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**Why wealthier people think people are wealthier, and why it matters: From social
sampling to attitudes to redistribution**

Rael J. Dawtry

A thesis submitted for the degree of Doctor of Philosophy in the Faculty of Social
Sciences at the University of Kent

Word count: 48,238

Declaration

The research reported in this thesis was conducted while the author was a full-time postgraduate student in the School of Psychology at the University of Kent (September 2012 – September 2015). The theoretical and empirical work herein is the independent work of the author. The author has not been awarded a degree by this or any other university for the work included in the thesis.

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TABLE OF CONTENTS

ABSTRACT	1
CHAPTER 1	3
1.1. Introduction.....	3
<i>1.1.2. Organisation of the chapter</i>	7
1.2. The Twilight of Probability: The Emergence of Probability Theory and Enlightenment Notions of Human Rationality.	8
1.3. Theories of Frequency Learning And Estimation.....	12
<i>1.3.2. Estimation by Direct Retrieval</i>	14
<i>1.3.3. Estimation by Inference</i>	15
<i>1.3.4. Heuristics versus Frequencies</i>	18
1.4. Competing Visions of Rationality?.....	20
<i>1.4.1. Ecological Rationality</i>	25
<i>1.4.2. Bias and Rationality in Contemporary Social Psychology</i>	26
1.5. The Cognitive-Ecological Approach.....	28
<i>1.5.1. Biases in Samples</i>	30
<i>1.5.2. The “Naïve” Intuitive Statistician</i>	36
1.6. Consequences of Sampling Processes for Social and Political Attitudes.....	38

1.6.1. <i>Homophily, Social Sampling and Attitudes toward Inequality</i>	45
1.7. Chapter Summary.....	47
1.8. Outline of Following Chapters.....	49
CHAPTER 2	52
2.1. Introduction.....	52
STUDIES 1A AND 1B	55
2.2. <i>Study 1a and 1b Methods</i>	55
2.3. <i>Study 1a and 1b Results</i>	60
STUDY 2	69
2.4. <i>Study 2 Methods</i>	70
2.5. <i>Study 2 Results</i>	73
2.6. Ch. 2 General Discussion.....	74
CHAPTER 3	78
3.1. Introduction.....	84
STUDY 3A	84
3.2. <i>Study 3a Method</i>	84
3.3. <i>Study 3a Results</i>	88
3.4. <i>Study 3a Discussion</i>	91

STUDY 3B	92
3.5. <i>Study 3b Method</i>	92
3.6. <i>Study 3b Results</i>	94
3.7. <i>Study 3b Discussion</i>	96
3.8. Ch. 3 General Discussion.....	97
CHAPTER 4	105
4.1. Introduction.....	105
STUDY 4A	117
4.2. <i>Study 4a Method</i>	117
4.3. <i>Study 4a Results & Discussion</i>	121
STUDY 4B	127
4.4. <i>Study 4b Method</i>	127
4.5. <i>Study 4b Results & Discussion</i>	128
4.6. Ch. 4 General Discussion.....	133
CHAPTER 5	143
5.1. Introduction.....	143
5.2. Summary of Chapters 1-4.....	143
5.3. Implications of Social Sampling for Attitudes toward Inequality.....	150

5.4. Social Sampling and Political Polarisation.....	155
5.5. Social Sampling and Bias In Subjective Rank.....	157
5.6. The Content of Social Samples.....	158
5.7. Preventing (or not) Social Sampling.....	160
5.8. The Role of Cognitive-Ecological Processes in Political Cognition.....	163
5.9. Limitations and Future Directions.....	168
REFERENCES.....	176
APPENDIX I.....	215
APPENDIX II.....	208
APPENDIX III.....	221
APPENDIX IV.....	222

INDEX OF TABLES, FIGURES AND FOOTNOTES

Tables:

Table 2.1.....	62-63
Table 3.1.....	89
Table 4.1.....	122
Table 4.2.....	122
Table 4.3.....	129

Table S1.....	221
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Figures

Figure 1.1.....	24
Figure 2.1 (a, b).....	61
Figure 2.2 (a, b).....	61
Figure 2.3 (a-c).....	67
Figure 3.1.....	90
Figure 3.2.....	91
Figure 3.3.....	95
Figure 3.4.....	96
Figure 4.1.....	125
Figure 4.2.....	131

Footnotes:

Footnote 1.1.....	34
Footnote 1.2.....	39
Footnote 2.1.....	52
Footnote 2.2.....	57
Footnote 2.3.....	65

Footnote 3.1.....	85
Footnote 3.2.....	93
Footnote 4.1.....	123
Footnote 4.2.....	125

Abstract

Drawing on research and theory (discussed in Chapter 1) emphasising *cognitive-ecological interaction* and sampling processes in judgment (e.g., Fiedler, 2000), the present research investigated the role of social sampling (Galesic, Olsson & Reiskamp, 2012) in preferences for wealth redistribution. Two studies (Ch. 2) provide evidence that social sampling leads wealthier people to oppose redistributive policies. Wealthier participants reported higher levels of wealth in their social circles (Studies 1a and 1b) and, in turn, estimated wealthier population distributions, perceived the distribution as fairer and were more opposed to redistribution. Study 2 (Ch. 2), drawing data from a nationally representative survey, revealed that neighbourhood-level deprivation – an objective index of social circle wealth – mediated the relation between income and satisfaction with the economic status quo. In Studies 3a and 3b (Ch. 3), participants experimentally presented with a low (high) wealth income sample subsequently estimated poorer (wealthier) population distributions, demonstrating reliance upon the novel samples. The effect of the manipulation on redistributive preferences was sequentially mediated via estimated population distributions and fairness, such that participants shown a high wealth sample estimated less unequal (3a) or wealthier (3b) distributions, perceived the distribution as fairer and were more opposed to redistribution. Studies 4a and 4b (Ch. 4) tested whether warning against social sampling, providing an alternative sample or both interventions together might serve to reduce social sampling. Whereas providing an alternative sample alone was sufficient to eliminate social sampling (4a and 4b), providing a warning had no effect (4a), and providing both an alternative sample and a warning lead to an increase in social sampling (4a and 4b). Taken together, the findings suggest that a) social sampling produces systematic differences in wealthier and poorer peoples' perceptions of the income distribution, b) social sampling contributes to divergence in the economic

preferences of wealthy and poor and c) social sampling is likely immune to deliberate control efforts.

Chapter 1

1.1. Introduction

Economic inequality – disparity in levels of wealth, income and consumption – has increased markedly across developed nations over the past few decades, and presently stands at its highest level for the past half a century (Organisation for Economic Cooperation and Development, 2011). A growing body of literature documents negative social, political and economic consequences of high levels of inequality. For example, in comparisons between developed nations, and between US states, Wilkinson and Pickett (2010) found that income inequality is strongly associated with reduced life expectancy, and greater prevalence of mental illness, infant mortality, homicides and imprisonment rates. In the economic domain, analyses conducted by the International Monetary Fund (IMF; 2011) found that lesser inequality is associated with more stable and enduring economic growth, and explains more variance in the longevity of growth spells than freedom of trade, government corruption, or levels of foreign investment and debt. In the political domain, inequality is associated with greater polarisation amongst both political elites and electorates (Garand, 2010; McCarty, Poole & Rosenthal, 2003, 2006). Furthermore, theorists have argued that economic inequality translates into political inequality, serving to distort the democratic process (Dahl, 2006; Gilens, 2005). Political gridlock, rent seeking amongst wealthy individuals and business, and reduced electoral turnout amongst poorer sections of society partly explain why majoritarian electoral systems have apparently done little to limit rising inequality (Bonica, McCarty, Poole & Rosenthal, 2012). Research also suggests that, in parallel with rising inequality, tax policies in developed nations have tended to become increasingly less progressive since the 1960s, potentially both a cause and consequence of inequality (Piketty & Saez, 2006).

Correspondingly, various organisations, political actors and theorists have stressed the importance of enacting policy measures, such as redistribution via progressive tax and transfer mechanisms, to reduce historically high levels of inequality (e.g., IMF, 2011; OECD, 2011; Piketty, 2014). President Obama, for example, recently stressed the importance of building bipartisan consensus on tackling income inequality in the 2015 State of The Union Address (Reuters, 2015). Research and theory, however, suggests that building consensus around reducing inequality, and around the means employed to reduce it, is likely to be a difficult and complex task. Notably, attitudes toward inequality diverge strongly as result of ideological preferences, as well as due to the differing material interests of poorer and wealthier individuals (Jost, Glaser, Kruglanski & Sulloway, 2003; Pratto, Sidanius, Stallworth & Malle, 1994; Sears & Funk, 1991).

Although people often view equality as an important justice principle in the abstract (e.g., Deutsch, 1975; Rawls, 1971), research demonstrates important individual and situational differences in attitudes toward inequality. Theoretical perspectives in political psychology place a strong emphasis upon the role played by motivational processes and individual differences in ideologies, in determining such attitudes. For example, attitudes toward inequality differ between politically right and left-leaning individuals, and relative opposition to equality appears to be a core feature of conservative ideology (Giddens, 1998; Jost et al., 2003). A further defining feature of conservatism is resistance to change and a desire to preserve the prevailing social order (Conover & Feldman, 1981; Jost et al., 2003). As such, conservatives are inclined to maintain social arrangements, structures and authorities that maintain and perpetuate inequality (Jost, Federico & Napier, 2009; Jost et al., 2003).

Much theorising in political psychology emphasises the role of motivated social cognition, suggesting that people adopt conservative, anti-egalitarian political ideologies as a means of satisfying certain epistemic and existential needs related to the management of uncertainty and fear (Jost, et al., 2009; Jost et al., 2003; Jost, Napier, Thorisdottir, Gosling, Palfai & Ostafin, 2007; Kruglanski, 1996). This reasoning is supported by research demonstrating associations between conservatism and intolerance of ambiguity (Frenkel-Brunswik, 1954), and higher needs for order, closure and structure (Altemeyer, 1998; Jost et al., 2007; Webster & Kruglanski, 1994). Conservatives, compared to liberals, also perceive that the world is more threatening and dangerous (Duckitt, 2001; Jost et al., 2007). Conservative ideology also serves an apparent a palliative function by providing a buffer against the negative hedonistic consequences of economic inequality, partly explaining greater happiness and subjective wellbeing amongst conservatives compared to liberals (Napier & Jost, 2008).

Relatedly, system justification theory also stresses the motivational basis of anti-egalitarian political attitudes, suggesting that people are generally motivated to justify prevailing social systems and arrangements that serve to perpetuate inequality (Jost, 1995; Jost, Banaji & Nosek, 2004; Kay & Zanna, 2009). Drawing on cognitive dissonance theory (Festinger, 1957) and just-world theory (Lerner, 1980), system justification theory suggests that people are motivated to rationalise the social arrangements to which they are unavoidably subjected as fair, just and legitimate, and further, that it is psychologically adaptive to do so (Jost & Hunyady, 2003). System justification can explain why people often hold beliefs that are seemingly at odds with their interests, such as anti-egalitarian political beliefs or outgroup-favouring stereotypes (e.g., of the “deserving rich” and “undeserving poor”) amongst low-income individuals (Jost, 2001; Jost, Pelham, Sheldon & Sullivan, 2003).

Other theories in political psychology, such as social dominance theory, have emphasised the role that socio-political processes play in giving rise to belief systems that serve to maintain inequality (Pratto, 1999, Pratto et al., 1994; Sidanius & Pratto, 1993). Social dominance theory suggests that societies strive to maintain order and reduce intergroup conflict by developing ideologies that legitimise hierarchies of power, status and wealth (Sidanius & Pratto, 1999). This involves the propagation of “legitimising myths”, which are shared cultural beliefs that justify the hegemony of some groups over others, via claims to paternalistic duty, mutual benefit or divine right (Sidanius, 1993). *Social dominance orientation*, defined as individuals’ preference for group-based inequality, is associated with endorsement of beliefs and attitudes which underpin support for economic inequality, such as belief in meritocracy and economic conservatism (Pratto et al., 1994).

Current theory in political psychology tends to emphasise the intra-psychic, motivational underpinnings of individuals’ attitudes toward inequality. Such accounts suggest that political beliefs generally are a reflection of opaque existential, epistemic and group-based motivations, and are adopted and maintained in the service of satisfying these needs and motives. Broadly, this motivated social-cognitive perspective implies that motives influence beliefs and attitudes by imposing selectivity upon information search, attention, processing and recall (Ditto & Lopez, 1992; Frey, 1986; Kruglanski, 1996; Kunda, 1987). This renders political cognition a constructive process, allowing the same social and political reality to be construed differently across people and situations, in accordance with extant goals and motives. Anti-egalitarian political attitudes are assumed to reflect motivated attempts to fend off uncertainty or fear in the face of threat (Jost et al., 2003), rationalise inescapable social arrangements that are disadvantageous to oneself or one’s group (Jost, Pelham, Sheldon & Sullivan, 2003), or legitimise the hegemony of one’s own group over others (Sidanius & Pratto, 1999).

Crucially, the locus of these tendencies lies inside the mind – political attitudes are assumed to reflect the top-down, goal directed operation of psychological processes.

In a departure from theories emphasising the top-down, intra-psychic determinants of political attitudes, the present research proposes a novel approach which highlights the importance of bottom-up (i.e., stimulus driven), ecological processes, in addition to psychological processes, in determining attitudes toward inequality. The present work draws upon theorising in the domain of judgment and decision making which highlights processes of *cognitive-ecological interaction* (e.g., Fiedler, 2000; Fiedler & Wänke, 2009). Echoing theorists such as Brunswik (1955), Lewin (1951) and Simon (1982), this perspective emphasises the importance of understanding the ecological input that impinges upon the mind, in pursuing understanding of the mind itself. It assumes that human judgment is relatively sensitive to the statistical properties of environments (e.g., event frequency). Further, it assumes that input from the environment is often selective and systematic, irrespective of motivational influences or cognitive shortcomings (Fiedler, 2000; Simon, 1982). Hence the organisation of information in the external environment, in tandem with the sampling processes by which information is acquired from it, serve to shape knowledge of the external world in a systematic and often biased manner. The present research proposes to investigate, specifically, the role that such sampling processes play in shaping perceptions of the wealth distribution, and the consequences that this may entail for attitudes toward this distribution.

