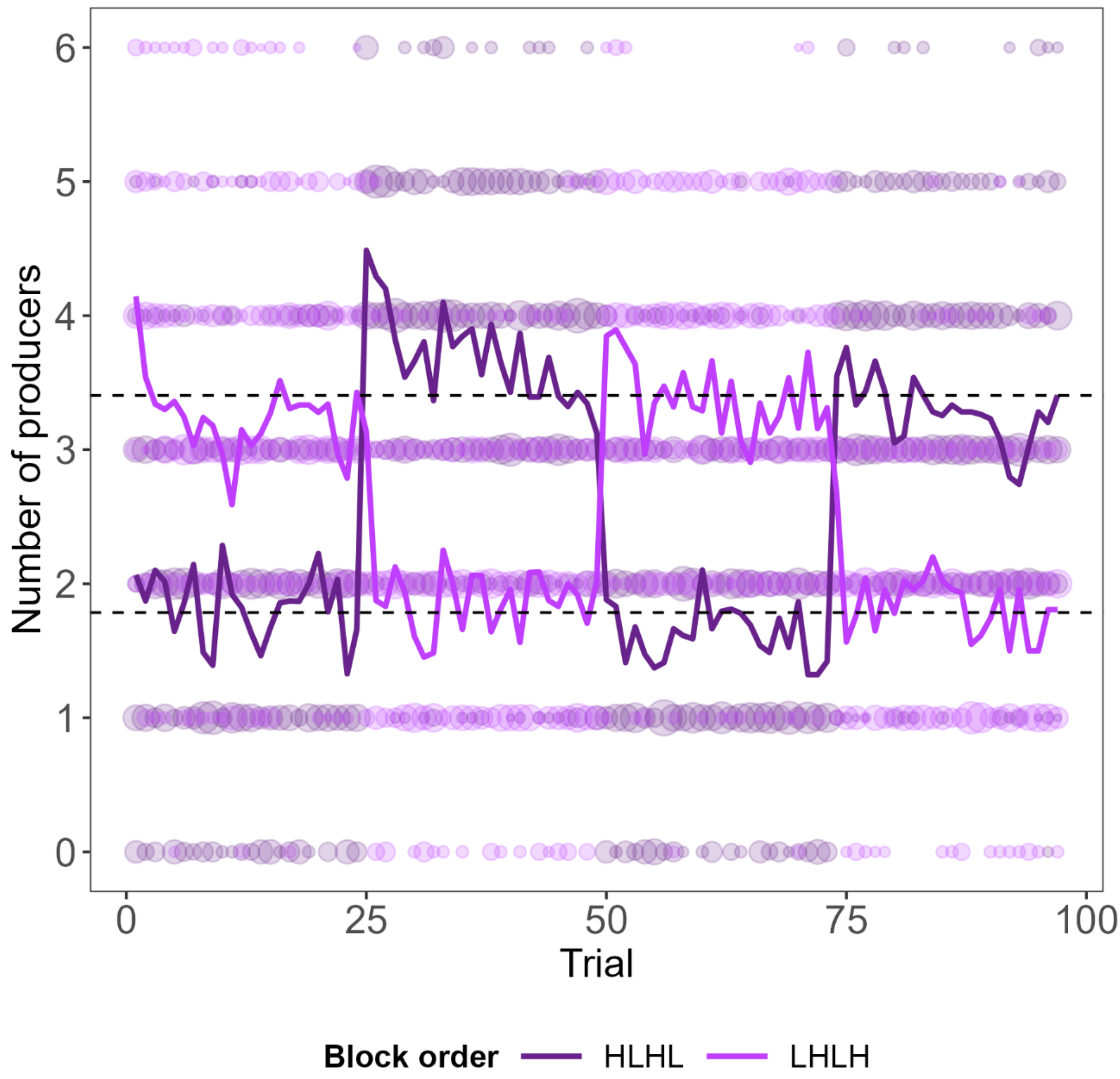

Figure 3.4 | Mean level of production across all participants per trial – sample 1
Dotted lines show the grand mean for each condition (High and Low-cost), and lines are coded according to the block order.

3.4.5 Task questionnaire

Having examined pre-registered hypotheses and behaviour versus benchmarks of prosociality, I looked at the questionnaire results to understand participants' strategies and approach to the game (Table 3.8). Each of these questions addressed a different aspect of the participant experience, scored on Likert scales. Participants reported relatively high motivation to maximise their own reward (M = 6.86, SD = 1.96), and indicated strong positive feelings when observing others search (M = 7.03, SD = 1.91). It was also notable that overall, people showed a bias towards cooperative rather than competitive play (M = 1.10, SD = 3.07, on a -5 to +5 scale). A one-sided t-test showed that the mean rating of cooperative play was significantly greater than zero ($t(205) = 5.1$, $p < .001$). Participants were moderately concerned with maximising group reward (M = 5.23, SD = 2.68), and avoiding the negative reward (M = 5.48, SD = 2.72). In terms of playing the game most people agreed to some extent that they were influenced by others' decisions (M = 5.07, SD = 2.71) and kept track of how others were playing (M = 5.35, SD = 2.78). There was the least agreement with the idea that they used their own decision to influence others (M = 4.01, SD = 2.83), tended to follow suit when others produced (M = 3.75, SD = 2.76), or that they felt others were reacting to their decisions (M = 3.91, SD = 2.59).

	Scale	**Mean**	**SD**
How did I feel when others searched	0-9	7.03	1.91
Extent I tried to maximise my reward	0-9	6.86	1.96
To what extent play game cooperatively (+ve) vs competitively (-ve)	-5 to +5	1.10	3.07
How concerned were you to avoid the zero berries outcome	0-9	5.48	2.72
Extent I tried to maximise group reward	0-9	5.23	2.68
What extent did you keep track of how others were playing	0-9	5.35	2.78
To what extent were you influenced by how others played	0-9	5.07	2.71
What was impact when others stayed	0-9	4.86	2.94
To what extent did you try to influence others	0-9	4.01	2.83
What extent did you feel others were reacting to the way you played	0-9	3.91	2.59
What was impact when others searched	0-9	3.75	2.76

Table 3.8 | Raw scores of task-based questionnaire – sample 1
Ranked in order from highest to lowest by mean. Cooperative/competitive is presented in correct rank order as if it were transformed to a 0-9 scale.

5 Replication sample

3.5.1 Pre-registered hypotheses

Next, I sought to replicate the findings in an independent online sample of participants. As with the first sample, a Shapiro test showed that the data for player behaviour was non-normal for low and high-cost conditions (W_{high} = 0.869, p < .001, W_{low} = 0.946, p < .001). It is important to note that overall levels of production were significantly lower in sample two (Sample 1; M = 0.433, SD = 0.496; Sample 2, M = 0.334, SD = 0.471) tested with between samples t-test based on average production levels per player (t(423) = 4.35, p < .001, r = 0.42). This was also true for each condition separately (t_{high}(421) = 3.55, p < .001, r = 0.34); t_{low}(424) = 3.85, p < .001, r = 0.37).

A paired Wilcoxon signed rank test showed that, as predicted, people chose to search more in the low-cost environment (W(234) = 2094, p < .001, r = 0.72) (Figure 3.5a). As with the first sample, the Wilcoxon test showed that player production was significantly above the pure Nash solution in high-cost, but was lower in the low-cost condition (W_{high}(234) = 16243, p = .016, r = 0.16; W_{low} = 11346, p = .034, r = 0.14).

3.5.2 Exploratory analyses – alternative benchmarks

I repeated the exploratory analyses using the mixed Nash, group optimality, ABM and prospect-theory adjusted Nash benchmarks. Using mixed Nash solution and found that results were fully consistent with the first sample in that production was higher than Nash in the high-cost condition and lower in low-cost (W_{high}(234) = 20649, p = < .001, r = 0.44; W_{low} = 5578, p = < .001, r = 0.52) (Figure 3.5b). There was a significant difference in producing behaviour versus Nash in high-cost versus low-cost [t(234, 234) = 25656, p < .001].

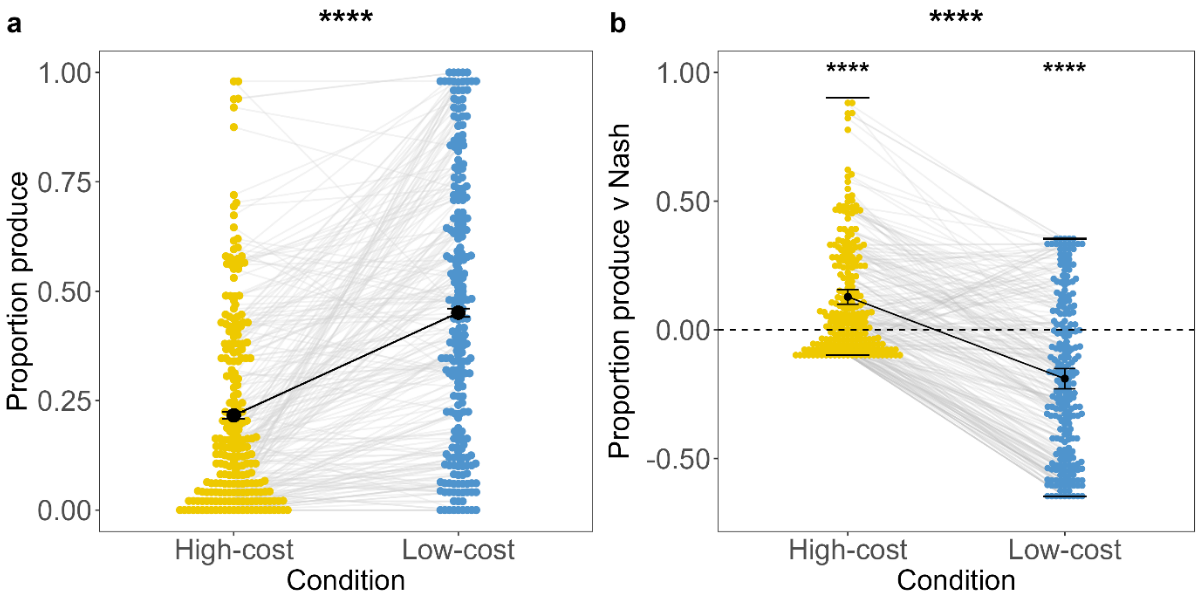

Figure 3.5 | Proportion of 'produce' trials per participant - sample 2
a. there was significantly more producing behaviour in low-cost compared to high-cost, [W(212, 212) = 1686, $p < .001$], **b.** producing was higher than Nash in the high-cost condition and lower in low-cost ($W_{high}(234) = 20649$, p = < .001, r = 0.44; $W_{low} = 5578$, p = < .001, r = 0.52). There was a significant difference in producing behaviour versus Nash in high-cost versus low-cost [t(234, 234) = 25656, $p < .001$]. Error bars represent 95% confidence interval.

In both conditions the actual mean producing was significantly lower than the point of maximum group return for each condition ($W_{high} = 1740$, $p < .001$, r = 0.76; $W_{low} = 0$, $p < .001$, r = 0.87), and was significantly lower than the ABM prediction ($W_{high} = 5559$, $p < .001$, r = 0.52; $W_{low} = 4957$, $p < .001$, r = 0.55). Against the prospect-theory adjusted Nash equilibria producing behaviour remained higher than the benchmark in the high-cost condition and lower in low cost ($W_{high} = 17116$, p = .001, r = 0.21; $W_{low} = 5158$, $p < .001$, r = 0.54) in line with the objective mixed Nash equilibria and consistent with sample one, though against typical heuristics the effect size in the high-cost condition was notably smaller (Cohen 2013).

	Pure NE	Mixed NE	Prospect theory NE	Group maximum	RL-ABM prediction
			High cost		
Mean actual producing v benchmark	+0.050	+0.118	+0.065	-0.284	-0.132
Statistic (W)	16243	20649	17116	1740	5559
p- value	.016	< .001	.001	< .001	< .001
Effect size (r)	0.11	0.44	0.21	0.76	0.52
			Low-cost		
Mean actual producing v benchmark	-0.049	-0.195	-0.207	-0.549	-0.121
Statistic (W)	11346	5578	5158	0	4957
p-value	.034	< .001	< .001	< .001	< .001
Effect size (r)	0.14	0.52	0.54	0.87	0.55

Table 3.9 | Summary of grand mean producing behaviour by condition – sample 2
*C*ompared to each specified benchmark*, t*able shows absolute difference between actual and benchmark plus the Wilcoxon signed rank test statistics with commensurate p value and effect size

Task questionnaire results (Table 3.10) replicated those of sample 1 very closely, with participants most focused on maximising their own reward (M = 7.14, SD = 2.01) and responding positively to others producing (M = 6.97, SD = 1.86). The one-sided t-test once again showed that the mean rating of cooperative play was significantly greater than zero (t(150) = 2.3, p = .020).

	Scale	**Mean**	**SD**
Extent I tried to maximise my reward	0-9	7.14	2.01
How did I feel when others searched	0-9	6.97	1.86
How concerned were you to avoid the zero berries outcome	0-9	5.68	2.82
To what extent play game cooperatively (+ve) vs competitively (-ve)	-5 to +5	0.63	3.32
What extent did you keep track of how others were playing	0-9	5.51	2.58
Extent I tried to maximise group reward	0-9	5.45	2.66
To what extent were you influenced by how others played	0-9	5.13	2.61
What was impact when others stayed	0-9	4.84	2.95
What extent did you feel others were reacting to the way you played	0-9	4.14	2.70
To what extent did you try to influence others	0-9	4.09	2.94
What was impact when others searched	0-9	3.53	2.94

Table 3.10 | Raw scores from task questionnaire - sample 2

3.5.3 Linear mixed models

I refitted the set of models previously evaluated on the original sample to the new data (Appendix 5) which showed the same winning model (Appendix 6). The interaction effect between condition and previous choice (OR = 1.83, [1.19, 2.81], z = 2.76, p = .006) was robust to replication, as were the main effects of incidence of producing in the low-cost versus high-cost condition (OR = 3.01 [2.32 3.89], z = 8.38, p < .001), and the decline in production per trial within block (OR = 0.97, [0.95, 0.97], z = -10.07, p < .001). though once more the decline in production per trial was marginal (r = -0.01; Figure 3.6).

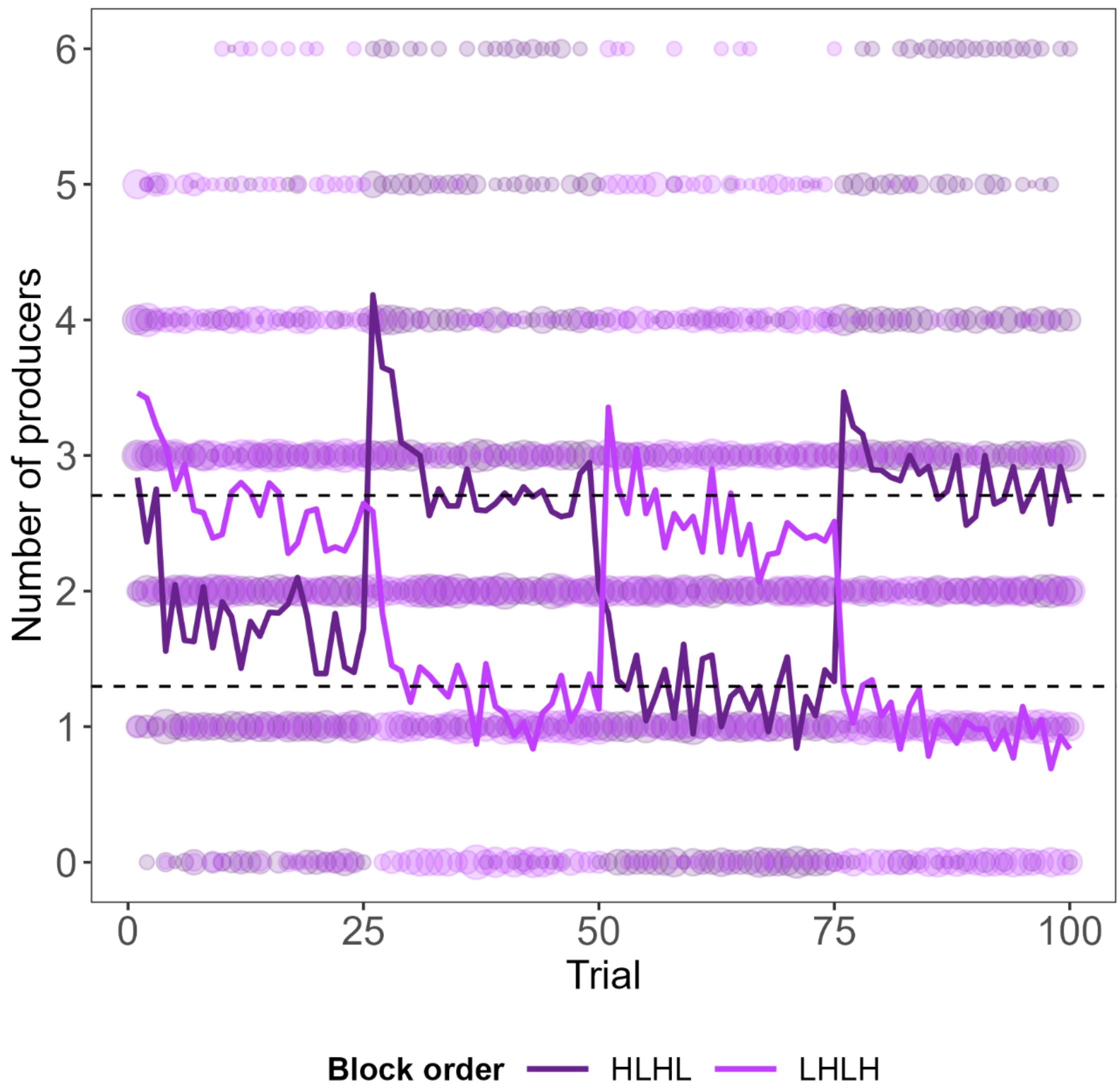

Figure 3.6 | Mean level of production across all participants per trial – sample 2
Dotted lines show the grand mean for each condition (High and Low-cost), and lines are coded according to the block order.

The effects were visualised in the choice data conditioned on choice and group outcome (Figure 3.7). In this sample, there was no apparent increase in production after zero-producer trials. In fact, as can be observed from Figure 3.7 production was slightly below the mean in this case (t = -2.87, p = .024, d = -0.05).

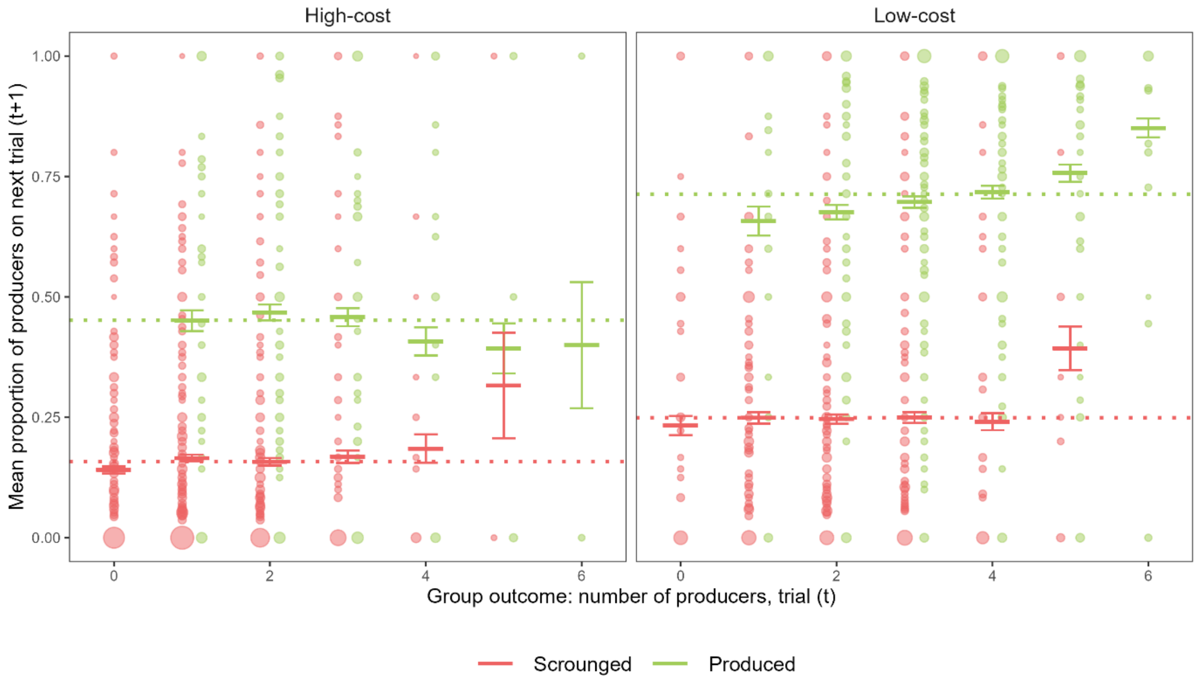

Figure 3.7 | Participant mean probability of producing on trial t + 1, depending on number of producers on trial t – sample 2
Points represent the mean of all individual choices across the relevant trials for each combination of choice on trial t and group outcome, with size proportionate to incidence in each case. Solid horizontal lines show the mean producing behaviour on trial t+1 for all instances of that particular group outcome, with bars showing standard error. Dashed lines show the grand mean behaviour on t+1 for those that produced (green) or scrounged (red) on trial t.

6 Results across both samples

Results from the original and replication samples showed a number of important similarities, in particular that people produced more in the low-cost condition, and that producing versus the (pure and mixed) Nash equilibria was higher in the high-cost condition compared to the low-cost condition. However, there was one key difference in the results between the two samples. In sample one, the low-cost condition players produced at a level higher than pure Nash, whereas in the second sample, players produced at a level lower than the pure Nash equilibrium. By combining the samples and repeating this analyses I obtained a definitive answer to this discrepancy and also analysed participant level influences on choice behaviour.

A Wilcoxon signed rank test showed people chose to search more in the low-cost environment ($W(448, 448) = 6778$, $p < .001$, $r = 0.73$) (Figure 3.8a). Across both samples, for the pre-registered pure Nash solution player producing was significantly higher than the NE in high-cost, but not significantly different from the NE in the low-cost environment ($W_{high}(448) = 68139$, $p < .001$, $r = 0.31$; $W_{low}(448) = 50544$, $p = .498$). As expected versus the mixed Nash optimality point, producing was higher in high-cost, but lower in low-cost ($W_{high}(448) = 81557$, $p < .001$, $r = 0.54$; $W_{low}(448) = 28660$, $p = < .001$, $r = 0.37$; Figure 3.8). The difference in the average producing behaviour versus (mixed) Nash between high- versus low-cost was also significant ($W(448, 448) = 151895$, $p = < .001$, $r = 0.55$).

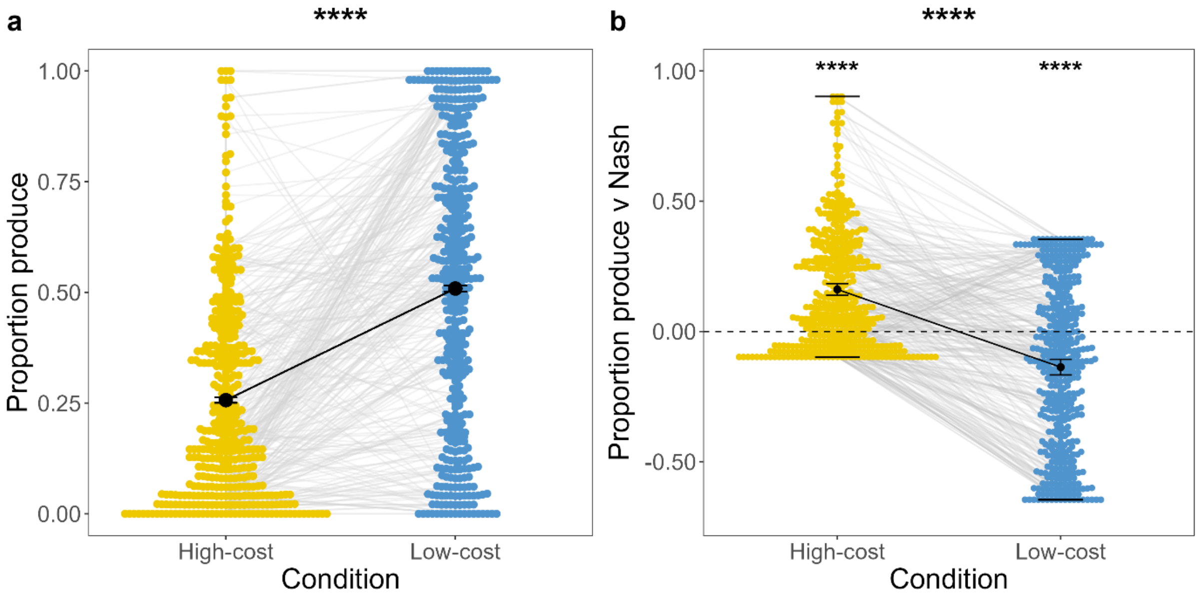

Figure 3.8 | Proportion of 'produce' trials per participant – samples combined
a. there was significantly more producing behaviour under low-cost compared to high-cost, ($W(448, 448) = 6778$, $p < .001$, $r = 0.73$) **b.** producing behaviour was higher than the optimal (mixed) Nash equilibrium in high-cost, but lower in low-cost (($W_{high}(448) = 81557$, $p < .001$, $r = 0.54$; $W_{low}(448) = 28660$, $p = < .001$, $r = 0.37$). The difference in the average producing behaviour versus (mixed) Nash was significant in high-cost versus low-cost ($W(448, 448) = 151895$, $p = < .001$, $r = 0.55$).

The differences against the other benchmarks were as follows (Figure 3.9): actual mean producing was significantly lower than the point of maximum group return for each condition ($W_{high} = 9219$, $p < .001$, $r = 0.71$; $W_{low} = 0$, $p < .001$, $r = 0.87$), and was significantly lower than the ABM prediction ($W_{high} = 27250$, $p < .001$, $r = 0.40$; $W_{low} = 26253$, $p < .001$, r

= 0.41). Against the prospect-theory adjusted Nash equilibria producing behaviour remained higher than the benchmark in the high-cost condition and lower in low cost (W_{high} = 71194, $p < .001$, r = 0.36; W_{low} = 27067, $p < .001$, r = 0.40).

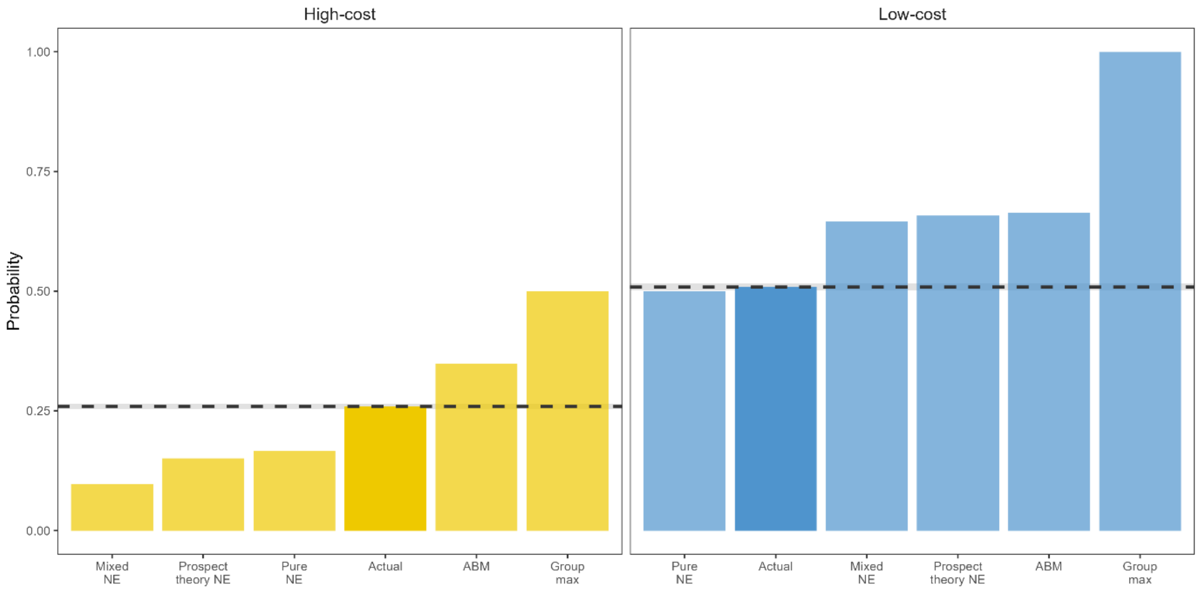

Figure 3.9 | Comparison of actual behaviour across all trials, per condition – samples combined
*M*ean producing shown by the dashed line against each benchmark for the combined samples. Grey shaded areas represents 95% CI for the grand mean.

One other key difference between the samples, was that the winning model fit in sample two showed a main effect of previous trial choice that was not present in sample one. I applied the same model previously fitted onto the individual samples onto the combined dataset. This revealed the same main effects of higher production in the low-cost condition (OR = 2.64, [2.16, 3.23], z = 9.54, $p < .001$), and of the decrease in producing period by period over the course of a trial block (OR = 0.86, [0.84, 0.88], z = -10.81, $p < .001$), as well as the positive interaction effect between condition and previous choice (OR = 2.38, [1.69, 3.34], z = 5.00, $p < .001$). These effects were common to both samples and hence to the combined sample. The combined sample did however also confirm the main effect of previous choice present in the model fit of sample two (OR = 1.57, [1.23, 2.00], z = 3.60, $p < .001$). In addition there was a negative interaction effect of previous trial group outcome and previous trial choice (OR = 0.92, [0.85, 1.00], z = -2.06, p = .039), although the size of this effect (r = -0.023) was negligible by conventional standards (H. Chen, Cohen, and Chen 2010).

3.6.1 Individual differences

Across both studies, aggregate levels of production were higher than the Nash equilibria in the high-cost condition, and lower than or equal to the Nash equilibria in the low-cost condition. However, behavioural responses displayed substantial inter-individual variability, with some participants behaving in a consistently selfish manner and others consistently aligning with (or exceeding) the selfish equilibrium prediction (Figure 3.10).