1.1.2. Organisation of the Chapter

The following section discusses the evolution of probability theory during the Enlightenment, which provides the historical and philosophical context for contemporary theorising on the nature of knowledge acquisition and belief, and debates

regarding human rationality. The subsequent section introduces the notion of frequency learning, and competing theories of estimation which describe how humans make quantitative judgments about the external world. Whereas some perspectives optimistically suggest that human belief and judgment reflects the laws of probability and statistics (e.g., Hasher & Zacks, 1979; Peterson & Beach, 1967), other perspectives instead emphasise the constructive, irrational and bias-prone nature of cognitive processes (e.g., Tversky & Kahneman, 1973, 1974). The following section discusses these divergent perspectives in the context of debates regarding human rationality. It also addresses theorising in contemporary social psychology in relation to rationality, highlighting particularly how the notion of bias within social psychology is construed primarily in terms of intrapsychic processes. The next section introduces the cognitive-ecological approach, which serves as a counterpoint to theories implying that human judgement is either “unboundedly” rational or hopelessly irrational and error prone. This approach, emphasising the role of sampling processes, demonstrates how biased judgment need not stem from biased cognitive processes, and is often parsimoniously explained in terms of biased environmental input. The penultimate section discusses how a cognitive-ecological approach might apply to social and political-psychological phenomena and outlines in brief how such processes may serve to influence attitudes toward inequality. The final section summarises the present chapter, and briefly describes the research reported in the subsequent chapters.

1.2. The Twilight of Probability: The Emergence of Probability Theory and Enlightenment Notions of Human Rationality

The notion that the human mind is attuned to natural frequencies has a long and nuanced history dating back to the Enlightenment, and is grounded in the interplay between the theories of mind proposed by scholars such as David Hume (1738) and

Richard Hartley (1749) on the one side, and classical probability theory emerging originally from the correspondence between Pierre de Fermat and Blaise Pascal in 1654, on the other. Historical treatments have argued that the classical interpretation of probability that emerged during the 17th and 18th century was of a contradictory or “Janus faced” character (Gigerenzer, 1994; Hacking, 1975). One face was materialist, concerned with objective, observable frequencies, such as co-occurrence between comets and the death of kings, or between the presence of fever and disease. The other was epistemic, concerned with subjective probabilities and degrees of, or the “reasonableness” of, belief. In contemporary interpretations of probability, objective probability is that associated with random physical systems, such as roulette wheels or coin tosses, which yield stable outcomes at a persistent rate over long-run series of observations. Subjective probability, on the other hand, can be assigned to any statement to represent the extent to which a proposition is supported by available evidence, and is formally represented as the posterior probability assigned via Bayes’ theorem. Whereas objective probability is concerned with the true, physical tendency of a given event to occur, subjective probability refers instead to strength of belief as warranted by prior experience.

Although the dichotomy between subjective and objective probability is familiar to, and well established in, present-day interpretations (e.g., in the distinction between subjectivist Bayesian inference vs. the frequentist Neyman-Pearson approach to hypothesis testing), this was not the state of affairs during the early evolution of probability theory in the 17th and early 18th century. As Gigerenzer (1994) notes, a puzzle exists insofar as the Enlightenment probabilists showed little acknowledgement of the dichotomy between subjective and objective interpretations of probability, vacillating unperturbed between these different meanings.

According to Daston (1988), the explanation for this inconsistency can be found in the close relationship between probability theory and the associationist psychology of the era. Daston argues that the classical probabilists were able to reconcile the inconsistencies between the subjective and objective faces of probability by recourse to the theories of mind proposed by Locke, Hartley and Hume. Both Locke and Hume believed that the mind is highly attuned to frequencies of events, unconsciously and automatically tallying frequencies and apportioning degrees of belief accordingly. In Hume's (1739) view, probabilistic thinking is synonymous with rationality, or "reasonableness" in the parlance of the time. David Hartley (1749) went a step further, combining Locke's ideas with Newton's physiological theory on the vibratory basis of sensations, explicitly linking the laws of the mind with the laws of mathematical probability. In Hartley's view, repeated associations produce cerebral vibrations which, with repetition, imprint grooves of mental habit onto the surface of the brain. According to Hartley, this physical mapping of frequency information onto the brain allows human judgment to imitate the law of large numbers; given a sufficient number of observations, belief will approximate the true likelihood of a given event.

In the early nineteenth century, then, probability and human reasoning were understood as two sides of the same coin; the laws of classical probability were grounded in Enlightenment theories of human reasoning, and the psychological theories of Hume and others invoked probability theory to describe the mechanics of reasoning. Associationist psychology enabled the classical probabilists to blend subjective degrees of belief and objective, observed frequencies, and probability theory provided associationist psychology a normative standard by which to describe and evaluate reasoning. A strict distinction between beliefs and frequencies was not required by the Enlightenment thinkers because probabilities were understood to be inherently subjective in nature, an emergent feature of human experience with the world as

opposed to a property of the physical world itself. Given the assumption, widely held by the Empiricists, that the world is deterministic, an omniscient, all-knowing being such as God or Laplace's secularised demon could dispense with probability altogether, as it could directly *know* the laws of nature. Human beings, on the other hand, are bounded in their knowledge by the constraints of time, space and cognitive capacity and, as argued by Locke (1689), are thus condemned to live in the twilight of probability, drawing inferences on the basis of limited experience.

In the view of the Empiricists, then, reasoning by probabilities was to be considered rational insofar as probabilities are built upon, and reflective of, objective, real-world frequencies. Given human beings' arbitrarily limited knowledge of the world, probability represented the best option available to mere mortal beings, and thus the Empiricists considered reasoning according to the laws of probability as the de-facto standard of rationality. Insofar as the biasing effects of passion and interest could be controlled, human judgement could conceivably approximate that of a Laplacean demon by applying the laws of probability and statistics.

But such an optimistic view of human reasoning was not set to endure. Historical accounts (e.g., Daston, 1988) suggest that the French revolution and its aftermath undermined the confidence of the 18th century intellectual and political elites in the notion of a shared standard of reasonableness. Psychological theories guaranteeing the apportioning of frequencies to degrees of reasonable belief soon gave way to theories emphasising, instead, the illusory nature of human belief. Etienne de Condillac (1754) expressed doubt concerning the link between frequency and belief postulated by Hume and others, pointing to the disruptive influence of needs, wants and temperament on human judgement. Such pathologies were argued to influence how the mind distributes attention, and as a consequence, the organisation of experience

(Daston, 1988). The notion of correspondence between objective frequency and subjective belief fell from favour, and in 1837, the mathematician Siméon-Denis Poisson became the first to explicitly distinguish in print between the subjective and objective meanings of probability.

1.3. Theories of Frequency Learning and Estimation

Darwin (1872/1965) noted that people use facial cues, such as wavering eyes or low-hanging lids, to make an inference as to another person's guilt. Male cane toads use the pitch of a rival's croak to infer its likely size and decide whether to fight or flee (Krebs & Davies, 1987). Indeed, the capacity to make inductive inferences about unknown aspects of the external world has been demonstrated in various species, from insects to birds to mammals (for a review, see Hutchinson & Gigerenzer, 2005). This capacity is assumed in models employing Bayes' theorem to describe hunting and foraging behaviour in nonhuman animals (Stephens & Krebs, 1986). These models describe how animals make use of the statistical properties of the environment in maximizing the efficiency of hunting and gathering behaviour, and consequently, their survival.

Ecological analyses of "optimal foraging" provide a rational analysis of animal behaviour grounded in the evolutionary assumption that an organism should strive to maximize its rate of energy intake rather than simply consuming all the available food in a particular area, and should target sources of food that provide the greatest returns (for reviews see Pyke, 1984; Pyke, Pulliam & Charnov, 1977). According to optimal foraging theory, an organism will only attempt to acquire a food item if the calorific return per unit time is greater than the return obtainable by continuing to search for another item. In order for prey or foraging locations to be ranked and prioritised according to net return, they must first be classified according to their statistically distinct return-rates (time spent in pursuit and processing) and encounter rates (per-unit

search time). Optimal foraging thus functionally relates food choice and search strategies to organisms' fitness via the ability to suitably adapt behaviour to the statistical properties of the environment, in such a way that maximizes energy consumption and minimizes energy expenditure.

Optimal foraging theory has been primarily applied to the analysis of animal behaviour, for example in identifying when, rationally, a bird should stop feeding in one tree and move to another, or why a bear may favour hunting salmon over other prey. Some research does indeed suggest that human behaviour may follow similar patterns (e.g., Hutchinson, Wilke & Todd, 2008; Pirolli, 2007). For example, the anthropologist Alden-Smith (1991; cited in Hertwig, Hoffrage & Martignon, 1999) argues that a "contingency prey" model best explains why Inajjuamiut eskimos in Canada undertake dangerous and time-consuming Beluga whale hunts rather than pursuing easier prey such as ducks or seals. Cane, Clark and Mitroff (2012) extended the logic of optimal foraging theory to the domain of visual search. In line with the predictions of the theory, participants adjusted search times to account for expectations regarding target prevalence and adjusted expectations on-line to account for the higher order, inter-trial target distributions. In short, participants spent relatively more or less time searching for a target, depending on the frequency at which it occurred across previous trials.

Whether applied to humans or the most rudimentary of animals, it is a prerequisite of optimal foraging models that organisms possess a cognitive mechanism for monitoring and learning environmental frequencies and are able to effectively adapt behaviour to best exploit this statistical structure. These models imply that both humans and animals behave like "intuitive statisticians" (Brunswik, 1955; Peterson & Beach, 1967), learning and updating probabilities on the basis of frequency information sampled from the environment and adjusting behaviour accordingly. Theoretical

assumptions aside, what is the evidence that humans, specifically, do indeed possess such a capacity for learning frequencies and if so, how might it operate? The following sections review prior research on quantitative estimation, focusing on how people make frequency estimates of events such as objects, people or episodes. Following the taxonomy employed by Hertwig, Hoffrage, and Martignon (1999), a distinction is employed between two broad classes of estimation mechanisms, specifically, estimation by direct retrieval versus estimation by inference.

1.3.2. Estimation by Direct Retrieval

Enlightenment theories of mind assumed that human beings possess a mechanism for frequency learning, automatically and unconsciously tallying frequencies of events and deriving degrees of belief accordingly. In Hume's (1738, p. 141) view, "When the chances or experiments on one side amount to ten thousand, and on the other to ten thousand and one, the judgement gives preference to the latter, upon account of that superiority". Indeed, a body of evidence is supportive of the notion that humans are attuned to frequency information, although in light of contemporary evidence, Hume's position appears slightly over-optimistic concerning the sensitivity of this mechanism. Nevertheless, numerous studies report that humans can learn and estimate frequencies with reasonable accuracy (e.g., Barsalou & Ross, 1986; Hasher & Zacks, 1979; Hintzmann & Block, 1972; Jonides & Jones, 1992). Shedler, Jonides and Manis (1985), for example, asked participants to estimate the number of restaurants in various fast food chains and found that these estimates were closely related to the actual number of extant outlets. Participants have also been shown to make fairly accurate judgements of the frequency of experimentally presented stimuli (e.g., Hintzmann, 1969), and judgements of the frequency of real-world events such as single words (Shapiro, 1969),

single letters (Attneave, 1953) and pairs of letters (Underwood, 1971) that correlate with their objective relative frequency of occurrence.

Like Hume (1739), Hasher and Zacks (1979, 1984) assume that individuals store a fine-grained count of frequency information and directly access this count from memory when required to make a quantitative estimate of a given property. In their view, people's ability to make reasonably accurate frequency judgements about events as mundane and meaningless as bigrams (letter pairs) suggests that frequency learning is likely an automatic process and hence requires little or no attentional capacity. Frequency learning also bears several other hallmarks of automaticity. For example, sensitivity to frequency does not improve with either task practice or explicit feedback on the accuracy of judgements, and develops at an early age showing no subsequent improvement in later life (Hasher & Chromiak, 1977). From this perspective, frequency is one of the few attributes of stimuli that is encoded automatically (others include spatial location, temporal information and word meaning), although the notion of automaticity in frequency learning is not uncontroversial (see Barsalou, 1992). Nevertheless, there appears to be broad agreement with the general conclusion that people possess some capacity to learn domain-specific frequency information and can generate reasonably accurate estimates from this information when required (Hertwig, Hoffrage & Martignon, 1999).

1.3.3. Estimation by Inference

In contrast to the notion that people can directly access a more-or-less accurate count of actual events in a class, an opposing group of theories suggest that it is not the actual events themselves that are retrieved from memory during estimation (Brunswik, 1952, 1955; Tversky & Kahneman, 1973, 1974). Rather, these approaches contend that frequencies are inferred from the value of cues that are related to the to-be-estimated

event. For example, an inference about the relative population size of different cities could be made by reference to the sense of familiarity elicited by the names of those cities, following the logic that cities with which one is familiar (vs. unfamiliar) are likely larger and more populous (Hertwig, Hoffrage & Martignon, 1999). Similarly, one could make an inference about people's relative income ranking by considering the clothes they wear, the cars they drive or the neighbourhood in which they live. Note that these two examples are not equivalent. Estimation in the first example relies upon a *subjective* cue (familiarity), whereas the latter relies on an *ecological* (environmental) cue or cues. This distinction divides estimation by inference theories into two classes, one specifically emphasising the role of heuristics, and the other ecological cues, in quantitative estimation.

Heuristics

Research within the heuristics and biases framework initiated by Tversky and Kahneman (1973, 1974) suggests that people use a set of general purpose heuristics such as availability, representativeness and anchoring in estimating quantitative properties. In a now classic study, Tversky and Kahneman (1973) asked participants to judge whether each of five consonants appears more frequently in either the first or third position of words in the English language. Although all letters presented are more common in the third position, two-thirds of participants judged the first position to be more likely for the majority of presented letters.

According to Tversky and Kahneman, biases of this kind in quantitative estimates can be accounted for by the availability heuristic. According to availability, estimates of frequency reflect the number of instances that can be readily brought to mind. For example, in judging whether the letter "R" is more common in the first or third letter position, a person can draw samples of words that have "R" in the first and

third position from memory, or assess the ease with which such a sample could be drawn, and make an estimate based on the sample statistics or perceived ease of sampling. Samples, however, may not be representative of a population, for example if it is easier to recall words with a specific letter in the first rather than third position, thus leading to systematic biases in people's estimates. The availability cue is subjective rather than ecological because the relationship between the sample and the criterion can only be evaluated with respect to the sample drawn by any one particular individual.

In support of this perspective, a large body of research shows that real-world quantitative estimates are often biased in the direction predicted by availability and other heuristics. For example, people estimate that better publicised causes of death, such as accidents, are more probable than less publicised but more frequent causes, such as strokes (Lichtenstein, Slovic, Fischhoff, Layman & Coombs, 1978; Slovic, Fischhoff & Lichtenstein, 1982), and that self-rated knowledge of countries is strongly correlated with estimates of population size (Brown & Siegler, 1992).

Ecological cues

Brunswik (1952, 1955) observed that environmental objects or events are often not in immediate contact with the subject, and hence these *distal* properties must be inferred on the basis of available *proximal* cues. For example, object size (in the absence of a tape measure), must be inferred from proximal cues indicating distance, and projected video size on the retina. According to Brunswik, the environment represents a "causal texture" in which proximal cues and distal events are regularly, but not invariably, linked together. Because these causal linkages are not absolutely invariant, they entail some degree of uncertainty as to their correspondence. Thus the relationship between proximal cues and distal events is necessarily probabilistic. As such, not all cues are created equal; different cues vary in the extent to which they facilitate successful

estimation, or “achievement”, of a given distal property, and intercorrelation leads to redundancy between overlapping cues. In Brunswik’s terminology, cues vary in terms of their “ecological validity”, that is, in their degree of correlation with a distal variable to which they relate. In estimating a given real-world quantity, the task of the organism is to make an inference based on the cue or cues that have the strongest relationship with the estimated event, the most ecologically valid cues.

In this view, then, event frequencies themselves are not stored and directly accessed during estimation. Rather, a set of related proximal cues and information as to their corresponding cue validities are stored in and retrieved from memory. This is arguably a less cognitively demanding process, in that it requires the storage and retrieval of only the discrete correlations between proximal cues and distal events, as opposed to the storage and retrieval of a complete tally of discrete events themselves. This reduces the need to store vast amounts of frequency information in memory, and consequently, for extensive memory search during estimation. Theories of inference via ecological cues, and via direct retrieval, however, share an important similarity: both processes require that people accurately perceive event frequencies and their co-occurrences to enable either later retrieval from memory, or so that cue validities may be learned and updated.

1.3.4. Heuristics versus Frequencies

The preceding discussion highlights a clear contradiction in the research findings and theoretical assumptions derived from direct theories of quantitative estimation versus heuristics. On the one hand, Hasher and Zacks (1979, 1984) and others argue, and provide evidence to show, that people store a frequency count of experienced events and access this count when estimation is required. Jonides and Jones (1992, p368) summarised these findings as follows: “Ask about the relative numbers of many kinds

of events, and you are likely to get answers that reflect the actual relative frequencies of the events with great fidelity”. On the other hand, Tversky and Kahneman (1973) do not assume that people store a count of frequencies, but rather *construct* a sample of an event ad-hoc and infer frequencies from the sense of ease with which the sample could be constructed. Contrary to the advocates of direct retrieval, they interpret their findings to be indicative of systematic biases in human judgement. Brown and Siegler (1993, p. 511) draw attention to this paradox, noting that:

“[...] research has not culminated in any theory of estimation, not even in a coherent framework for thinking about the process. This gap is reflected in the strangely bifurcated nature of the research in the area. Research on heuristics does not indicate when, if ever, estimation is also influenced by domain-specific knowledge; research on domain specific knowledge does not indicate when, if ever, estimation is also influenced by heuristics”.

The tension between these two perspectives –frequency learning versus inference by heuristics – cannot be easily resolved by assuming that they apply under different circumstances. Conceivably, quantitative estimates may rely upon stored frequencies when the relevant events have been experienced and stored in memory, and rely on inference from subjective cues when relevant domain-specific knowledge is unavailable. Tversky and Kahneman (1973), however, also report experiments in which participants sequentially experienced series of events. For example, in one study participants were serially presented with a list of names, including those of well-known celebrities of either gender, and were required to judge whether the list included more men or women. Participants erroneously judged the class (men/women) containing the higher proportion of famous names to be more frequent. In a further study, participants were serially presented with word pairs that were either highly (phonetically or

semantically) related, or unrelated. Although related and unrelated word pairs were presented with the same frequency, participants judged related pairs to have appeared more frequently during presentation than unrelated pairs. Both studies imply that availability applies to directly experienced events as well as events that have not been directly experienced and must necessarily be inferred from available cues.

1.4. Competing Visions of Rationality?

The theories of Hume and his 20th century intellectual descendants appear to paint a very different picture of human reasoning capacities than that arising from the heuristics and biases literature. For the Empiricists and, to a lesser degree, the advocates of direct retrieval mechanisms such as Hasher and Zacks (1979, 1984), the laws of human inference are assumed to reflect the laws of probability and statistics. Much contemporary research in psychology, economics and behavioural ecology has been grounded in the assumption that statistical tools provide both a normative benchmark and a descriptive model of human (and animal) inference. In the 1960s, early research in the domain of judgment and decision making using “bookbags” and “pokerchips”, (i.e., bags with varying proportions of different coloured chips from which participants drew samples) compared participants’ revision of beliefs in response to repeated sampling against the normative prescriptions specified by Bayes’ rule (Peterson & Beach, 1967; Peterson & Miller, 1964; Ulehla, 1966). Despite some systematic deviations from normative prescriptions (e.g., conservatism in the updating of beliefs), this research broadly suggested that people respond in qualitatively appropriate ways to new evidence (Hahn & Harris, 2014). Drawing on Brunswik’s (1955) metaphor, researchers likened human reasoning capacities to those of an intuitive statistician; the laws of probability and statistics were concluded to provide a good approximation of inference processes in humans (e.g., Peterson & Beach, 1967). Elsewhere, Bayes’ theorem has

been employed to model the hunting and foraging behaviour of animals (Stephens & Krebs, 1986), human memory processes (Anderson, 1990) and economic behaviour (Harsanyi, 1967). Such theories characterised by the notion of the intuitive statistician embody a highly optimistic view in which judgement is considered to be *unboundedly rational* (see Gigerenzer & Todd, 1999), that is, constrained only by limitations on information available in the context of judgement.

Research in the heuristics and biases tradition, on the other hand, points to a very different and considerably less optimistic conclusion: that human inference is systematically biased and error prone, and functions through the application of “quick and dirty” heuristics rather than the laws of probability (Kahneman, Tversky & Slovic, 1982). On first glance, this perspective is diametrically opposed to accounts emphasising the probabilistic and statistical nature of human judgment, highlighting instead an inherent irrationality. This appearance is, however, misleading since both perspectives employ the same normative standard, stemming from the classical view held by Locke, Hume and others, which equates rationality with the laws of probability and statistics. In both traditions, error is defined as a departure in reasoning from the dictates of classical rationality, as epitomised by phenomena such as availability (Tversky & Kahneman, 1973), base-rate neglect (Bar-Hillel, 1980; Tversky & Kahneman, 1981) or the conjunction fallacy (Tversky & Kahneman, 1983), within heuristics and biases research. In short, both views accept the laws of probability and statistics as an appropriate normative benchmark for rational inference although they disagree as to whether humans can live up to this standard (Gigerenzer & Goldstein, 1996).

Much research has been conducted to examine the validity of these two perspectives, identifying conditions under which reasoning either adheres to, or departs

from, this standard. But how realistic is such a benchmark? And what does this research tell us about human judgment as it occurs in complex, ill-defined, real-world environments, as opposed to the well-defined and highly controlled laboratory setting? Gigerenzer and Goldstein (1996, p. 2) argue that the prospects do not look promising from either perspective:

“If one would apply the classical view to such complex real-world environments, this would suggest that the mind is a supercalculator like a Laplacean Demon (Wimsatt, 1976) – carrying around the collected works of Kolmogoroff, Fisher or Neyman [...] On the other hand, the heuristics and biases view of human irrationality would lead us to believe that humans are hopelessly lost in the face of real-world complexity”.

The implication is that neither view provides a fair, ecologically valid representation of human reasoning capacities. Whereas the classical view holds seemingly unrealistic expectations about the computational capacities of the human mind, heuristics and biases research equates deviations from this very same unrealistic standard with poor judgement in the real-world.

Funder (1987) highlights an important distinction between the notion of “error” and the notion of a “mistake”. In psychology, the term error is used in a technical rather than an evaluative sense, denoting the deviation of a response from an objectively defined standard, such as a population or sample mean, a predictive model or Bayesian rules of inference. Errors reflect misjudgements of well-defined artificial stimuli which depart from a given normative model (e.g., Bayes theorem). Theories of direct retrieval, and the heuristics and biases perspective, are both grounded firmly in the study of error as defined in this way. Phenomena such as conservatism or base-rate neglect, for example, are defined completely by departures in judgment from the normative

prescriptions of classical rationality – the laws of probability and statistics – which provides unambiguously “true” answers to the kind of judgment problems frequently studied in the laboratory.

A mistake, on the other hand, can be defined as a misjudgement of a poorly defined, real-world stimulus that may entail tangible consequences for the organism, and, “Although it is relatively easy to detect an error, because the nature of the stimulus is known and the normative judgement of it can be modelled with some certainty, it is much more difficult to determine that a judgement, perhaps even the same judgement, is also a mistake, because the criteria must be broader and located in the real world” (Funder, 1987, p. 78). Although errors may reveal important information about the *processes* by which judgments are made, such as whether these processes cohere to the classical model of rationality, they do not necessarily tell us about the accuracy or utility of judgment as it occurs in the real-world (Cosmides & Tooby, 1995; Funder, 1987).

Funder (1987) provides an example from the domain of perceptual constancy that serves to illuminate this point. Consider the Ponzo illusion (Figure 1.1); the lower horizontal line appears, incorrectly, to be longer than the upper line. Outside the laboratory, however, perception would almost always be correct; objects near the horizon that produce an equivalent retinal image to nearer objects *are* genuinely larger. If the criterion for accuracy in the Ponzo illusion is taken to be the literal length of the two dimensional lines as printed on the page, then judging the lines to be non-equivalent is an error. If, however, the figure is considered a representation of three dimensional space, then the two lines could indeed be non-equivalent. Furthermore, if the image were substituted for a photograph shot along a railroad track, then the two objects (if not the lines printed on the photo) really *are* of different lengths.

Figure 1.1



An example of the Ponzo illusion, first demonstrated by Mario Ponzo in 1911.

Such illusions may be useful insofar as they tell us something about how different cues are used in maintaining perceptual constancy, but perceptual errors in the context of visual illusions do not imply perceptual *mistakes* in the real world. In order to make informed statements about the accuracy of visual perception, it must be evaluated with respect to the real world contexts and stimuli upon which it is adapted to function, rather than the engaging but zero-stakes realm of visual illusion paradigms.

A similar argument has been made in regards to the study of human judgment by Savage (1954), who draws a distinction between “small worlds” (e.g., monetary gambles, the ultimatum game, neoclassical economic models) in which all relevant alternatives, consequences and their probabilities are known, and “large worlds” in which some of the relevant information is missing or must be estimated from samples. Small worlds are certain and, like visual illusions, contain all the necessary information to determine an optimal, rational solution to a given problem. These conditions, however, rarely hold in the large, uncertain, complex world in which people live their day-to-day lives and, consequently, nor do the requirements (e.g., complete and unbiased information) of classical rationality.

Cognitive limitations aside, this observation calls into question the assumption that everyday human reasoning can ever live up to the dictates of classical rationality, either descriptively or normatively, since the input to cognitive processes is constrained and shaped by the environment. If classical rationality represents an ideal that will generally be unobtainable in practice, then asking whether and to what extent judgment coheres to such a standard is perhaps not the most informative question to ask if the goal is to understand how, and how well, reasoning functions in the real world. In this vein, Simon (1989, p. 377) poses a rather different question, namely “How do human beings reason when the conditions for rationality postulated by the model of neoclassical economics are not met?”

1.4.1. Ecological Rationality

Whereas classical rationality is concerned with the construction of objectively accurate and general representations of the world, the kind of everyday, practical reasoning in which human beings most frequently engage is first and foremost concerned with making quick and effective judgments to solve problems that confront the organism (Todd, Fiddick & Krauss, 2000). As argued previously, the requirements of classical rationality rarely hold in the real world and such a view seemingly involves unrealistic assumptions about the cognitive capacities of the human mind. Correspondingly, an alternative perspective emphasising both the psychological and ecological, questions whether classical rationality provides an appropriate norm for evaluating judgment. Simon (1956, 1982) observed that reasoning mechanisms must operate effectively within the kind of real-world constraints that decision makers face and argued for the replacement of classical models of rationality with a concept of *bounded* rationality.