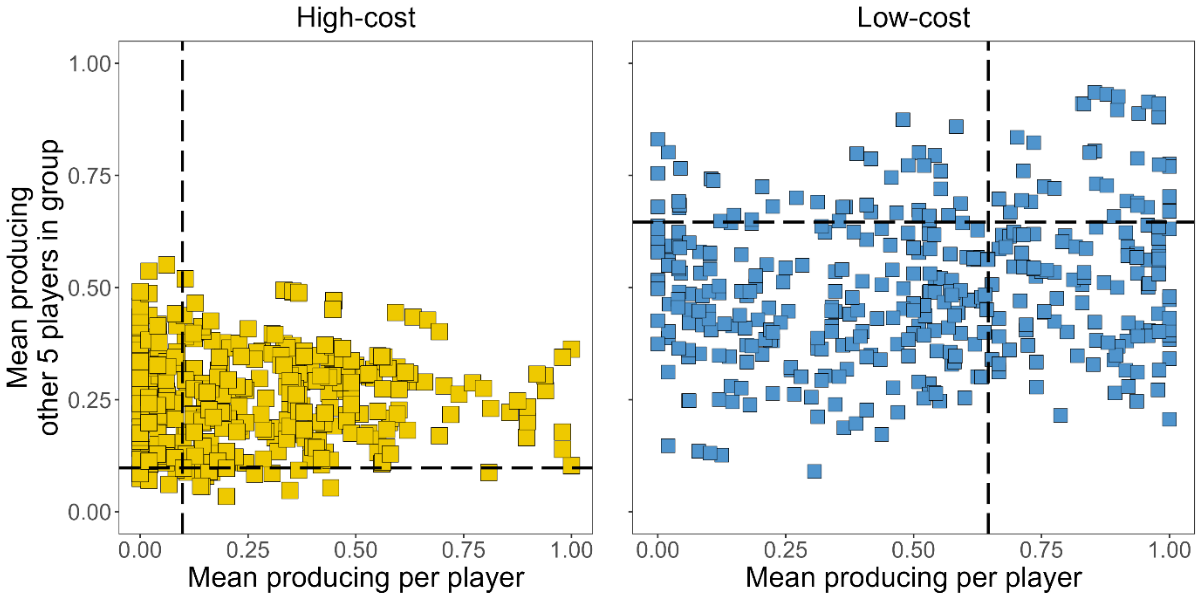

Figure 3.10 | Intra-individual variation in producing levels – samples combined
Plots show mean producing proportion for each player across both experimental samples on the x-axis with the mean procuring behaviour of the rest of their group on the axis, split by condition. Dotted lines show the mixed Nash equilibrium for each condition.

Combining the samples was helpful in maximising statistical power for the participant level analyses. In order to examine top-line relationships in the data I first created correlation matrices for each set of participant level variables: (i) task questionnaires, (ii) psychological traits and (iii) financial/income variables. Each of these sets was correlated at an item level, using the Benjamini–Hochberg FDR correction across all correlation tests, along with four measures of mean producing behaviour: overall, for high and low-cost conditions separately and the difference between conditions.

3.6.2 Task questionnaire

The correlation matrix revealed that many of the eleven task-related questionnaire results had significant relationships with behaviour (Appendix 8), and that there were also multiple significant relationships between the task variables themselves. To make the data more interpretable, I conducted an exploratory factor analysis (orthogonal 'varimax' rotation, R 'stats' package) on the task variables.

First, I established the number of factors which would best explain the data. I found three eigenvalues greater than 1: 3.57, 1.61 and 1.10. Using the Kaiser criterion (eigenvalues > 1) would therefore suggest a three-factor solution, although the third factor was close to the cut-off. Because of the closeness of the third eigenvalue to one, I compared this to a two-factor solution (Appendix 9). The two-factor solution explained cumulative variance of 37%, compared to the three-factor solution 42%. Given this and the fact that the third factor was biased heavily in its factor loadings to a single item, the two-factor solution was chosen.

In analysing the factor loadings, there are no definitive rules about what constitutes significance (Howard, 2016) but based on the various heuristics and our sample size, I applied a filter of 0.6 or above, representing a strong association, 0.4 – 0.6 as a moderate association. The first factor was strongly correlated with maximising group reward and playing co-operatively, whilst moderately correlated with concern for zero outcome, and maximising personal reward (as a negative relationship). I termed this factor "Group-focused co-operation". The second factor loaded strongly onto being influenced by how others played and keeping track of others' play, and trying to influence others, whilst moderately loading on feeling like others were reacting to their decisions and being concerned to avoid zero berries. I termed this factor "Engaged influence".

COMBINED SAMPLE	Factor1: Group focused cooperation	Factor2: Engaged influence
Extent I tried to maximise my reward	**-0.565**	-
Extent I tried to maximise group reward	**0.756**	0.294
How did I feel when others searched	-	0.136
How concerned were you to avoid the zero berries outcome	**0.401**	**0.455**
To what extent play game cooperatively (+ve) vs competitively (-ve)	**0.823**	0.142
To what extent were you influenced by how others played	0.238	**0.637**
What was impact when others searched	0.398	0.126
What was impact when others stayed	**0.439**	0.168
To what extent did you try to influence others	0.220	**0.649**
What extent did you feel others were reacting to the way you played	-	**0.495**
What extent did you keep track of how others were playing	-	**0.681**
Proportion of variance	0.203	0.163
Cumulative variance	0.203	0.366

Table 3.11 | Factor loadings of task questionnaire items - samples combined
Moderate (0.4 – 0.6) and high (> 0.6) factor loadings are shown in bold.

This factorisation was then applied at an individual level. Individual level factor scores represent how strongly each person expresses the latent factors identified in the EFA. Higher scores indicate stronger alignment with the patterns of responses that define that factor. Thus, I could correlate the individual factor scores such as the extent to which someone was a 'group-focused cooperator', with traits and behaviours.

I examined individual level correlations between the factor scores representing task orientation, with behavioural variables such as the overall level of production and reward. Once again, significant correlations were measured using the Benjamini–Hochberg FDR correction ($p < .05$). There were significant correlations between the group-focused cooperation factor and overall mean producing levels including both conditions individually ($r = 0.64$, $p < .001$; $r_{high} = 0.62$, $p < .001$; $r_{low} = 0.50$, $p < .001$), but no significant correlations with the engaged influencer factor. This result indicated that the group-focused cooperation factor was predictive of overall production, suggesting that individual differences might be systematically important. To test how stable individual behaviour was across blocks, I ranked each player's producing behaviour within the eight block-condition combinations as a percentile and an intraclass correlation coefficient (ICC) was calculated to assess the consistency of participants' percentile ranks across the four blocks. The ICC indicated moderate consistency (ICC(C,1) = .53, 95% CI [.48, .58], $F(436, 1311) = 5.52$, $p < .001$; Cicchetti 1994).

3.6.3 Zero aversion

Given the over-production that was evident in the high-cost condition, I wanted to understand the extent to which an aversion to the zero-producer outcome was driving behaviour. The post-task question 'While playing, how concerned were you to avoid the situation where nobody searched and so everybody lost berries?' probed this facet of participant choice. Answers to this question were significantly correlated with overall producing behaviour ($r = 0.26$, $p < .001$) and in high- and low-cost conditions separately ($r_{high} = 0.23$, $p < .001$; $r_{low} = 0.22$, $p < .001$). We also observed that the incidence of group failure, defined as when a trial when zero group members chose to produce and therefore everyone lost berries, was higher in the high-cost condition ($X^2(1, 8504) = 380$, $p <.001$) (Figure 3.11).

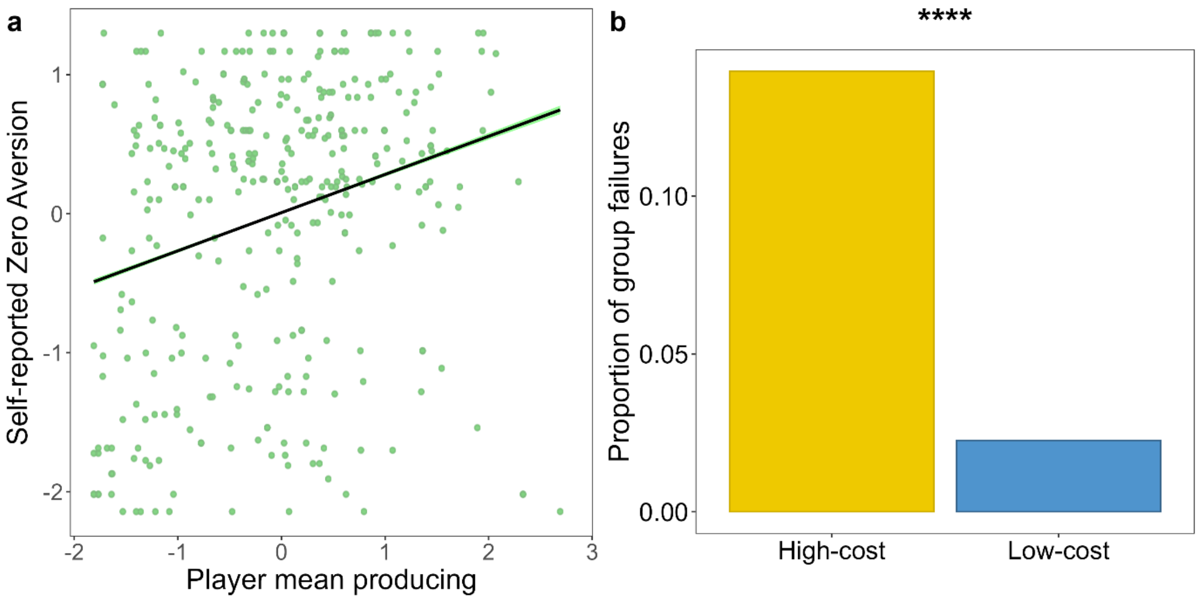

Figure 3.11 | Zero aversion in high- v low-cost conditions – samples combined
Panel (a) shows a scatterplot of and best fit line of self-reported aversion to the zero-producer outcome and overall producing behaviour ($r = 0.26$, $p < .001$). Panel (b) shows the difference in count between instances of zero producer outcomes per condition over all trials per group, chi-squared proportion test showed that the difference was significant [$X^2(1, 8504) = 380$, $p <.001$].

I then tested to find whether the self-reported zero aversion was a factor in predicting the extent to which participants searched after zero-producer group outcomes on a trial-by-trial basis. To do this, I fitted a linear regression model predicting the mean producing behaviour of participants from the interaction of condition and self-reported zero-aversion, filtering the data on zero-producer trials only. This showed that where there was a behavioural effect of zero-producer trials in sample one, this was significantly correlated with self-reported zero aversion ($t(555) = 3.43$, $p < .001$). However, this effect was absent in sample two, where, as previously shown, there was no overall behavioural effect following zero-producer trials.

I then repeated the correlation tests of behavioural data against financial measures and trait-based psychological traits.

3.6.4 Psychological traits

I examined the relationships between choices to produce and psychological traits. There were no significant relationships with empathy, prosociality, group affiliation or psychopathy on overall production, or with production in high- or low-cost conditions separately, with one exception (Appendix 8). There was a small, but significant *negative* correlation between the Prosocialness Scale in Adults and producing behaviour in the low-cost condition only ($r = -0.13$, $p = .013$). Bayes factors were calculated for all the null results to determine the strength of evidence in favour of no effect. This showed moderate support for the null (ie. no relationship with overall level of producing) with prosociality (BF = 0.16), empathy (BF = 0.15), and psychopathy (BF = 0.13) and strong support for no relationship with group affiliation (BF = 0.07). There were significant, but small correlations with the difference between participants' low- and high-cost producing levels with prosociality ($r = -0.18$, $p < .001$), group affiliation ($r = -0.15$, $p = .003$) and psychopathy ($r = 0.13$, $p = .011$).

As a way of assessing whether or not the threat of zero-producer trials was related to people's risk perception, risk preference was also measured just in sample two using the DOSPERT risk attitude scale (Blais & Weber, 2006). There were no significant correlations between behaviour and risk preference, nor any of its subscales (Appendix 8). Bayes showed moderate support for the null hypothesis (BF = 0.12).

3.6.5 Financial measures

Behavioural measures of production were correlated against personal income, precarity, socio-economic status and financial wellbeing across the combined samples, but once more no significant correlations were present (Appendix 8). Bayes factors showed strong support for no relationship with overall levels of production and personal income (BF = 0.08), precarity (BF = 0.08), SES (BF = 0.07) and financial wellbeing (BF = 0.07).

7 Discussion

The present study examined the decision-making of humans in a group context when acting in a social dilemma based on the Producer-Scrounger model of animal foraging. In these studies, it is apparent that the structure of the data reveals different aspects of prosocial behaviour at an aggregate level compared to an individual level, therefore I have split the discussion into two parts.

3.7.1 Aggregate level effects

The key measure in this task was how levels of production compared to the mathematically optimal solution ie. the Nash equilibrium. Choices to produce up to the Nash equilibrium point yield incremental reward for both self and other, and were thus cooperative in nature. Production levels above the NE benefit the group at a cost to self, are thus unambiguously prosocial (ie. they are not conflated with selfish reward) and may be described as altruistic. Both studies confirmed that participants produced more frequently when the personal benefit of doing so was higher. This shows that participants responded to the different equilibria of the payoff matrices, making choices on the basis of their own reward as would be expected from rational choice theory, and is in line with existing producer-scrounger models and experimental findings (Afshar & Giraldeau, 2014). We cannot directly measure prosocial versus selfish motivation in this task, but we can infer that selfish reward motivations were dominant from the main experimental manipulation as production levels increased in line with increased rewards to self, even though the net reward for the group was lower. This is in keeping with studies which show that people will generally prioritise their own interests over those of others, learning faster and making greater effort (Lockwood et al., 2016, 2017). Thus, the difference between the aggregate production levels in the low- versus high-cost condition can most simply be explained by sensitivity to personal reward, though it is likely that some level of other-focused motivation was present. Participants were all told in the task instructions that producing benefited the group, and the strong correlation of the group-focused cooperator factor and production across the board suggests an awareness of the impact of those decisions, at least in retrospect.

In order to understand this further, I compared behaviour to several benchmarks, each providing a different account of optimal behaviour in the task. The mixed Nash solution is arguably a more valid benchmark than the pure Nash because it is closer to how participants behave (i.e. probabilistically), and due to the indeterminacy resulting from the non-unique pure Nash solutions in the way the game was presented to participants. Using this benchmark, altruistic behaviour was absent in the low-cost condition and in fact, production levels were below the mixed NE, showing that participants were not even cooperating at an optimal level. There was, however, *apparent* evidence of altruistic behaviour in the high-cost condition, in that mean production was above the mixed Nash solution across both samples. The lack of association and Bayes factor scores of known trait-based correlates of prosociality with individual levels of production suggests that over-production may not be driven by psychologically motivated prosociality or related phenomena such as empathy or group affiliation. In order to establish whether prosocial behaviour was present in the high-cost condition, I consider several possible alternative explanations.

The apparently counter-intuitive result, that there was a greater level of prosociality when its' cost was higher, could potentially be accounted for by the fact that in the high-cost condition the optimal number of producers was close to zero, but that this was not the case in the low-cost condition. One possible explanation might be if participants were *disproportionately* averse to the zero-producer outcome, compared to just the negative reward. Such biased weighting of losses would be in line with the well-established findings of prospect theory (Kahneman and Tversky 1979) as well as findings that financial shocks can cause a temporary increase in loss aversion, particularly when it is perceived to be the result of one's own decision (Pammi et al. 2017). The task included a direct measure of self-reported zero-aversion, which correlated with overall levels of production in both samples in both high- and low-cost conditions. In the case of sample one, results also showed that participants were more likely to produce on the trial immediately following a zero-producer outcome, and that this was directly related to their level of zero aversion. This suggests that indeed participants were averse to this condition, though it should be noted that in the replication sample, where overall levels of production were lower, there was no increase in production following zero-producer trials.

Comparison with both the group-optimal and the agent-based model benchmarks provided useful comparisons. Actual production levels were significantly lower in both cases across both conditions. What can we interpret from this? In the case of the group-optimality benchmark, we can say that as a group people were not generating the highest possible return. This is very common in humans (Alberti, Cartwright, and Cartwright 2021), and reflects a generalised preference to focus on personal rather than collective returns. This helps to frame conclusions about the extent of prosociality in the task in the sense that this is the most prosocial outcome available to participants, and therefore is a useful comparison compared to Nash, which in a sense represents a lower bound to altruistic behaviour at least. Given previous ecological validation (Afshar & Giraldeau, 2014) the ABM results in particular highlight that a simple reinforcement learning model predicts over-production versus Nash. It seems that learning steers choices away from those that result in negative outcomes towards safer, more reliable choices which, whilst less than optimal for the individual, produce reliably positive rewards. This is interesting in the sense that it shows a mechanism, well-established in human decision-making, that results in a level of apparent altruism, though it is not driven by any kind of prosocial motive. Previous analysis on iterated PGGs has shown that simple reinforcement learning models fit human behaviour well, outperforming such models with loss aversion and other strategies such as fictitious play (Cotla 2015).

Thus, the evidence for prosociality at an aggregate level seems unconvincing. There is some cooperative behaviour in evidence, but its motivations (self v other) are unknown and, as others have pointed out, *any* level of cooperation should not necessarily be taken as meaningful (Schäffer et al., 2025). I have also argued that the apparent altruism in the high-cost condition was driven by aversion to the zero-producer outcome. However, there was a great deal of variation in behaviour at an individual level, from indisputable prosociality to extreme selfishness.

3.7.2 Individual differences

At an individual level, choices to produce when below the NE choices benefit *both* the individual and the group, so these choices could be motivated by a mixture of benefits to the self and others. It is not possible to directly measure these motivations in a purely behavioural study such as this (though this could be done in future studies, by detecting self-versus-other neural responses to reward using fMRI, for example). Interestingly, in relation to social preference theory, there were no significant positive relationships between overall producing levels and the traits of prosociality, empathy, group affiliation, or psychopathy, with Bayesian evidence showing moderate to strong support for no relationship. This is in contrast to previous findings that personality traits can reliably predict prosocial behaviour (Balliet et al., 2009; Thielmann et al., 2020), but as outlined in the general introduction, this finding should not be over-interpreted on its own and instead considered alongside other evidence.

The EFA of the task-based questionnaires did reveal a relationship between the prevalence of the ‘group-focused cooperation’ factor and behaviour, suggesting that those who prioritised group over self-reward and cooperative over competitive game play produced more often. Despite the lack of trait-based evidence, this suggests that some level of intentional prosociality is influencing decision-making at an individual level, consistent with previous findings (Balliet et al., 2009; Fischbacher et al., 2001; Thielmann et al., 2020). However, this evidence should be interpreted cautiously for two reasons. First, the EFA-derived factor has not been externally validated and may partly reflect post-hoc self-justification or context-specific reasoning rather than a stable underlying construct. Second, although there were detectable individual differences in behaviour, the block-level consistency was only moderate, indicating that while there is some stable variance, a substantial proportion of within-person variability remains unexplained. Future studies could explore this by repeating the experiment, testing behavioural plasticity (Kim et al., 2019), or by including some other task to externally validate prosociality.

3.7.3 Conclusions and future research

In summary, this task reveals several interesting features of prosocial decision-making. Overall, at an aggregate level, I differentiate between two forms of prosociality – cooperation and altruism. This delineation is in line with what we know about the neural and behavioural basis of prosocial decision-making (Böckler et al., 2016; Rhoads et al., 2021; Wu & Hong, 2022). Across both studies, the existence of altruism at an aggregate level is highly questionable, and the frequency of decisions to produce can be explained by a combination of self-interest and aversion to negative reward, rather than any prosocial motive. At the individual level, however, behaviour is highly heterogeneous, with some individuals demonstrating altruistic prosociality while others behave in line with (or even more extremely than) selfish predictions. Any decision to produce could be classified as prosocial in the sense that it is cooperative, and this is supported to some extent by the relationship between producing and self-reported attitudes in the post-task questionnaire. However, cooperation can be considered a relatively weak form of prosociality when, as in this case, it is wholly conflated with reward for self, so it is difficult to draw any firm conclusions about whether or not this behaviour is meaningfully prosocial. As a result, we should be healthily sceptical of simplistic measurements that allocate any deviations from 'always defect' optimality to the construct of prosociality as is used in classic economic games (Chaudhuri, 2011; Engel, 2011; Tisserand, 2014). There are good reasons to think carefully about the existence or magnitude of deliberate prosociality in these experiments, a subject to which I return in the general discussion.

This study has several strengths and limitations. There are relatively few group-based human studies on prosocial behavior with most experiments conducted on individuals or dyads (Penner et al., 2005). Often, group-based behaviour is created with simulated responses, which leads to questionable validity. Thus, having a group-based task with all human respondents conducted live with simultaneous and dynamic decision-making over a large sample size, such as this, gives a unique new dataset with which to analyse prosociality. In the behavioural analysis, I have used multiple benchmarks which, it could be argued, risk ambiguity in interpretation as conclusions are different depending on the benchmark. There is no single game theoretic solution concept, and different solutions can be applied to the same problem (Albrecht et al., 2021). Although a formal multiverse analysis would require systematic variation across all analytic decisions (Mazei et al. 2025) I adopted a conceptual analogue to examine whether interpretations of prosociality are robust across several distinct game-theoretic benchmarks.

Another strength of this study is that I ran a full replication to corroborate the effects, which showed consistency across the key findings regarding the levels of prosociality versus the benchmarks. The modelling results showed consistent effects for condition and the interaction effect of previous choice and condition. There were some differences between the two samples, however, and the second sample overall showed lower levels of producing behaviour, even to the extent of negative effects on participant reward. This is puzzling but suggests that perhaps the incentives on offer were not fully sufficient to motivate behaviour, or possibly even that continued scrounging was being used by some as an ad-hoc costly punishment mechanism for non-cooperators. These are hypotheses that could be addressed in future studies.

Other possible future developments could be to focus more explicitly on what drives individual differences, and the extent to which these are situational or have some degree of plasticity depending on the behaviour of the rest of the group – would pro-selfs change to being more prosocial if the situation demands it, as has been shown in other species (Reichert et al. 2021) and to some extent in humans (Baader et al., 2024)? It would also be interesting to rigorously test whether the over-production in the high-cost scenario contains any element of motivated prosociality at all, which could be done using the ‘black box’ method (Burton-Chellew & West, 2022).

Following the successful establishment of this new task, I sought to validate the findings by attempting to replicate them using an in-person rather than online version of the paradigm. The measurement of prosocial behaviour in human participants is highly sensitive to social or contextual clues, which may not be a part of the explicit experimental design, such as whether they are in the presence of other participants (Haley & Fessler, 2005; Zizzo, 2010). I created a pre-registered (As Predicted #220,880) single participant in-person version of the original task. This allowed the experimental design to be adjusted so it could be used with other neuroimaging or in-person modalities, where testing six people in parallel can be practically demanding. It also provided a further opportunity to replicate the findings and test whether in-person settings affected prosociality.

Chapter 4
Replicating effects in-person

Prosocial behaviours are highly sensitive to social contexts in humans and subtle experimental cues. Building on my previous experiments, I tested if these online results could be replicated with an in-person version of the producer-scrounger 'desert island' task, which would also serve as a precursor to future fMRI work.

1 Introduction

In this study, I recreated the producer-scrounger task, but adapted it to work with a single in-person participant with simulated responses for the other five participants. This enabled it to be run efficiently, as it required only one person for each experimental session rather than six. This served as a replication as well as testing of the previously observed behaviours in a social context where the experimenter and other human participants were present.

Behaviour in social decision-making tasks may be influenced by both experimenter demand and social cue effects, which can confound inferences about genuine prosocial motivation. Work on demand characteristics (Haley & Fessler, 2005; Orne, 1962; Rosnow & Rosenthal, 1997) has demonstrated that participants often attempt to behave in line with perceived expectations. In economic games, such effects can manifest as prosocial behaviour. Subtle changes in instructions or perceived goals can inflate cooperation or generosity (de Quidt et al., 2018; Zizzo, 2010), and participants' prosociality may be motivated by the presence of the experimenter as well as other players (Fleming and Zizzo 2013). In group-based social dilemmas, observability and reputational considerations have been shown to increase cooperative contributions (Rege and Telle 2004; Andreoni and Petrie 2004), and explicit social norms can influence expressed preferences (Kimbrough & Vostroknutov, 2013). Together, these findings indicate that apparent prosociality in laboratory settings may partly reflect sensitivity to experimenter expectations or perceived social monitoring, rather than intrinsic prosocial motivations.

My pre-registered hypotheses (As Predicted #220,880) were based on the previous results, namely that we would see

1. More production in low-cost than high-cost
2. Over-production versus (pure) Nash in high-cost but not in low-cost, and
3. Greater production versus Nash in high-cost when compared to low-cost.

In addition, in preparation for future possible fMRI studies, the visual display sequencing was adapted to temporally differentiate neural responses to group outcome and personal reward so that these could be analysed separately (see 'Method' below).

2 Method

4.2.1 Recruitment and sample

Thirty-nine 18-35 year-old adults were recruited through university participant mailing lists and Facebook advertisements. Other than age, inclusion criteria were English-speaking, with no previous or current neurological or psychiatric disorder, and that they were not currently, or had not previously, studied psychology. The latter criterion was employed as previous experience of psychological research could have compromised participants' belief in the deception. The final sample consisted of 18 males and 21 females, with a mean age of 21 (SD +/- 4); self-reported ethnicity 18 White (European/US), and 12 Asian (Chinese/Indian subcontinent); 32 were students, and 12 were in employment.

Participants were reimbursed at a rate of £10 per hour, plus up to £3 bonus depending on task performance (number of berries collected), with most experimental sessions taking 75 - 90 minutes. All participants completed the full experiment, and there were no exclusions.

The study was approved by the University of Birmingham STEM Review Committee, approval number: ERN_20-1897AP10.

4.2.2 Procedure

For this task, we used a deception to make participants believe they were playing with other live players, rather than with a computer simulation. Participants were recruited on the premise that they were taking part in a group decision-making task. Upon arrival, they were brought to a large computer laboratory and seated at individual desks. They were informed that they would first complete an instruction and practice phase, then they would enter the main task, running on a local network, where they would be put into randomised and anonymous groups of six players, with the final stage being to complete a series of questionnaires. They were also informed that there were other players in a different room, and were asked to confirm that they were in the correct room, to add credibility to the deception. The 'other room' deception was necessary to avoid participants questioning the task if they noticed that there was not a perfect multiple of six players in the room.

As per the online study, the instructions and practice phase included the three comprehension check questions, which had to be answered correctly to progress. After successfully completing this, the participants began the main task. To begin with, they saw a series of screens to add further credence to the deception. The first was designed to look like a computer operating system, taking several seconds to connect to a network, and then once this had completed, a ‘lobby’ screen which counted down how many more participants were required to complete the group, to simulate others joining the game. This mimicked the experience of the online players, with randomised delays to simulate real joining behaviour. After these screens, the participants saw a screen informing them that their group was full and they could begin the main task.

After the main task, they were directed to a web browser to complete questionnaires on the Qualtrics platform. They had been told that their bonus payment depended on the number of berries they collected, although for ease of administration, all participants were all paid the full £3 bonus. Payments were handed out individually in sealed envelopes, with participants leaving the experimental sessions at different times depending on when they finished to eliminate the possibility that they would compare bonus amounts.

4.2.3 Task

After being matched into a group, participants completed the same real-time decision-making game as in **Chapter 3**. To recap, this was a form of producer-scrounger paradigm gamified as a group-based foraging task (Figure 4.1). Participants were instructed that they were stranded on a desert island with five other people, and to survive, they needed to collect berries. Each day (trial), they had to choose whether to ‘search’ for berries to be shared amongst the group (produce), or ‘stay’ at the camp (scrounge) and benefit from the berries that other members of the group collected. Participants completed the task in two different environments, summer (high-cost of production) and winter (low-cost of production).

The costs and rewards of producing and scrounging in each condition were identical to those in **Chapter 3** (Table 3.5), setting up the same Nash equilibria.

The task consisted of 140 trials, split into four blocks of 35 trials per block, alternating between high- and low-cost, and counterbalanced for order effects. Each block was separated by a break of 30 seconds, with the subsequent block condition (Summer/Winter) being cued at this stage. In this version of the task, the outcome of each trial was separated into a group outcome screen (how many others had produced), and a reward screen showing how many berries the participant had gained/lost on that trial (Figure 4.1).

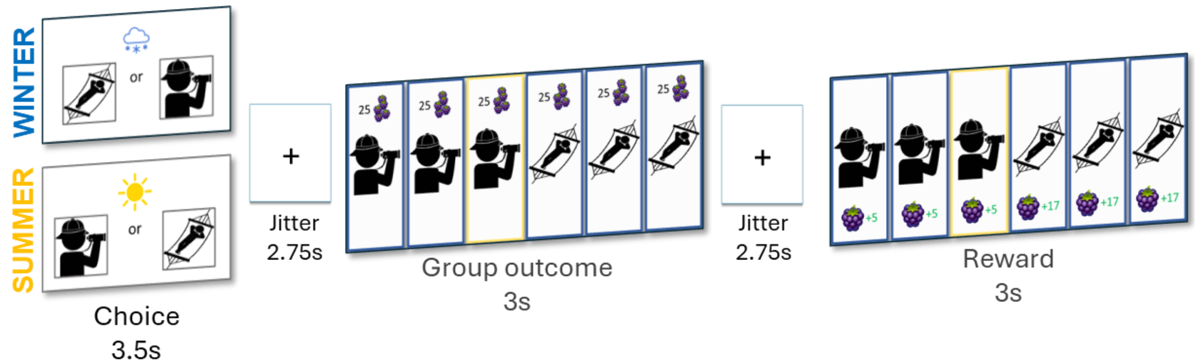

Figure 4.1 | Producer-Scrounger in-person task
Participants were presented with a scenario in which they were stranded on a desert island and each day had to choose whether to 'search' for berries (produce) to be shared amongst all group members, or 'stay' at the camp, which generated no berries but came at a lower metabolic cost. Choice, outcome and reward screens were separated by a jittered fixation cross to disaggregate the neural responses to each stimulus screen.

Each stimulus presentation was jittered using a truncated gamma distribution applied over a range of 2-8 seconds with a mean of 2.75 seconds. Anticipating any future fMRI analysis, we carried out a pre-emptive check on the design matrix of this task structure to ensure that our parameters would not be overly correlated.

On each trial, the choices of the other five participants were simulated by computer using the following procedure.

1. For each block/condition combination (e.g. high-cost / block 1, low-cost / block 2), a discrete probability distribution was calculated based on the combined online samples such that each outcome of (0-5) producers had a fixed probability

2. A random sample was drawn from this distribution for each of the 35 trials within each block/condition combination, and this provided the simulated response for the 'other' players
3. This procedure was repeated three times, to give four pseudo-randomised sets of simulated data, which were then used to run the experiment

In this way, the simulated responses were fixed and did not change based on the live player's choices, other than in one specific circumstance. Based on our experience with the online task, we noticed that some participants continually scrounged for all or almost all trials. This could have been problematic for some analyses, as it would prevent us from comparing produce and scrounge decisions and would also mean that the trial-by-trial parameters from computational models would be impossible to calculate. Thus, we added a 'repeating scrounger' threshold trigger action to prevent this. If a player scrounged for more than ten successive trials within a block, then the pseudo-random value of other producers from the process outlined above was changed to zero. This continued until the participant next chose to produce or moved to the next block. Thus, if the participant continually scrounged, then they would experience that all their fellow group members would turn to scrounging until they changed their behaviour. The idea here was to force the participant to change their behaviour enough times to provide data that was suitable for analysis, without materially affecting their overall statistics or strategy.

Questionnaire measures were the same as in the replication sample in **Chapter 3**. They consisted of trait measures of empathy (Reniers et al., 2011), prosocialness in adults (Caprara et al., 2005), psychopathy (Paulhus et al., 2009), group affiliation (Leach et al., 2008), and risk (Blais & Weber, 2006) as well as measures for socio-economic status (Adler et al., 2000), income, experience of precarity, financial well-being, and questions designed to assess how participants understood and strategised in the task.

For this single-player version, the task was coded in MATLAB (v23.2) with Psychtoolbox extensions (v3.0.20) (Kleiner, Brainard, and Pelli 2007), using the PsyBuilder graphical experiment builder (v0.1) (Lin et al. 2022) plus additional manual coding.

4.2.4 Data, outliers and exclusions

All players completed the whole experiment, and there were no exclusions. Overall, there were 0.8% missed trials, with no player missing more than five of the 140 trials.