This perspective takes the functional view that judgment processes should be evaluated in terms of their effectiveness in solving real-world problems, irrespective of

whether they produce accurate representations of the world. According to Simon (1990, p.7), “Human rational behaviour is shaped by a scissors whose two blades are the structure of task environments and the computational capacities of the actor”. Simon’s dictum captures the notion that human reasoning necessarily involves an interaction between both internal cognitive processes, and present and past external ecological conditions, and as such cannot be adequately understood when either dimension is neglected. Furthermore, the notion of bounded rationality emphasises that the structure of information in the environment can serve to either constrain or facilitate the operation of reasoning mechanisms. The relative accuracy of judgement is thus to be understood as a function of the fit between cognitive processes and the nature of the information that is fed in from the environment.

Simons’ (1990) dictum hence entails that rationality is bounded not only by internal cognitive constraints, as exemplified by heuristics and biases research, but also by the environment or task structure in which judgments are made. The environment may impact upon judgment via the adjustment of individuals to the structure of local environments or tasks, as well as via the adaptation of reasoning mechanisms throughout our evolutionary history (Cosmides & Tooby, 1996).

1.4.2. Bias and Rationality in Contemporary Social Psychology

Whereas Simon’s notion of bounded rationality emphasises the interplay between both the mind and environment in understanding judgement (Fiedler & Wänke, 2009; Todd & Gigerenzer, 2007), theorising in social and cognitive psychology has largely focused upon how limitations of the human mind serve to bias judgement in various ways (Fiedler & Wänke, 2009; Kreuger & Funder, 2004). The “miserliness” of human cognition is argued to force reliance on heuristics which lead to rationally sub-par judgement strategies and outcomes. For example, availability suggests that frequency

estimates reflect the ease with which available information is retrieved from memory rather than actually experienced frequencies (Tversky & Kahneman, 1973).

Representativeness suggests that people judge the probability that an uncertain event belongs to a given category by the extent to which it is subjectively experienced as typical of that category, at the expense of neglecting base-rates and sample size (Kahneman & Tversky, 1974). As discussed previously, these models imply that the mind employs subjective, internal cues (e.g., ease or fluency of recall, perceived typicality) in arriving at a judgement, and hence fails to represent frequencies, probabilities and contingencies as they exist in the external world.

In addition to cognitive shortcomings, much contemporary research in social psychology emphasises how processes of motivated social cognition serve to bias judgment in various ways (e.g., Kruglanski, 1996). Motivational biases ostensibly lead people to engage in varieties of “wishful thinking” in service of maintaining desired views of the self and others. For example, individuals engage in self-serving attributions, attributing success to internal dispositions and failure to external constraints (Fiske & Taylor, 2008), and distort or selectively recruit self-referent information in order to enhance or maintain positive self-esteem (Green, Sedikides & Gregg, 2008; Sedikides, Green & Pinter, 2004). Similarly, people are said to engage in motivated search or processing of information in service of reaching a desired conclusion (De Dreu, Nijstad & Knippenberg, 2008; Kunda, 1990). Further, people display an apparent preference for information that is consistent with prior beliefs (Baron, 1995), for example, selectively processing information that is congruent with stereotype-based expectancies (Bodenhausen, 1989; Hamilton & Sherman, 1990).

In the context of social judgement, such biases are rarely judged explicitly against normative standards of rationality, given that what constitutes a “true” or

“correct” judgement cannot easily be defined (Hahn & Harris, 2014; Kruglanski & Ajzen, 1983). Unlike judgements of probability or frequency, there is rarely a clear, objective criterion for determining the absolute correctness of individuals’ beliefs about another person or group, their attitudes toward a given political issue or the veracity of positive self-views. However, this research entails that the top-down operation of motives or expectancies introduce a lack of impartiality into the acquisition and use of information in judgement (see Hahn & Harris, 2014), for example via the selective recruitment of evidence consistent with preferences or beliefs. Hence a violation of rationality in principle is heavily implied, insofar as rational judgment requires impartiality and independence from emotions, instincts, desires and preconceptions. Further, akin to heuristics, it is typically assumed that such motivational or expectancy-driven biases exert negative, systematic effects on the accuracy of peoples’ beliefs (Baron, 2008), although questions of accuracy are often not addressed directly in such research (Hahn & Harris, 2014; Funder, 1987).

1.5. The Cognitive-Ecological Approach

The varied conceptions of bias discussed above, whether stemming from cognitive shortcomings or motivations and expectancies, all involve a conception of bounded rationality that is grounded exclusively in the mind. Bias is internally attributed to either cognitive shortcomings or the top-down operation of motives and expectancies. Further, these perspectives emphasise that it is the inherently constructive nature of cognition that gives rise to biased judgement. Heuristics, motives and expectancies serve to render information selectively accessible by determining the course of information search, and also how such selectively accessed information is subsequently utilised in judgement. These models tacitly assume that the input upon which judgment is based is in itself unbiased, and that any apparent bias in output can be accounted for by biasing processes

operating exclusively within the mind (Fiedler & Wänke, 2009). The potential role played by the information input itself in the generation of judgement biases is, however, rarely considered in such models.

An alternative approach is to turn this assumption upside down, placing the explanatory burden at the opposite end of the scale – the information samples upon which judgements are based (Einhorn & Hogarth, 1978; Fiedler, 2000; Fiedler & Juslin, 2006; Fiedler & Wänke, 2009). This “sampling approach” begins with the working assumption that cognitive processes are generally consistent with normative principles, and biases in judgement are instead said to arise from pre-existing biases in information samples drawn from the environment, even when the sampling process itself is unbiased. This view is supported by research demonstrating that people can provide high-fidelity descriptions of samples (Fiedler, 2000; Gigerenzer & Murray, 1987; Zacks & Hasher, 2002), but that pre-existing biases in (accurately judged) samples carry over into judgements of populations (Fiedler, Brinkmann, Betsch & Wilde, 2000; Juslin, Winman & Hanson, 2007). Whereas the cognitive and social psychological models of bias discussed earlier emphasise cognitive *processes*, the sampling approach emphasises the role of informational input upon which cognitive *representations* are formed (Fiedler & Juslin, 2006). Biases in judgement can emerge despite, and perhaps because of, the normative accuracy of cognitive processes. Pre-existing biases in samples will inevitably carry over into judgments of populations where (biased) samples are judged accurately.

It is important to note that this approach does not attribute biased judgment exclusively to environmental constraints. Rather, consistent with Simon’s notion of bounded rationality as shaped by the “scissor blades” of both mind and environment, this approach emphasises a dynamic interaction between cognition and environmental

information. Information samples are constrained by the distribution of stimuli in a given individual's environment, and conversely, individuals' attention, cognitive capacity and processing goals can serve to influence the inclusion of stimuli in samples, which act as the interface between mind and environment (Fiedler, 2000). Similarly, external information is partly a function of an individual's decisions, activities and search strategies in the external environment, and internal mental states depend on environmental input through communication, social interaction and culture (Fiedler & Wänke, 2009). For example, different environments (e.g., urban vs. rural) differ in the extent to which they afford opportunities for contact with outgroups, and peoples' intergroup attitudes also determine their inclination toward interacting with members of outgroups when given the opportunity to do so. Contact shapes intergroup attitudes, which in turn influences both the quantity and quality of subsequent contact, and feeds back in to attitudes.

1.5.1. Biases in Samples

As noted previously, much psychological theory assumes that information is selectively accessible, and variations in judgement or behaviour are assumed to reflect a subset of all knowledge that is potentially available in memory. Concepts such as priming, selective retrieval, domain specificity, schematic knowledge and resource limitation impose constraints on what is accessible under a given set of circumstances, and consequently, how incoming information from the environment will be treated by the mind.

A similar selectivity also applies to information as originally encountered in the external world – spatial and temporal constraints, density of information, social distance, and cultural and economic restrictions all impose boundaries upon what information a person can potentially acquire from the social and physical world. For

example, people have greater access to information about themselves than they do of other people. On a broader social level, they also have more knowledge of their own culture and in-groups, than out-groups and socially or spatially distant cultures. In an analogous fashion, social and conversational norms place limits on what can be communicated between people, commercial imperatives determine what consumers may learn about different goods and services, and media policies shape what information is made available to the public about policy and politicians.

These examples demonstrate how information is often rendered selectively accessible by the environment alone. In many circumstances, environmental constraints are sufficient to render information selectively accessible, irrespective of cognitive shortcomings or motivated processing (Fiedler & Juslin, 2006). Availability biases, for example, might reflect accurate, veridical probability judgements of information encountered, but where environmental samples are biased, for example due to external media coverage, biases will inevitably manifest in judgment (Lichtenstein et al., 1978).

In addition, irrespective of the influence of cognitive or motivational biases, a person's sampling strategies vis-à-vis the environment can serve to shape the information it affords in various ways. Different internet search engines, for example, prioritise results in differing order, and events receive varying degrees of coverage across varied media outlets. The impression of a person solicited from an interview will vary depending on whether an interviewer asks questions focusing on introvert or extrovert behaviour (Snyder, 1984). Small, relative to large samples, are prone to underrepresent rare events (Hertwig, Barron, Weber & Erev, 2004) and are more prone to regression than larger samples. As such, features of sampling strategies including the source, extent and direction of sampling will influence the constitution of samples in various ways, and consequently, judgments that are based upon these samples.

It has been demonstrated that in hypothesis testing, for example, people tend to examine cases that are expected or known to possess a focal property more often than cases known or expected to lack the property, a tendency known as the positive-test strategy (Klayman & Ha, 1987). Although such a strategy runs counter to the logic of falsification (e.g., Popper, 1959), it is arguably rational when considered in light of inductive, probabilistic (versus deductive, logic-based) models of inference, and possesses functional value in uncertain, real-world contexts, for example when the costs of a Type II error (i.e., a false-negative) are high (Chater & Oaksford, 1994; Oaksford & Chater, 2003). Oaksford and Chater (2003) cite the example of a person testing the hypothesis that drinking from a particular well causes illness. This person may place greater value on evidence of illness after drinking from the well than upon absence of illness. In such a situation, positive-testing is clearly adaptive due to the high risk associated with erroneously rejecting the hypothesis that the well is contaminated - many people would become ill.

Positive testing potentially provides a sampling-based explanation of phenomena often attributed to confirmation bias, a tendency to seek or give preference to evidence that supports pre-existing beliefs, expectations or hypotheses (for a review, see Nickerson, 1998). Fiedler, Walther, Freytag and Plessner (2002) conducted a simulated classroom study in which teachers were instructed to test the stereotype-consistent hypothesis that boys perform better in science whereas girls perform better in languages. Consistent with positive testing, teachers tended to focus on questioning boys in science more so than girls, and girls more so in language than boys (the rate of correct answers was held equal across gender and subject). The resulting unequal sample sizes of correct responses led to biased, stereotype-consistent impressions; smart boys were judged to be better than smart girls in science, and vice-versa in language. When asked to test the opposing hypothesis, however (i.e., boys are better in languages

and girls are better in science), the opposite positive test strategy yielded larger samples, and more favourable evaluations, for girls in science and for boys in language. In sum, sample size, an ecological feature determined by the sampling strategy, overrode any internal influence of gender-stereotypical expectancies.

In some judgement contexts, the structure of information in the environment can serve to prompt, or will necessitate, sampling strategies that produce biased samples. Consider the bias toward confirmatory results in publication practices. Because null findings are rarely published, irrespective of cognitive or motivational biases on the part of individual researchers, samples of research on a given phenomenon will inevitably be skewed toward supportive findings. Similarly, therapists and clinical psychologists are only exposed to patients they treat, such that the spontaneous recovery of untreated individuals is unobserved (Fiedler & Wänke, 2009). The true base-rate of recovery is hence unknown to these practitioners. This is of course unavoidable, given that non-patients suffering from a given condition are by definition absent from patient records, and ethical concerns may preclude the use of non-intervention controls in clinical validation studies.

Even where complete information on base rates is available, the organisation of information in the environment may elicit sampling strategies that serve to distort base rates represented in samples, for example in the domain of medical diagnosis. Judging the likelihood that a patient has breast cancer or AIDS given a positive test is a task of conditional inference, the normative solution of which requires the calculation of a posterior probability according to Bayes' theorem^{1.1}. The likelihood of a positive mammogram given breast cancer (i.e., the hit rate) is approximately 80%; however the reverse probability, the rate of breast cancer given a positive mammogram (i.e., the posterior probability equating to diagnosticity of the test) is below 10%. This is because

the base rate of breast cancer, at around 1%, is roughly 8 times lower than the likelihood of a positive test (Gigerenzer & Hoffrage, 1995). An analogous situation occurs in the case of HIV.

Research examining problems of this form has consistently demonstrated a tendency of judges to substantially overestimate posterior probabilities. This phenomenon has been attributed to a tendency of judges to neglect base rate information^{1.1} (Bar-Hillel, 1980, Kahneman & Tversky, 1973). However, research conducted by Fiedler, Brinkmann, Betsch and Wilde (2000) points to a rather different conclusion. Participants were asked to estimate the posterior probability of breast cancer given a positive mammogram on the basis of information freely sampled from a population of index cards organised by either the criterion (i.e., cancer vs. no cancer) or predictor category (i.e., positive vs. negative mammogram), which preserved the aforementioned probabilities. Under the former circumstances, participants sampled approximately equal numbers of cases with and without breast cancer, hence drastically inflating the rate of breast cancer in the sample, and in turn gave typically inflated

Footnote 1.1

Bayes' theorem estimates the posterior probability of an event (e.g., breast cancer) based upon observed conditions (e.g., a positive mammogram) that are associated with the event. Where A and B are events, the posterior probability is given by;

$$P(A|B) = \frac{P(A) P(B|A)}{P(B)},$$

where $P(A)$ and $P(B)$ are the independent probabilities of events A and B (e.g., the population base rate of breast cancer and the probability of a positive mammogram, respectively), $P(A|B)$ is the conditional probability of A where B is true (e.g., the probability of breast cancer given a positive mammogram) and $P(B|A)$ is the probability of B given that A is true (i.e., the probability of a positive mammogram given breast cancer; the hit rate for the test).

estimates of diagnosticity. Under the latter conditions, however, participants were able to identify that only a small proportion of positive mammograms were associated with breast cancer, and provided relatively accurate estimates of diagnosticity.