4.2.5 Analysis

Models of behaviour were fitted to the data using the same sets of models which had been previously developed (Chapter 3, Section 3.4.4). All statistical analyses, including our two pre-registered hypotheses, were carried out using linear mixed models from the glmmTMB (v1.1.11) package of R (v4.1.0) in R Studio (v2024.04.0).

3 Results

4.3.1 Pre-registered results

A Shapiro test showed that the data for player behaviour was non-normal in the high-cost condition ($W_{high} = 0.942$, $p = .044$). Thus, I used non-parametric Wilcoxon signed rank tests to test the key hypothesis. A paired test comparing production in high- versus low-cost conditions showed that, as predicted, participants chose to search more in the low-cost condition ($W(39) = 76$, $p < .001$, $r = 0.70$), confirming the first pre-registered hypothesis. Compared to the pure Nash solution, production was higher in high-cost, but not significantly different in low-cost ($W_{high}(39) = 668$, $p < .001$, $r = 0.62$; $W_{low}(39) = 374$, $p = .823$), confirming the second pre-registered hypothesis. The third pre-registered hypothesis was also confirmed results showing a significant difference between the deviation from Nash between conditions ($W(39, 39) = 694$, $p = < .001$).

These tests of the pre-registered hypotheses were consistent with the online sample in Chapter 3. I then examined the results versus non-preregistered benchmarks to test whether these also showed consistency in terms of the assessment of prosocial behaviour.

4.3.2 Exploratory analyses – alternative benchmarks

Producing was higher compared to the mixed Nash equilibrium in the high-cost condition, but lower in low-cost ($W_{high}(39) = 753$, $p < .001$, $r = 0.81$; $W_{low}(39) = 150$, $p < .001$, $r = 0.54$). The difference in the average producing behaviour versus mixed Nash was also significant between high-cost and low-cost ($W(39, 39) = 779$, $p = < .001$; Figure 4.2). These results were consistent with those in Chapter 3 (Section 3.4.3). I then ran the same set of tests for the three other benchmarks (Figure 4.3). Against the prospect-theory weighted benchmark, producing behaviour was also higher in the high-cost condition and lower in low cost ($W_{high} = 690$, $p < .001$, $r = 0.67$; $W_{low} = 132$, $p < .001$, $r = 0.58$). Actual producing levels were lower in high- and low-cost conditions against group optimality benchmarks ($W_{high} = 104$, $p < .001$, $r = 0.64$; $W_{low} = 0$, $p < .001$, $r = 0.87$). Compared to the ABM producing was on a par with the ABM prediction for high-cost and lower than the ABM prediction in the low-cost condition (W_{high} = 332, p = .422; W_{low} = 137, p < .001, r = 0.57).

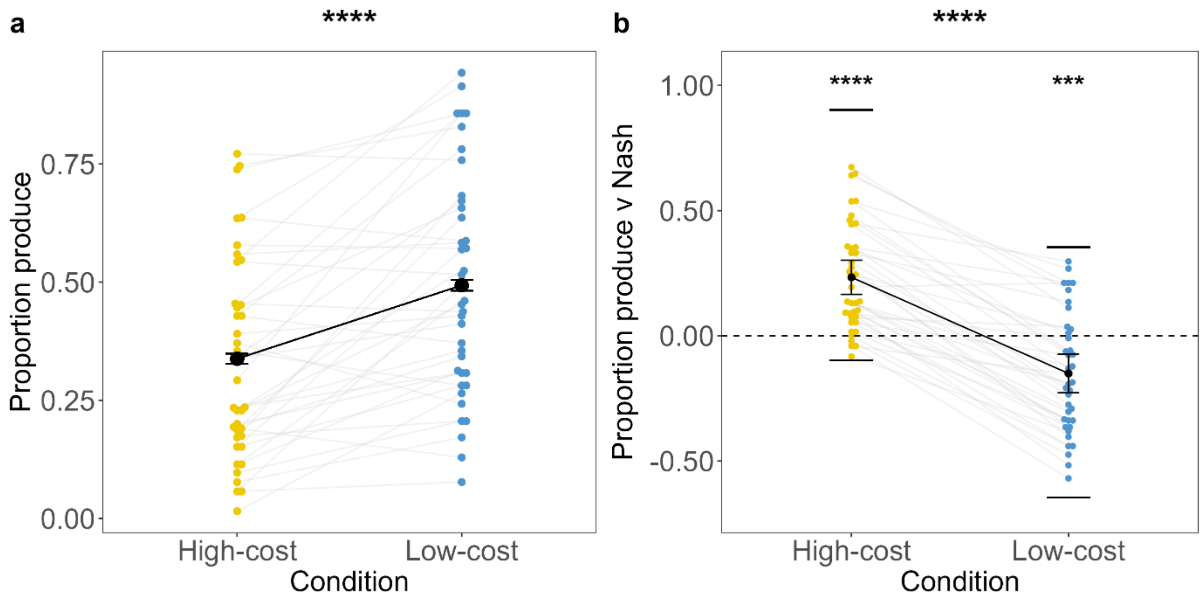

Figure 4.2 | Proportion of 'produce' trials per participant – experiment 2
a there was significantly more producing behaviour in low-cost compared to high-cost, [W(39) = 76, p < .001, r = 0.70], (b) producing was higher versus the mixed Nash under high-cost, but lower under low-cost ($W_{high}(39)$ = 753, p < .001, r = 0.81; $W_{low}(39)$ = 150, p < .001, r = 0.54), ***b*** *the* difference in the average producing behaviour versus (mixed) Nash was significant in high-cost versus low-cost [W(39, 39) = 779, p = < .001]. Error bars represent 95% confidence interval.

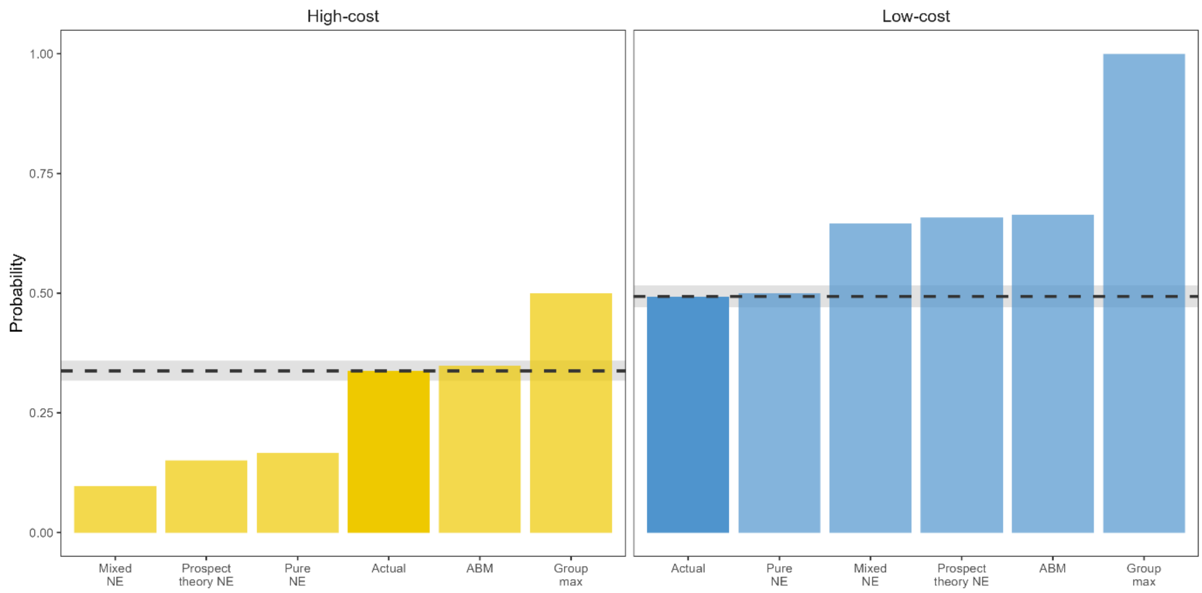

Figure 4.3 | Comparison of actual behaviour across all trials, per condition – experiment 2
*M*ean producing shown by the dashed line against each benchmark for the combined samples. Grey shaded areas represents 95% CI for the grand mean.

4.3.3 Task questionnaire responses

I next examined the participants' responses to the task itself to confirm that the deception had been successful. To measure this, I compared responses to the self-report question “I felt other players were reacting to how I was playing the game”. An independent-samples t-test was conducted, comparing scores in this sample ($M = 4.28$, $SD = 2.53$, $n = 39$) with those from the online dataset ($M = 4.05$, $SD = 2.59$, $n = 357$). The difference was not statistically significant ($t(47.1) = 0.788$, $p = .435$), indicating that the simulated responses were perceived in the same way as real responses from online participants, confirming that the deception had worked. I then compared the remaining self-report task questionnaire results between the two studies, correcting for multiple comparisons using the Benjamini-Hochberg procedure and found no significant differences, showing that players were responding to the game in the same way across all task measures.

4.3.4 Comparison of in-person and online results

Having shown that the deception had been successful, and people believed they were playing with other humans (at least to the same extent as they did in the online task), I compared levels of production between the two studies. I fitted a general (logit) linear mixed model, predicting trial-by-trial choice from the interaction of study and condition, with random effects of condition. This showed a significant interaction between study and condition ($OR = 0.68$ [0.11 1.24], $z = 2.34$, $p = .019$). Estimated marginal means showed that this was due to the relatively high producing levels in the high-cost condition in the in-person study, compared to online (Figure 4.4).

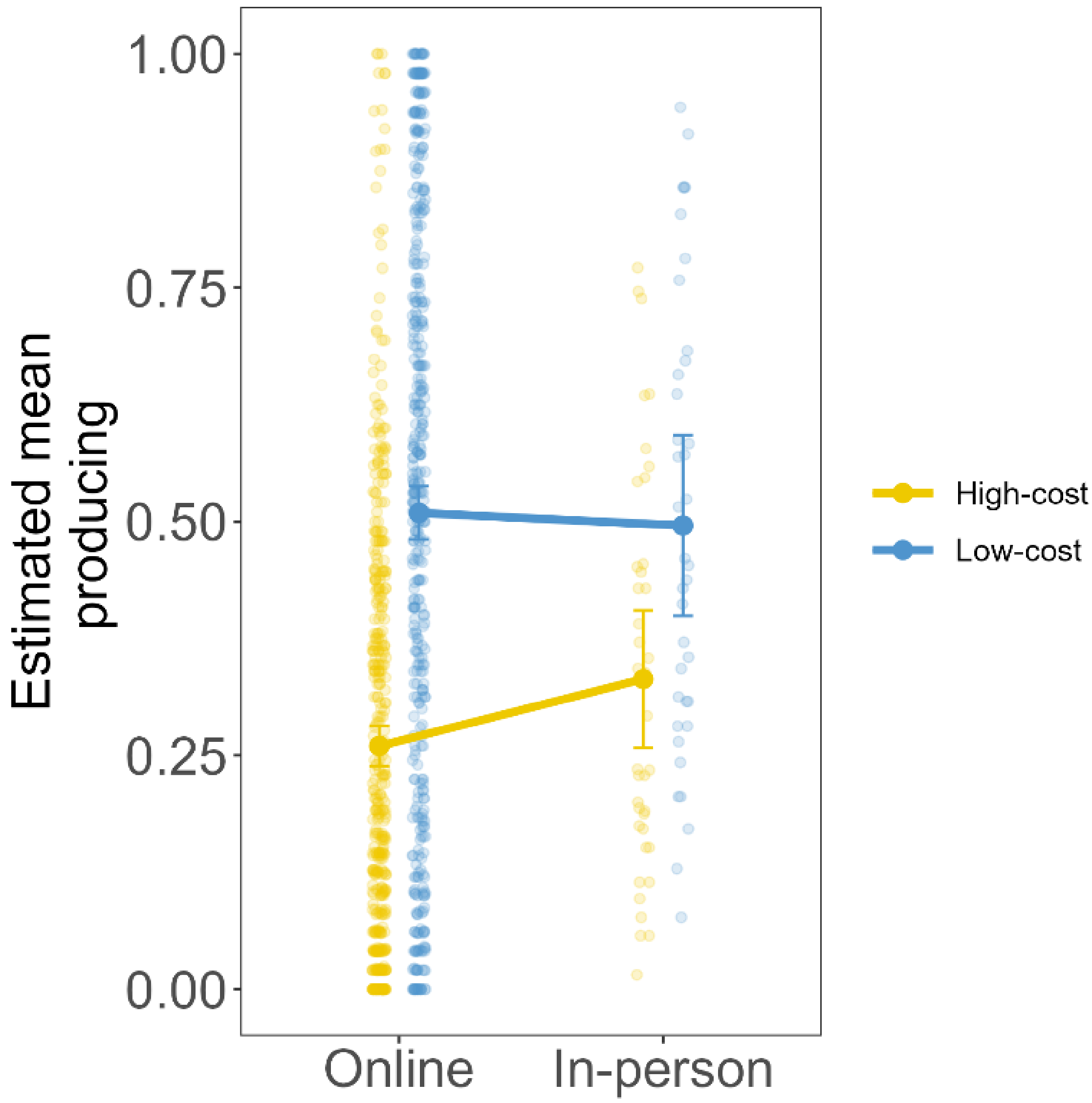

Figure 4.4 | Estimated marginal means of producing compared between online and in-person producer-scrounger game – experiment 2
*S*howing interaction effect between study and condition (OR = 0.68 [0.11 1.24], z = 2.34, p = .019). Data points represent individual participants and error bars represent 95% confidence intervals.

4.3.5 Linear mixed models

I refitted the set of models previously evaluated on the online samples to the new data (Appendix 5), evaluated on AIC scores and weighting for model comparison. This revealed a slightly different winning model from the previous study analysis, the in-person data was better fitted without the three-way interaction between condition, and previous group outcome and choice (Akaike weight 0.965, Appendix 5). The only significant effect was of greater production in the low-cost condition (OR = 2.76 [1.63 4.65], z = 3.80, p < .001).

4.3.6 Individual differences

I then examined the data to look for significant relationships between participant-level measures of task responses, psychological traits and financial status.

4.3.7 Task responses

Once more, there was a clear structure observed in relationships between the various task-related questions, and so I ran an exploratory factor analysis to simplify the analysis and interpretation. I found three eigenvalues greater than 1: 4.05, 2.28, and 1.27. Using the Kaiser criterion (eigenvalues > 1) would therefore suggest a three-factor solution. However, it is generally understood that the Kaiser criterion tends to overestimate the number of factors, and parallel analysis is a more accurate method (Hayton, Allen, and Scarpello 2004). Further investigation using parallel analysis (package 'paran' 1.5.4), accounting for expected random variation, showed adjusted eigenvalues of 2.41, 0.83 and 0.06, demonstrating that the eigenvalue of the third factor was very close to what would be expected through random variation. Thus, I compared this to a two-factor solution (Table 4.1). The two-factor solution explained 49% of the variance and was also almost identical in structure to the two-factor solution found in **Chapter 3**.

	Factor1	Factor2
Extent I tried to maximise my reward	**-0.672**	**0.436**
Extent I tried to maximise group reward	**0.883**	0.210
How did I feel when others searched	0.311	0.195
Concerned about avoiding the zero berries outcome	**0.680**	0.211
Play game cooperatively (+ve) vs competitively (-ve)	**0.890**	0.113
Influenced by how others played	0.160	**0.765**
What was the impact when others searched	**0.682**	-0.174
What was the impact when others stayed	**0.643**	0.171
Try to influence others	0.253	**0.496**

Feel others were reacting to the way you played	**0.469**	**0.469**
Keep track of how others were playing	**0.610**	**0.613**
Proportion of variance	32.3	16.5
Cumulative variance	32.3	48.8

Table 4.1 | Two-factor solution from EFA – experiment 2
Factor loadings with moderate (0.4 – 0.6) and high (> 0.6) factor loadings are shown in bold.

This factor structure's similarity to that of **Chapter 3** was evaluated by carrying out a Procrustean rotation and then calculating the congruence coefficient between the two. These were highly similar across samples (Tucker's $\phi_{factor1} = .99$, Tucker's $\phi_{factor2} = .98$), indicating complete structural equivalence (Lorenzo-Seva and ten Berge 2006).

I then applied the factor scores at the individual level and examined correlations between the individual factor scores representing task orientation, with behavioural variables such as overall level of production and reward, financial measures, and psychological traits. Significant correlations were measured using the Benjamini–Hochberg FDR correction ($p < .05$). There were significant correlations between the group-focused cooperation factor and overall mean producing levels across both conditions ($r = 0.77$, $p < .001$; $r_{high} = 0.60$, $p < .001$; $r_{low} = 0.77$, $p < .001$), but no significant correlations of the engaged influencer factor and choices to produce. These results were fully consistent with the studies in **Chapter 3**.

4.3.8 Zero aversion

Answers to the zero-aversion question were significantly correlated with overall producing behaviour ($r = 0.50$, $p = .001$) and in high- and low-cost conditions separately ($r_{high} = 0.38$, $p = .018$; $r_{low} = 0.53$, $p < .001$). We also observed that the incidence of group failure, defined as when a trial when zero group members chose to produce and therefore everyone lost berries, was higher in the high-cost condition (Figure 4.5).

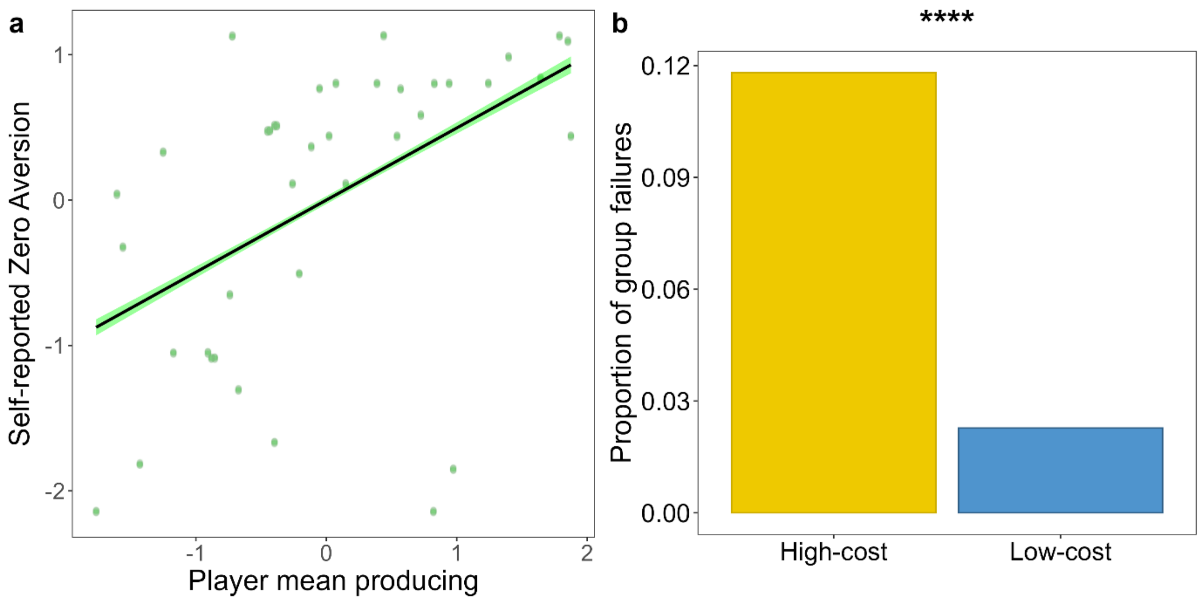

Figure 4.5 | Zero aversion in high- v low-cost conditions – experiment 2
Panel (a) shows a scatterplot of and best fit line of self-reported aversion to the zero-producer outcome and overall producing behaviour (r = 0.50, p = .001). Panel (b) shows the difference in count between instances of zero producer outcomes per condition over all, trials per group, chi-squared proportion test showed that the difference was significant [$X^2(1, 3900) = 133$, p <.001].

4.3.9 Psychological traits

There were no significant relationships with empathy, prosociality, group affiliation or psychopathy on overall production, with production in high- or low-cost conditions individually, or with the difference in production between the two conditions (full results Appendix 8). Bayes factors were calculated for all the null results to determine the strength of evidence in favour of no effect. This showed anecdotal support for the null (ie. no relationship with overall level of producing) with prosociality (BF = 0.71), psychopathy (BF = 0.44) and group affiliation (BF = 0.34), and moderate support for no relationship with empathy (BF = 0.24).

4.3.10 Financial measures

In this sample, there was a significant negative correlation between financial well-being and mean production in the high-cost condition (r = -0.41, p = .032), indicating that those who feel better off were less likely to produce than those who feel less well off when the cost of doing so is high. The effect was directionally the same, but not statistically significant in the low-cost condition, or overall (Figure 4.6). However, there were no significant relationships with self-reported income, socio-economic status or precarity measures (Appendix 8).

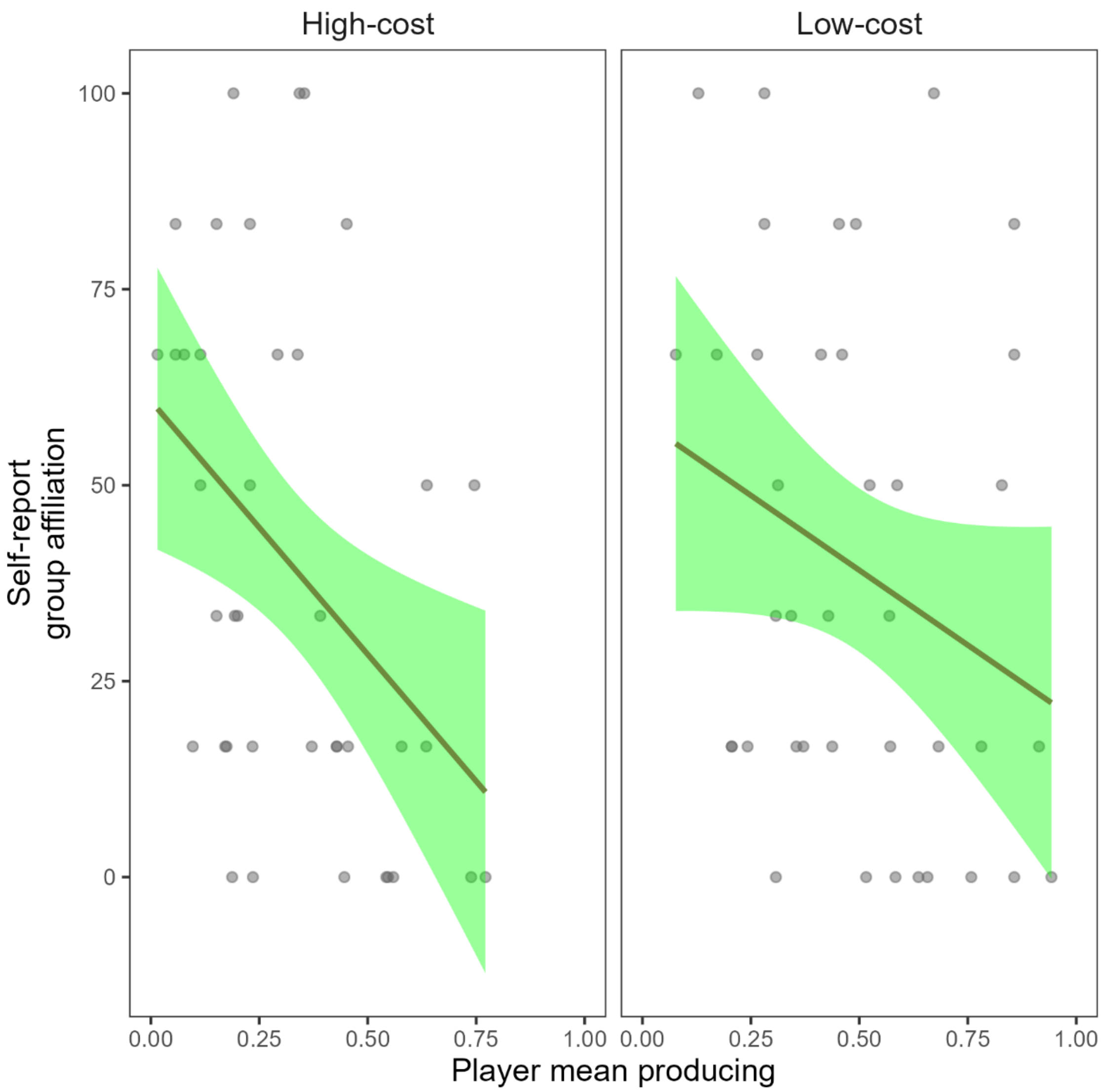

Figure 4.6 | Association of producing with financial well-being (FWB) in high- and low-cost conditions – experiment 2
FWB was significantly correlated with producing in the high-cost condition (r = -0.41, p = .009), though the association was not significant in the low-cost condition.

4.3.11 Group outcome influence on decision-making

I examined the extent to which players were reacting to the collective group choices for each round. To do this, I looked at whether the total number of producers on a trial influenced the choices on the following round. A one-way Anova confirmed significant differences between group outcome for a single trial, and mean producing behaviour on the subsequent trial in both high- and low-cost for those that had scrounged ($F_{high}(5)$ =

4.0, *p* = .003; $F_{low}(5) = 3.6$, *p* = .004) and in low-cost for those that had produced ($F_{low}(5) = 6.7$, $p < .001$). Post-hoc tests (Appendix 10) showed no consistent pattern (Figure 4.7), but it was notable that in both high- and low-cost conditions there was a significant increase in choice to produce on the trial after a group failure (i.e. one where all players chose to scrounge and no berries were found).

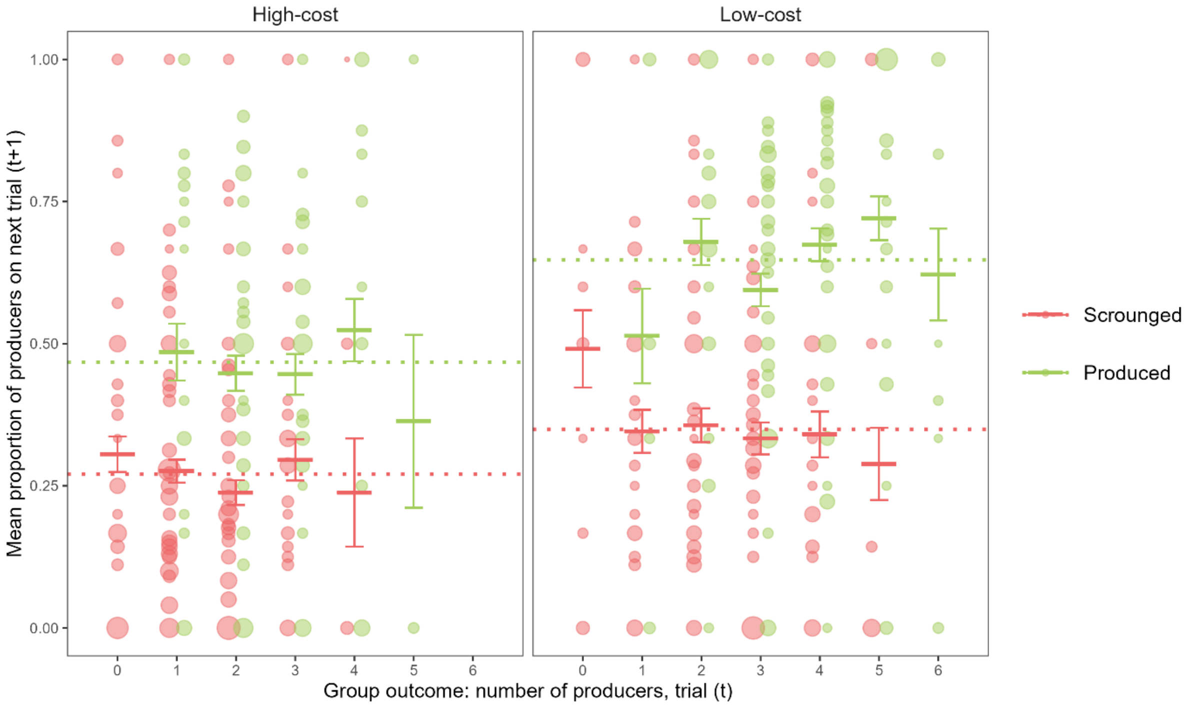

Figure 4.7 | Participant mean probability of producing on trial t + 1, depending on the number of producers on trial t – experiment 2
Points represent the mean of all individual choices across the relevant trials for each combination of choice on trial t and group outcome, with size proportionate to incidence in each case. Solid horizontal lines show the mean producing behaviour on trial t+1 for all instances of that particular group outcome, with bars showing standard error. Dashed lines show the grand mean behaviour on t+1 for those that produced (green) or scrounged (red) on trial t.

Based on the association of zero aversion and producing behaviour, the deviations from the mean when the previous trial group outcome was zero were of particular interest in this dataset. Hence, I tested for variations of each group against the overall mean for each condition-choice combination (shown by the dashed lines, Figure 4.7). To do this, I used weighted linear contrasts of the fitted ANOVA model. This approach tests whether any given group significantly deviates from the overall mean level of production on the subsequent trial. Across both the high- and low-cost conditions, the tendency of players to produce was higher if they had just experienced a zero trial ($t_{high} = 2.35$, p = .019;

$t_{low} = 3.85$, $p < .001$), demonstrating the impact of zero-producer trials on subsequent trial behaviour.

4 Discussion

In this pre-registered study, I tested a single-player in-person version of the producer-scrounger paradigm to assess whether the effects observed in the online studies in **Chapter 3** would be replicated. The new task was conducted in the presence of other participants and the experimenters themselves. It involved a deception whereby participants were told they were playing against other humans, whereas in fact the rest of the group's responses were simulated based on the results from the online version. As predicted, participants chose to search more in the low-cost condition, overproduced versus the pure Nash equilibrium in the high-cost condition, but not in the low-cost condition, and showed a significant difference in production versus Nash between the two conditions. These results were fully consistent with the online studies. Post-task questionnaires confirmed that participants perceived no difference to the online task and thus the deception had been successful.

Exploratory analyses compared levels of production to multiple alternative benchmarks, and these results were also highly consistent, with the over-production versus all three variants of the NE in the high-cost condition, but not in the low-cost condition, across both studies. Comparing levels of production between the two studies directly showed higher absolute mean producing behaviour in the high-cost scenario in the in-person study, though there was no difference in producing levels in the low-cost condition.

Overall, these results represent a successful replication of the previously observed effects. As with the online studies, there was some evidence of cooperation from the correlation with post-task questionnaire items, but no altruism was evident. The level of production in the low-cost condition was not above any of the prosocial behaviour benchmarks, indicating that behaviour could be accounted for by selfish motivations alone. In the high-cost condition, the over-production versus the Nash benchmarks was observed as before, but this was uncorrelated with any of the psychological traits, which might evidence a prosocial motivation, though it should be noted that power was relatively low compared to the online studies as a result of a much smaller sample size. Bayes factors suggested anecdotal-moderate evidence of no relationship between behaviour and traits.

The difference in producing levels between the studies is notable. This could be due to social cues, experimenter demand effects, and/or the changes to the visual presentation of the results, though it would require a separate experiment to confirm the extent to which each of these was causal. It is possible that the separation of group and personal reward in the visual presentation could have affected the levels of production, although this would be a somewhat speculative conclusion without any known empirical or theoretical grounding. However, the impact of social cues and demand effects is well-documented (Andreoni & Bernheim, 2009; de Quidt et al., 2018; Fleming & Zizzo, 2013; Haley & Fessler, 2005; Zizzo, 2010) and therefore represents a more likely explanation.

It is interesting that, in contrast to the global survey findings of **Chapter 2** and behavioural evidence from **Chapter 3**, in this experiment, those with a higher sense of financial well-being were less likely to behave prosocially in the high-cost condition at least. As previously noted, few studies address subjective financial well-being in isolation, as it is often conflated in measurements of socio-economic status (SES). Those that do, including the research presented in **Chapter 2**, find that increased financial well-being is associated with greater prosociality (see also Wiepking and Breeze 2012). The fact that the effect was observed only in the high-cost condition is notable because in this condition, the payoff matrix is such that there is greater jeopardy for the participants, and the threat of a negative reward is much more present than in the low-cost condition. Evidence of the effect of this was seen in the correlation of self-reported zero-aversion with producing and in the trial-by-trial data. There are two key differences between the studies which may account for this difference in result – firstly, this study was behavioural, whilst the global survey is based on self-report, and secondly, the difference in the sample, thirty-nine 18-35 year-olds, predominantly students, compared to 80,337 with a robust sampling method designed to be representative of 76 countries. Of these, the second factor would seem to be most important. Others have shown that loss aversion is heterogeneous, indicating that student samples in particular have greater loss aversion under risk (Blake, Cannon, and Wright 2021). It's also clear from the literature that the effects of resource scarcity on prosociality are mixed and highly situationally dependent (Civai, Elbaek, and Capraro 2024), so noteworthy though it is, this result may represent a sample-based anomaly rather than a generalisable finding.

There is some evidence in the literature that SES and social class are negatively correlated with positive other-regarding behaviours (Elbæk et al., 2023; Guinote et al., 2015; Kraus et al., 2012; Piff et al., 2010, 2012), although the Piff work in particular has been criticised for its methodology and failed replications (Balakrishnan et al., 2017; Korndörfer et al., 2015; Schmukle et al., 2019; Stamos et al., 2020). This study used the MacArthur scale measure of SES, commonly used in many of the other studies reported here, which did not show an association with prosocial behaviour, so would be consistent with the failed replications and thus further build the evidence base against a generalisable negative association of SES and prosociality.

Having established that the effects in the online studies were replicated in person in my final experiment, I wished to integrate and test the conclusion from the global study linking prosociality and financial status. In the following experiment, I examined whether levels of prosociality in the producer-scrounger game were affected by a between-subjects manipulation of in-game wealth. Using the same pre-registered design (As Predicted #237,817), I also sought to extend my understanding of the zero-aversion effect by altering the payoff matrix to have a high-cost equilibrium even closer to zero. If the zero-aversion effect was driving producing behaviour, the new condition should be even higher above Nash than in the original. The following chapter covers my final experiment, testing these two hypotheses.

Chapter 5
Testing the influence of wealth experimentally

The global study presented in **Chapter 2** showed a positive association between wealth and self-reported prosocial behaviour. To explore whether this was a causal link, in this study, I tested whether the effect could be replicated with a between-subjects experimental manipulation of rich and poor conditions. I retained the within-subjects manipulation of two different payoff structures but varied it to compare the high-cost to a new very-high cost condition. This was designed to test the effect of moving the optimality threshold closer to the zero point, where always scrounging was even more favoured and very close to being a dominant strategy.

1 Introduction

Having previously found that participants choose to produce more often in low-cost environments, but higher than the Nash equilibrium in high-cost environments (AsPredicted #168,138 and #200,528) I adapted the original online task to test two new hypotheses. Firstly, whether endowing people with different levels of in-game wealth (rich or poor) changes how willing people are to benefit the group and secondly in the effect of changing the environment such that the optimal behaviour was closer to the zero-producer point.

For the rich-poor manipulation, different lines of evidence suggest alternative hypotheses. For example, studies have suggested that those with higher incomes are more prosocial (Andreoni et al., 2021; Bekkers & Wiepking, 2011; Kosse & Tincani, 2020; Nettle et al., 2011; Vanags et al., 2025; Zwirner & Raihani, 2020) whereas others have suggested that, in experimentally manipulated poor environments, in terms of reward rates, people act more prosocially in poor compared to rich environments (Vogel et al., 2024).

The impact of the new very-high cost condition payoff matrix was to shift the Nash equilibrium closer to zero producers. This is the point at which zero producers becomes optimum ie. there is no rational benefit to producing, and scrounging becomes a dominant strategy. As such, this represents the equivalent of the ‘always defect’ zero-point in classic economic games. Moving the optimal equilibrium closer to this should theoretically reduce the amount of production overall (Afshar & Giraldeau, 2014). However, I also hypothesised that if zero-aversion was influencing behaviour, then the over-production versus the Nash optimality point might be greater when the threat of the zero-producer outcome was more proximal. This manipulation was thus designed to distinguish between two competing explanations for above-Nash producing observed in Chapters 3 and 4: prosocial motivation versus zero aversion. If zero aversion is the primary driver, bringing the Nash equilibrium closer to zero should amplify above-Nash producing, allowing the two explanations to be empirically dissociated.

Specifically, I hypothesised that:

H1: People will choose the 'produce' option more when the costs of doing so are lower.

H2a: People will produce more than the (mixed) Nash equilibrium in the original high-cost environment previously tested, as well as a new higher-cost environment where fewer producers are required to reach an equilibrium.

H2b: The effect of producing above Nash will be larger in higher-cost environment compared to the lower-cost environment due to zero aversion.

H3a: Participants allocated to the poor condition will produce less on average than those allocated to the rich condition (cf. Vanags et al., 2025; Wu et al., 2025)

Or

H3a: Participants allocated to the poor condition will produce more on average than those allocated to the rich condition (cf. Vogel)

2 Method

In this study I adapted the task in two ways to examine whether (1) whether endowing people with different levels of in-game wealth (rich or poor) changes people's prosocial behaviour, and (2) whether in environments closer to the threshold whereby zero producers is optimal, whether the zero-aversion effect would push people to over-produce (versus NE) in greater proportions.

In order to test (1), I created a between-subjects manipulation whereby half of the participants were randomly allocated to a "rich" group and half to a "poor" group. All participants were informed that they had been randomly allocated, and all six participants in each group had the same status, either all rich or all poor. Those in the rich group were told that they began the game with 500 berries in their store, whereas those in the poor group were told that they started the game with zero berries in their store. As the bonus was based on the total number of berries at the end of the game, this equated to real money rewards for the participants.

In order to test (2), I changed the payoff matrices of the high and low cost conditions such that one was high cost ie. with the NE close to zero producers, but one was even closer to that threshold than the previous high cost condition in **Chapters 3 and 4**. I called this the very-high cost condition. This was designed to test if the zero-aversion effect was driving producing behaviour, the new condition should be even higher above Nash than in the original.

5.2.1 Recruitment and sample

A total of 40 groups of 6 participants of the same sample criteria (N = 219, after exclusions) were recruited once again using the 'Prolific' platform. Experiments were run in batches between 23rd July and 31st August 2025.

The final sample consisted of 220 people (109 self-reported male, 106 female, M (SD) age = 28.65 (4.50); self-reported ethnicity 153 White (European/US), 18 Asian (Chinese/Indian subcontinent) and 29 Black (African, American, Caribbean); 47 were students; and 170 were in employment). Twenty participants who dropped out or were disconnected before completing 70% of trials were excluded from all individual level analyses, but valid responses before the dropout point were retained in group level analyses. We also applied the 70% completed trials criterion at a group level, but all groups passed this check. Three attention check trials were present in the task. One participant was excluded from the analyses for failing all three attention checks, as per the pre-registered criteria.

The study was approved by the University of Birmingham Science, Technology, Engineering and Mathematics Ethical Review Committee, approval number: ERN_20-1897AP10. Participants were presented with information about the study before giving their consent to participate. Players were reimbursed at an hourly rate of £6 / hour minimum, with an incentive bonus of up to £5 (cf. £3 in experiment one), dependent on the amount of in-game reward they collected in the task. The bonus incentive was set higher than in experiment one to encourage people to engage fully in the game mechanic.

5.2.2 Procedure

The procedure was identical to experiment one, except that it included one additional instruction screen which explained the rich/poor group allocation and informed the participants which of the two groups they had been allocated to.

5.2.3 Task

The task was very similar to that in **Chapter 3** (study 2), with the adaptation of the rich/poor manipulation and the change in payoff matrices for the very-high and high-cost conditions.

Half of the groups were informed that they had been randomly allocated to the rich group and would start the game with 500 berries (equivalent to £1.50 in task bonus, although the participants were not aware of the conversion rate), or the poor group, where they would start with zero berries in their store. Based on the experience of the earlier studies, to decrease the time (and therefore cost) required for the task, I reduced the number of trials to 80 from 100. The groups were counterbalanced for order effects of the blocked very-high and high-cost conditions. In the very-high cost condition, the cost of producing was 15 berries (compared to 12 berries in experiment one), whereas in the standard high-cost condition, it was 12 berries (compared to 2 berries in experiment one).

5.2.4 Nash Equilibria

As a result of the changes, the payoff curves were more similar to one another than in experiment one, with the new high-cost condition mixed Nash equilibrium being very close to zero (Figure 5.1).

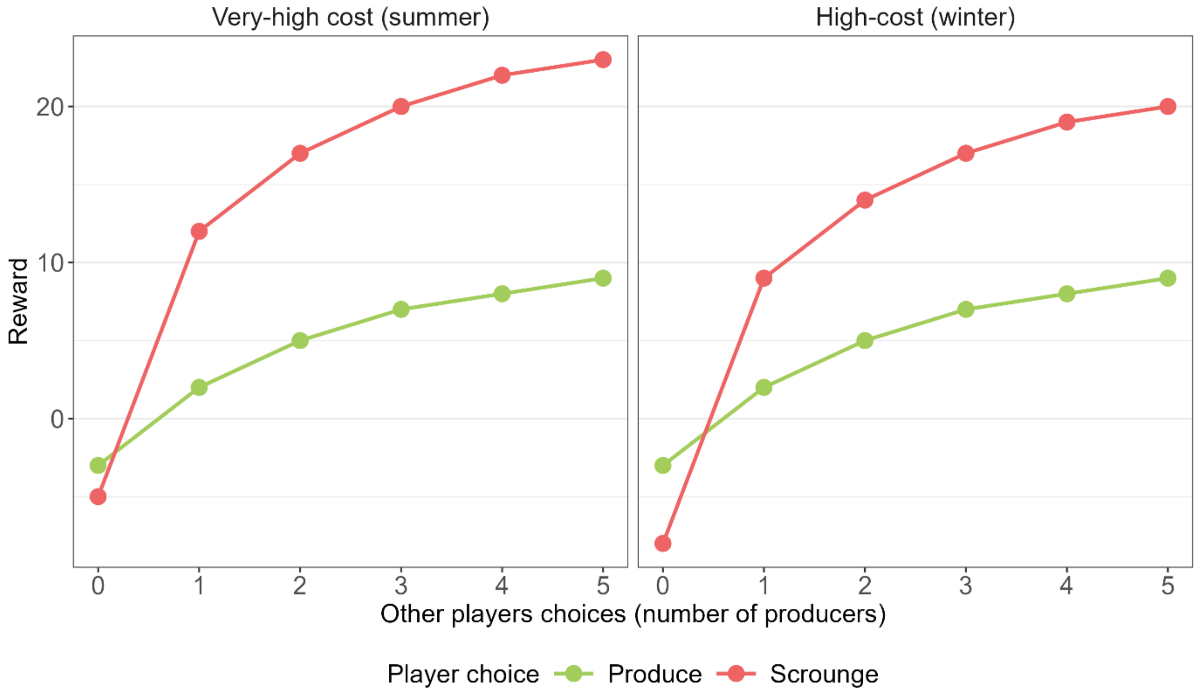

Figure 5.1 | Payoffs and Nash equilibria (very-high cost v high-cost) - experiment 3
Net payoffs for each choice per condition show diminishing returns with greater numbers of producers, showing the proximity of the equilibrium point to zero in the very-high cost condition.

As previously, we calculated these new equilibrium points of the mixed Nash solution by calculating the expected values for all probabilities between 0 and 1 in 0.001 increments and finding the first point where the difference in payoffs was less than 0.001 (Appendix 2). The optimal solution in the high-cost solution was for each player to choose to produce with a probability of .035 (equivalent to ~0.2 players), and in the low-cost solution .098 (equivalent to ~0.6 players).

Number of producers	Total resources available	Total resources per person	Producer metabolic requirement	Scrounger metabolic requirement	P payoff	S payoff	Group total payoff
			VERY-HIGH COST				
0	0.0	0.0	20	5	--	-5.0	-
1	100.0	16.7	20	5	-	11.7	55.2
2	133.3	22.2	20	5	2.2	17.2	73.2
3	150.0	25.0	20	5	5.0	20.0	75.0
4	160.0	26.7	20	5	6.7	21.7	70.2
5	166.7	27.8	20	5	7.8	22.8	61.8
6	171.4	28.6	20	5	8.6	-	51.6
			HIGH COST				
0	0.0	0.0	20	8	-	-8.0	-
1	100.0	16.7	20	8	-	8.7	40.2
2	133.3	22.2	20	8	2.2	14.2	61.2
3	150.0	25.0	20	8	5.0	17.0	66.0
4	160.0	26.7	20	8	6.7	18.7	64.2
5	166.7	27.8	20	8	7.8	19.8	58.8
6	171.4	28.6	20	8	8.6	-	51.6

Table 5.1 | Payoff matrices for producer-scrounger game - experiment 3

Questionnaire measures were the same as those in **Chapters 3 and 4** excluding the risk scale.

5.2.5 Exclusions and data quality

Data was screened for quality at the participant and group level as per sample details. As previously, any reaction times more than 3 SD's over the mean were excluded. Drop-outs ie. people becoming disconnected, or terminating their participation early by shutting their browser, were the main source of missing data. Missed trials (time-outs) were recorded, and the simulated choice data were excluded from individual analyses. In total, across the whole experiment, there were 372 (2.1%) missed trials, with no single participant missing more than 11 trials (13.8%).

5.2.6 Analysis

Models of behaviour were fitted to the data using the same sets of models which had been developed in previous studies. All statistical analyses, including our two pre-registered hypotheses was carried out using R (v4.1.0) in R Studio (v2024.04.0), and linear mixed models from the glmmTMB (v1.1.11) package.

3 Results

5.3.1 Pre-registered hypotheses

A Shapiro test showed that the distribution of participant level mean production was non-normal in both very-high and high-cost conditions (W_{high} = 0.929, p < .001, W_{low} = 0.927, p < .001). A paired Wilcoxon signed rank test showed that participants chose to search more in the high cost (standard) condition (W(219) = 6084, p < .001, r = 0.35), confirming the first pre-registered hypothesis. In this experiment, production was higher than the (mixed) Nash in both very-high and high-cost conditions (W_{high}(219) = 22868, p < .001, r = 0.78; W_{low}(219) = 21534, p < .001, r = 0.68), confirming hypothesis 2(a). The difference in the average producing behaviour versus Nash was also significant between very-high cost and high-cost conditions (W(219, 219) = 14121, p = < .001, r = .15), though the effect size was small (Lovakov & Agadullina, 2021).

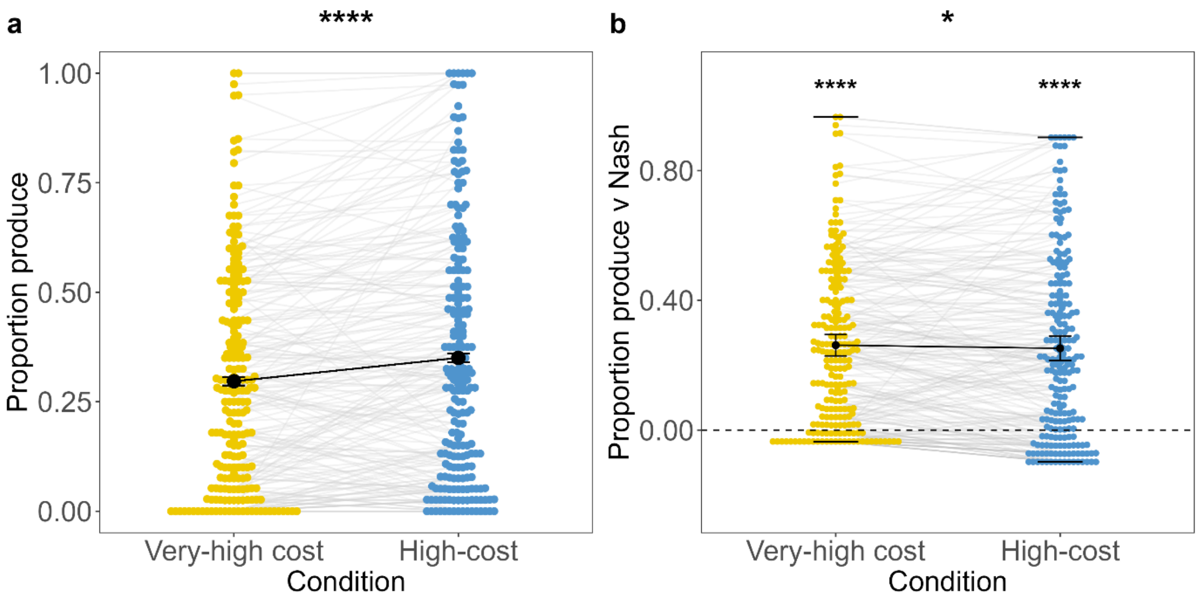

Figure 5.2 | Proportion of 'produce' trials per participant - experiment 3
a there was significantly more producing behaviour in low-cost compared to high-cost, [W(219) = 6084, p < .001, r = 0.35], (b) producing was higher than the (mixed) Nash in both high- and low-cost conditions [W_{high}(219) = 22868, p < .001, r = 0.78; W_{low}(219) = 21534, p < .001, r = 0.68], ***b*** *t*he difference in the average producing behaviour versus (mixed) Nash was also significant

between high-cost and low-cost [W(219, 219) = 14121, p = < .001, r = .15]. Error bars represent 95% confidence interval.

5.3.2 Exploratory analyses – alternative benchmarks

I then ran the same set of tests for the three other benchmarks (Figure 5.3). Against the prospect-theory weighted benchmark, producing behaviour was higher in both conditions ($W_{v\text{-}high}$ = 22062, p < .001, r = 0.72; W_{high} = 19992, p < .001, r = 0.58), lower against group optimality benchmarks ($W_{v\text{-}high}$ = 3004, p < .001, r = 0.64; W_{high} = 5502, p < .001, r = 0.47). Compared to the ABM producing was not significantly different to the ABM prediction for both conditions ($W_{v\text{-}high}$ = 11991, p = .955; W_{high} = 137, p = 0.478).

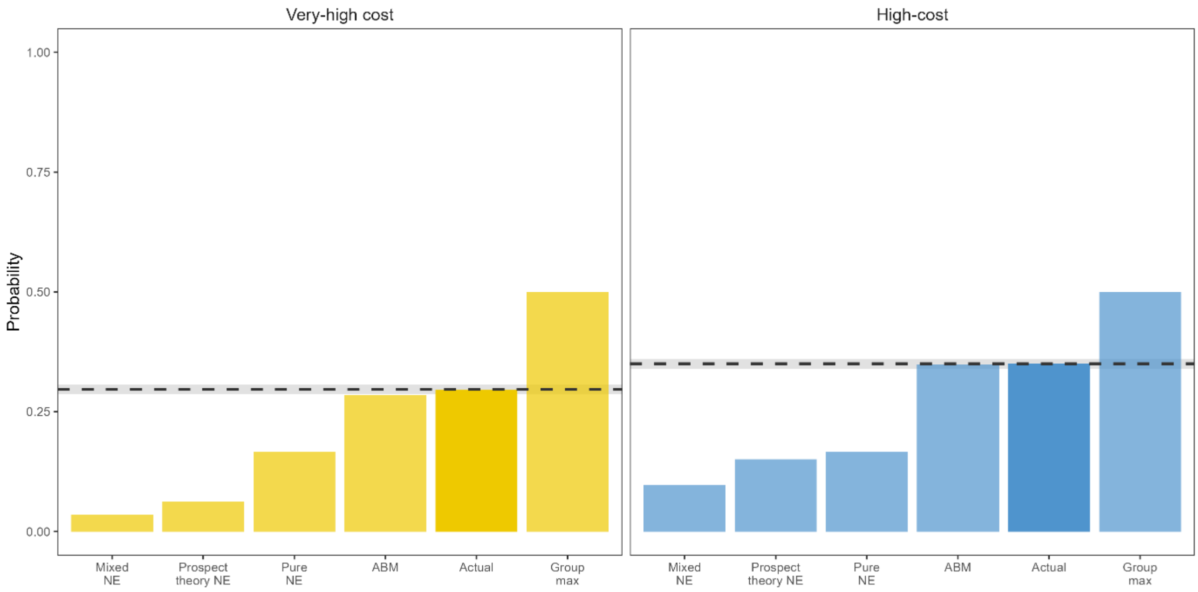

Figure 5.3 | Comparison of actual behaviour across all trials, per condition - experiment 3
Mean producing shown by the dashed line against each benchmark for the combined samples. Grey shaded areas represents 95% CI for the grand mean.

5.3.3 Rich – poor manipulation

I next tested for the between-subjects effect of being allocated to either the rich or poor group. There was no difference between overall levels of production between the two groups (W(214, 224) = 24430, p = .727; Figure 5.4).

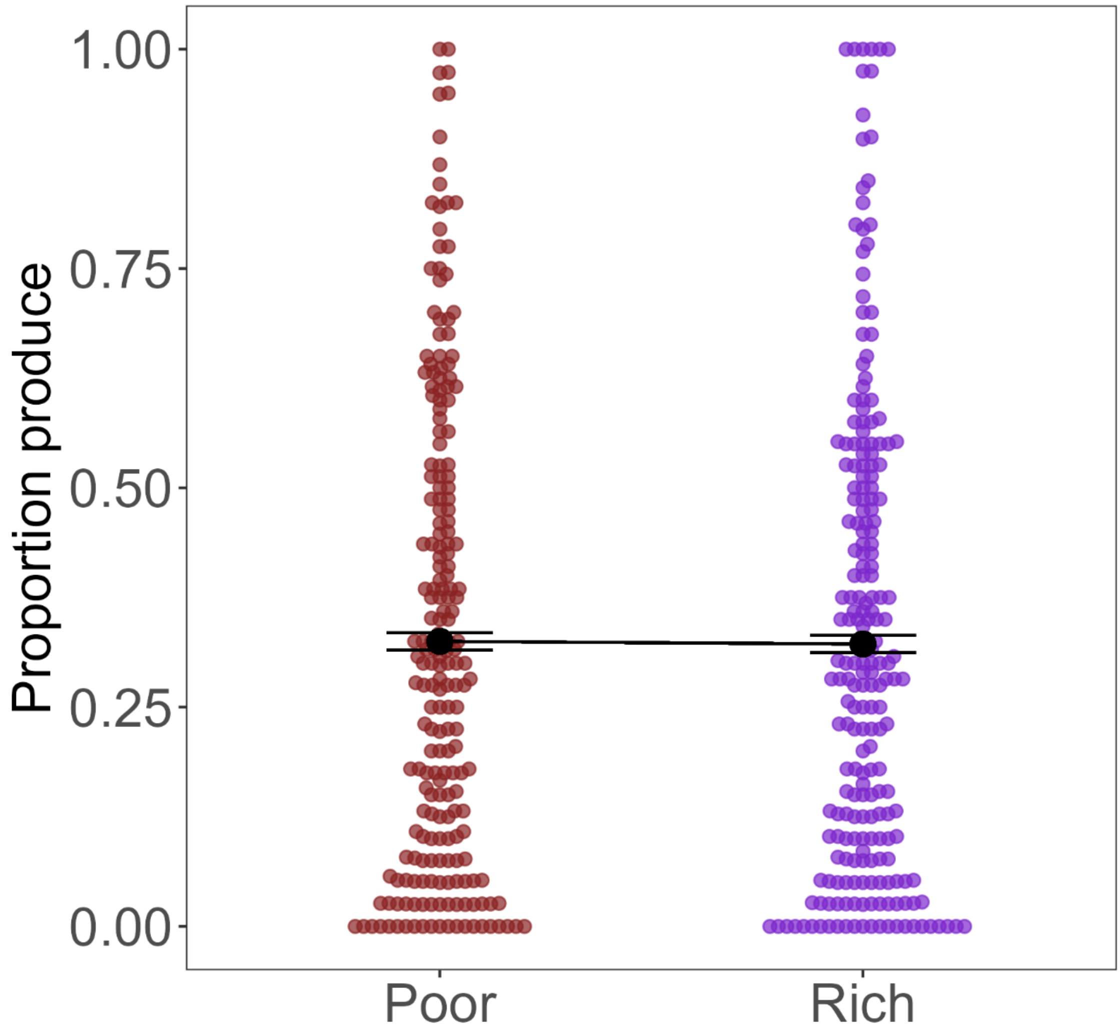

Figure 5.4 | Distribution of player mean producing scores compared between Rich-Poor groups – experiment 3
There was no difference between overall levels of production between the two groups (W(214, 224) = 24430, p = .727).

Given that the optimal probability of producing in both conditions was close to zero (0.035 or 0.098), the effect of a player becoming disconnected with their choices being simulated at the equivalent of 0.500 probability of producing could potentially have a significant impact on other players' choices, making it easier for the remaining players to scrounge with a relatively generous simulated player. To test this, I repeated the Wilcoxon signed rank test but using only data from full groups ie. those containing no computer-simulated trials, which also failed to show a significant effect (W(120, 132) = 6850, p = .064). I next tested whether the previously established difference between production and Nash levels by condition (high-cost > low-cost; Figure 2b) was moderated by the rich/poor manipulation. To account for the structure of the data, I fitted a linear mixed-effects model with a random intercept for participant. Condition, rich/poor grouping, and their interaction were included as fixed effects predicting deviation from Nash production. There was no evidence that the effect of condition differed by grouping ($t(217) = 0.84$, $p = .40$, $\beta = 0.017$, $SE = 0.020$). Neither the main effect of condition nor grouping was significant, indicating that the previously observed condition difference was not moderated by the manipulation. Finally, reasoning that the rich/poor manipulation effect may also depend on someone's real-world wealth, I also tested a linear model predicting player mean producing from the interaction of the rich/poor grouping with personal income and then subsequently with financial well-being, but once more there were no significant interaction effects ($t(163) = -1.51$, $p = .133$; $t(163) = -1.42$, $p = .157$ respectively).

5.3.4 Group outcome influence on decision-making

I examined the extent to which players were reacting to the collective group choices for each round (Figure 4.7). A one-way Anova confirmed significant differences between group outcome for a single trial, and mean producing behaviour on the subsequent trial in both high- and low-cost for those that had scrounged ($F_{high}(5) = 13.9$, $p < .001$; $F_{low}(5) = 77.3$, $p < .001$) and those that had produced ($F_{high}(5) = 12.4$, $p < .001$; $F_{low}(5) = 31.8$, $p < .001$). Post-hoc tests in this experiment revealed consistent patterns, although different to those found in **Chapter 3**. Across both conditions and previous trial choices, there was a direct positive relationship between the group outcome and the next trial choice (Figure 4.7, Appendix 10).

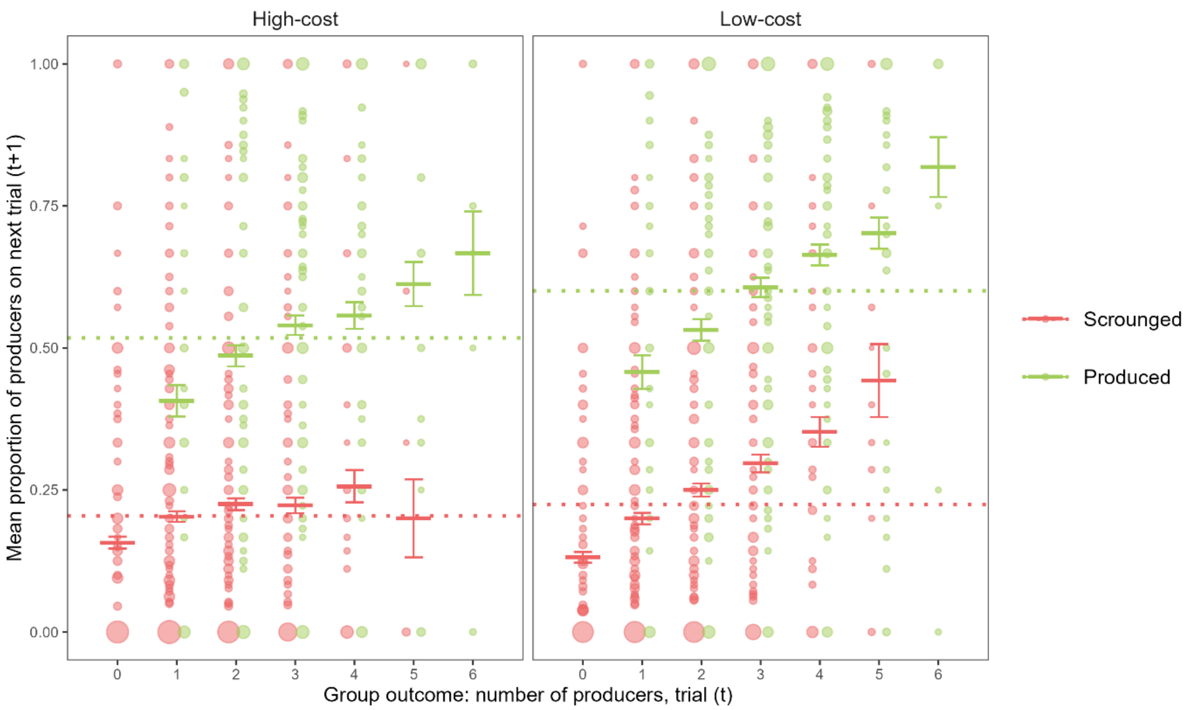

Figure 5.5 | Participant mean probability of producing on trial t + 1, depending on number of producers on trial t - experiment 3
Points represent the mean of all individual choices across the relevant trials for each combination of choice on trial t and group outcome, with size proportionate to incidence in each case. Solid horizontal lines show the mean producing behaviour on trial t+1 for all instances of that particular group outcome, with bars showing standard error. Dashed lines show the grand mean behaviour on t+1 for those that produced (green) or scrounged (red) on trial t.

5.3.5 Linear mixed models

I refitted the set of models previously evaluated on the online samples to the new data (Appendix 5), evaluated on AIC scores and weighting for model comparison. The winning model was the same as in the previous combined online sample (Akaike weight 0.971, Appendix 6). The previously observed decline in production per trial within block (OR = 0.97, [0.96, 0.97], z = -8.76, p < .001) was also present in this sample.

However, in contrast to the Wilcoxon test between high- and low-cost conditions, in this data, there was no significant main effect of condition within the model. There were significant interaction effects of condition and previous group outcome (OR = 1.25 [1.14 1.38], z = 4.62, p < .001), condition and previous choice (OR = 2.07 [1.34 3.21], z = 3.26, p = .001), previous group outcome and previous choice (OR = 1.20 [1.07 1.35], z = 3.11, p = .002), and a three-way interaction between condition, previous group outcome and choice (OR = 0.76 [0.65 0.88], $z = -3.56$, $p < .001$). The three-way interaction can be clearly observed in Figure 5.5, as the difference between the producer and scrounger slopes between conditions. In the very-high cost condition, these diverge to a greater extent, showing that the relative production is higher for producers compared to scroungers as the group outcome goes up, compared to the standard high-cost condition. This pattern was notably different to that observed in the previous online and in-person experiments, and was found to be consistent across all combinations of block and condition (Appendix 10), including the first standard high-cost block.