Far from suggesting base rate *neglect*, this finding implies that relative *sensitivity* to base rates is sufficient to produce bias in conditional probability judgements due to sampling processes. Sampling conditionalised on a criterion (e.g., breast cancer vs no breast cancer) can produce base rate inflation in samples, and hence an accurate assessment of sampled information will carry over into biased judgement. When sampling is appropriately conditionalised on a predictor category, however, population base rates are preserved relatively intact in samples, leading to relatively accurate judgement of posterior probabilities relative to the population.

This is of clear practical importance given how information is often organised in the real world. For example, medical statistics are organised according to diseases rather than by positive versus negative test results. Relatedly, media framings and narratives often focus disproportionately upon socially undesirable behaviour, such as alcoholism, drug use and welfare dependency, amongst specific social groups such as the poor or ethnic minorities (Bullock, Wyche & Williams, 2001; Clawson & Trice, 2000; Gilens, 1999, 1996). Such a tendency presumably inhibits unbiased assessment of the relative prevalence of differing (positive and negatively valenced) behaviours and attributes within and across different social groups. A disproportionate media or political focus on the undesirable behaviour of specific social groups (e.g., the poor), or otherwise higher exposure to members of certain groups due to their greater density in the population, can presumably serve to distort base rates. In turn, this may lead to overestimation of certain attributes or behaviours given membership in a certain social category (e.g., drug or alcohol abuse given low socioeconomic status). Unrepresentative media

portrayals may hence lead to illusory correlations (e.g., Hamilton & Gifford, 1976) between specific groups and certain negative behaviours or attributes.

1.5.2. The “Naïve” Intuitive Statistician

The re-examination of prominent judgement biases in light of environmental sampling processes suggests that, on the one hand, internally attributing biased judgement to cognitive shortcomings seemingly paints an unfairly pessimistic picture of human rationality. Such a dim view is also difficult to reconcile with those findings demonstrating impressive performance in certain judgement tasks such as frequency estimation, optimistically likening the mind to an “intuitive statistician” (e.g., Zacks & Hasher, 2002; Peterson & Beach, 1967), or demonstrating the functional, ecological rationality of judgement in real-world contexts (e.g., Todd & Gigerenzer, 2007). On the other hand, it is also clear that cognitive processes cannot be completely exonerated. Irrespective of biases inherent in the environment, biases can manifest in judgement due to the sampling schemes employed by the individual as in the case of positive testing, a strategy that under some circumstances might indeed stem from goals or expectancies (e.g., Sanitioso, Kunda & Fong, 1990). To fully localise the source of judgement bias in either the mind or environment is hence misleading. Density effects resulting from unequal samples exemplify this point (Fiedler et al., 2002). Judges may be privy to unequal observations as a result of selectivity imposed by either the external environment (e.g., differing motivation or attendance of equally able students; greater exposure to the behaviour of majority vs. minority social groups), due to the search strategies relied on by the individual (e.g., positive testing), or decisions which serve to shape feedback from the environment (e.g., avoidance of contact with outgroups).

In attempting to integrate these contradictory perspectives and research findings, researchers have employed a further metaphor: the *naive intuitive statistician* (Fiedler &

Juslin, 2006; Juslin, Winman & Hansson, 2007). In agreement with research demonstrating relative accuracy in frequency and probability judgement (Hasher & Zacks, 1979; Peterson & Beach, 1967; Zacks & Hasher, 2002), and internally consistent judgements of samples themselves (Fiedler et al., 2000; Freytag & Fiedler, 2006), it is assumed under this framework that cognitive processes operating on incoming information are generally unbiased and provide normatively accurate descriptions of samples. It is further assumed, however, that people are naïve or “myopic” in respect to the external constraints imposed on samples by the environment, and also in respect to the more sophisticated properties of samples and sampling processes (Fiedler, 2012; Fiedler & Juslin, 2006; Juslin, Winman & Hansson, 2007). For example, people tend to assume that the samples they encounter are representative of relevant populations, failing to account for selectivity imposed by either the environment itself or by the sampling processes employed to extract information from it. Research further indicates that people are naïve in regards to constraints imposed by statistical properties such as sample size (Kareev, Lieberman & Lev, 1997), skewness (Fiedler & Freytag, 2004), and the negative relation between sample size and variance (Kareev, Arnon & Horwitz-Zeliger, 2002).

As opposed to emphasising biased processing in the mind, then, this perspective re-constructs biased judgement as a result of *meta-cognitive myopia* (MM; Fiedler, 2012). This term refers to accurate, unbiased judgement of information samples in the absence of any critical reflection on their origin or statistical properties, or appropriate top-down correction for biases in samples related to these factors. Biases in judgments of conditional probability, for example, appear to result not from base-rate neglect (samples are judged with relative accuracy), but from ignorance of systematic biases introduced by the sampling strategy itself (i.e., criterion sampling).

Similarly, the finding that positive testing of expectancy-congruent and expectancy-incongruent hypotheses leads to opposing biases implies naivety in regards to the effect of sampling strategy on the density of observations (e.g., correct and incorrect answers) across combinations of variable levels (e.g., girls/boys in languages/science; Fiedler et al., 2002). Positive testing need not reflect a motivated or expectancy driven bias toward seeking confirmatory information, but simply an innocent tendency to sample evidence in line with a focal hypothesis.

A further example of MM comes from research demonstrating that people are unable to ignore repeated (and hence redundant) information, even when it is clearly understood that repeated observations are irrelevant and misleading (Unkelbach, Fiedler & Freytag, 2007). Unkelbach et al., (2007) showed participant's news reports of share pricings sequentially over 16 days, with information on some winning shares selectively repeated across different news shows. Although estimates of daily winning rates were relatively accurate, evaluations and purchasing intentions were higher for shares whose winning was repeated, even when those shares in fact performed worse than other, lesser-repeated shares. This bias toward often repeated shares persisted even when participants were forewarned of the distorting effect of repetition and explicitly instructed to ignore redundant information.

1.6. Consequences of Sampling Processes for Social and Political Attitudes

The preceding discussion suggests various ways in which sampling processes might serve to influence perceptions of people, groups, events and other features of the social world. As mentioned, spatial and temporal constraints, density of information, social distance, and cultural and economic restrictions serve to shape and constrain the information samples to which people are exposed. Such selective exposure due to the informational structure of peoples' day-to-day environment, or the sampling processes

used to extract information from it, presumably lead to divergent social perceptions across different people and groups. Research on sampling processes shows that, in many circumstances, no cognitive or motivational bias need be present in order for biased perceptions to emerge (Fiedler, 2000). For example, irrespective of their attitudes or prejudices, people are exposed to more information about majority rather than minority groups, and those groups which are spatially and temporally more proximal. In other circumstances, sampling processes might interact with cognitive or motivational factors to bias perceptions. Political ideology, for example, inclines people toward sources of information that align with their political beliefs (Iyengar & Hahn, 2009), and prejudice can discourage interaction with members of outgroups (Plant & Devine, 2003).

The relationship between these internal and external sources of bias can be conceived of as bidirectional and self-reinforcing^{1.2}. Biased environments can serve to elicit biased perceptions, motivating information search strategies or behaviours that further distort feedback from the environment. Such processes of cognitive-ecological interaction presumably play an important role in shaping knowledge of the social and political world, and consequently in determining important social and political attitudes.

Footnote 1.2

Note that the studies reported in forthcoming chapters do not examine effects of person-environment interaction on judgment from a bidirectional perspective. Rather, the present studies focus predominantly upon the role of information samples (e.g., of others incomes) in systematically determining judgements of the wider environment (e.g., the population income distribution), and in turn, political attitudes (e.g., support for redistribution), in a unidirectional sequence. The present research does not directly examine whether judgments or attitudes reciprocally influence sampling strategies (e.g., positive testing; Iyengar & Hahn, 2009), or otherwise affect behaviour in a manner that causes changes in a person's environment (and consequently in the constitution of available samples).

The domain of intergroup attitudes provides a neat illustration of this point – research suggests that differences in sample size can account for certain in-group serving biases (Kunda, 1990). For two groups with the same rate of positive behaviour, the group for which a higher number of observations are available will be judged more positively (Sanbonmatsu, Shavitt & Gibson, 1994). Where the base-rate of positive behaviours is high, evaluations of the in-group will more accurately reflect the high rate of positive behaviour insofar as observations of the behaviour of in-group members are denser, and hence judgements are less regressive, relative to judgements of outgroup members (Fiedler & Wänke, 2009; Meiser & Hewstone, 2001, 2004). Irrespective of any motivational bias in intergroup comparison, then, evaluations of in-group members may be conferred a positivity advantage over evaluations of outgroups, due to the regressive nature of judgements based on small samples. Similar logic can be applied to outgroup homogeneity; the tendency to provide simplistic and homogeneous judgments of outgroups. Where in-groups and outgroups share the same variety of attributes, relative underexposure can render fewer attributes recognisable amongst outgroups relative to in-groups (Fiedler, Kimmelmeir & Freytag, 1999; Linville, Fischer & Salovey, 1989).

Sampling-based accounts complement research demonstrating complex and sometimes contradictory relationships between out-group prejudice and the density of minorities (e.g., Pettigrew, Wagner & Christ, 2010). Intergroup contact serves to reduce prejudice and should be more likely to occur in places where minorities are greater in number, but opposing relationships are often observed (higher or lower prejudice in the presence of either greater or fewer minorities). These inconsistencies can be accounted for by factors that moderate the extent of contact between majority and minority group members, such as the level of segregation between groups (e.g., Pettigrew et al., 2010; Pettigrew & Tropp, 2006). Contact might serve to reduce prejudice via the

aforementioned sampling mechanisms, although the extent of contact may in turn be a function of ecological variables (e.g., physical segregation in urban, educational and work environments) rather than the actual density of minorities per se. This is not to dismiss the role of social-psychological factors (e.g., threat), and of course, prejudice itself begets segregation. Rather, it highlights the kind of interdependence and interaction between ecological and psychological processes suggested previously. Threat, for example, is determined partly by the *perceived* availability of resources such as jobs, housing and healthcare, and such perceptions are subject in turn to the sampling processes and constraints described earlier.

As discussed at the outset of the chapter, research in political psychology emphasises the ideological and motivational underpinnings of peoples' attitudes towards important social and political issues such as inequality, immigration, foreign policy and domestic security (Bonanno & Jost, 2006; Duckitt & Fisher, 2003; Jost, Ledgerwood & Hardin, 2008; Pratto, Liu, Levin, Sidanius, Shih, Bachrach & Hegarty, 2000; Pratto, Stallworth & Conway-Lanz, 1998). Undoubtedly, ideology plays a powerful role in determining these attitudes, but beliefs regarding relevant facts and figures – factual political knowledge - also exert a strong influence on political attitudes and responses to policy (Gilens, 2001; Kuklinski et al., 2000; Nyhan & Reifler, 2010). Whether people correctly estimate that 5% of people in the UK are Muslims, or incorrectly estimate the proportion at 21% (Ipsos Mori, 2014), is likely not a trivial factor in determining public attitudes toward immigration or issues of domestic security. Similarly, whether people correctly estimate that less than 1% of UK welfare spending is lost to fraud annually, versus 27% (You Gov, 2013), presumably exerts a strong influence on attitudes toward both government welfare policy and welfare claimants themselves.

Accordingly, research shows that judgements regarding important social statistics and political facts of this kind are often wildly inaccurate, and further, that such misinformed judgements are indeed related to attitudes and policy preferences (Bartels, 2005; Gilens, 2001; Kuklinski et al., 2000). A sampling approach, emphasising the interactivity of cognitive and ecological processes, can contribute to explaining the emergence and persistence of such widespread distortions in political knowledge, in more and less obvious ways.

Selectivity in media coverage, for example, plays a relatively obvious role in the genesis of misperceptions. A large part of people's political knowledge is accrued vicariously via the media, which serves to select and filter information for public consumption (Barabas & Jeritt, 2009; Graber, 2002, 2004). Such filtering is not completely unbiased. The content of news media is determined by editorial priorities, such as the perceived appeal of a given story to a target audience. It is also influenced by political and commercial priorities, such as the partisan alignment of editorial departments or media financiers, and the business concerns of advertisers (e.g., Hermann & Chomsky, 1988). More fundamentally, limitations upon resources, such as time, column inches and access to sources, constrain reporting to a subset of all potential news stories, and editorial and commercial priorities in turn determine which stories are selected for reporting. As such, news media provides non-random, unrepresentative information samples, and renders information about politically relevant events and circumstances selectively accessible to the public.

Sampling models discussed earlier imply that biases in information samples will inevitably become manifest in judgement, for example in the form of structural (i.e., environmental) availability biases (Dawes, 2006; Lichtenstein et al., 1978). Over-reporting of objectively rare events in the media, for example benefit fraud or

unemployment, can render such events subjectively more frequent than they are in reality, and vice-versa for relatively common events (e.g., in-work poverty). Selection biases in news media might also carry over to related judgements in which unknown, distal features of the social and political world, such as the division of government spending or the proportion of immigrants from a given ethnic group, are inferred via frequency of exposure. Respondents to a poll about the division of UK welfare spending, for example, estimated on average that 41% of the entire UK welfare budget is spent on unemployment benefits, whereas the actual figure for the year reported was in fact 3% (You Gov, 2013). Speculatively considered, this severe level of distortion in public perception perhaps reflects the relative preoccupation of politicians and the media with spending on the unemployed relative to other categories of welfare spending, such as pensions. In a similar vein, a disproportionate focus on particular social groups in news media (e.g., benefit claimants, Muslims) can foster illusory correlations such that the likelihood of negative behaviour given membership in a particular social category is prone to overestimation (Hamilton & Gifford, 1976).