5.3.6 Individual differences

I then examined the data to look for significant relationships between participant level measures of task responses, psychological traits and financial status.

5.3.7 Task responses

To further test the effect of the rich/poor manipulation, I used a two-sample t-test to test for differences between the groups on each task-related questionnaire item. Three of the measures were significantly lower in this experiment compared to the combined sample of the online task; being 'influenced when others stayed' (scrounged), 'used your decision to try to influence others' and 'felt like other were reacting to my choices' ($t(432, 326) = 4.36$, $p < .001$, $r = 0.36$; $t = 4.48$, $p < .001$, $r = 0.37$; $t = 4.73$, $p < .001$, $r = 0.39$ respectively). The other eight measures showed no difference between the two studies (Appendix 11).

I ran an exploratory factor analysis to simplify the analysis and interpretation to the task questionnaires. I found four eigenvalues greater than 1: 3.40, 1.89, 1.13, and 1.08, but two of these were very close to the threshold. Further investigation using parallel analysis (package 'paran' v1.5.4), accounting for expected random variation, showed adjusted eigenvalues of 2.41, 0.91, 0.20, and 0.16, demonstrating that the eigenvalues of the third and fourth factors were very close to what would be expected through random variation. Thus, I compared this to a two-factor solution (Table 5.2). The two-factor solution explained 38% of the variance. I compared this solution to the two-factor solution found in **Chapter 3** by carrying out a Procrustean rotation and then calculating

Tucker's congruence coefficient, which indicated complete structural equivalence (Lorenzo-Seva & ten Berge, 2006).

	Factor	Factor
Extent I tried to maximise my reward	**-0.608**	**-**
Extent I tried to maximise group reward	**0.870**	0.176
How did I feel when others searched	-	-
Concerned to avoid the zero berries outcome	**0.515**	0.292
Play game cooperatively (+ve) vs competitively (-	**0.803**	0.142
Influenced by how others played	0.206	**0.476**
What was impact when others searched	**0.468**	0.135
What was impact when others stayed	**0.439**	0.107
Try to influence others	0.200	**0.652**
Feel others were reacting to the way you played	-	**0.629**
Keep track of how others were playing	-	**0.683**
Proportion of variance	0.23	0.15
Cumulative variance	0.23	0.38

Table 5.2 | Two-factor solution from EFA - experiment 3
Factor loadings with moderate (0.4 – 0.6) and high (> 0.6) factor loadings are shown in bold.

I applied the factor scores at the individual level and examined correlations between the individual factor scores representing task orientation, with behavioural variables such as overall level of production and reward, financial measures, and psychological traits. There were significant correlations between the group-focused cooperation factor and overall mean producing levels across both conditions ($r = 0.73$, $p < .001$; $r_{very\text{-}high} = 0.71$, $p < .001$; $r_{low} = 0.70$, $p < .001$), but no significant correlations of the engaged influencer factor and choices to produce. These results were fully consistent with the online studies.

5.3.8 Zero aversion

Being averse to the zero producers group outcome was significantly correlated with overall producing behaviour ($r = 0.43$, $p < .001$) and in high- and low-cost conditions separately ($r_{high} = 0.39$, $p < .001$; $r_{low} = 0.44$, $p < .001$). However, a proportion test indicated no significant difference in failure rates between the two groups with proportions of 14.6% and 13.8%, respectively ($\chi^2(1) = 0.37$, $p = .543$, 95% CI for the difference [−0.017, 0.033]).

5.3.9 Psychological traits

There were no significant relationships between any of the trait variables or any of the measures of producing behaviour. Bayes factors were calculated for all the null results to determine the strength of evidence in favour of no effect. This showed moderate evidence for the null with prosociality (BF = 0.10), psychopathy (BF = 0.13), group affiliation (BF = 0.15), and anecdotal support for no relationship with empathy (BF = 0.49). Full correlation coefficients and test statistics are reported in appendix 8.

5.3.10 Financial status

Behavioural measures were correlated against personal income, precarity, financial wellbeing and SES, but once more, no significant correlations were present. Bayes factors showed strong support for no relationship with overall levels of production and personal income (BF = 0.14), precarity (BF = 0.18), SES (BF = 0.11) and financial wellbeing (BF = 0.18). Full correlation coefficients and frequentist test statistics are reported in appendix 8.

4 Discussion

In this pre-registered study (As Predicted #237,817), I sought to replicate the effects previously seen across the online and in-person experiments. In addition, I examined two new hypotheses; firstly, whether the change to a very high-cost environment would result in even greater overproduction versus the Nash and secondly, whether allocating participants to either a rich or a poor group with real money reward consequences would alter their behaviour. Because this chapter draws heavily on the paradigm, design, and overall theoretical rationale described in **Chapter 3**, the present discussion focuses on the novel aspects of the current study. Specifically, I consider the implications of the replication results or deviations from the original findings, and the methodological features unique to this version of the task, namely the very-high cost condition and rich-poor manipulation.

As predicted, participants chose to search more in the lower of the two cost conditions and overproduced versus the Nash equilibrium in both the very high-cost condition and standard high-cost conditions. The level of overproduction versus Nash was higher in the very-high cost condition than in the standard high-cost condition, and overall production was correlated with zero aversion, suggesting that this may be the cause rather than a prosocial motive. The effect size was small, but this is likely to be because the payoff matrices were very similar and the optimality point was so close to zero that there was very little scope for difference between the two conditions. Over-production versus the Nash benchmarks was uncorrelated with any of the psychological traits which might have evidenced a prosocial motivation. The group-focused cooperator score was once more correlated with production, though previous caution about the interpretation of both of these findings remains.

The trial-by-trial data and mixed-effect model showed a different pattern from the previous experiments. There was a consistent positive association of individuals producing with higher levels of production in the group, but notably, the likelihood of producing on a trial after a zero trial was actually lower than the mean. The experiments differed, although those participants experiencing the first block of trials as standard high-cost would have been directly comparable to the previous studies. In this first block, the relationships were consistent with all of the other block-condition segments within the rich-poor study, so it would appear that participants were indeed behaving differently compared to previous studies. Due to the similarity of the two online studies, these divergences do not have an obvious theoretical or methodological explanation. As no substantive procedural differences distinguish the two studies, the most straightforward interpretation is that the variation simply reflects between-sample variability and the dynamics that might arise in multi-player interactive tasks. This interpretation should, however, be regarded as tentative, and the source of these differences remains an open question for future work.

It is also important to note, regarding the negative association of lower production and financial well-being seen in the high-cost condition in **Chapter 4**, that this was not replicated in either condition here, providing further evidence that the previous result may have been sample specific. Previously documented negative relationships between SES and prosociality (Guinote et al., 2015; Piff et al., 2010, 2012) were not borne out in the results, in keeping with the findings of both **Chapter 3** and **Chapter 4**. Overall, these results reinforce the main conclusions from the previous online and in-person studies using this task.

Contrary to the pre-registered hypothesis, the rich/poor manipulation did not result in any significant differences in behaviour. This was unexpected, and given the well-established positive effect of wealth on prosocial behaviour (Andreoni et al., 2021; Bekkers & Wiepking, 2011; Kirkpatrick et al., 2015; Kosse & Tincani, 2020; Nettle et al., 2011; Zwirner & Raihani, 2020) most likely reflects that the manipulation was not effective in producing the desired change in psychological state. This is supported by the finding that there were no differences in any of the task-related questions such as the extent to which people tried to maximise their personal reward or played the game cooperatively, and also by the lack of interaction with real-world income. This suggests that this specific operationalisation of resource disparity failed to alter participants' subjective state in the intended way. As such, I interpret this primarily as a failed manipulation rather than evidence that resource disparity has no effect on the behaviour under study. Future work should pre-test the impact of such manipulations in smaller pilot samples, using direct measures of perceived resources and prosocial behaviour, and potentially stronger or more ecologically valid differences between conditions, such as larger endowment gaps, or real-world differences in income.

Chapter 6
General discussion

1 Introduction

In this thesis, I have investigated two aspects of prosociality: its relationship with wealth at a global level and how group dynamics and non-linear payoffs shape prosocial decision-making in a novel experimental task. In this final chapter, I begin by summarising the key findings from each empirical section and outlining their broader implications and significance, along with thoughts on future work which stem from these. I then return to the themes introduced in the opening chapter, in terms of the conceptualisation and measurement of prosociality, and consider what the present findings suggest about these longstanding issues. Finally, I highlight several avenues for future research that may deepen our understanding of prosocial behaviour and the mechanisms that support it.

2 How does wealth shape prosociality globally?

6.2.1 Key findings

In **Chapter 2,** I revealed a positive association between wealth and prosociality. This association was robust across a range of different measures, some of which had been externally validated (altruism, trust, positive and negative reciprocity), with a globally representative sample. There was one exception in the negative relationship between trust and self-reported income. Linear mixed models also revealed interaction effects from precarity (operationalised in this study as a self-reported lack of reliable access to food and shelter) on the wealth-prosociality association. Experiencing precarity enhanced wealth's associations with prosocial behaviours but weakened how strongly wealth was associated with prosocial preferences. These results confirmed our pre-registered hypotheses that income and prosociality would be positively correlated, and that the effect sizes would be greater for financial compared to non-financial measures. The surprising finding in this study was that contrary to our original hypothesis, subjective financial well-being was also positively associated with prosociality.

6.2.2 Causality and mechanism

The results of this study are consistent and conclusive in supporting a generalised and globally consistent positive association with prosociality and financial resources. This adds weight to the existing evidence base in support of this conclusion (Andreoni et al., 2021; Falk et al., 2021; Kosse & Tincani, 2020; Nettle et al., 2011; Wiepking & Bekkers, 2012; Zwirner & Raihani, 2020), on a question that has been the subject of some previous debate (Balakrishnan et al., 2017; Clerke et al., 2018; Francis, 2012; Greitemeyer, 2023; Jung et al., 2023; Korndörfer et al., 2015; Stamos et al., 2020). Of particular note is the finding that subjective financial well-being is positively associated with prosociality. Little previous work had isolated the effect of financial well-being specifically, though many studies have found the inverse correlation with the related but independent construct of socio-economic status (Cutler et al., 2021; Elbaek et al., 2021b; Guinote et al., 2015; Piff et al., 2010, 2012).

It seems intuitive to think that greater levels of income may result in greater levels of prosociality, but the correlational method used in this study cannot definitively establish that. Though unlikely, it is possible that higher levels of prosociality cause greater income, as has been shown in the case of volunteering (Hackl et al., 2007), perhaps through improved reputation, or the effect may be bi-directional. An obvious causal mechanism could be that greater levels of income yield greater levels of material comfort and security, which in turn enable people to spend more time or money on others. Evidence to support this idea comes from a study which shows that the differences in prosociality between rich and poor are not associated with social preferences, but simply with how much money they have (Andreoni et al., 2017; Frankenhuis & Nettle, 2020). It also chimes with modern interpretations of need satisfaction. Whilst the specifics of Maslow's hierarchy have been largely left behind by contemporary psychological science (Kenrick et al. 2010; Wahba and Bridwell 1976), the foundational principle remains useful. People with more resources are more able to satisfy a broader range of needs, of which some, for example, love and belonging, esteem, and self-transcendence (Koltko-Rivera 2006) may be met through prosocial behaviours such as donating, volunteering and helping. There is evidence that need fulfilment is associated with subjective well-being regardless of income (Tay and Diener 2011), and also that income can satisfy physiological needs but only has a minor role in those of safety, love and belonging, esteem, and self-actualisation (Rojas, Méndez, and Watkins-Fassler 2023). Need fulfilment could therefore be a potential explanatory mechanism for the relationship between prosociality and wealth.

6.2.3 Implications and future work

Given the myriad of physical and mental benefits of prosociality detailed in the general introduction (Kubzansky et al., 2023), the obvious implication is that improving income and financial well-being could be highly beneficial to individuals and society as a whole. Future work could experimentally test need fulfilment as a possible framework, namely that greater levels of income create greater levels of prosociality through this mechanism. This could be done experimentally by measuring individual levels of need in different domains, and then manipulating their experience of financial security in the task, through income shocks, or through trial blocks with different levels of volatility. A more ecological test could be done by giving people small cash grants and measuring their prosociality over time (e.g. de Milliano et al. 2021), which as a randomised-control trial would represent a high level of causal evidence indeed, although it would be resource and time-intensive to execute.

Within this, regarding the precarity findings, it would also be interesting to test whether basic physiological needs (food, shelter) are prioritised to any extent. Modern interpretations of need satisfaction would suggest not, as they downplay the role of hierarchy, emphasising instead that humans have a variety of needs that require balancing (Rojas, Méndez, and Watkins-Fassler 2023), but this is a testable hypothesis. Understanding causal factors and finding an explanatory mechanism would help to give confidence to social policies and interventions designed to increase prosociality.

3 How do group dynamics and non-linear payoffs shape behaviour in a public goods game?

6.3.1 Key findings

Chapters 3-5 concerned the creation of three separate experiments using the producer-scrounger paradigm. The collective findings across these experiments at an aggregate level were: (1) participants were sensitive to their personal reward, producing more when in line with their own self-interest, and (2) in the condition where rewards for producing were lower, the relative production versus Nash was, surprisingly, higher (Table 6.1). At an individual level, (3) there was evidence that participants had an aversion to the zero-producer outcome, (4) there were no associations between any of the psychological trait measures, including prosociality, and (5) production behaviour was correlated with cooperative and pro-group motivations from the post-task questionnaire. The aggregate level findings were contrary to my original expectations, given the large body of experimental work in support of systematic and reproducible prosocial bias in humans (Camerer, 2013; Engel, 2011; Tisserand, 2014) as well as the one previous study that had measured prosociality using a producer-scrounger game (Kameda et al. 2010).

Study	Cost condition	N	Mean production	Mixed Nash production	Deviation
Online 1	High	214	0.297	0.098	0.199
Online 2	High	234	0.225	0.098	0.127
Online 1	Low	214	0.567	0.646	-0.078
Online 2	Low	234	0.456	0.646	-0.190
In-person	High	39	0.337	0.098	0.239
In-person	Low	39	0.493	0.646	-0.152
Rich-Poor	Very high	219	0.296	0.035	0.261
Rich-Poor	High	219	0.350	0.098	0.252

Table 6.1 | Summary table of mean production levels for all experiments
Split by cost condition with deviation from the mixed Nash equilibrium. All of the deviations from Nash were significant with $p < .001$.

6.3.2 Deviations from Nash: boundary effects, confusion and coordination

Overall, the main results from all three producer-scrounger experiments were very consistent. The conclusion is clear enough in the case where the Nash equilibria were comfortably distanced from the zero-production point – there was no evidence of aggregate prosociality. In fact, in the standard high-cost condition, compared to the mixed Nash equilibria, there is a significant under-production which might be described as anti-social. However, there was aggregate over-production in the case where the Nash equilibria were relatively close to the zero point.

Taken together, at the aggregate level, it seems that the evidence supporting the presence of deliberate and meaningful prosocial behaviour in this task is relatively thin. To summarise, (i) the high-low cost manipulation shows that participants are primarily responsive to their own reward, (ii) actual production levels were lower than all benchmarks in the low-cost condition when the reward stakes and margin for coordination error were relatively low, (iii) there was no evidence for a relationship between prosocial behaviour with traits such as prosociality, empathy and group affiliation which would be expected from social preference theory. In addition several non-exclusive reasons could explain why production was greater than the benchmarks in the high-cost condition, but not in the low-cost condition namely; (iv) both samples showed behavioural evidence of zero-aversion, (v) the theoretical effect of loss-aversion as a reduction in magnitude of the over-production is quantified by the use of prospect-theory adjustment, (vi) in both conditions the level of producing is significantly lower than that represented by optimal group-level welfare maximization, (vii) the ABM shows a simple reinforcement learning model conditioned on self-reward only can easily account for the levels of production observed in the task and in fact predict higher levels of production than those observed in either sample. In addition to these factors, it is also important to consider the possibility of zero-inflation or floor effects (Ferr 2025).

To this end, it is informative to consider features of the probability space of the game when the optimality point is close to the boundary. Perfect play in the standard high-cost condition can be modelled using a simple binomial distribution with the Nash optimal probability. This leads to the following spread of outcomes: 55% zero-producer trials, 36% one-producer trials, and 9% two or more producers (the actual values were:

20%, 34% and 46% respectively). Thus, to achieve Nash optimality, participants would have to tolerate zero-producer group failure and the resultant negative personal reward on over half of all trials. The features of the binomial probability space thus act to compound any aversion to zero-producer outcomes, such that even small biases can result in relatively large deflections from optimality.

It is also possible that the proximity of the Nash equilibria to the lower boundary contributes to the over-production versus Nash because of performance errors. This is in keeping with previous evidence that confusion can account for large proportions of superficially prosocial behaviour in economic games (Andreoni, 1995; Bayer et al., 2013; Houser & Kurzban, 2002; Koppel et al., 2025). The recent studies of Koppel et al (2025) suggest that this may be as high as to 53% in lab-based PGGs. In addition to any kind of basic misunderstanding about the game, performance errors may have been induced by the relative difficulty of the group successfully coordinating to achieve a one-producer outcome. There are only 6 out of 64 possible combinations of group choices which lead to a one-producer outcome (e.g. 1 producer 5 scroungers), whereas there are 57 out of 64 combinations of 2 or more producers. In the absence of any ability to coordinate or plan within the group, it is more difficult to hit the optimum one-producer outcome compared to a 2-or-more producer outcome, introducing a further potential source of bias in the high- cost compared to the low-cost scenario.

Thus, there are multiple potential factors with the potential to explain some or all of the deviation from Nash when close to the lower boundary, without needing to invoke prosociality as an explanation. It is not possible to quantify each of these factors and demonstrate conclusively that all of the over-production is attributable to them, and thus, prosociality could still be a factor. However, recall that this over-production did *not* occur in the low-cost condition when the Nash equilibria were distanced from the zero-producer point. The over-production is only observed when the Nash point is close to the boundary, whereas the incentives to behave prosocially were actually higher in the other case, so along with all of the mechanisms previously discussed, it seems reasonable to think that the over-production does not represent prosociality. At an individual level, however, there is more reason to believe that some level of intentional prosociality can help to explain behaviour.

6.3.3 Individual differences

There was notable variation at an individual level, the key question here is whether this is just statistical noise or something more meaningful. Many lines of experimental research over decades strongly support the phenomenon of deliberate prosociality in humans (Camerer 2013; Wyss and Knoch 2022; Penner et al. 2005; Thielmann, Spadaro, and Balliet 2020; Jensen, Vaish, and Schmidt 2014; Jaeggi, Burkart, and Van Schaik 2010; Batson 2010; Henrich and Muthukrishna 2021) and the correlation of production with group-focused cooperator factor supports the idea that people were choosing to behave in a prosocial manner.

Factor analysis of the post-task questionnaire responses showed a strong correlation between the group-focused cooperation factor and production. The factor loadings, which were highly consistent across the three experiments, were primarily related to a focus on group over personal reward, and cooperative over competitive behaviour. This correlated with production overall, and in each of the experimental conditions individually. The discussion in **Chapter 3** highlights why this result should not be over-interpreted, namely, the moderate levels of consistency of the individual behaviour across blocks, the lack of external validation of the EFA measure and the possibility of post-hoc rationalisation.

However, the result cannot be ignored, given the heterogeneity in individual responding, with some participants producing consistently at a personal cost and others consistently scrounging. It seems clear, therefore, that despite the lack of evidence for a population-wide prosocial bias, there is support for some level of individual differences. This result is in keeping with many studies which identify consistent prosocial typologies within the human population, for example, the individualists, prosocials, altruists, and competitors measured by Social Value Orientation (Murphy et al., 2011) or the conditional cooperator/free-rider classification (Fischbacher et al., 2001). However, for these individual differences to be meaningful they need to be independently predictive of behaviour outside of one particular task. The literature suggests that prosocial preferences do influence behaviour systematically, but are heavily affected by the particular circumstances, such as the structure of the task and its context. For example Baader et al (2024) ran a series of social dilemma games with varying payoffs and elicited social preferences through modelling, showing that underlying social preferences influenced behavioural strategies, but also that these varied systematically with payoffs. Another group foraging study paired the most- or least-cooperative members with one another in a second phase of the same experiment. The result was that cooperating and defecting “types” did not completely persist across groups, with some of the most cooperative members switching to become the least cooperative and vice versa (Kim et al., 2019). This remains an open question in research, but these studies suggest that prosocial preferences exert a weak but persistent influence on humans’ behavioural choices.

The coexistence of a lack of population-level effect with a wide span of individual differences in behaviour aligns with accounts that emphasise how individuals interact with situational specifics. Under these accounts, prosocial tendencies are not expected to appear uniformly across contexts; instead, they emerge selectively for those whose preferences, beliefs, or motivational orientations favour prosocial action under particular conditions (Thielmann, Spadaro, and Balliet 2020; Mullett, McDonald, and Brown 2020; Fleeson 2004; Popov and Thielmann 2025). A nuanced interpretation may allow for some level of prosociality to account for a portion of behaviour whilst admitting that other factors, such as loss-aversion or learning, may come into play. Future work could repeat the producer-scrounger game using an externally validated prosociality measure, by testing over multiple experimental sessions, or by systematically varying the payoffs as other studies have done.

The lack of significant association of producing with the PSA and known correlates of prosociality, such as empathy, group affiliation, and psychopathy, with support from Bayes factors, was taken as supporting evidence against the presence of prosociality in the experiments. It is possible, however, that other instruments may have been better at detecting prosocial motivations. The PSA measures prosocial orientation (Caprara et al., 2005; Luengo Kanacri et al., 2021), and is particularly suited to real-world prosociality, using as it does a battery of questions about actual behaviour unrelated to economic games. Another common measure is the Social Value Orientation (SVO), which is also widely used (Murphy and Ackermann 2014). The SVO-slider method (Murphy et al., 2011) uses economic game style trade-offs to infer prosocial orientation and so is more suited to predicting behaviour in similar contexts. The link between SVO and cooperation in social dilemmas has been shown to be significant with a small-medium effect size (Balliet et al., 2009). Studies comparing the two have drawn a clear distinction between SVO and PSA (Böckler et al., 2016), differentiating self-report prosociality from altruistically motivated, norm-based, and strategically motivated types. This study and others have found no significant correlation between PSA and SVO (Böckler et al., 2016; Pelowski et al., 2024). As such, the SVO may have revealed different aspects of individual prosocial orientation not captured by the PSA.

Overall, the producer-scrounger experiments provide a clear demonstration that at an aggregate level, when humans do not know and cannot identify one another, and there is no expression of need or request for help, people are primarily sensitive to their own reward and that prosociality is not required to explain behaviour. There is no question that behaviour is motivated by sensitivity to individual reward (Dayan & Niv, 2008; Sutton & Barto, 1998), and therefore it is the principle of parsimony here that suggests ruling out prosocial motivations, they are simply not required (as Laplace famously said, “Je n’avais pas besoin de cette hypothèse-là.”[1]). Individual differences are certainly present, but in keeping with other research, their effect seems relatively weak. Prosocial motivation may be relevant to explain some degree of individual differences, though these effects may be relatively small compared with the effects of context.

4 Implications and future work

The crux of this debate centres on the extent to which humans are motivated prosocially or selfishly. It is a long-standing question. It seems likely that human behaviour is a product of both motivations to some extent (Balliet et al., 2009; Henrich et al., 2005; Shen et al., 2024) and that there are a huge range of context-specific factors which make drawing generalisable conclusions difficult (Van Lange et al., 2013). In this set of experiments, the most parsimonious account does not require prosociality at an aggregate level, but it appears that some systematic individual differences exist. Prosociality clearly motivates a great deal of inspiring and beneficial behaviour, but the more pertinent questions here are how those behaviours are best reproduced in lab-

[1] “I had no need of that hypothesis”, attributed to Pierre-Simon Laplace, when questioned by Napoleon on the absence of God in his celestial model (early 19th century).

based social dilemmas, and critically, how can we be confident in measures which attribute behaviour to either selfish or prosocial motivations?

Possible avenues for future work that would extend the present findings can be grouped in terms of some broad research questions:

Conceptual foundations of prosociality

These questions concern what prosociality *is* and whether it should be treated as a unitary construct.

- If prosociality is not a coherent construct, how might we develop a more robust and predictive taxonomy of prosocial behaviours? What would such a taxonomy look like, and what implications would it have for future research?
- Is it philosophically and psychologically justified to distinguish between intentional and unintentional prosociality, and if so, how should this distinction be operationalised?

Measurement and experimental approaches

These questions focus on how prosociality can be identified and differentiated empirically, particularly in laboratory settings.

- What experimental methods can be used to distinguish intentional from unintentional prosociality within lab-based behavioural paradigms?
- Within the producer–scrounger task, can additional experimental manipulations be introduced to better isolate prosocial motivation and provide convergent evidence in observed decision-making?
- How meaningful and stable are the individual differences observed in prosocial behaviour across tasks and over time?

Decision dynamics and social representation in groups

These questions address how individuals make prosocial decisions in dynamic group contexts.

- In the group decision-making task, how dynamic are individual decisions relative to the group as a whole?

- Do participants represent the group as a single entity (analogous to a one-armed bandit), or as a collection of distinct individuals, and does this distinction meaningfully affect how they respond?
- To what extent can trial-by-trial decision-making be better understood using computational approaches, such as modelling learning dynamics or cyclical response patterns?

Space precludes a comprehensive treatment of each of these questions. Instead, the discussion below highlights a small number of illustrative directions that follow directly from the present findings and that, taken together, point toward a more precise account of prosociality.

On developing a taxonomy of prosociality, it's clear from many previous attempts that adoption and use are critical. The best way to do this would be to firstly ground it in a systematic theoretical framework, and secondly to make it useful ie. be able account for observable phenomena better than other taxonomies. Classification is fundamental to the scientific method (Bowker and Star 2021) and philosophy of science has tools to give a solid theoretical base, which can be applied to prosociality specifically. In particular, the idea that natural systems have consistent structures which allow them to be meaningfully classified, for example, in the way that the periodic table classifies the chemical elements (Kendig, 2023). It is more complex to attribute discrete kinds of mental state or function in human psychology (Hacking, 1996; Khalidi, 2010; Laimann, 2020), but has also been applied to the classification of psychopathologies and other cognitive functions (Kincaid and Sullivan 2014; Sullivan 2016; Khalidi 2023). The application of natural kinds to the study of prosociality could provide a theory-led, empirically informed way to improve our understanding. Candidate, competing taxonomies could be developed based on existing empirical and neuroscientific knowledge, and then tested experimentally using a battery of prosociality paradigms.

The extent to which intentionality is present in a prosocial behaviour could be assessed in a number of ways. One simple improvement could be to employ both the PSA and SVO, as well as the ABC approach (Weber et al. 2023), to probe different levels of prosocial orientation based on each method's proximity to the specific task. Another possibility would be to repeat with multiple studies over time and measure the consistency (test-retest reliability) of the individual behaviours. Repeating the experiment with a design and measures targeted on drawing out the prosocial versus selfish motivations would also be an interesting next step. One way that this could be done is through experimental design, such as the case of the "others only" PGG in which, as the name suggests, decisions to cooperate with the group carry no self-benefit (De Silva et al. 2010), or using the 'black box' method (Burton-Chellew and West 2022) which uses an asocial control as a benchmark for prosocial decision-making. This could be very effectively supplemented by inference from neural activity (fMRI or other modalities) to discern whether the different behaviours manifested themselves in distinctive neural activation patterns (Hein et al., 2016; Hu et al., 2023; Morelli et al., 2015; Paulus, 2018; Saulin et al., 2021). Computational modelling could supplement such studies, deriving parameter values which can be attributed to individual motivations based on the model (Lockwood & Klein-Flügge, 2020; Lugrin et al., 2023).

With respect to group decision-making, traditional single-agent reinforcement learning (RL) modelling yielded inconsistent results, likely reflecting their assumption that each decision-maker learns solely from individual reward signals in a stationary environment. In group contexts, however, the environment is inherently social and non-stationary, as outcomes depend on the evolving behaviour of others. To better capture these dynamics, future work could draw on the emerging field of multi-agent reinforcement learning (MARL), which explicitly models interacting agents whose learning processes are coupled through shared environments, group-level states, and interdependent rewards (Albrecht et al., 2021). Although MARL has primarily been developed to solve optimisation problems in cooperative multi-agent systems—such as task allocation, scheduling, and autonomous coordination—simplified formulations may be valuable as descriptive models of human group behaviour. In particular, MARL frameworks that allow agents to weight individual and collective rewards, or to learn from aggregate rather than individual-specific feedback. The literature in this area is sparse, though researchers have started to apply these approaches (Leibo et al., 2017; McKee et al., 2021). One benefit of this route is that MARL techniques could be applied to existing producer–scrounger data without requiring additional data collection.

5 The conceptualisation and measurement of prosociality

The discussion so far has focused on the pertinent findings, significance and implications of each empirical section; the links of prosociality and wealth globally, and the findings of the producer-scrounger experiments. I now turn to a theme that is common to both, described in the general introduction, concerning the conceptualisation and measurement of prosociality. In the vast literature on prosociality across different scientific disciplines, there are a range of perspectives which represent the particular needs and interests of each research community. This leads inevitably, and rightly, to definitional differences. For example, evolutionary biologists are generally interested in the ultimate cause, whereas psychologists will care more about the proximal cause, and the concepts and terminology they use will be honed for that purpose. Within a particular field though, these definitions should be clear, sharp and consistent, but as I and others have shown, this is not the case for the psychology of prosociality (Pfattheicher et al., 2021). Moreover, I suggest that sub-types of prosociality (such as cooperation, or altruism) make more coherent and fruitful objects of study. I now turn to reconsider both empirical studies in this context.

6.5.1 Global wealth study

A key strength of the prosociality and wealth study is that it shows a large degree of convergent validity and directionally uniform results. This lends support to the presence of a consistent effect across the different facets of prosociality and thus allows us to draw a generalisable conclusion. However, if we are being rigorous about measurement, it is also important to note the limits of the conclusion, given the negative relationship found between income and trust. If we accept that trust is a type of prosociality, as many researchers do (Gallup, Inc, n.d.; Kimbrough & Vostroknutov, 2013; Korndörfer et al., 2015; Zwirner & Raihani, 2020), and then make the general claim that prosociality increases with wealth, the fact that trust declines with increasing income is contrary to this claim. If we had asserted the link between prosociality and wealth as a law of nature (Carroll 2003), then that law would be falsified by the single anomalous result. But in general, psychological science does not make claims of this type because humans and human decision-making are too complex and statistically noisy, and instead tends to use inductive methods to draw generalised conclusions (Dienes 2008). Hence, claims like this are inherently probabilistic (even if this is not formalised mathematically), and this is a necessary and expected feature of psychological science.

The example of the trust-income relationship also serves to illustrate the point that single measures of prosociality (e.g. charity donations or giving behaviour in an economic game) should be treated with caution, given the manifest variation in neural and behavioural types of prosociality discussed in the introduction. If we had conducted a study *only* on trust and income, we might have concluded that prosociality *declines* with wealth. Another good example here is provided by meta-studies, which look at the variation of age with prosociality. They show that across a range of measures, the association of age and prosociality, the effects depend on the type of prosociality being tested (Li et al., 2024; Pollerhoff et al., 2024). Thus, it is not possible to conclude anything about prosociality per se, though conclusions may be drawn about its sub-types, such as trust or altruism, which vary across the lifespan. The consequence of this conclusion could be to revise what is included under the concept of prosociality so that it includes only subtypes that show consistent relationships to one another, or else to entirely segregate them so that only individual subtypes are measured.

6.5.2 Producer-scrounger games

The producer-scrounger experiments provide another example of the importance of clear conceptualisation and measurement. The producer-scrounger game is in essence a variant of the public goods game (PGG). Its unique features are that it uses a non-linear diminishing return curve and is played in a live group of human participants (except for the in-person study) over multiple rounds. Its comparative set therefore, is other lab-based economic games designed to measure prosociality, and social dilemmas such as PGGs in particular (Van Lange et al., 2013). In the general introduction, I suggest that, based on neural and behavioural evidence, prosociality should not be regarded as a singular construct and instead can be thought of as a loose collection of closely related attitudes and behaviours. I also posit that intention should play a key role in differentiating what is and isn't meaningfully prosocial concerning the psychology of human decision-making in line with many (though not all) accounts of prosociality (Bar-Tal, 1986; Batson & Powell, 2003; Eisenberg & Miller, 1987; K. Jensen, 2016; Mikulincer & Shaver, 2010). There are two distinct issues raised by this analysis, one of quantity – how much prosociality is happening, and relatedly, one of form – what *type* of prosociality is happening. I consider each of these now in turn.

With regard to quantity, what is striking about these results is that they show no evidence for prosocial behaviour at an aggregate level, contrary to previous interpretations of default human behaviour from classical economic games (Engel, 2011; Matsumoto et al., 2016; Tisserand, 2014; Van Lange et al., 2013). The key point is that the attribution of deviation from optimality in these classic games represents an untested assumption, rather than an externally validated measure. The measurement of psychological constructs is not straightforward, and the link between a construct and its operational definition is crucial (Uher 2023). More specifically, those studies do not take account of known human biases such as loss aversion or exploration-exploitation dynamics, nor do they have any means of differentiating stochastic noise from genuine prosocial behaviour or account for learning. This is of course, not to deny the insights furnished over many decades by such experiments – it is merely to point out that the assumptions of the measures may contain inaccuracies. Even if experiments such as the ultimatum game, dictator game and prisoner's dilemma contain systematic errors, it may still be that they are reliable, if biased, measures of prosociality. However, the fact that aggregate level prosociality disappears when we move away from a zero-based Nash equilibrium should be cause for healthy questioning of these assumptions. Indeed, this was a primary motivation of the early studies using producer-scrounger games (Kameda et al. 2010).

This is perhaps the most important contribution of these experiments. When non-zero equilibria are created in a public goods style game, this leads to non-zero responding that does not *require* a prosocial explanation, even a cooperative one, and can be entirely accounted for by self-interest. This suggests that some level of reinterpretation of the accuracy of classical economic games is required. Clearly, against such a large body of validated and replicable work, we should be cautious about over-interpreting results on the basis of one set of experiments, so here I turn to other studies that have reached similar conclusions.

One study used a PGG which had three different information conditions: the ’black box’ where participants were unaware that their choices benefited others, the standard condition where they were informed of how their choices would benefit others, and an ‘enhanced’ condition where the benefit to others was reinforced after each round of play (Burton-Chellew & West, 2013). Results showed that participants contributed the same amounts in the black box and standard conditions i.e. participants showed the same behaviour even when they had no possible prosocial motive. This supports the idea that play in PGG games is payoff-driven, not prosociality-driven. Furthermore, in the enhanced condition, there was significantly less cooperation. Not only should this have no effect if behaviour in the standard condition was prosocially motivated, but it also suggests the opposite hypothesis, that greater awareness of the benefit to others reduces prosociality. The same data was analysed using three different behavioural models, using basic personal reward-based learning, and then two variants: prosocial behaviour learning incorporating rewards for others, and conditional cooperation incorporating equality of incomes. The payoff-based learning model was clearly superior for all three versions of the PGG, in contrast to the pro-social and conditional-cooperation rules, which were typically non-significant or significant in the wrong direction (Burton-Chellew et al., 2015). In a later meta-study, which pitted a payoff-based learning rule against a variety of prosocially motivated alternatives and found that the payoff-based learning rule provided a better fit to the data across 237 linear public goods games, endorsing the view that participants are best described as “self-interested confused learners” (Burton-Chellew and West 2021). Shen et al (2024) attempted to directly test this confused learner hypothesis against that of prosocial preference in a PGG using hyper-cooperative partners. They demonstrated that neither could perfectly account for the observed outcomes, suggesting that humans may employ a blend of the two motivations. Their methodology (one-shot games) and conclusions focus more on refuting the ‘confusion’ aspect of the confused learner hypothesis, however. Furthermore, they do not thoroughly evaluate the possibility of payoff-based motivations, but focus on individual levels of cooperation. Their experiments did, however, replicate the black box findings (Burton-Chellew and West 2021), and so the evidence against a pure prosocial preference (the classical assumption) appears robust.

This is not to suggest that prosociality doesn't exist in this task, merely that there are good reasons to doubt the presence of a population level prosocial bias.

When considering the form of prosociality that is in evidence, we can clearly distinguish, from a theoretical perspective at least, two distinct types. The first is cooperation, the classic definition of which is a goal of mutual benefit, combined with the understanding that others will cooperate (Pruitt & Kimmel, 1977). Cooperation, therefore, could motivate any decision to produce up to the Nash equilibrium as it rewards both the self and others. The second is altruism, for which I use the standard definition as incurring some personal cost to benefit others (Fehr & Fischbacher, 2003). This could motivate decisions to produce above the Nash equilibria, where a personal cost takes effect. Previous neurobiologically grounded research has identified cooperation, equity and altruism as clusters of prosocial choice (Rhoads et al., 2021). Differentiating between types of prosociality is helpful as it provides a more nuanced understanding of the decisions people are making. It also allows the researcher to attribute different qualities to the different forms. Cooperation, by definition, results in benefits for both the self and others and is thus arguably a weaker form of prosocial behaviour compared to altruism (Claidière et al., 2015; West et al., 2007). Furthermore, any cooperative act may have both selfish and prosocial motives across a continuum of relative weights, from almost entirely selfish through equally balanced, to almost entirely prosocial. Some researchers have questioned whether *any* level of cooperation should be regarded as meaningfully cooperative behaviour, or whether this is should be stratified to some extent (Schäffer et al., 2025). Assessment of motivation is one of the fundamental challenges of psychological science, and not easily solved. In these experiments, post-task questionnaires and psychological trait measures helped to make an assessment of motivation, but future work already outlined would take this a step further.

In summary, when the non-linear nature of the payoff matrices in a PGG establishes non-zero Nash equilibria, this *necessitates* an alternative interpretation of non-zero responding (i.e. any decision to produce), which does not have to rely on any form of prosociality, at least for this experimental design. This conclusion aligns with the work of West and Burton-Chellew (2025) who argue against the canonisation of potentially incorrect conclusions in experiments that lack competing or null hypotheses.

6 In conclusion

In this thesis, I set out to consider how two different factors, wealth and group dynamics, affect prosocial decision-making in humans. The empirical findings stand independent of my subsequent higher-level interpretation. Firstly, there is a clear and consistent positive, globally robust relationship between prosociality and wealth. Secondly, when clear of any boundary, a non-linear payoff matrix can account for behaviour in a PGG purely based on self-reward. The course of these investigations led me to think more deeply about what constitutes prosociality and the implications for each of these studies.

The experiments I carried out were not designed to carve nature at these particular joints. The endless sub-division of categories is almost always possible, but is not always a desirable goal, rather, finding where the joints are that provide the most parsimonious and powerful explanations of behaviour (Magnus 2012). That these delineations exist is backed by extensive neural and behavioural evidence previously cited as well as that of well-established individual response typologies such as conditional cooperators, unconditional cooperators and free-riders (Fischbacher et al., 2001; Thöni & Volk, 2018). It would be fascinating in considering the nature of prosociality to determine whether different motivations could distinguish cooperative from altruistic behaviours, or whether they are similarly motivated. It is hoped that further work in this area might lead to more precise measurement of prosociality in all its forms, more robust and replicable findings and a better understanding of the workings of this vital human phenomenon.

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Appendices

1 Taxonomies of prosociality

Author(s)	Source type	Proposed taxonomy including sub-type definitions where available	Segmenting factors	Method and Sample (if applicable)
(K. Jensen, 2016)	Review	<u>Primary</u> **Informing:** “providing information that someone else needs, such as warning someone of danger” **Comforting:** “decreasing the distress of someone else, such as hugging them when they are sad” **Sharing:** “resource is given up, for example offering a piece of food to someone who is hungry” **Helping:** “recognising the goals of other individuals and working to see those goals achieved, such as opening a door for someone who is unable to” <u>Secondary</u> Rescuing Adopting Teaching Punishing free-riders		
(Pfattheicher et al., 2021)	Review	Provides a purely descriptive, rather than a normative account Prosociality (the highest level classification), containing: **Helping** **Co-operation** **Prosocial behaviour:** acts *intended* to benefit others (regardless of consequences)	Differentiated on three dimensions: 1)Intentions and motives 2)Costs and benefits (consequences) 3)Societal norms	Literature review

Author(s)	Source type	Proposed taxonomy including sub-type definitions where available	Segmenting factors	Method and Sample (if applicable)
		Altruism: acts intended to benefit others without the expectation (intention) of reward		
(Dunfield, 2014)	Review	**Helping:** "e.g., retrieving an out of reach object" **Sharing:** "e.g., giving up a limited resource" **Comforting:** "e.g., offering verbal or physical support"	Response to the type of need: Helping = instrumental need (depends on recognising goal directed behaviour in others), Sharing = unmet material need, Comforting = others negative emotional states	Analysis of child development onset stages
(Nezlek, 2022)	Survey	Defines two domains **Interpersonal:** caring for friends and people around you **Ideological:** cares for nature, respects others opinions even if different, believes everyone should be treated equally.	Ideological correlates with liberal attitudes on environment, sexuality, income equality, immigration, whereas interpersonal has zero or negative correlations with these things.	European Social Survey (23 countries) Measures based on Schwartz Universalism (ideological) and Benevolence (interpersonal)
(Rhoads et al., 2021)	Meta-study	**Co-operative:** decisions that depend on others, or contain uncertainty **Equitable:** adherence to social norms and unilateral decisions by single agent **Altruistic:** outcomes with no benefit to deciding agent, or unilateral decisions in response to distress/need of others		Meta-study (N=43) feature-based clustering of decision making tasks and fMRI
(Dovidio et al., 2006)	Book chapter	**Prosocial behaviour:** "actions that are defined by society as generally beneficial to other people and to the ongoing political system" Subcategories,		

Author(s)	Source type	Proposed taxonomy including sub-type definitions where available	Segmenting factors	Method and Sample (if applicable)
		Helping: 'an action that has the consequence of providing some benefit to the well-being of another person' e.g. giving a gift. Further sub-categories provided by McGuire (1994) **Altruism:** 'often seen as a specific type of helping' with the added characteristic that the *motivation* is to benefit another **Co-operation:** a group of actors contribute together in an attempt to achieve a common goal.		
(McGuire, 1994)	Behavioural study	**Casual helping:** low cost, low consequence, do not require close bond e.g. lending a pen, laughing at a joke **Substantial personal helping:** higher cost, require more personal bond, e.g. lending a car **Emotional helping:** high intimacy, but no material cost e.g. comforting, listening **Emergency helping:** response to acute need e.g. accident victim, protecting someone from harassment	Reported behaviours were ranked and then factor analysed by 22 scales, categorised into: frequency, characteristics (e.g. cost/benefit), antecedents, immediate consequences, long-term consequences	
(Batson & Powell, 2003)		**Prosocial behaviour** contrasted with altruism as a motivational state (not a behaviour) An act is altruistic if the primary motivation is other's welfare even if self-benefits are accrued 'Altruism is a motivational state with the ultimate goal of increasing another's welfare'		
(Pearce & Amato, 1980)	Survey	Proposed three dimensions of helping: Planned, formal <– > Spontaneous, informal (social setting) Serious <– > Non-serious (need of recipient) Giving, indirect <– > Doing, direct (type of help)		Multi-dimensional scaling of self-report survey items amongst 72 Australian undergraduates

Author(s)	Source type	Proposed taxonomy including sub-type definitions where available	Segmenting factors	Method and Sample (if applicable)
(Wispé, 1972)	Review	**Altruism:** a regard for the interest of others without concern for one's self interest **Sympathy:** a concern with, or a sharing of, the pain or sadness of another person, or even an animal **Cooperation:** the willingness and ability to work with others, usually but not always for a common benefit **Donating:** the action of making a gift or giving a contribution, usually to a charity **Helping:** to the giving of assistance or aid toward a definite object or end		
(Carlo & Randall, 2002)	Survey	**Compliant:** 'helping others in response to a verbal or nonverbal request' **Anonymous:** 'helping performed without knowledge of whom helped' **Altruistic:** 'voluntary helping motivated primarily by concern for the needs and welfare of another' **Public:** 'conducted in front of an audience are likely to be motivated, at least in part, by a desire to gain the approval and respect of others' **Emotional:** 'helping others under emotionally evocative circumstances' **Dire:** 'helping in crisis or emergency situations'		Survey, factor analysis and replication. College students (N1 = 249) and Psychology students (N=40)
(Luengo Kanacri et al., 2021)	Survey	**General prosociality disposition,** supplemented by two sub-factors, **Prosocial Actions:** helping, sharing, caring **Prosocial Feelings:** empathic responses		Multi-country replication and CFA (N = 1,630) on Prosociality Scale for Adults (Caprara et al., 2005)

2 Python code for mixed-strategy Nash equilibria

```python
# Required packages
import numpy as np
from scipy.stats import binom
import matplotlib.pyplot as plt

# Number of other players
n = 5

# Original (rounded) payoff values
# reward_scrounge = [-18, -1, 4, 7, 9, 10] # LOW_COST (winter)
reward_scrounge = [-8, 9, 14, 17, 19, 20] # HIGH-COST (summer)
reward_produce = [-3, 2, 5, 7, 8, 9]

# Probability values from 0 to 1
p_values = np.linspace(0, 1, 1001)

# Define expected payoff functions
def expected_payoff(p):
  probs = binom.pmf(range(6), n, p)
  E_S = sum(p_d * r_d for p_d, r_d in zip(probs, reward_scrounge))
  E_P = sum(p_c * r_c for p_c, r_c in zip(probs, reward_produce))
  return E_S, E_P

# Compute payoff differences
diffs = []
for p in p_values:
  E_S, E_P = expected_payoff(p)
  diffs.append(E_S - E_P)

# Find equilibrium point(s)
diffs = np.array(diffs)
equilibrium_indices = np.where(np.abs(diffs) < 1e-3)[0]
equilibrium_probs = p_values[equilibrium_indices]

# Plot payoff difference
plt.plot(p_values, diffs, label='E[Scrounge] - E[Produce]', color='blue')
plt.axhline(0, color='gray', linestyle='--')
plt.xlabel("Probability of producing (p)")
plt.ylabel("Expected payoff difference")
plt.title("Mixed Strategy Nash Equilibrium (Rounded Payoffs)")
plt.grid(True)
plt.legend()
plt.tight_layout()
plt.show()

# Display equilibrium probabilities
print("Equilibrium cooperation probability:", equilibrium_probs)
```

3 Supplementary materials method and results

Italicised text taken directly from Gallup World Poll methodology, September 2012 (Gallup, Inc, n.d.)

Wealth measures

Subjective wealth measure

"The Financial Wellbeing Index measures respondents' personal economic situations and the economics of the community where they live."

Index Questions

"Which one of these phrases comes closest to your own feelings about your household's income these days: living comfortably on present income, getting by on present income, finding it difficult on present income, or finding it very difficult on present income?"
"Are you satisfied or dissatisfied with your standard of living, all the things you can buy and do?"
"Right now, do you feel your standard of living is getting better or getting worse?"
"Right now, do you think that economic conditions in the city or area where you live, as a whole, are getting better or getting worse?"

Index Construction

"Index scores are calculated at the individual record level. For each individual record, respondents who say they are "living comfortably on present income" are recoded as "1," and all other answers are recoded as "0." The remaining items are recoded so that positive answers are scored as a "1" and all other answers (including don't know and refused) are assigned a score of "0." A record's final index score is the average of the mean for responses to the first question and the mean of the three other items multiplied by 100."

In line with Gallup's standard procedures, for the first question respondents who said they were "living comfortably on present income" were scored as 1 and all other answers as 0. The remaining three items were scored with positive answers as a 1 and all other answers as 0. The final index score was calculated using the average of the mean for responses to question (i) and the mean of the three other items multiplied by 100.

On examining the data, it was observed that household income had a typical Lorenz curve distribution, with extreme outliers at high incomes and was best modelled using a transformed logarithmic scale yielding an approximately normal distribution in line with other studies (Kosse & Tincani, 2020). This was then standardised, and the resultant z-score used as the predictor variable. As a result of this transformation, quadratic effects were difficult to interpret and only linear relationships are reported in the main results section, with quadratic effects included for completeness (Table S1). In addition to the high-income outliers, there were also a notable number of very low incomes, suggestive of measurement error. Thus, we cropped the log income scale at +/- 3 standard deviations from the mean, resulting in 143 observations (0.2%) being removed from the analysis. Of these 133 were smaller than -3 SD i.e. less than $199 per annum total household income, and 10 were larger than +3 SD, i.e. greater than $382,825.

Prosociality measures

The GPS defines positive reciprocity as the propensity to return a prosocial act, measured by two items (i) the level of financial gift (€5-30 equivalent) that respondents would give as a gift after a stranger gives them directions, and (ii) self-report Likert scale response 0-11 on how willing they are in general to return favours, these items receiving roughly equal weights. Altruism was defined as (i) self-report Likert scale response 0-11 how willing they respondents would be to give to good causes without expecting anything in return, and (ii) how much of an unexpected €1,000 windfall they would donate to charity. These two items were weighted about equally. Trust was measured by a single item Likert scale 0-10 response on whether respondents assume that other people only have the best intentions (Likert scale, 0-10). Trust in this instance therefore represents social, rather than institutional, trust. Negative reciprocity is the tendency to punish others for unfair behaviour and was measured by three items, (i) Likert scale response 0-10 on how willing respondents were to take revenge if treated unjustly, even if it there is a cost to doing so, (ii) willingness to punish someone for unfair behaviour, either towards themselves or, (iii) towards a third person (prosocial punishment). Each of the three items received roughly equal weighting.

These measures were defined prior to the data collection for the GPS, via the process of independent external experimental validation. In this process, respondents completed multiple survey items along with incentivised behavioural tasks. The items which correlated most strongly (highest R2) with actual behaviour in the tasks were then selected for use in the GPS. The behavioural experiment for altruism was measured using amounts donated to charity in a dictator game. Trust was based on the amounts sent as first moved in two investment games. Similarly, positive reciprocity was measured as the amounts returned as receiver in two investment games. Negative reciprocity was measured by the amount given to punishment after an opponent's defection in a prisoner's dilemma game, and by the minimum offer deemed acceptable in an ultimatum game.

The GWP (which is linked to the GPS at an individual level) provides three measures of prosocial behaviour; donating money, volunteering, or helping a stranger. In each case respondents were asked "Have you done any of the following in the past month? How about (i) “donated money to a charity?”, (ii) “volunteered your time to an organization?”, and (iii) “helped a stranger or someone you didn’t know who needed help?”. Each item had a simple binary response such that positive answers were scored as a 1 and all other answers given a score of 0. All prosociality measures from the GWP and GPS were rescaled to standardised scores prior to analysis.

Moderators

Precarity (Food & Shelter Index) measure:

“The Food and Shelter Index measures whether a respondent has experienced deprivation in the areas of food and shelter. Two items that ask about respondents’ ability to afford food or shelter in the past year compose this index. Lower scores on this index indicate that more respondents reported struggling to afford food and shelter in the past year, while higher scores indicate fewer respondents reported such struggles.”

Index Questions

“Have there been times in the past 12 months when you did not have enough money to buy food that you or your family needed?”

“Have there been times in the past 12 months when you did not have enough money to provide adequate shelter or housing for you and your family?”

Index Construction

“Index scores are calculated at the individual record level. For each individual

record the two items are recoded so that positive (or favorable) answers are scored a “1” and all other answers (including don’t know and refused) are assigned a score of “0.” An individual record has an index calculated if it has valid scores for both questions. A record’s final index score is the mean of valid items multiplied by 100.”

This index was modelled as a discrete variable with four levels: no precarity, food precarity only, shelter precarity only, and both, testing for fixed effects and their interaction effect with wealth. We used ANOVA omnibus tests to identify for each model whether there was a significant interaction effect overall with the wealth variable (Table S4). For each significant interaction we then conducted post-hoc significance tests using (G)LMM’s to establish which specific factor levels were responsible for the differences (Table S5).

We also examined whether three distinct country-level factors moderated associations between income / financial well-being and prosociality by including their fixed effects and the interaction effect with wealth.

Gross National Income measure

For this we used per capita Gross National Income from the World Bank (GNI per capita, Atlas method (current US$), n.d.) (pre-registered).

Family Ties measure

The Family Ties measure (Alesina & Giuliano, 2014) is constructed from the World Values Survey. It measures the strength of relationships within family units at intra- and inter-country levels. The measure is correlated with a range of highly relevant social and economic outcomes; trust, labour market participation, stress, and well-being suggesting good external validity. On obtaining the data it was found that the Family Ties metric as described in in the pre-registration was not available due to a change in the WVS questionnaire for Wave 6 (2010-2014). Instead, the weighted PCA measure was substituted with a single-item response to the question 'How important is family in your life' scaled; 1=Very important, 2=Rather important, 3=Not very important, 4=Not at all important (reverse scored in the analysis) that was available within the same time period of the GPS data.

Individualist-Collectivist culture measure

The individualism-collectivism dimension (Minkov et al., 2017) uses data taken from over 50,000 respondents across 56 countries.

We ran additional models including GNI and family ties respectively as country-level predictors, testing for potential interaction effects with wealth prosociality (Table S7-8). As GNI and HH Income were expected to be correlated we tested models which included both with the variance inflation factor (VIF) to check for collinearity issues. Though there are no strict rule for what constitutes a problematic VIF measure, values of 1 represent complete orthogonality, 5-10 are generally considered cautionary, whereas 10+ is regarded as problematic (Hair, 1992; Thompson et al., 2017). These were found not to be of material concern in our case, with the maximum being 3.96 in behavioural models, and all close to 1 in the preference models (Table S11).

Supplementary Results

For all tables β represents regression coefficients, OR = odds ratios and CI = confidence intervals.

Table S1: Quadratic effects of all wealth-prosociality models

Wealth measure	Prosociality measure	β (x^2)	CI low	CI high	*p*-value
		Preferences (LMM standardized regression coefficients)			
Subjective	Altruism	-3.97	-5.96	-1.98	**<0.001**
Objective	Altruism	0.39	-2.38	3.15	0.780
Subjective	Negative reciprocity	-1.27	-3.33	0.79	0.230
Objective	Negative reciprocity	1.68	-1.26	4.62	0.260
Subjective	Positive Reciprocity	-0.75	-2.81	1.31	0.470
Objective	Positive Reciprocity	3.03	0.10	5.96	0.043
Subjective	Trust	-5.35	-7.43	-3.27	**<0.001**
Objective	Trust	-3.36	-6.13	-0.58	0.018
		Behaviours (GLMM odds ratios)			
Subjective	Donated	-16.43	-21.58	-11.28	**<0.001**
Objective	Donated	12.78	4.79	20.76	**0.002**
Subjective	Volunteered	-8.24	-13.65	-2.84	**0.003**
Objective	Volunteered	9.34	2.30	16.38	**0.009**
Subjective	Helped	-9.73	-14.37	-5.09	**<0.001**
Objective	Helped	4.17	-2.43	10.77	0.220

Note: the effects reported here are orthogonal to the lower order main effects, and as a result the coefficients are not directly comparable. This is done so that independent significance of quadratic effects in the models can be seen.

Table S2: Main effects of all wealth-prosociality models

Wealth measure	Prosociality measure	Main effect	CI low	CI high	*p*-value
		Preferences (LMM standardized regression coefficients)			
Subjective	Altruism	0.085	0.072	0.098	**<.001**
Objective	Altruism	0.097	0.077	0.117	**<.001**
Subjective	Negative reciprocity	0.025	0.009	0.041	**0.002**
Objective	Negative reciprocity	0.051	0.027	0.075	**<.001**
Subjective	Positive Reciprocity	0.065	0.050	0.081	**<.001**
Objective	Positive Reciprocity	0.112	0.089	0.136	**<.001**
Subjective	Trust	0.033	0.018	0.049	**<.001**
Objective	Trust	-0.027	-0.046	-0.008	**0.005**
		Behaviours (GLMM odds ratios)			
Subjective	Donated	1.384	1.334	1.436	**<.001**
Objective	Donated	1.562	1.443	1.690	**<.001**
Subjective	Volunteered	1.221	1.177	1.267	**<.001**
Objective	Volunteered	1.157	1.099	1.218	**<.001**
Subjective	Helped	1.200	1.153	1.250	**<.001**
Objective	Helped	1.271	1.210	1.336	**<.001**

Note: significant effects ($p<.01$) in bold

Table S3: Comparison of regression coefficients from main models using z-score

Comparison pair	β_1 / OR_1	β_2 / OR_2	z-score	*p*-value
OBJECTIVE WEALTH - PREFERENCE MODELS				
Altruism - Trust	0.097	-.027	12.25	**< .001**
Altruism – Positive reciprocity	0.097	0.112	-0.98	.167
Altruism – Negative reciprocity	0.097	0.051	4.07	**< .001**
Trust – Positive reciprocity	-.027	0.112	-11.07	**< .001**
Trust – Negative reciprocity	-.027	0.051	-12.48	**< .001**
Positive reciprocity – negative reciprocity	0.112	0.051	4.53	**< .001**
OBJECTIVE WEALTH - BEHAVIOUR MODELS				
Donating – Volunteering	0.446	0.146	3.24	**< .001**
Donating – Helping	0.446	0.240	2.11	.017
Volunteering - Helping	0.146	0.240	-2.10	.018
SUBJECTIVE WEALTH - PREFERENCE MODELS				
Altruism – Trust	0.085	0.033	6.90	**< .001**
Altruism – Positive reciprocity	0.085	0.065	2.21	.014
Altruism – Negative reciprocity	0.085	0.025	8.26	**< .001**
Trust – Positive reciprocity	0.033	0.065	-4.93	**< .001**
Trust – Negative reciprocity	0.033	0.025	2.20	.014
Positive reciprocity – negative reciprocity	0.065	0.025	6.48	**< .001**
SUBJECTIVE WEALTH - BEHAVIOUR MODELS				
Donating – Volunteering	0.325	0.200	2.38	**.009**
Donating – Helping	0.325	0.182	2.75	**.003**
Volunteering - Helping	0.200	0.182	0.47	.320

Note: significant effects ($p<.01$) in bold

Table S4: Model comparison results of precarity as a moderator on the wealth-prosociality association

	Objective wealth			Subjective wealth		
	X^2	*p*-value	Direction	X^2	*p*-value	Direction
			Preferences			
Positive reciprocity	8.6	0.036	-	**31.5**	**<.001**	**Negative**
Altruism	**15.1**	**0.002**	**Negative**	3.7	0.297	-
Trust	5.4	0.144	-	0.1	0.992	-
Negative reciprocity	1.2	0.756	-	4.9	0.176	-
			Behaviours			
Donating	5.5	0.136	-	**19.0**	**<.001**	**Positive**
Volunteering	1.2	0.762	-	**15.4**	**0.002**	**Positive**
Helping	**21.2**	**<.001**	**Positive**	**11.8**	**0.008**	**Positive**

Note: significant effects (p<.01) in bold. X^2 is chi-square statistic.

Table S5: Main effects of precarity on prosocial preferences and behaviours controlling for wealth (showing only effects where p<.01)

	Precarity measure	Wealth control	Main effect coefficient	CI Low	CI High	*p*-value
			β			
Altruism	F/S	Objective	-0.0629	-0.0887	-0.0371	<0.001
	F/S	Subjective	-0.0696	-0.0939	-0.0452	<0.001
Positive reciprocity	F	Objective	-0.0343	-0.0597	-0.0089	0.008
	F/S	Objective	-0.0515	-0.0776	-0.0254	<0.001
	F	Subjective	-0.0387	-0.0645	-0.0129	0.003
	F/S	Subjective	-0.0707	-0.0960	-0.0455	<0.001
Negative reciprocity	F/S	Objective	0.0636	0.0372	0.0900	<0.001
	F/S	Subjective	0.0630	0.0376	0.0883	<0.001
Trust	F/S	Objective	0.0510	0.0246	0.0774	<0.001
	F/S	Subjective	0.0781	0.0525	0.1036	<0.001
			log-odds			
Donating	F	Objective	-0.1196	-0.1954	-0.0439	0.002
Volunteering	S	Objective	0.1830	0.0780	0.2880	<0.001
	F/S	Objective	0.2215	0.1412	0.3019	<0.001
	S	Subjective	0.2221	0.1177	0.3265	<0.001
	F/S	Subjective	0.2700	0.1993	0.3407	<0.001
Helping a stranger	F	Objective	0.1254	0.0573	0.1935	<0.001
	S	Objective	0.1286	0.0382	0.2191	0.005
	F/S	Objective	0.1266	0.0565	0.1968	<0.001
	F	Subjective	0.1300	0.0654	0.1946	<0.001
	S	Subjective	0.1337	0.0429	0.2246	0.004

Note: F = Food precarity (in the last 12 months), S = Shelter, F/S = Food and Shelter

Table S6: Per country model effect sizes

Effect	Proportion of positive country effects	Proportion of negative country effects	Binomial test ($H_0 = .50$; $H_1 > H_0$) p-value	Proportion of positive effects significant (*p*<.05)	Proportion of negative effects significant (*p*<.05)
Altruism	67/76 **(88%)**	9/76 (12%)	**< .001**	36/67 (54%)	1/9 (11%)
Positive reciprocity	69/76 **(91%)**	7/76 (9%)	**<.001**	41/69 (59%)	1/7 (14%)
Negative reciprocity	53/76 **(70%)**	23/76 (30%)	**<.001**	23/53 (43%)	6/23 (26%)
Trust	33/76 (43%)	43/76 **(57%)**	.900	2/33 (6%)	12/43 (28%)
Donating	52/57 **(91%)**	5/57 (9%)	**< .001**	45/52 (87%)	0/5 (0%)
Volunteering	45/57 **(79%)**	12/57 (21%)	**< .001**	11/44 (24%)	1/12 (8%)
Helping	48/57 **(84%)**	9/57 (16%)	**< .001**	26/48 (54%)	0/9 (0%)
Altruism	64/68 **(94%)**	4/68 (6%)	**< .001**	45/64 (70%)	0/4 (0%)
Positive reciprocity	56/68 **(82%)**	12/68 (18%)	**< .001**	30/56 (54%)	0/12 (0%)
Negative reciprocity	44/68 **(65%)**	24/68 (35%)	**.010**	12/44 (27%)	6/24 (25%)
Trust	50/68 **(74%)**	18/68 (26%)	**<.001**	18/50 (36%)	4/18 (22%)
Donating	59/59 **(100%)**	0/59 (0%)	**< .001**	52/59 (88%)	0/0 -
Volunteering	57/59 **(97%)**	2/59 (3%)	**< .001**	29/57 (51%)	0/2 (0%)
Helping	49/59 **(83%)**	10/59 (17%)	**< .001**	30/49 (61%)	0/10 (0%)

Note: Columns show numbers of countries matching the criteria with proportions in parentheses. The proportions shown in **bold** match the direction of the global effect. The third column shows whether those proportions were significant at *p*<.01. The final two columns show the proportion of each effect direction that were found to be significant at *p*<.01 for a main effect.