All the aforementioned examples imply that bias resides in available information (i.e., news reporting) prior to sampling, and as such, no cognitive or motivational bias is *necessary* for biased judgement to emerge. Sampling processes employed by individuals themselves, such as positive testing (e.g., sampling evidence of Muslims committing terrorist acts) or criterion sampling (e.g., sampling reported instances of terrorism), further serve to introduce or exacerbate potential biases in information acquired via the media. A person testing the hypothesis that benefit fraud is more common than tax evasion will presumably sample relatively more stories concerning benefit fraud, irrespective of any underlying (environmental) bias in reporting, and even in the absence of any *motivation* to confirm this hypothesis (Klayman & Ha, 1987). Although motivational processes might not always be necessary to explain biased political

judgments of the kind discussed, however, such processes certainly play an important and obvious role - the sources from which people sample, the hypotheses they entertain and the search strategies they employ will, of course, often be related to goals, expectations, attitudes and ideologies.

Research and theorising in the domain of socioecological psychology serves to further underscore the importance of social and physical environments in shaping political attitudes (e.g., Oishi & Graham, 2010; Oishi, 2014). Although this perspective does not emphasise sampling processes, it is grounded in a similar, objectivist epistemology to the sampling approach, examining how objective features of the environment, and not only subjective construal of environments, affect cognition, emotion and behaviour, and vice-versa. Research in this vein examines both how individuals adapt to distal, macro-environmental properties (e.g., climate and geography, demography, economies, institutions), and conversely, how individuals give rise to and maintain specific environments via processes of “niche construction” (Oishi, 2014; Yamagishi, 2011). Correspondingly, much research supports the notion that political attitudes are influenced by macro-level properties of the social and political environment. For example, cross-national variations in attitudes toward the welfare state are systematically related to country-level differences in the structure of welfare regimes (Larsen, 2008), levels of unemployment (Blekesaune & Quadagno, 2003) and levels of immigration (Eger, 2008). Findings of this kind broadly support the notion that political attitudes are adapted to the particular social and political environments in which people reside. Via an opposite, and perhaps more familiar process, individual attitudes lead to changes in macro-level political environments, via both more and less subtle mechanisms. An unambiguous example is the effect of political attitudes upon voting tendencies, which in turn determine the character of government and policies enacted. People also shape the political context to which they are exposed in a more

direct manner, and on an individual level. For example, individuals choose to live in communities in which their political ideology is widely shared, and members of political minorities in a given community show an increased desire to migrate compared to members of political majorities (Motyl, Iyer, Oishi, Trawalter & Nosek, 2014). This highlights how individuals own attitudes and behaviour serve to shape the ideological environment in which they exist, in a manner that potentially leads to relative overexposure to political attitudes similar to ones own.

1.6.1. Homophily, Social Sampling and Attitudes Toward Inequality

The sampling approach also implies that people's own social and psychological attributes serve to shape the social environment around them, and consequently, the information samples to which they are exposed (Galesic, Olson & Rieskamp, 2012). A large body of research indicates that a basic organising principle of social networks is a tendency toward homophily – the principle that contact between similar people occurs more frequently than contact between dissimilar people (Lazarsfeld & Merton, 1954; Marsden, 1987; McPherson, Smith-Lovin & Cook, 2001; Reagans, 2011). Social environments are spatially and demographically clustered such that people tend to associate with others who share similar sociodemographic attributes (e.g., ethnicity, age, education and socioeconomic status) to themselves, and are geographically proximal. Furthermore, homophily extends to psychological attributes, such as beliefs, attitudes and preferences (Huston & Levinger, 1978). Like-minded individuals may selectively associate with each other (Festinger, 1957) or conformity may be bred via processes of social influence (Cialdini & Goldstein, 2004; Asch, 1954). Correspondingly, research in political science highlights the influence that social networks exert on political attitudes and behaviour (Newman, 2013; Klofstad, Sokhey & McClurg, 2013; Mutz,

2002), demonstrating for example that people who interact frequently share similar voting preferences (Pattie & Johnston, 2000).

Galesic et al. (2012) investigated how homophily, in tandem with sampling processes, influences judgments of population-level social distributions. Specifically, the authors propose that, in estimating unknown properties of their social environments, people draw on samples of people they know and regularly encounter, such as their family, friends, colleagues and acquaintances – their social circles. Due to homophily, these *social samples* are not random but are conditioned upon peoples' own attributes, and hence are not representative of the wider population. Correspondingly, the authors found that social sampling produces systematic deviation in estimates of population-level distributions, in line with peoples' own standing on the attribute under estimation. For example, wealthier, relative to poorer participants, reported moving in wealthier social circles, and consequently estimated wealthier population-level wealth distributions. Comparable results were obtained for various other attributes including incidence of work stress, education, health problems and number of friends. The authors show how this interaction between sampling processes and the informational structure of social environments can parsimoniously account for both self-enhancement and self-depreciation effects. Alternatively, explanations involving motivational influences such as self-esteem, or cognitive shortcomings, struggle to account for these seemingly contradictory biases in a unitary model (e.g., Alicke, Klotz, Breitenbacher Yurak & Vredenburg, 1995; Kruger & Dunning, 1999).

Social sampling processes might also be expected to play a role in determining political and economic attitudes. Sampling processes systematically mould peoples' perceptions of how incomes and wealth are distributed across society. Wealth is considered an important determinant of economic attitudes, for example in classical

economics, due to the differing material interests of wealthier and poorer individuals (Alesina & Guiliano, 2011; Meltzer & Richard, 1981; Sears & Funk, 1991). Poorer people are expected to oppose inequality and favour redistribution to a greater extent than the wealthy insofar as it is in their material self-interest to do so. Social sampling suggests an additional, less obvious avenue by which wealth can create divergence in economic attitudes – sampling from homophilous social circles presumably leads wealthier and poorer individuals to hold differing perceptions of the wealth distribution. Because they are disproportionately exposed, via their social contacts, to other similarly wealthy individuals, it follows that wealthier, relative to poorer people, are prone to overestimate levels of affluence across wider society. Although important ideological differences exist in political and economic preferences, conservatives and liberals alike are sensitive to distributive outcomes, and employ knowledge of those outcomes in judging the fairness of social arrangements (Deutsch, 1975; Mitchell, Tetlock, Mellers & Ordonez, 1993; Rawls, 1971). This is the theoretical rationale underpinning the present research, which will investigate the role played by social sampling processes in determining perceptions of the income distribution, and whether sampling processes can consequently help to explain divergence in economic attitudes.

1.7. Chapter Summary

The importance of reducing historically high levels of economic inequality has been increasingly stressed from various quarters, due to its deleterious social, economic, political and psychological consequences (IMF, 2011; OECD, 2011; Picketty, 2014; Wilkinson & Pickett, 2010). Research and theory in political psychology suggests that ideological and motivational forces present a barrier to such efforts by creating wide divergence in attitudes toward inequality (e.g., Jost et al., 2003). Whereas theory in political psychology has generally stressed the intrapsychic bases of political attitudes,

certain lines of research in the domain of judgment and decision making imply that ecological processes might also play an important role. Even where beliefs and judgments are unbiased by cognitive shortcomings or motivational factors, systematic biases in information due to environmental structure can lead to biased perception and judgment (Fiedler, 2000).

Enlightenment philosophy equated human belief with the laws of probability and statistics, suggesting that human beings possess an inherent capacity for “reasonableness” (Hartley, 1749; Hume, 1739). This view later fell from favour in light of evidence that belief and judgment are easily swayed by passions and interests, casting doubt upon humans’ capacity for rational thought and behaviour. Developments in psychology in the 20th century paralleled these circumstances. Whereas some lines of research suggested that humans are well attuned to the statistical properties of their environments, and reason akin to “intuitive statisticians” (e.g., Brunswik, 1955; Hasher & Zacks, 1979; Peterson & Beach, 1967), subsequent research suggested that belief and judgment is inherently constructive in nature (e.g., Kruglanski, 1996; Tversky & Kahneman, 1973, 1974). This latter epistemological perspective entails that motivational influences or cognitive shortcomings impose selectivity upon information search, attention, processing and recall (Ditto & Lopez, 1992; Frey, 1986; Kruglanski, 1996; Kunda, 1987). Consequently, theories of this kind cast serious doubt on the capacity of human beings to live up to the normative dictates of classical rationality.

Theories emphasising the interactive nature of both cognitive and ecological processes pose a counterpoint to perspectives that construe human belief and judgment as either unboundedly rational, or hopelessly distorted, biased and error-prone. This line of reasoning also offers a potential means of unifying contradictory findings and images of human rationality. Cognitive-ecological perspectives demonstrate how selectivity in

information can arise from the environment as well as the mind (Fiedler, 2000; Fiedler & Wänke, 2009; Simon, 1982). Hence environmental structures, and the sampling processes by which information is acquired, need also be considered in explaining biases in belief and judgment. This requires a recasting of rationality in light of the ecological constraints in which humans operate, epitomised by Simon's (1990) analogy of rational behaviour as shaped by the twin scissor blades of mind and environment.

Such processes of cognitive-ecological interaction have clear implications for political cognition, and consequently, they may also play a role in determining political attitudes, including attitudes toward inequality. One avenue by which this might occur is via processes of social sampling (Galesic et al., 2012). People's own wealth systematically determines the wealth of others they are exposed to in their day-to-day lives, such that wealthy people are exposed to relatively more wealthy others, and vice versa. As such, wealthier and poorer people may have different perceptions of how wealthy their society is. Because people are sensitive to distributive outcomes, such divergence in perceptions of the distribution may in turn influence attitudes toward it (Deutsch, 1975; Mitchell et al., 1993; Rawls, 1971).

1.8. Outline of Following Chapters

Chapter 2 addresses, in more specific detail, how social sampling processes might serve to influence attitudes toward inequality, and reports three studies to support this basic proposal. Using a correlational design, Studies 1a and 1b show how household income indirectly influences attitudes toward redistribution of wealth via social sampling, partially explaining wealthier people's greater opposition toward redistributive efforts. Study 2 presents a conceptual replication of these findings drawing data from a large-scale, nationally representative survey conducted in New Zealand, the New Zealand Attitudes and Values Survey (NZAVS). Unlike Studies 1a and 1b which relied upon

participants' subjective estimates, Study 2 uses an objective proxy for social circle wealth (levels of deprivation in participants' neighbourhood).

Chapter 3 examines whether providing novel, alternative and ostensibly real information about the distribution of incomes can influence perceived fairness of the distribution and support for redistributive measures. Both Studies 3a and 3b attempted to manipulate perceptions of the population-level income distribution by presenting more versus less "efficient" (i.e., high vs. low mean) samples of income. These attempts were met with mixed success. In both studies, participants estimated relatively more or less wealthy population distributions in line with the samples presented, demonstrating that they relied upon the novel samples in inferring population distributions. The efficiency manipulation, however, did not translate into between-group differences in economic attitudes as predicted. However, in line with the findings described in Chapter 2, the manipulation did indirectly affect preferences for redistribution sequentially via inequality (Study 3a) or efficiency (Study 3b) of population estimates, and fairness, such that wealthier samples reduced support for redistribution.

Chapter 4 seeks to address whether the tendency to estimate population-level income distributions via social sampling can be prevented or attenuated either by introducing awareness of the unrepresentative nature of social circles and warning against social sampling, providing an alternative sample, or both interventions in combination. In both Studies 4a and 4b, providing an alternative sample alone was sufficient to prevent social sampling, although warning against social sampling was not. Providing both an alternative sample and introducing awareness of social sampling biases ironically backfired, producing stronger social sampling effects. In line with prior research and theory, the findings suggest that social sampling is difficult to prevent, and deliberate attempts at exerting metacognitive control over sampling are likely to fail.

Chapter 5 summarises the findings of Studies 1a – 4b, and discusses the present findings in the broader context of related research and theory on normative justice judgements, political attitudes and political economy. Alternative avenues by which social sampling processes might influence political beliefs and attitudes are explored, as well as the implications of social sampling for democratic process and public policy. Some speculation upon the nature of social samples, and why and how they might influence for economic attitudes, is provided. The present findings are also discussed in the context of theory and research on sampling processes.

Chapter 2

2.1. Introduction

Recent decades have witnessed marked increases in economic inequality across developed nations (Organisation for Economic Cooperation and Development, 2011). Although people generally view equality as an important justice principle in the abstract (e.g., Deutsch, 1975), there is weaker consensus about adopting policies to reduce inequality (e.g., Hochschild, 1986). One source of dissensus is wealth itself: wealthier (vs. poorer) people tend to be more opposed to redistribution (Alesina & Guiliano, 2011). This is no surprise from a classical economic standpoint, since the material burden of redistributive policies falls on wealthier people (Meltzer & Richard, 1981), whereas redistribution is aligned with the self-interest of poorer people (Bartels, 2005; Sears & Funk, 1991; Feldman, 1982). Further, wealthier people are more likely to adopt ideological positions that militate against redistribution (Pratto, Sidanius, Stallworth & Malle, 1994). The present chapter proposes and tests a complementary psychological mechanism that leads wealthier people to be less supportive of redistribution than poorer people, independent of biases stemming from self-interest and ideology^{3.1}.

Social Sampling: Extrapolating from Social Circles to the Population

Inferences about inequality, poverty and affluence in society are constrained, like all social judgments, by the cues the environment affords (e.g., Fiedler, 2000; Gibson,

Footnote 2.1

Chapter 2 appears as a published article in *Psychological Science*:

Dawtry, R. J., Sutton, R. M., & Sibley, C. G. (2015). Why Wealthier People Think People Are Wealthier, and Why It Matters From Social Sampling to Attitudes to Redistribution. *Psychological Science*, 0956797615586560.