Table S7: Interaction effects between country GNI and wealth

Wealth measure	Prosociality measure	Interaction coefficient	95% CI	*p*-value
		β		
Subjective	Altruism	0.00	(-0.01, 0.02)	0.708
Objective	Altruism	0.00	(-0.02, 0.02)	0.891
Subjective	Negative reciprocity	0.00	(-0.02, 0.02)	0.795
Objective	Negative reciprocity	0.03	(0.00, 0.05)	0.043
Subjective	Positive Reciprocity	-0.01	(-0.03, 0.01)	0.231
Objective	Positive Reciprocity	0.01	(-0.15, 0.01)	0.427
Subjective	Trust	0.00	(-0.02, 0.02)	0.938
Objective	Trust	0.02	(0.00, 0.04)	0.049
		log-odds		
Subjective	Donated	-0.06	(-0.10, -0.02)	**0.005**
Objective	Donated	0.10	(0.01, 0.19)	0.036
Subjective	Volunteered	-0.06	(-0.09, -0.02)	**0.001**
Objective	Volunteered	-0.02	(-0.09, 0.06)	0.695
Subjective	Helped	-0.06	(-0.10, -0.02)	**0.001**
Objective	Helped	-0.05	(-0.10, -0.00)	0.060

Note: significant effects (p<.01) in bold

Table S8: Interaction effects between Individualism-Collectivism and wealth

Wealth measure	Prosociality measure	Interaction coefficient	95% CI	*p*-value
		β		
Subjective	Altruism	0.00	(-0.01, 0.02)	0.682
Objective	Altruism	0.00	(-0.03, 0.02)	0.657
Subjective	Negative reciprocity	0.00	(-0.03, 0.03)	0.921
Objective	Negative reciprocity	0.03	(0.00, 0.06)	0.048
Subjective	Positive Reciprocity	-0.02	(-0.04, -0.00)	0.042
Objective	Positive Reciprocity	0.00	(-0.03, 0.03)	0.964
Subjective	Trust	0.00	(-0.02, 0.02)	0.785
Objective	Trust	0.01	(-0.00, 0.03)	0.232
		log-odds		
Subjective	Donated	-0.03	(-0.07, 0.00)	0.110
Objective	Donated	0.10	(0.01, 0.18)	0.022
Subjective	Volunteered	-0.05	(-0.09, -0.02)	**< .001**
Objective	Volunteered	0.03	(-0.03, 0.08)	0.367
Subjective	Helped	-0.06	(-0.10, -0.02)	**0.005**
Objective	Helped	-0.04	(-0.10, 0.02)	0.168

Note: significant effects (p<.01) in bold

Table S9: Interaction effects between Family Ties and wealth

Wealth measure	Prosociality measure	Interaction coefficient	95% CI	*p*-value
		β		
Subjective	Altruism	-0.01	(-0.04, 0.01)	0.178
Objective	Altruism	0.00	(-0.02, 0.03)	0.817
Subjective	Negative reciprocity	0.00	(-0.03, 0.03)	0.921
Objective	Negative reciprocity	-0.01	(-0.05, 0.02)	0.425
Subjective	Positive Reciprocity	-0.03	(-0.06, -0.01)	0.023
Objective	Positive Reciprocity	0.01	(-0.02, 0.05)	0.374
Subjective	Trust	0.00	(-0.02, 0.02)	0.960
Objective	Trust	0.00	(-0.03, 0.02)	0.667
		log-odds		
Subjective	Donated	0.00	(-0.05, 0.06)	0.842
Objective	Donated	0.08	(-0.08, 0.25)	0.331
Subjective	Volunteered	-0.02	(-0.07, 0.03)	0.400
Objective	Volunteered	0.00	(-0.08, 0.08)	0.974
Subjective	Helped	0.00	(-0.08, 0.06)	0.798
Objective	Helped	0.06	(0.00, 0.12)	0.048

Note: significant effects (p<.01) in bold

Table S10: Cited papers for income-wealth relationships showing cognitive ability controls

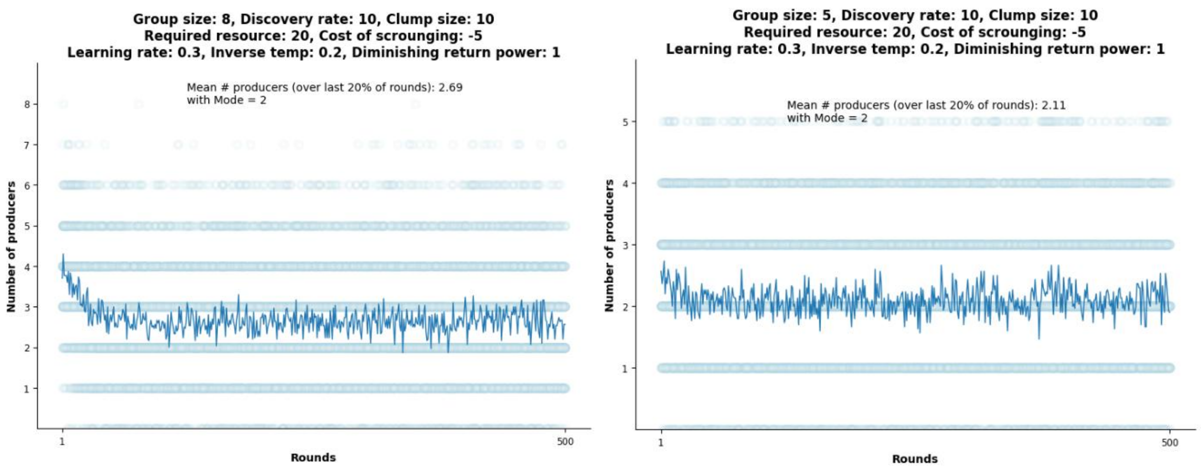
Paper	Cognitive ability controls
(Ananyev & Guriev, 2019)	Educational level included, but none for cognitive ability
(Steijn & Lancee, 2011)	Educational level included, but none for cognitive ability
(Morrone, 2009)	None
(Brandt et al., 2015)	None

Table S11: Variance Inflation Factors for GNI x Household Income

Wealth measure	Prosociality measure	VIF GNI main effect	VIF GNI interaction effect
Objective	Altruism	1.014	1.010
Objective	Negative reciprocity	1.017	1.012
Objective	Positive Reciprocity	1.014	1.010
Objective	Trust	1.027	1.018
Objective	Donated	1.520	1.519
Objective	Volunteered	3.641	3.595
Objective	Helped	3.956	3.878

4 ABM validation

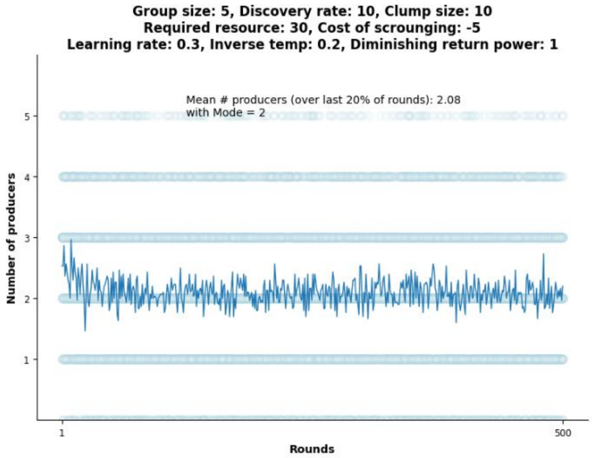
Validation: Tables and plots
A.Group size: Larger group size -> more scroungers (Group size of 5 has around 4 scroungers, group size of 8 has around 6 scroungers)

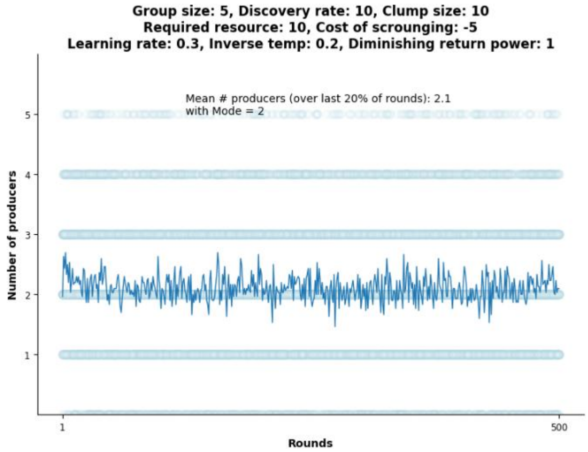


	Group size = 5			Group size = 8		
Simulation	Mean # producers (last 20% of rounds)	Std. dev	Mode	Mean # producers (last 20% of rounds)	Std. dev	Mode
1	2.10	1.08	2	2.85	1.59	3
2	1.94	1.12	2	2.53	1.31	3
3	1.97	1.04	2	2.70	1.38	3
4	2.10	1.14	2	2.74	1.47	3
5	2.07	1.17	2	2.62	1.39	2
6	2.25	1.11	2	2.56	1.32	2
7	2.18	1.12	2	2.71	1.35	3
8	2.15	1.14	2	2.69	1.40	3
9	2.35	1.25	2	2.64	1.12	3
10	2.14	1.07	2	2.59	1.30	3
11	2.13	1.06	2	2.63	1.38	3
12	2.22	1.22	2	2.78	1.39	2
13	1.96	0.96	2	2.66	1.33	2
14	2.22	1.28	2	2.46	1.33	2
15	2.03	1.06	2	2.91	1.52	2
16	1.99	1.10	2	2.80	1.48	2
17	2.01	1.04	2	2.76	1.39	2
18	2.05	0.94	2	2.60	1.40	3
19	2.14	1.02	2	2.96	1.73	3

20	1.99	1.13	2	2.70	1.38	3
21	2.31	1.19	2	2.66	1.40	2
22	1.95	1.04	2	2.54	1.31	2
23	2.10	1.16	2	2.68	1.37	2
24	2.02	1.11	2	2.77	1.33	2
25	2.04	1.08	2	2.52	1.34	2
26	2.30	1.20	2	2.65	1.24	3
27	2.21	1.18	2	2.85	1.36	3
28	2.16	1.03	2	2.78	1.43	2
29	1.96	1.01	2	2.54	1.42	3
30	2.12	1.05	2	2.74	1.14	3

B.Metabolic requirement: no effect

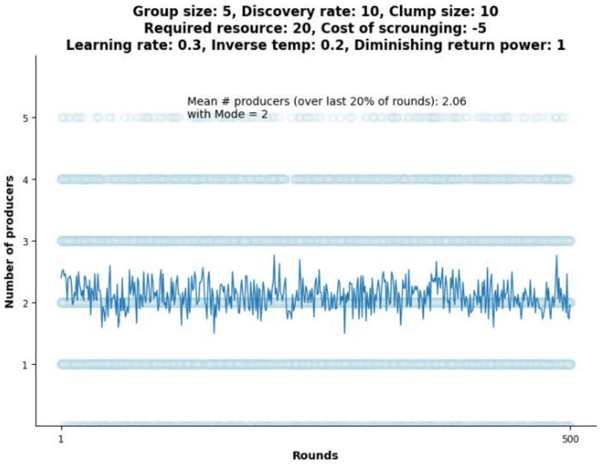


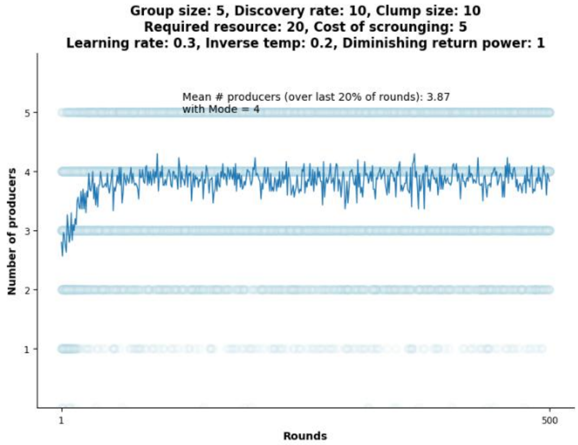


	Metabolic requirement = 10			Metabolic requirement = 30		
Simulation	Mean # producers (last 20% of rounds)	Std. dev	Mode	Mean # producers (last 20% of rounds)	Std. dev	Mode
1	2.04	1.10	2	2.20	1.15	2
2	2.15	1.16	2	1.97	1.13	2
3	2.08	1.15	2	2.09	1.15	2
4	2.10	1.12	2	1.90	1.17	2
5	2.01	1.17	2	1.91	1.06	2
6	2.00	0.93	2	2.10	1.10	2
7	2.27	1.32	2	2.18	1.17	2
8	2.15	1.13	2	2.24	1.18	2
9	2.13	1.08	2	1.98	1.06	2
10	2.19	1.05	2	2.17	1.12	2
11	2.09	1.13	2	1.96	1.12	2
12	2.26	1.11	2	2.24	1.15	3
13	1.75	0.98	2	2.10	1.10	2
14	2.01	1.07	2	2.02	1.04	2
15	2.07	1.12	2	2.08	1.06	2
16	2.29	1.17	2	2.33	1.20	2
17	1.89	1.08	2	2.08	1.11	2
18	2.09	1.04	2	2.04	1.05	2
19	2.12	1.04	2	1.89	1.05	2
20	2.20	1.26	2	1.99	1.01	2
21	2.35	1.11	2	2.25	1.23	2
22	2.05	1.08	2	1.95	1.02	2
23	2.25	1.15	2	2.32	1.16	2
24	1.95	1.08	2	2.19	1.07	2
25	2.27	1.25	2	2.08	1.03	2
26	2.03	1.21	2	1.96	1.06	2
27	2.09	1.02	2	1.86	1.04	2

28	2.25	1.24	2	2.21	1.16	2
29	1.96	1.07	2	1.97	1.08	2
30	2.01	0.98	2	2.10	1.12	2

C.Cost of scrounging

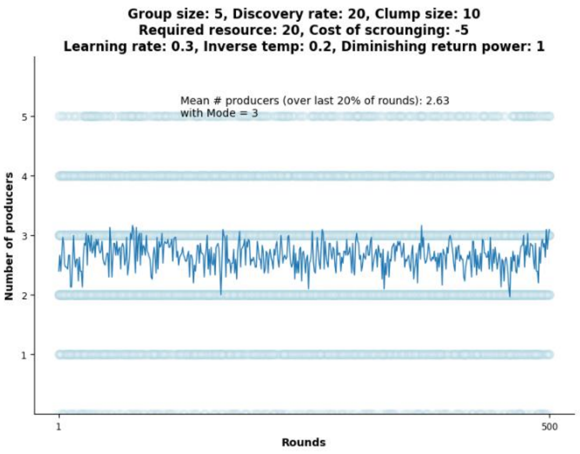


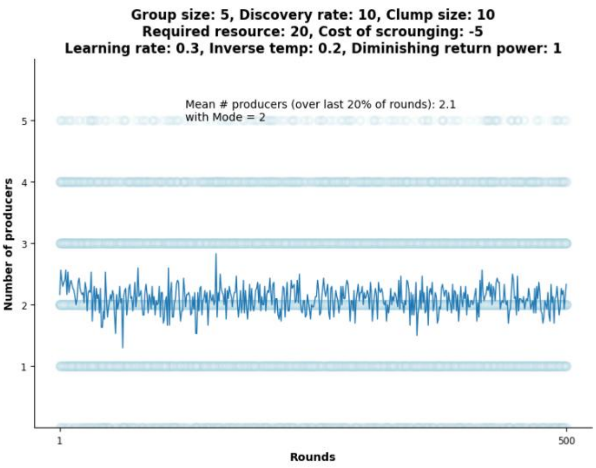


	Cost of scrounging = -5			Cost of scrounging = +5		
Simulation	Mean # producers (last 20% of rounds)	Std. dev	Mode	Mean # producers (last 20% of rounds)	Std. dev	Mode
1	3.88	0.90	4	2.00	1.11	2
2	3.65	1.05	4	2.27	1.22	2
3	3.88	0.89	4	1.85	0.93	2
4	3.94	1.05	4	1.94	1.07	2
5	3.71	0.92	4	2.23	1.16	2
6	3.98	0.91	4	2.15	1.15	2
7	3.78	1.00	4	1.95	1.13	2
8	3.90	0.92	4	2.12	1.16	2
9	3.87	1.00	4	1.97	1.14	2
10	3.87	0.83	4	2.11	1.08	2
11	3.96	0.88	4	1.87	0.98	2
12	3.96	0.98	4	1.90	1.11	2
13	3.70	0.99	4	1.90	1.10	2
14	3.88	0.98	4	1.97	1.08	2
15	3.83	0.86	4	2.04	1.06	2
16	4.20	0.87	4	2.17	1.08	2
17	3.79	0.94	4	2.23	1.27	2
18	3.84	0.95	4	2.21	1.20	2
19	3.81	0.91	4	2.09	1.20	2
20	3.84	0.97	4	1.86	1.00	2
21	3.86	0.91	4	2.24	1.15	2
22	3.90	0.94	4	2.27	1.15	2
23	3.77	0.90	4	1.96	0.94	2

24	3.98	0.86	4	1.94	0.96	2
25	3.90	0.96	4	2.24	1.10	2
26	3.95	0.93	4	2.07	1.10	2
27	4.09	0.93	4	2.06	1.08	2
28	3.80	0.85	4	2.01	1.04	2
29	3.81	0.98	4	2.08	1.10	2
30	3.85	1.04	4	2.17	1.19	2

D.Discovery rate





	Discovery rate = 20			Discovery rate = 10		
Simulation	Mean # producers (last 20% of rounds)	Std. dev	Mode	Mean # producers (last 20% of rounds)	Std. dev	Mode
1	2.48	1.01	3	2.22	1.18	2
2	2.76	1.24	3	2.37	1.29	2
3	2.66	1.06	3	2.13	1.08	2
4	3.27	1.33	3	2.05	1.09	2
5	2.75	1.25	3	2.20	1.16	2
6	2.65	1.25	2	2.16	1.32	2
7	2.74	1.14	3	1.90	1.04	2
8	2.58	1.20	3	2.13	1.21	2
9	2.31	1.07	2	2.03	1.08	2
10	2.60	1.17	3	2.14	1.17	2
11	2.16	1.06	2	2.32	1.15	2
12	2.43	1.12	2	2.24	1.15	1
13	2.58	1.25	3	2.20	1.20	2
14	2.92	1.20	2	2.13	1.06	2
15	2.42	1.13	3	1.90	0.99	2
16	2.73	1.14	3	2.04	1.09	2
17	2.46	1.14	2	1.98	1.08	2
18	2.34	1.00	2	2.02	1.03	2
19	2.85	1.23	3	2.26	1.21	2

20	2.92	1.18	3	2.01	1.02	2
21	2.77	1.20	3	1.85	1.08	2
22	2.29	1.03	3	2.03	1.03	2
23	2.86	1.20	3	1.97	0.94	2
24	2.73	1.16	3	2.28	1.06	2
25	2.61	1.22	3	2.07	1.10	2
26	2.76	1.23	3	2.15	0.98	2
27	2.45	1.02	3	1.94	1.06	2
28	2.72	1.22	3	2.07	1.18	2
29	2.56	1.06	3	2.17	1.07	2
30	2.48	1.13	2	1.97	1.11	2

5 Linear mixed models – model comparison

SAMPLE 1

Model	Fixed effects (RHS)	Random effects	logLik	AIC	Akaike weight
m0	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr)	-10098	20208	0.000
m1	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + condition | playerNr)	-9649	19314	0.000
m2	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + gp_rnd_search_minus1 | playerNr)	-9960	19933	0.000
m3	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + choice_minus1 | playerNr)	-9562	19138	0.000
m4	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + period_bl | playerNr)	-10097	20208	0.000
m5	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + condition | playerNr) + (0 + choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1 | playerNr)	-9293	18607	0.000
m6	period_bl + condition * gp_rnd_search_minus1 * choice_minus1	(1 | playerNr)	-10013	20046	0.000
m7	period_bl + condition + gp_rnd_search_minus1 + choice_minus1 + condition:gp_rnd_search_minus1 + condition:choice_minus1 + gp_rnd_search_minus1:choice_minus1 + condition:gp_rnd_search_minus1:choice_minus1	(1 | playerNr) + (0 + condition | playerNr) + (0 + choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1 | playerNr) + (0 + condition:choice_minus1 | playerNr) + (0 + condition:gp_rnd_search_minus1 | playerNr) + (0 + choice_minus1:gp_rnd_search_minus1 | playerNr) + (0 + condition:choice_minus1:gp_rnd_search_minus1 | playerNr)	-9221	18477	0.814
m8	period_bl + gp_rnd_search_minus1 + condition + choice_minus1 + gp_rnd_search_minus1:condition + choice_minus1:condition + choice_minus1:gp_rnd_search_minus1	(1 | playerNr) + (0 + period_bl | playerNr) + (0 + gp_rnd_search_minus1 | playerNr) + (0 + condition | playerNr) + (0 + choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1:condition | playerNr) + (0 + condition:choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1:choice_minus1 | playerNr)	-9224	18480	0.186

<u>Variable key:</u> period_bl = the trial number within each block; condition = either high- or low-cost; gp_rnd_search_minus1 = the previous trial group outcome expressed as a number (0-6); choice_minus1 = participant choice (produce or scrounge) on the previous trial

SAMPLE 2

Model	Fixed effects (RHS)	Random effects	logLik	AIC	Akaike weight
m0	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr)	-10454	20920	0.000
m1	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + condition | playerNr)	-10050	20116	0.000
m2	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + gp_rnd_search_minus1 | playerNr)	-10296	20607	0.000
m3	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + choice_minus1 | playerNr)	-9927	19867	0.000
m4	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + period_bl | playerNr)	-10450	20914	0.000
m5	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + condition | playerNr) + (0 + choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1 | playerNr)	-9716	19452	0.000
m6	period_bl + condition * gp_rnd_search_minus1 * choice_minus1	(1 | playerNr)	-10428	20876	0.000
m7	period_bl + condition + gp_rnd_search_minus1 + choice_minus1 + condition:gp_rnd_search_minus1 + condition:choice_minus1 + gp_rnd_search_minus1:choice_minus1 + condition:gp_rnd_search_minus1:choice_minus1	(1 | playerNr) + (0 + condition | playerNr) + (0 + choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1 | playerNr) + (0 + condition:choice_minus1 | playerNr) + (0 + condition:gp_rnd_search_minus1 | playerNr) + (0 + choice_minus1:gp_rnd_search_minus1 | playerNr) + (0 + condition:choice_minus1:gp_rnd_search_minus1 | playerNr)	-9694	19425	0.756
m8	period_bl + gp_rnd_search_minus1 + condition + choice_minus1 + gp_rnd_search_minus1:condition + choice_minus1:condition + choice_minus1:gp_rnd_search_minus1	(1 | playerNr) + (0 + period_bl | playerNr) + (0 + gp_rnd_search_minus1 | playerNr) + (0 + condition | playerNr) + (0 + choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1:condition | playerNr) + (0 + condition:choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1:choice_minus1 | playerNr)	-9697	19427	0.244

IN-PERSON

Model	Fixed effects (RHS)	Random effects	logLik	AIC	Akaike weight
m0	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr)	-2227	4466	0.000
m1	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + condition | playerNr)	-2212	4439	0.000
m2	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + gp_rnd_search_minus1 | playerNr)	-2226	4465	0.000
m3	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + choice_minus1 | playerNr)	-2209	4434	0.000
m4	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + period_bl | playerNr)	-2221	4457	0.000
m5	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + condition | playerNr) + (0 + choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1 | playerNr)	-2195	4409	0.022
m6	period_bl + condition * gp_rnd_search_minus1 * choice_minus1	(1 | playerNr)	-2218	4457	0.000
m7	period_bl + condition + gp_rnd_search_minus1 + choice_minus1 + condition:gp_rnd_search_minus1 + condition:choice_minus1 + gp_rnd_search_minus1:choice_minus1 + condition:gp_rnd_search_minus1:choice_minus1	(1 | playerNr) + (0 + condition | playerNr) + (0 + choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1 | playerNr) + (0 + condition:choice_minus1 | playerNr) + (0 + condition:gp_rnd_search_minus1 | playerNr) + (0 + choice_minus1:gp_rnd_search_minus1 | playerNr) + (0 + condition:choice_minus1:gp_rnd_search_minus1 | playerNr)	-2187	4410	0.014
m8	period_bl + gp_rnd_search_minus1 + condition + choice_minus1 + gp_rnd_search_minus1:condition + choice_minus1:condition + choice_minus1:gp_rnd_search_minus1	(1 | playerNr) + (0 + period_bl | playerNr) + (0 + gp_rnd_search_minus1 | playerNr) + (0 + condition | playerNr) + (0 + choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1:condition | playerNr) + (0 + condition:choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1:choice_minus1 | playerNr)	-2185	4401	0.965

RICH-POOR

Model	Fixed effects (RHS)	Random effects	logLik	AIC	Akaike weight
m0	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr)	-8022	16057	0.000
m1	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + condition | playerNr)	-7974	15965	0.000
m2	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + gp_rnd_search_minus1 | playerNr)	-7989	15992	0.000
m3	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + choice_minus1 | playerNr)	-7744	15501	0.000
m4	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + period_bl | playerNr)	-8019	16052	0.000
m5	period_bl + condition + gp_rnd_search_minus1 + choice_minus1	(1 | playerNr) + (0 + condition | playerNr) + (0 + choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1 | playerNr)	-7722	15464	0.000
m6	period_bl + condition * gp_rnd_search_minus1 * choice_minus1	(1 | playerNr)	-8008	16036	0.000
m7	period_bl + condition + gp_rnd_search_minus1 + choice_minus1 + condition:gp_rnd_search_minus1 + condition:choice_minus1 + gp_rnd_search_minus1:choice_minus1 + condition:gp_rnd_search_minus1:choice_minus1	(1 | playerNr) + (0 + condition | playerNr) + (0 + choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1 | playerNr) + (0 + condition:choice_minus1 | playerNr) + (0 + condition:gp_rnd_search_minus1 | playerNr) + (0 + choice_minus1:gp_rnd_search_minus1 | playerNr) + (0 + condition:choice_minus1:gp_rnd_search_minus1 | playerNr)	-7706	15449	0.971
m8	period_bl + gp_rnd_search_minus1 + condition + choice_minus1 + gp_rnd_search_minus1:condition + choice_minus1:condition + choice_minus1:gp_rnd_search_minus1	(1 | playerNr) + (0 + period_bl | playerNr) + (0 + gp_rnd_search_minus1 | playerNr) + (0 + condition | playerNr) + (0 + choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1:condition | playerNr) + (0 + condition:choice_minus1 | playerNr) + (0 + gp_rnd_search_minus1:choice_minus1 | playerNr)	-7712	15456	0.029

6 Linear Mixed models – winning model results

SAMPLE 1

Winning model specification: period_bl + condition + gp_rnd_search_minus1 + choice_minus1 + (1 | playerNr) + (0 + condition * choice_minus1 * gp_rnd_search_minus1 || playerNr) + condition:gp_rnd_search_minus1 + condition:choice_minus1 + gp_rnd_search_minus1:choice_minus1 + condition:gp_rnd_search_minus1:choice_minus1

Parameter	**Coefficient**	**SE**	**CI**	**CI_low**	**CI_high**	**z**	**p**
(Intercept)	0.25	0.03	0.95	0.20	0.33	-10.31	<0.001
period_bl	0.90	0.02	0.95	0.87	0.94	-5.24	<0.001
conditionWinter	2.20	0.36	0.95	1.60	3.03	4.81	<0.001
gp_rnd_search_minus1	1.00	0.04	0.95	0.94	1.08	0.11	0.91
choice_minus1	1.19	0.21	0.95	0.84	1.69	0.96	0.34
conditionWinter:gp_rnd_search_minus1	1.10	0.06	0.95	0.98	1.22	1.65	0.099
conditionWinter:choice_minus1	3.55	0.96	0.95	2.09	6.02	4.70	<0.001
gp_rnd_search_minus1:choice_minus1	0.97	0.05	0.95	0.87	1.08	-0.61	0.54
conditionWinter:gp_rnd_search_minus1:choice_minus1	0.95	0.08	0.95	0.81	1.11	-0.61	0.54

SAMPLE 2

Winning model specification:
period_bl + condition + gp_rnd_search_minus1 + choice_minus1 + (1 | playerNr) + (0 + condition * choice_minus1 * gp_rnd_search_minus1 || playerNr) + condition:gp_rnd_search_minus1 + condition:choice_minus1 + gp_rnd_search_minus1:choice_minus1 + condition:gp_rnd_search_minus1:choice_minus1

Parameter	**Coefficient**	**SE**	**CI**	**CI_low**	**CI_high**	**z**	**p**
(Intercept)	0.13	0.02	0.95	0.10	0.17	-16.31	<0.001
period_bl	0.82	0.02	0.95	0.79	0.86	-10.00	<0.001
conditionWinter	3.01	0.39	0.95	2.32	3.89	8.38	<0.001
gp_rnd_search_minus1	1.07	0.04	0.95	1.00	1.15	1.83	0.067
choice_minus1	2.00	0.35	0.95	1.42	2.81	4.00	<0.001
conditionWinter:gp_rnd_search_minus1	1.00	0.05	0.95	0.90	1.10	-0.05	0.96
conditionWinter:choice_minus1	1.83	0.40	0.95	1.19	2.81	2.76	0.006
gp_rnd_search_minus1:choice_minus1	0.89	0.05	0.95	0.79	1.01	-1.88	0.061
conditionWinter:gp_rnd_search_minus1:choice_minus1	1.01	0.08	0.95	0.86	1.17	0.07	0.95

SAMPLES COMBINED

Winning model specification:
period_bl + condition + gp_rnd_search_minus1 + choice_minus1 + (1 | playerNr) + (0 + condition * choice_minus1 * gp_rnd_search_minus1 || playerNr) + condition:gp_rnd_search_minus1 + condition:choice_minus1 + gp_rnd_search_minus1:choice_minus1 + condition:gp_rnd_search_minus1:choice_minus1

Parameter	**Coefficient**	**SE**	**CI**	**CI_low**	**CI_high**	**z**	**p**
(Intercept)	0.18	0.02	0.95	0.15	0.21	-18.79	<0.001
period_bl	0.86	0.01	0.95	0.84	0.88	-10.81	<0.001
conditionWinter	2.64	0.27	0.95	2.16	3.23	9.52	<0.001
gp_rnd_search_minus1	1.04	0.03	0.95	0.99	1.10	1.61	0.11
choice_minus1Search	1.57	0.20	0.95	1.23	2.00	3.60	<0.001
conditionWinter:gp_rnd_search_minus1	1.04	0.04	0.95	0.96	1.12	0.96	0.34
conditionWinter:choice_minus1Search	2.38	0.41	0.95	1.69	3.34	5.00	<0.001
gp_rnd_search_minus1:choice_minus1Search	0.92	0.04	0.95	0.85	1.00	-2.06	0.039
conditionWinter:gp_rnd_search_minus1:choice_minus1Search	1.00	0.06	0.95	0.90	1.12	0.04	0.97

IN-PERSON

Winning model specification:
period_bl + gp_rnd_search_minus1 + condition + choice_minus1 + gp_rnd_search_minus1:condition + choice_minus1:condition + choice_minus1:gp_rnd_search_minus1 + (1 + period_bl + gp_rnd_search_minus1 + condition + choice_minus1 + gp_rnd_search_minus1:condition + choice_minus1:condition + choice_minus1:gp_rnd_search_minus1 || playerNr)

Parameter	**Coefficient**	**SE**	**CI**	**CI_low**	**CI_high**	**z**	**p**
(Intercept)	0.47	0.11	0.95	0.29	0.74	-3.19	0.001
period_bl	0.99	0.01	0.95	0.98	1.00	-1.18	0.24
gp_rnd_search_minus1	1.09	0.08	0.95	0.95	1.25	1.25	0.21
conditionWinter	2.76	0.74	0.95	1.63	4.65	3.80	<0.001
choice_minus1Stay	1.04	0.24	0.95	0.67	1.63	0.18	0.86
gp_rnd_search_minus1:conditionWinter	0.98	0.07	0.95	0.85	1.13	-0.29	0.77
conditionWinter:choice_minus1Stay	0.72	0.14	0.95	0.48	1.06	-1.67	0.095
gp_rnd_search_minus1:choice_minus1Stay	0.88	0.06	0.95	0.76	1.01	-1.86	0.063

RICH-POOR

Winning model specification:
choice.r ~ period_bl + condition + gp_rnd_search_minus1 + choice_minus1 +
(1 | playerNr) + (0 + condition * choice_minus1 * gp_rnd_search_minus1 || playerNr) +
condition:gp_rnd_search_minus1 + condition:choice_minus1 + gp_rnd_search_minus1:choice_minus1 +
condition:gp_rnd_search_minus1:choice_minus1

Parameter	**Coefficient**	**SE**	**CI**	**CI_low**	**CI_high**	**z**	**p**
(Intercept)	0.31	0.04	0.95	0.24	0.41	-8.40	<0.001
period_bl	0.97	0.00	0.95	0.96	0.97	-8.76	<0.001
conditionWinter	0.90	0.09	0.95	0.73	1.10	-1.03	0.30
gp_rnd_search_minus1	1.04	0.04	0.95	0.96	1.12	0.96	0.34
choice_minus1	0.83	0.16	0.95	0.57	1.21	-0.96	0.34
conditionWinter:gp_rnd_search_minus1	1.25	0.06	0.95	1.14	1.38	4.62	<0.001
conditionWinter:choice_minus1	2.07	0.46	0.95	1.34	3.21	3.26	0.001
gp_rnd_search_minus1:choice_minus1	1.20	0.07	0.95	1.07	1.35	3.11	0.002
conditionWinter:gp_rnd_search_minus1:choice_minus1	0.76	0.06	0.95	0.65	0.88	-3.56	<0.001

7 Linear contrast anova

Sample 1 linear contrast anova for high-cost/scrounge choice:

Group outcome, trial t	Estimate	Std. Error	t	value
0	0.04828	0.00778	6.20	<0.001
1	0.01012	0.00387	2.61	0.051
2	-0.01583	0.00438	-3.61	0.002
3	-0.02953	0.00709	-4.16	<0.001
4	-0.02506	0.01688	-1.48	0.551
5	0.06354	0.04662	1.36	0.639

Table shows difference from mean producing of all outcomes on trial t+1

8 Correlation matrices

Correlation matrix of behaviour with task questionnaire items combining original and replication samples. All significant ($p < .05$, FWE adjusted) correlations are marked with a coloured circle, green for a positive association, red for negative. Numbers in each circle show the value of Pearson correlation coefficient, r.

CHAPTER 3 - TASK QUESTIONNAIRE & BEHAVIOUR

Table section 1

Variable	**Choice mean**	**Choice mean (high-cost)**	**Choice mean (low-cost)**	**Choice diff (L - H)**	**Max personal**	**Max group**	**Feel search**
Choice mean							
Choice mean (high-cost)	0.82 (<0.001****)						
Choice mean (low-cost)	0.90 (<0.001****)	0.50 (<0.001****)					
Choice diff (L - H)	0.31 (<0.001****)	-0.29 (<0.001****)	0.69 (<0.001****)				
Max personal	-0.58 (<0.001****)	-0.55 (<0.001****)	-0.46 (<0.001****)	-0.05 (0.322)			
Max group	0.49 (<0.001****)	0.52 (<0.001****)	0.36 (<0.001****)	-0.04 (0.446)	-0.41 (<0.001****)		
Feel search	0.16 (<0.001***)	0.10 (0.057.)	0.17 (<0.001***)	0.11 (0.032*)	-0.03 (0.64)	0.03 (0.565)	
Zero averse	0.27 (<0.001****)	0.23 (<0.001****)	0.24 (<0.001****)	0.07 (0.178)	-0.10 (0.06.)	0.45 (<0.001****)	0.09 (0.063.)
Co-operative	0.55 (<0.001****)	0.53 (<0.001****)	0.44 (<0.001****)	0.04 (0.447)	-0.47 (<0.001****)	0.66 (<0.001****)	0.06 (0.272)
Was influenced	0.21 (<0.001****)	0.18 (<0.001***)	0.18 (<0.001***)	0.05 (0.381)	-0.11 (0.031*)	0.33 (<0.001****)	0.11 (0.023*)
Inf other search	0.49 (<0.001****)	0.40 (<0.001****)	0.44 (<0.001****)	0.16 (0.002**)	-0.26 (<0.001****)	0.35 (<0.001****)	0.08 (0.097.)
Inf other stay	0.41 (<0.001****)	0.42 (<0.001****)	0.31 (<0.001****)	-0.01 (0.868)	-0.29 (<0.001****)	0.41 (<0.001****)	-0.03 (0.532)
Tried to influence	0.24 (<0.001****)	0.22 (<0.001****)	0.20 (<0.001****)	0.04 (0.477)	-0.20 (<0.001****)	0.37 (<0.001****)	0.09 (0.094.)
Others reacted	0.03 (0.64)	0.06 (0.231)	-0.01 (0.858)	-0.06 (0.211)	-0.01 (0.858)	0.22 (<0.001****)	-0.01 (0.902)
Kept track	0.10 (0.055.)	0.09 (0.093.)	0.08 (0.102)	0.02 (0.689)	0.01 (0.902)	0.23 (<0.001****)	0.13 (0.012*)

Table section 2

Variable	Zero averse	Co-operative	Was influenced	Inf other search	Inf other stay	Tried to influence	Others reacted
Co-operative	0.40 (<0.001****)						
Was influenced	0.47 (<0.001****)	0.29 (<0.001****)					
Inf other search	0.21 (<0.001****)	0.36 (<0.001****)	0.15 (0.003**)				
Inf other stay	0.37 (<0.001****)	0.36 (<0.001****)	0.25 (<0.001****)	0.01 (0.902)			
Tried to influence	0.33 (<0.001****)	0.26 (<0.001****)	0.47 (<0.001****)	0.19 (<0.001****)	0.12 (0.013*)		
Others reacted	0.18 (<0.001***)	0.15 (0.002**)	0.31 (<0.001****)	0.08 (0.128)	0.08 (0.129)	0.40 (<0.001****)	
Kept track	0.31 (<0.001****)	0.12 (0.019*)	0.42 (<0.001****)	0.12 (0.013*)	0.14 (0.004**)	0.46 (<0.001****)	0.35 (<0.001****)

Variable	Choice mean	Choice mean (high-cost)	Choice mean (low-cost)	Choice diff (L - H)	Empathy	Prosociality	Gp affiliation	Psychopathy
Choice mean								
Choice mean (high-cost)	0.82							
	(<0.001****)							
Choice mean (low-cost)	0.90	0.50						
	(<0.001****)	(<0.001****)						
Choice diff (L - H)	0.31	-0.29	0.69					
	(<0.001****)	(<0.001****)	(<0.001****)					
Empathy	-0.06	-0.02	-0.07	-0.07				
	(0.286)	(0.754)	(0.159)	(0.217)				
Prosociality	-0.06	0.05	-0.13	-0.18	0.60			
	(0.256)	(0.366)	(0.013*)	(<0.001***)	(<0.001****)			
Gp affiliation	-0.01	0.08	-0.07	-0.15	0.21	0.37		
	(0.837)	(0.132)	(0.159)	(0.003**)	(<0.001****)	(<0.001****)		
Psychopathy	0.05	-0.03	0.10	0.13	-0.32	-0.38	-0.16	
	(0.334)	(0.587)	(0.064.)	(0.011*)	(<0.001****)	(<0.001****)	(0.002**)	

Variable	Choice mean	Choice mean (high-cost)	Choice mean (low-cost)	Choice diff (L - H)	SES	Precarity	Income (personal)	Income (HH)	Fin wellbeing
Choice mean									
Choice mean (high-cost)	0.82								
	(<0.001****)								
Choice mean (low-cost)	0.90	0.50							
	(<0.001****)	(<0.001****)							
Choice diff (L - H)	0.31	-0.29	0.69						
	(<0.001****)	(<0.001****)	(<0.001****)						
SES	0.00	-0.00	0.01	0.01					
	(0.971)	(0.971)	(0.971)	(0.971)					
Precarity	-0.02	-0.01	-0.02	-0.02	0.23				
	(0.867)	(0.971)	(0.813)	(0.853)	(<0.001****)				
Income (personal)	-0.00	-0.05	0.04	0.09	-0.23	-0.19			
	(0.971)	(0.553)	(0.725)	(0.214)	(<0.001****)	(0.001**)			
Income (HH)	0.05	0.06	0.03	-0.01	-0.37	-0.15	0.63		
	(0.528)	(0.398)	(0.725)	(0.971)	(<0.001****)	(0.003**)	(<0.001****)		
Fin wellbeing	-0.00	0.03	-0.03	-0.06	-0.30	-0.18	0.23	0.28	
	(0.971)	(0.725)	(0.725)	(0.351)	(<0.001****)	(<0.001***)	(<0.001****)	(<0.001****)	

CHAPTER 3 - RISK MEASURES (SAMPLE 2 ONLY)

Variable	Choice mean	Choice mean (high-cost)	Choice mean (low-cost)	Choice diff (L - H)	Risk preference	Risk ethical	Risk financial	Risk H&S	Risk recreational	Risk social
Choice mean										
Choice mean (high-cost)	0.81									
	(<0.001****)									
Choice mean (low-cost)	0.90	0.48								
	(<0.001****)	(<0.001****)								
Choice diff (L - H)	0.34	-0.27	0.71							
	(<0.001****)	(<0.001****)	(<0.001****)							
	(0.694)	(0.835)	(0.433)	(0.268)						
Risk pref	0.04	-0.01	0.06	0.08						
	(0.694)	(0.914)	(0.525)	(0.416)						
Risk ethical	-0.02	-0.05	0.01	0.05	0.57					
	(0.862)	(0.608)	(0.914)	(0.608)	(<0.001****)					
Risk financial	-0.05	-0.08	-0.01	0.05	0.65	0.29				
	(0.617)	(0.388)	(0.873)	(0.617)	(<0.001****)	(<0.001****)				
Risk H&S	0.05	-0.01	0.09	0.11	0.67	0.46	0.21			
	(0.585)	(0.873)	(0.324)	(0.187)	(<0.001****)	(<0.001****)	(0.00354**)			
Risk recreational	0.09	0.06	0.09	0.05	0.71	0.20	0.26	0.37		
	(0.326)	(0.552)	(0.326)	(0.608)	(<0.001****)	(0.0047**)	(<0.001***)	(<0.001****)		
Risk social	0.02	0.03	0.00	-0.02	0.52	0.05	0.26	0.11	0.20	
	(0.835)	(0.75)	(0.953)	(0.835)	(<0.001****)	(0.585)	(<0.001***)	(0.176)	(0.00585**)	

CHAPTER 4 (IN-PERSON) – TASK & BEHAVIOURAL MEASURES

Table Section 1

Variable	Choice mean	Choice mean (high-cost)	Choice mean (low-cost)	Choice diff (L - H)	Max personal	Max group	Feel search
Choice mean							
Choice mean (high-cost)	0.88 (<0.001****)						
Choice mean (low-cost)	0.91 (<0.001****)	0.67 (<0.001****)					
Choice diff (L - H)	0.17 (0.419)	-0.28 (0.157)	0.52 (0.00191**)				
Max personal	-0.69 (<0.001****)	-0.45 (0.0114*)	-0.72 (<0.001****)	-0.42 (0.0209*)			
Max group	0.67 (<0.001****)	0.54 (0.00151**)	0.69 (<0.001****)	0.28 (0.165)	-0.50 (0.00343**)		
Feel search	0.02 (0.929)	0.03 (0.897)	0.10 (0.669)	0.09 (0.694)	-0.03 (0.897)	0.39 (0.0349*)	
Zero averse	0.50 (0.00398**)	0.38 (0.0416*)	0.53 (0.0017**)	0.25 (0.22)	-0.38 (0.0416*)	0.67 (<0.001****)	0.42 (0.0209*)
Co-operative	0.68 (<0.001****)	0.55 (0.00112**)	0.64 (<0.001****)	0.20 (0.324)	-0.55 (<0.001***)	0.81 (<0.001****)	0.25 (0.215)
Was influenced	0.23 (0.278)	0.31 (0.115)	0.17 (0.415)	-0.13 (0.527)	0.24 (0.245)	0.35 (0.0591.)	0.08 (0.704)
Inf other search	0.57 (<0.001***)	0.44 (0.0127*)	0.56 (<0.001***)	0.22 (0.294)	-0.54 (0.0014**)	0.58 (<0.001***)	0.15 (0.468)
Inf other stay	0.58 (<0.001***)	0.55 (0.00104**)	0.48 (0.00555**)	-0.01 (0.964)	-0.35 (0.0645.)	0.56 (<0.001***)	0.18 (0.398)
Tried to influence	0.15 (0.489)	0.18 (0.377)	0.01 (0.964)	-0.20 (0.324)	0.02 (0.929)	0.20 (0.324)	-0.03 (0.928)
Others reacted	-0.18 (0.398)	-0.05 (0.848)	-0.14 (0.515)	-0.12 (0.561)	0.24 (0.232)	0.13 (0.527)	0.35 (0.0604.)
Kept track	-0.17 (0.406)	-0.09 (0.7)	-0.24 (0.252)	-0.21 (0.321)	0.22 (0.28)	0.08 (0.726)	0.09 (0.689)

Table section 2

Variable	Zero averse	Co-operative	Was influenced	Inf other search	Inf other stay	Tried to influence	Others reacted
Co-operative	0.57 (<0.001***)						
Was influenced	0.30 (0.123)	0.22 (0.294)					
Inf other search	0.37 (0.0466*)	0.60 (<0.001***)	-0.10 (0.657)				
Inf other stay	0.59 (<0.001***)	0.63 (<0.001***)	0.17 (0.406)	0.38 (0.0416*)			
Tried to influence	0.21 (0.316)	0.38 (0.0416*)	0.41 (0.0229*)	0.25 (0.224)	0.33 (0.0886.)		
Others reacted	0.01 (0.964)	-0.01 (0.964)	0.30 (0.128)	-0.09 (0.68)	0.05 (0.837)	0.22 (0.294)	
Kept track	0.08 (0.704)	0.08 (0.704)	0.44 (0.0131*)	-0.04 (0.887)	0.16 (0.459)	0.56 (<0.001***)	0.31 (0.11)

CHAPTER 4 (IN-PERSON) – PSYCHOLOGICAL TRAITS

Variable	**Choice mean**	**Choice mean (high-cost)**	**Choice mean (low-cost)**	**Choice diff (L - H)**	**Empathy**	**Prosociality**	**Gp affiliation**	**Psychopathy**
Choice mean								
Choice mean (high-cost)	0.88 (<0.001****)							
Choice mean (low-cost)	0.91 (<0.001****)	0.67 (<0.001****)						
Choice diff (L - H)	0.17 (0.415)	-0.28 (0.205)	0.52 (0.00215**)					
Empathy	0.01 (0.973)	0.06 (0.771)	-0.08 (0.718)	-0.17 (0.415)				
Prosociality	0.25 (0.296)	0.28 (0.205)	0.17 (0.415)	-0.11 (0.642)	0.58 (<0.001***)			
Gp affiliation	0.15 (0.478)	0.32 (0.141)	-0.02 (0.945)	-0.39 (0.0478*)	0.18 (0.415)	0.09 (0.7)		
Psychopathy	0.18 (0.415)	0.07 (0.718)	0.21 (0.384)	0.18 (0.415)	-0.23 (0.325)	-0.23 (0.323)	-0.20 (0.384)	

CHAPTER 4 (IN-PERSON) – FINANCIAL MEASURES

Variable	**Choice mean**	**Choice mean (high-cost)**	**Choice mean (low-cost)**	**Choice diff (L - H)**	**SES**	**Precarity**	**Income (personal)**	**Income (HH)**	**Fin wellbeing**
Choice mean									
Choice mean (high-cost)	0.88								
	(<0.001****)								
Choice mean (low-cost)	0.91	0.67							
	(<0.001****)	(<0.001****)							
Choice diff (L - H)	0.17	-0.28	0.52						
	(0.525)	(0.192)	(0.00257**)						
SES	-0.14	0.03	-0.25	-0.36					
	(0.573)	(0.954)	(0.275)	(0.0727.)					
Precarity	0.17	0.32	0.01	-0.36	0.10				
	(0.525)	(0.112)	(0.977)	(0.0726.)	(0.707)				
Income (personal)	-0.28	-0.33	-0.29	0.01	-0.04	-0.05			
	(0.573)	(0.525)	(0.573)	(0.977)	(0.961)	(0.957)			
Income (HH)	0.03	-0.09	0.10	0.24	-0.62	0.09	-0.36		
	(0.956)	(0.738)	(0.72)	(0.349)	(<0.001***)	(0.738)	(0.549)		
Fin wellbeing	-0.34	-0.41	-0.27	0.12	-0.47	-0.19	0.22	0.39	
	(0.0825.)	(0.0317*)	(0.198)	(0.651)	(0.00957**)	(0.455)	(0.681)	(0.0723.)	

Variable	Choice mean	Choice mean (V-high-cost)	Choice mean (high-cost)	Choice diff (H - VH)	Empathy	Low SES	Prosociality	Gp affiliation	Psychopathy
Choice mean									
Choice mean (V-high-cost)	0.96								
	(<0.001****)								
Choice mean (high-cost)	0.97	0.85							
	(<0.001****)	(<0.001****)							
Choice diff (H - VH)	0.24	-0.05	0.48						
	(<0.001***)	(0.733)	(<0.001****)						
Empathy	-0.12	-0.15	-0.09	0.08					
	(0.16)	(0.0653.)	(0.378)	(0.418)					
Low SES	0.01	0.01	0.00	-0.01	-0.11				
	(0.963)	(0.963)	(0.963)	(0.963)	(0.209)				
Prosociality	-0.00	-0.01	0.00	0.03	0.62	-0.19			
	(0.963)	(0.963)	(0.963)	(0.871)	(<0.001****)	(0.0104*)			
Gp affiliation	0.05	0.08	0.02	-0.08	0.35	-0.04	0.45		
	(0.733)	(0.449)	(0.922)	(0.417)	(<0.001****)	(0.794)	(<0.001****)		
Psychopathy	-0.04	-0.04	-0.04	-0.02	-0.35	0.17	-0.35	-0.22	
	(0.759)	(0.794)	(0.759)	(0.933)	(<0.001****)	(0.0337*)	(<0.001****)	(0.00254**)	

Variable	Choice mean	Choice mean (V-high-cost)	Choice mean (high-cost)	Choice diff (H - VH)	SES	Precarity	Income (personal)	Income (HH)	Fin wellbeing
Choice mean									
Choice mean (V-high-cost)	0.96								
	(<0.001****)								
Choice mean (high-cost)	0.97	0.85							
	(<0.001****)	(<0.001****)							
Choice diff (H - VH)	0.24	-0.05	0.48						
	(<0.001***)	(0.682)	(<0.001****)						
SES	0.01	0.01	0.00	-0.01					
	(0.98)	(0.98)	(0.98)	(0.98)					
Precarity	0.07	0.08	0.06	-0.03	0.37				
	(0.488)	(0.414)	(0.588)	(0.891)	(<0.001****)				
Income (personal)	0.04	0.07	0.02	-0.07	-0.30	-0.26			
	(0.755)	(0.585)	(0.98)	(0.548)	(<0.001***)	(0.00177**)			
Income (HH)	0.01	0.01	0.01	0.00	-0.45	-0.32	0.52		
	(0.98)	(0.98)	(0.98)	(0.988)	(<0.001****)	(<0.001****)	(<0.001****)		
Fin wellbeing	-0.07	-0.05	-0.09	-0.09	-0.36	-0.34	0.18	0.38	
	(0.488)	(0.69)	(0.366)	(0.353)	(<0.001****)	(<0.001****)	(0.0417*)	(<0.001****)	

9 Exploratory factor analyses

Three-factor results of EFA

COMBINED SAMPLE	Factor1	Factor2	Factor3
Extent I tried to maximise my reward		-0.535	-0.135
Extent I tried to maximise group reward	0.219	0.7	0.33
How did I feel when others searched	0.112		
How concerned were you to avoid the zero berries outcome	0.398	0.288	0.431
To what extent play game cooperatively (+ve) vs competitively (-ve)	0.135	0.716	0.359
To what extent were you influenced by how others played	0.616	0.127	0.339
What was impact when others searched	0.228	0.518	-0.123
What was impact when others stayed		0.206	0.671
To what extent did you try to influence others	0.668	0.213	
What extent did you feel others were reacting to the way you played	0.5		0.103
What extent did you keep track of how others were playing	0.636		
Proportion of variance	0.161	0.159	0.095
Cumulative variance	0.161	0.320	0.415

Two-factor results of EFA

COMBINED SAMPLE	Factor1	Factor2
Extent I tried to maximise my reward	**-0.565**	-
Extent I tried to maximise group reward	**0.756**	0.294
How did I feel when others searched	-	0.136
How concerned were you to avoid the zero berries outcome	**0.401**	**0.455**
To what extent play game cooperatively (+ve) vs competitively (-ve)	**0.823**	0.142
To what extent were you influenced by how others played	0.238	**0.637**
What was impact when others searched	0.398	0.126
What was impact when others stayed	**0.439**	0.168
To what extent did you try to influence others	0.220	**0.649**
What extent did you feel others were reacting to the way you played	-	**0.495**
What extent did you keep track of how others were playing	-	**0.681**
Proportion of variance	0.203	0.163
Cumulative variance	0.203	0.366

10 Post-hoc tests on influence of previous choice on decision-making

CHAPTER 4 (IN-PERSON)

Group1	Group2	Estimate	Conf low	Conf high	p-value	Signif
	HIGH-COST PRODUCE					
1	2	-0.031	-0.127	0.064	0.896	ns
1	3	-0.037	-0.136	0.063	0.852	ns
1	4	0.029	-0.090	0.149	0.963	ns
1	5	-0.119	-0.377	0.140	0.719	ns
2	3	-0.005	-0.082	0.072	1.000	ns
2	4	0.061	-0.041	0.162	0.473	ns
2	5	-0.087	-0.338	0.164	0.876	ns
3	4	0.066	-0.039	0.171	0.428	ns
3	5	-0.082	-0.335	0.170	0.901	ns
4	5	-0.148	-0.409	0.113	0.530	ns
	HIGH-COST SCROUNGE					
0	1	-0.027	-0.076	0.023	0.577	ns
0	2	-0.066	-0.117	-0.014	0.005	**
0	3	-0.008	-0.072	0.055	0.996	ns
0	4	-0.066	-0.205	0.073	0.699	ns
1	2	-0.039	-0.081	0.003	0.080	ns
1	3	0.018	-0.037	0.074	0.893	ns
1	4	-0.039	-0.174	0.097	0.936	ns
2	3	0.057	0.000	0.115	0.049	*
2	4	0.000	-0.136	0.137	1.000	ns
3	4	-0.057	-0.198	0.084	0.803	ns
	LOW-COST PRODUCE					
1	2	0.166	0.019	0.312	0.016	*
1	3	0.079	-0.058	0.217	0.565	ns
1	4	0.157	0.019	0.296	0.015	*
1	5	0.205	0.059	0.352	0.001	***
1	6	0.108	-0.075	0.291	0.543	ns
2	3	-0.086	-0.168	-0.004	0.033	*
2	4	-0.008	-0.092	0.075	1.000	ns
2	5	0.040	-0.056	0.136	0.843	ns
2	6	-0.057	-0.204	0.089	0.873	ns
3	4	0.078	0.012	0.144	0.011	*
3	5	0.126	0.045	0.207	0.000	***
3	6	0.029	-0.109	0.166	0.991	ns
4	5	0.048	-0.035	0.131	0.559	ns
4	6	-0.049	-0.188	0.089	0.912	ns
5	6	-0.097	-0.243	0.049	0.401	ns
	LOW-COST SCROUNGE					
0	1	-0.139	-0.258	-0.019	0.012	*
0	2	-0.128	-0.241	-0.014	0.017	*
0	3	-0.150	-0.262	-0.038	0.002	**
0	4	-0.143	-0.265	-0.021	0.011	*
0	5	-0.194	-0.342	-0.045	0.003	**
1	2	0.011	-0.066	0.088	0.999	ns
1	3	-0.011	-0.087	0.064	0.998	ns
1	4	-0.004	-0.093	0.085	1.000	ns
1	5	-0.055	-0.178	0.067	0.794	ns
2	3	-0.022	-0.088	0.043	0.926	ns
2	4	-0.015	-0.096	0.065	0.994	ns
2	5	-0.066	-0.183	0.051	0.588	ns
3	4	0.007	-0.072	0.086	1.000	ns
3	5	-0.044	-0.160	0.072	0.890	ns
4	5	-0.051	-0.176	0.074	0.857	ns

CHAPTER 5 (RICH-POOR)

Group1	Group2	Estimate	Conf low	Conf high	p-value	Signif
	HIGH-COST PRODUCE					
0	1	0.405	-0.616	1.426	0.905	ns
0	2	0.474	-0.547	1.494	0.818	ns
0	3	0.537	-0.484	1.557	0.713	ns
0	4	0.556	-0.465	1.577	0.678	ns
0	5	0.613	-0.410	1.635	0.571	ns
0	6	0.667	-0.365	1.698	0.476	ns
1	2	0.069	0.000	0.138	0.051	ns
1	3	0.132	0.064	0.199	0.000	****
1	4	0.151	0.076	0.226	0.000	****
1	5	0.207	0.108	0.306	0.000	****
1	6	0.262	0.094	0.429	0.000	****
2	3	0.063	0.011	0.115	0.007	**
2	4	0.082	0.021	0.144	0.002	**
2	5	0.139	0.049	0.228	0.000	****
2	6	0.193	0.031	0.355	0.008	**
3	4	0.019	-0.041	0.079	0.964	ns
3	5	0.076	-0.012	0.164	0.147	ns
3	6	0.130	-0.031	0.291	0.209	ns
4	5	0.056	-0.038	0.150	0.567	ns
4	6	0.111	-0.054	0.275	0.426	ns
5	6	0.054	-0.123	0.231	0.972	ns
	HIGH-COST SCROUNGE					
0	1	0.047	0.020	0.073	0.000	****
0	2	0.067	0.040	0.094	0.000	****
0	3	0.066	0.035	0.097	0.000	****
0	4	0.100	0.049	0.150	0.000	****
0	5	0.043	-0.079	0.166	0.913	ns
1	2	0.020	-0.004	0.045	0.182	ns
1	3	0.019	-0.010	0.048	0.395	ns
1	4	0.053	0.004	0.102	0.026	*
1	5	-0.003	-0.125	0.118	1.000	ns
2	3	-0.001	-0.030	0.029	1.000	ns
2	4	0.033	-0.017	0.083	0.406	ns
2	5	-0.023	-0.145	0.099	0.994	ns
3	4	0.034	-0.018	0.086	0.437	ns
3	5	-0.023	-0.146	0.100	0.995	ns
4	5	-0.056	-0.185	0.073	0.815	ns
	LOW-COST PRODUCE					
1	2	0.077	0.011	0.143	0.011	*
1	3	0.148	0.084	0.213	0.000	****
1	4	0.206	0.140	0.272	0.000	****
1	5	0.244	0.166	0.323	0.000	****
1	6	0.360	0.223	0.498	0.000	****
2	3	0.071	0.023	0.119	0.000	***
2	4	0.129	0.079	0.180	0.000	****
2	5	0.167	0.101	0.233	0.000	****
2	6	0.283	0.153	0.414	0.000	****
3	4	0.058	0.009	0.107	0.009	**
3	5	0.096	0.032	0.160	0.000	***
3	6	0.212	0.082	0.342	0.000	****
4	5	0.038	-0.028	0.104	0.576	ns
4	6	0.154	0.023	0.285	0.010	*
5	6	0.116	-0.021	0.254	0.154	ns
	LOW-COST SCROUNGE					
0	1	0.068	0.040	0.096	0.000	****
0	2	0.119	0.091	0.148	0.000	****
0	3	0.167	0.134	0.199	0.000	****

Group1	Group2	Estimate	Conf low	Conf high	p-value	Signif
0	4	0.221	0.176	0.266	0.000	****
0	5	0.312	0.215	0.408	0.000	****
1	2	0.051	0.024	0.078	0.000	****
1	3	0.099	0.067	0.130	0.000	****
1	4	0.153	0.109	0.197	0.000	****
1	5	0.244	0.148	0.339	0.000	****
2	3	0.047	0.015	0.079	0.000	***
2	4	0.102	0.058	0.147	0.000	****
2	5	0.192	0.097	0.288	0.000	****
3	4	0.055	0.007	0.102	0.013	*
3	5	0.145	0.048	0.242	0.000	***
4	5	0.090	-0.012	0.193	0.118	ns

11 Between studies comparison of task questionnaire results

	Est S1/S2	Est Rich/Poor	Diff	n1	n2	statistic	p	df	CI low	CI high	Effect size
Extent I tried to maximise my reward	7.003	6.995	0.008	437	216	0.043	0.965	402	-0.335	0.350	-
Extent I tried to maximise group reward	5.345	4.932	0.413	437	216	1.771	0.106	403.5	-0.045	0.871	-
How did I feel when others searched	7.005	6.854	0.151	437	216	0.947	0.378	419.8	-0.162	0.464	-
Concerned to avoid the zero berries outcome	5.577	5.094	0.482	437	216	2.076	0.073	425.6	0.025	0.939	-
Play game cooperatively (+ve) vs competitively (-ve)	0.850	0.4398	0.410	437	216	1.494	0.166	414.7	-0.129	0.949	-
Influenced by how others played	5.099	4.641	0.458	437	216	2.060	0.073	425.5	0.021	0.895	-
What was impact when others searched	3.634	4.222	-0.588	437	216	-2.336	0.055	399.7	-1.083	-0.093	-
What was impact when others stayed	4.848	3.784	1.06	437	216	4.361	< .001	431.6	0.584	1.544	0.362
Try to influence others	4.053	3.037	1.02	437	216	4.475	< .001	464.2	0.569	1.462	0.366
Feel others were reacting to the way you played	4.034	3.014	1.02	437	216	4.726	< .001	439	0.596	1.445	0.391
Keep track of how others were playing	5.432	5.032	0.400	437	216	1.814	0.1063	435.6	-0.033	0.833	-