1960). Lacking ready knowledge of how various (social and non-social) attributes are distributed, individuals draw on samples of the people they know, including family, friends, and colleagues (Galesic, Olsson & Rieskamp, 2012; Nisbett & Kunda, 1985). Crucially, these social circles are not representative of the overall population, since social environments are spatially clustered. That is, individuals with similar incomes generally live close together and move in similar social circles (McPherson, Smith-Lovin & Cook, 2001). Hence the social circles of wealthier (vs. poorer) individuals include relatively fewer low earners and relatively more high earners (see Figs. 1a and 2a).

Sampling from such unrepresentative sub-populations can lead to systematic differences in perceived population distributions. Relative to poorer people, wealthier individuals tend to estimate that higher incomes are more common and lower incomes less common in the wider population. As a result, people tend to perceive higher mean levels of wealth in society as their own wealth increases. Crucially, this *social sampling* process does not stem from a political or self-serving motivation, but reflects the operations of “an unbiased mind acting in a particular social structure” (Galesic et al., 2012, p. 7).

Political-Psychological Sequelae of Social Sampling

Rich and poor people alike judge wealth levels in society against normative criteria, including efficiency and equality (Deutsch, 1975; Rawls, 1971). Contemporary theories of distributive justice construe equality as a state in which people have approximately the same level of wealth, irrespective of privilege, effort, or merit. Efficiency refers to the extent to which inputs such as labor and economic resources produce a greater overall level of wealth. Increments in efficiency imply an increase in income for at least one person at no penalty to another (i.e., Pareto optimality: see Arrow & Debreu, 1954;

Okun, 1975). Thus, efficiency is reflected in a higher mean level of wealth for a given society, and is often operationalized in this way (e.g., Mitchell et al., 1993).

All else being equal, people prefer these efficient distributions, in which the mean wealth in society is higher. Similarly, all else being equal, people prefer egalitarian distributions to those that are highly unequal. In other words, people prefer their economic pies both big (efficient), and cut into similarly sized slices (equal). These criteria are also applied interactively; people become less concerned with inequality as efficiency increases. These preferences are revealed by increased satisfaction and ratings of fairness (Scott, Matland, Michelbach & Bornstein, 2001; Mitchell et al., 1993). It follows, to the extent that social sampling leads wealthier (vs. poorer) people to conclude that society is wealthier, they will be more satisfied with the status quo and perceive it as fairer. In turn, this can be expected to affect redistributive attitudes, since perceptions of fairness are an important proximal motivator of support for redistribution (Alesina & Angeletos, 2005; Fong, 2001; Smith & Tyler, 1996).

The Present Research

The present research investigates how social sampling, in tandem with normative justice judgments, informs people's attitudes to the redistribution, independently of political orientation and perceptions of self-interest. Normative principles of justice, such as equality and efficiency, condition how people respond to information concerning the distribution of wealth across society. The information people receive about distributive outcomes is, however, constrained by the structure of the social environment in which they are embedded. Consequently, richer and poorer citizens may differ on their attitudes to redistribution in part because they have a different experience of how rich their country is.

In Studies 1a and 1b, American participants indicated their own household income, and estimated how incomes are distributed across both their immediate social circles and the wider population. Participants then indicated how fair and satisfactory they perceived society to be, and whether they supported redistributive efforts. We hypothesised that, controlling for political orientation (Studies 1a and 1b) and perceived self-interest (Study 1b), wealthier (vs. poorer) participants would, via a sequential indirect path of mediation, report a higher level of mean wealth in their social circles, estimate a higher level of mean wealth in the USA, perceive the distribution of wealth in USA as fairer, and tend to oppose redistributive policies.

Study 2 examined data from a nationally representative survey in New Zealand. It utilized census measures of neighborhood-level economic deprivation to derive an objective index of wealth levels in participants' social circles. Since residents of more affluent (less deprived) areas are exposed to wealthier social samples, we predicted that they would show more satisfaction with New Zealand's economic status quo, independent of political attitudes and control factors. Analogous to Studies 1a and 1b, this entails that the relationship between household income and satisfaction is mediated by neighborhood deprivation.

2.2. Methods

Study 1a

Participants

US participants were recruited online ($N = 305$, 51.5% male; $M_{\text{age}} = 37.40$ years; $SD_{\text{age}} = 12.04$) via Amazon's Mechanical Turk (MTurk; Buhrmester, Kwang, & Gosling, 2011). Given our focus on the role of household income, it was desirable to minimise the number of individuals who are dependent on parental income. Hence we requested

that only individuals of 25 years or older complete the survey. Fifteen participants reported their age to be below 25. All analyses were conducted both with and without these participants' data and no substantive differences emerged, so reported analyses include all participants. In keeping with previous investigations of the representativeness of Mturk samples (Paolacci, Chandler & Ipeirotis, 2010), the incomes of the present sample were somewhat lower than, but similarly distributed to, the US population (based on the US Census Bureau, 2013). Thus, 10.27% of the sample reported household incomes placing them in the wealthiest 20% of the US population, and 20%, 26.89%, 21.64% and 20.32 % reported household incomes in the 2nd, 3rd, 4th, and 5th wealthiest quintiles respectively. Sample size was determined a priori based on budgetary considerations. Data collection proceeded until the predetermined sample size was reached. Although 300 participants were requested, an additional 5 did not complete the entire survey and provided only partial data. For all studies reported herein, ethical approval was obtained from the institutional Ethics Committee, and the research was conducted in full accordance with the World Medical Association Declaration of Helsinki.

Materials and Procedure

In accordance with the method used by Galesic et al. (2012), participants estimated complete income distributions as opposed to summary indicators such as the mean. This indirect method allowed for estimation of both within-participant Gini indices and mean incomes for reported social circle and population distributions. It was also expected to minimise any potential biases (e.g., from ideology or self-enhancement motives) introduced by explicitly asking participants about inequality and average incomes.

Participants first estimated the distribution of annual household income across their social contacts by indicating the percentage of contacts earning incomes within

each of eleven \$15,000 intervals (\$0 – \$15,000; \$15,000 - \$30,000... \$150,000+). The final interval was open-ended (all incomes of \$150,000 upward). Household income was defined as “...the combined annual earnings of all household members from all sources, including wages, commissions, bonuses, Social Security and other retirement benefits, unemployment compensation, disability, interest, and dividends”. Social contacts were defined as “...adults you have been in personal, face-to-face contact with at least twice this year, for example friends, family, colleagues and other acquaintances” (Galesic et al., 2012). Using an identical procedure, participants then estimated the distribution of annual household income across the “entire US population”. The order of the distribution estimation tasks was not counterbalanced^{2.2}.

Two questions assessing perceived fairness of and satisfaction with the US income distribution followed (e.g., “To what extent do you feel that household incomes are fairly-unfairly distributed across the US population”; 1 = *Extremely Fair*; 9 = *Extremely Unfair*, reverse-coded prior to analysis). These items were highly correlated ($r = .88$) and their mean formed a composite measure of perceived fairness.

Redistributive attitudes were assessed using four items ($\alpha = .81$) adapted from the Gallup Poll Social Audit Survey (1998) (e.g., “The government should redistribute

Footnote 2.2

To examine order effects, a further study was conducted in which a sample of US MTurkers ($N = 306$) estimated social circle and population distributions in counterbalanced order with a 2 minute filler task. In a moderated mediation analysis (PROCESS model 14, 10,000 resamples), presentation order did not moderate the indirect effect of own income on population mean income via social circles, $b = -.09$, $SE = .11$, $p = .38$. The indirect relationship between own income and population mean income was the same whether social circles were estimated first (BCa CI's of .11 and .43, point estimate effect = .23) or second (BCa CI's of .07 and .39, point estimate = .18).

wealth through heavy taxes on the rich”; 1 = *Strongly Disagree*; 6 = *Strongly Agree*). In a final section, participants provided demographic information, including annual household income, and rated their political orientations (1 = *Extremely Liberal*; 9 = *Extremely Conservative*). Examples of the Study 1a estimation tasks and a complete list of questionnaire items appear in Appendix I.

Study 1b

Participants

US participants were recruited online ($N = 321$, 48.4% male; $M_{age} = 35.06$ years; $SD_{age} = 10.92$) via Amazon’s Mechanical Turk (Mturk; Buhrmester et al., 2011). As in Study 1a, only individuals of 25 years or over were requested to complete the survey, but 24 participants reported being younger than this. Reported analyses include these participants; excluding them did not affect results. In Study 1b, 8.9% of the sample reported household incomes placing them in the wealthiest 20% of the US population, and 20.5%, 27.5%, 21.5%, and 20.9% reported household incomes in the 2nd, 3rd, 4th, and 5th wealthiest quintiles respectively. Sample size was determined a priori based on budgetary considerations. Data collection proceeded until the predetermined sample size was reached. Although 300 participants were requested, an additional 21 did not complete the entire survey and provided only partial data.

Materials and Procedure

Study 1b utilised a novel response method in which participants were asked to estimate mean incomes across quintiles (i.e., each 20%) of their social circles and the US population. Compared to the method used in Study 1a, participants are required to make use of the same “raw” data (available knowledge of incomes), and are equally subject to the environmental constraints proposed in the social sampling model. The

method here is more time-efficient, and the use of different response formats in the two studies builds confidence in the robustness of the findings. Participants also provided explicit estimates of social circle and population mean incomes and rated levels of inequality.

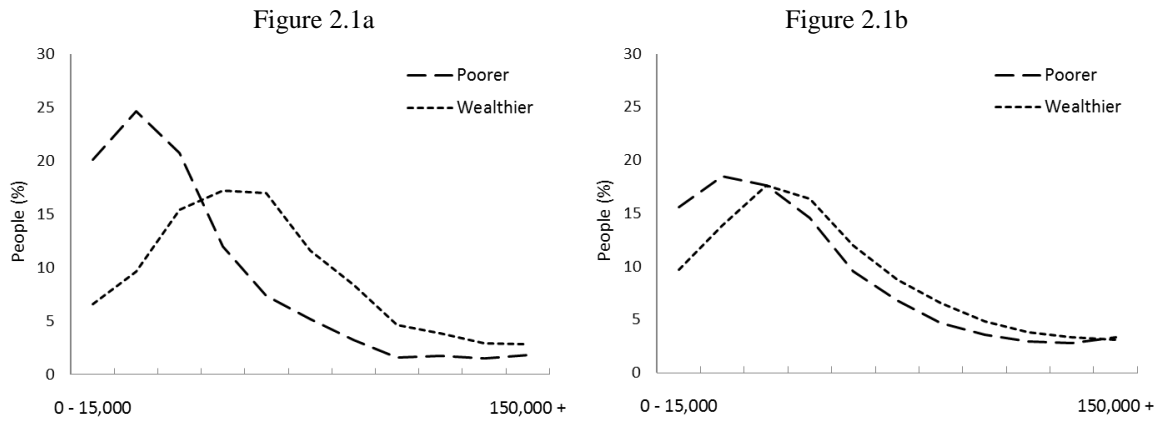
Participants first estimated the mean annual household income within each income quintile (i.e., lowest to highest earning 20%) of their social contacts, and then within each quintile of the US population as a whole, on sliding scales (ranging from \$1000 - \$250,000 in \$100 units). Social contacts and household income were defined as per Study 1a. In addition, participants provided an explicit estimate of the mean income across their social circles, and another for the entire US population, on a sliding scale (ranging from \$1000 - \$100,000 in \$100 units). Participants provided ratings of inequality across both their social circles and the US population (2 items for each, e.g., “To what extent are household incomes equally – unequally distributed across your social contacts (the population of the United States)”; 1 = *Very Equally*; 6 = *Very Unequally*; $r = .48$ and $r = .62$ respectively). Participants then responded to the same fairness and satisfaction items used in Study 1a ($r = .81$; 1 = *Extremely Fair/Satisfied*; 6 = *Extremely Unfair/Dissatisfied*, reverse-coded prior to analysis). Redistributive attitudes were assessed with the four-item scale used in Study 1a ($\alpha = .81$; 1 = *Strongly Disagree*; 6 = *Strongly Agree*). Three items ($\alpha = .82$) assessed perceived self-interest in redistribution (e.g., “To what extent do you feel that redistribution of wealth through tax and welfare is in agreement with your own financial interests”; 1 = *Strongly Disagree*; 6 = *Strongly Agree*). A further three items ($\alpha = .83$) assessed political orientation (“How would you describe your political attitudes”; 1 = *Very Liberal/Very Left-Wing/Strong Democrat*; 7 = *Very Conservative/Very Right-Wing/Strong Republican*). In a final section, participants provided demographic information including annual household

income. Examples of the Study 1b estimation tasks and additional questionnaire items (those not included in Study 1a) appear in Appendix II.

2.3. Results

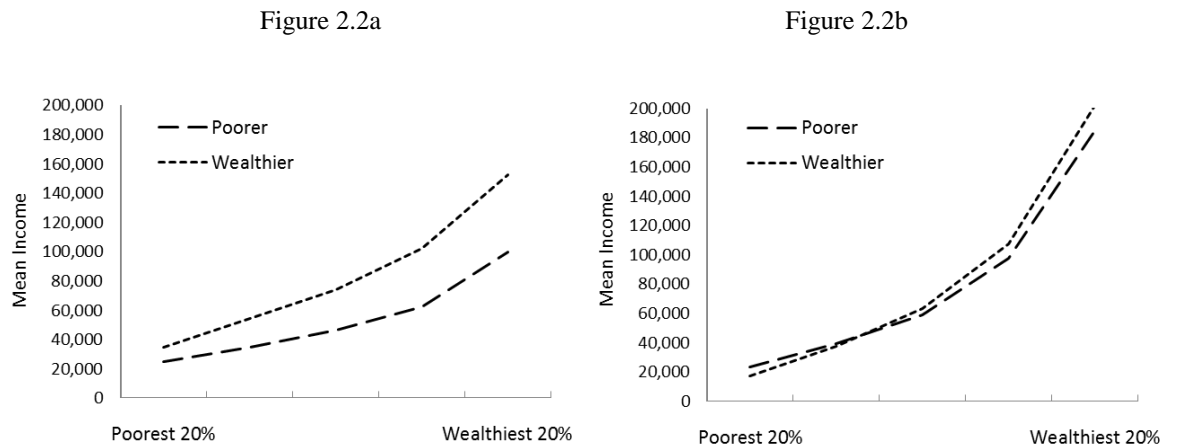
Figures 2.1a and 2.1b display estimated population and social circle distributions of household income, respectively, for high and low-income participants (the highest and lowest earning third of the sample) in Study 1a. Figures 2.2a and 2.2b display estimated population and social circle distributions of household income, respectively, for high and low-income participants (the highest and lowest earning third of the sample) in Study 1b. Correlations, means and standard deviations for the Study 1a and Study 1b variables are presented in Table 2.1. Due to the scaling of the measures, all analyses across both studies were conducted on standardized data.

In Study 1a, within-participant (social circle and population) mean incomes and Gini indices were estimated on the assumption of complete homogeneity of incomes within each income interval. Following the advice of Ravallion (1992), incomes in the lowest interval were set at 80% of the upper bound of the interval (\$12,000) and incomes in the highest interval were set at 30% above the lower bound (\$195,000). Incomes within all intervening intervals were assumed to be equivalent to the interval midpoint (e.g., all incomes in \$15,000 – \$30,000 were set at \$22,500). Weighted mean incomes were derived by calculating the total income at each interval, summing these totals, and dividing across the population (i.e., by $N = 100$). Cumulative proportions of total income at each X % of the population were derived following the same assumptions, allowing for approximation of the Gini index with trapezoids. Where X_k is the cumulative proportion of the population and Y_k is the cumulative proportion of income indexed in non-decreasing order, the resulting approximation for the Gini index is given by:



Study 1a average estimated social circle (Fig. 2.1a) and population (Fig. 2.1b) income distributions (estimated percent across income intervals) as a function of participant income.

Poorer and wealthier participants are the bottom and top third, respectively, ranked by household income (data for the middle income third is not displayed for clarity).



Study 1b average estimated social circle (Fig. 2.2a) and population (Fig. 2.2b) income distribution (estimated mean incomes across quintiles) as a function of participant income.

Poorer and wealthier participants are the bottom and top third, respectively, ranked by household income (data for the middle income third is not displayed for clarity).

Table 2.1. Means and intercorrelations of Study 1a and Study 1b variables (continued overleaf)

Variable	<i>M (SD)</i>	1	2	3	4	5	6	7	8
Study 1a									
1. Household Income	\$54,732 (\$47,238)	-							
2. Social circle mean income	\$54,294 (\$25,295)	.48***	-						
3. Population mean income	\$58,604 (\$17,230)	.19***	.34***	-					
4. Social circle inequality (Gini Index)	26.35 (9.97)	-.12*	-.11	-.09	-				
5. Population inequality (Gini Index)	35.51 (7.48)	-.07	-.15*	-.05	.21***	-			
6. Fairness/Satisfaction	3.54 (2.02)	.18**	.24***	.17**	-.08	-.16**	-		
7. Support for redistribution	3.91 (1.15)	-.21***	-.25***	-.18**	.06	.15**	-.70***	-	
8. Political Preferences	4.47 (2.23)	.15**	.15*	-.01	-0.05	-.14*	.42***	-.57***	-
Study 1b (derived mean income/inequality indices)									
1. Household Income	\$55,500 (\$55,999)	-							
2. Derived social circle mean income	\$65,980 (\$36,419)	.42***	-						
3. Derived population mean income	\$83,992 (\$28,214)	.11	.51***	-					
4. Derived social circle inequality (Gini Index)	30.31 (11.87)	-.06	.01	-.07	-				
5. Derived population inequality (Gini Index)	41.64 (11.09)	-.02	-.19***	-.14**	.34***	-			
6. Fairness/Satisfaction	2.28 (1.31)	.14*	.14*	.16**	-.22***	-.22***	-		
7. Support for redistribution	4.09 (1.23)	-.21***	-.21***	-.09	.14*	.18**	-.71***	-	
8. Political Preferences	3.53 (1.49)	.13*	.12*	.07	-.06	-.14*	.49***	-.61***	-
9. Self-interest in redistribution	3.53 (1.19)	-.38***	-.22***	-.02	.17**	.15*	-.46***	.58***	-.42***

Table 2.1 (continued).

Variable	<i>M (SD)</i>	1	2	3	4	5	6	7	8
Study 1b (direct mean income/inequality ratings)									
1. Household Income	\$55,500 (\$55,999)	-							
2. Estimated social circle mean income	\$48,184 (\$22,829)	.60***	-						
3. Estimated population mean income	\$44,054 (\$13,142)	.18**	.32***	-					
4. Estimated social circle inequality	4.06 (1.15)	-.01	-.05	.09	-				
5. Estimated population inequality	5.34 (.91)	.15*	.06	-.05	.20***	-			
6. Fairness/Satisfaction	2.28 (1.31)	-	.18**	.11	-.19**	-.41***	-		
7. Support for redistribution	4.09 (1.23)	-	-.19**	-.03	.09	.28***	-	-	
8. Political Preferences	3.53 (1.49)	-	.05	.03	-.08	-.18**	-	-	-
9. Self-interest in redistribution	3.53 (1.19)	-	-.23***	-.02	.06	.18**	-	-	-

Note. Higher values indicate more of each construct. Higher values for the political preferences measure indicate greater Conservatism, and higher values for evaluations indicate more favorable evaluations. Redundant coefficients have been deleted from the lower panel.

* $p < .05$, ** $p < .01$, *** $p < .001$

$$G_1 = 1 - \sum_{k=1}^n (X_k - X_{k-1})(Y_k + Y_{k-1})$$

In Study 1b, (social circle and population) mean incomes and Gini indices were calculated directly on estimated quintile mean incomes (i.e., the mean of estimated quintile mean incomes). These Gini indices capture the inequality between the mean incomes of the poorest through wealthiest quintiles (as opposed to inequality approximated continuously across each X percent of the population). These methods were chosen for computational simplicity, and enabled simultaneous computation of mean incomes and Gini's across all participants using a bespoke spreadsheet.

From Social Sampling Effects to Policy Preferences

We first examined whether participants' income was indirectly related to redistributive preferences sequentially via social circle mean income, population mean income and fairness/satisfaction. Bootstrapped mediation analyses (10,000 resamples) examined the indirect relationship of income to redistributive preferences via these mediators using the PROCESS macro for SPSS (model 6; see Hayes, 2012), separately for Study 1a and 1b participants. Designed to specifically test hypotheses of *serial* mediation in which the sequence of mediators represents an assumed causal chain, this procedure estimates path coefficients and 95% bias-corrected accelerated confidence intervals (BCa CI's) for the total and all possible specific indirect effects of X on Y . Political orientation was included as a covariate in analyses for both Study 1a and 1b data. Perceived self-interest in redistribution was an additional covariate in Study 1b. In both studies, we also controlled for population Gini since this was negatively related to estimated mean incomes. We repeated the analyses without the covariates included and obtained similar

results. Eight participants in Study 1a, and 26 in Study 1b, were excluded from these analyses due to missing data (principally, household income)^{2,3}.

As expected, in Study 1a the relationship between participant income and redistributive preferences was sequentially mediated through social circle mean income, population mean income, and then fairness/satisfaction, controlling for political orientation and population Gini (BCa CI's of -.02 and -.003, indirect effect = -.01). No other indirect effects attained significance. The direct effect of household income was not significant after accounting for the proposed mediators and covariates (BCa CI's of -.12 and -.05, direct effect = -.03).

In Study 1b, separate mediation analyses were conducted for mean incomes and inequality indices derived from estimated distributions, and then from participants' direct estimates. As predicted, for derived mean incomes, the relationship between participant income and redistributive preferences was sequentially mediated through social circle mean income, population mean income, and fairness/satisfaction, controlling for political orientation, population Gini and perceived self-interest in redistribution (BCa CI's of -.03 and -.01, indirect effect = -.02). An indirect path from household income to redistributive preferences via mean social circle income was also

Footnote 2.3

Two participants were additionally excluded from Study 1b analyses due to outlying income scores (+4.72 and +10.62 *SD*). The indirect effect of income on redistribution via directly estimated mean incomes is not significant when these participants are included. In Study 1a, two participants also reported household incomes above +4 *SD* (+4.66 and +6.25), however, excluding these participants did not affect the results and hence their data was retained in the reported analyses (when excluded, BCa CI's of -.02 and -.002, indirect effect = -.0 for the sequential mediation of income on redistributive preferences reported in Study 1a).

significant (BCa CI's of $-.08$ and $-.002$, indirect effect = $-.04$). The direct effect of household income was not significant after accounting for the proposed mediators and covariates (BCa CI's of $-.05$ and $.12$, direct effect = $.04$).

Repeating this analysis on the Study 1b direct estimates of mean social circle and population incomes produced similar results; the relationship between participant income and redistributive preferences was sequentially mediated through social circle mean income, through population mean income, through evaluations, controlling for political orientation and perceived self-interest in redistribution (BCa CI's of $-.02$ and $-.001$, indirect effect = $-.01$). The direct effect of household income was not significant after accounting for the proposed mediators and covariates (BCa CI's of $-.07$ and $.11$, direct effect = $.02$).

As shown in Figures 2.3a (Study 1a), 2.3b (Study 1b derived measures) and 2.3c (Study 1b direct estimates), higher (vs. lower) income participants estimated more efficient social circle distributions, and consequently, more efficient population distributions. In turn, increased efficiency was related to more fairness and lower support for redistribution.

In Study 1b, we also sought to examine the accuracy of both poorer (lowest income third of sample) and wealthier (highest income third) participants' estimates by comparing them with external data. The derived mean incomes of both poorer ($M = \$81,215$, $SD = \$31,228$) and wealthier ($M = \$86,249$, $SD = \$23,635$) participants were significantly above the mean US household income of $\$71,274$ (obtained from US Census Bureau data for 2012), $t(102) = 3.23$, $p = .002$ and $t(104) = 6.49$, $p < .001$ for poorer and wealthier participants, respectively. Derived estimates did not differ between poorer and wealthier participants, $t(206) = 1.31$, $p = .19$. In contrast, directly estimated mean incomes of both poorer ($M = \$39,859$, $SD = \$14,725$) and wealthier ($M =$

Figure 2.3a.

Study 1a indirect path via derived mean income indices (political ideology and derived population Gini's were included as covariates). The total effect is given in parentheses.

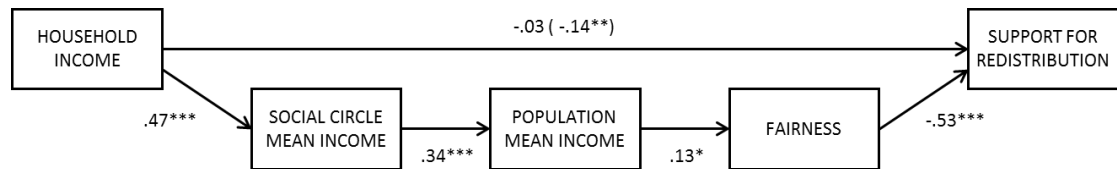


Figure 2.3b.

Study 1b indirect path via derived mean income indices (political ideology, perceived self-interest in redistribution and derived population Gini's were included as covariates). The total effect is given in parentheses.

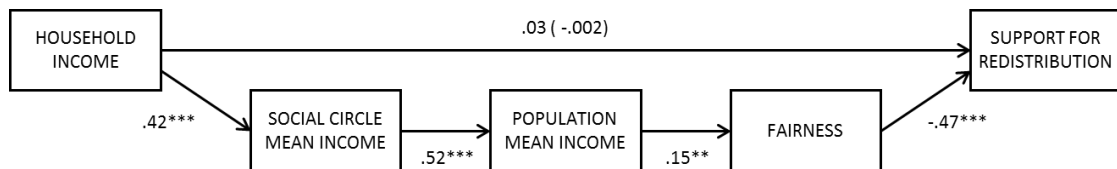
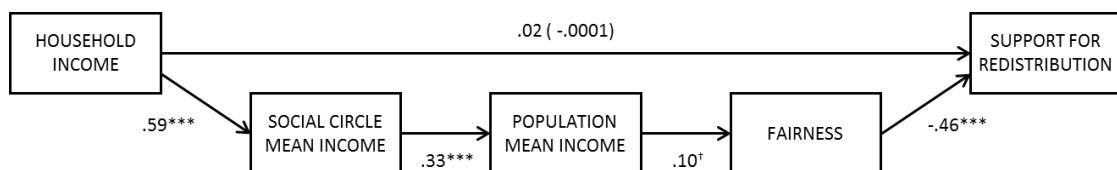


Figure 2.3c.

Study 1b indirect path via direct mean income ratings (political ideology and perceived self-interest in redistribution were included as covariates). The total effect is given in parentheses.



$^\dagger p < .10$, $* p < .05$, $** p < .01$, $*** p < .001$

\$47,643, $SD = \$11,536$) participants were significantly below the mean US household income of \$71,274, $t(100) = 21.44, p < .001$ and $t(102) = 20.79, p < .001$ for poorer and wealthier participants, respectively. Poorer participants' direct estimates were hence less accurate insofar as they were significantly lower than wealthier participants', $t(202) = 4.21, p < .001$.

Testing Alternative Mechanisms

We next sought to examine the potential mediating role of ideological variables in the relation between household income and redistributive preferences, and to compare the relative size of indirect relations via ideological and ecological pathways. Parallel mediation analyses (PROCESS model 4, 10,000 resample's) were conducted testing the relation of income to redistribution via social circle mean incomes and ideological variables (political ideology and self-interest) simultaneously, for both Studies 1a and 1b. In Study 1a, a contrast of the indirect relation of income to attitudes to redistribution via social circles (BCa CI's of -.15 and -.01, indirect effect = -.07) and political ideology (BCa CI's of -.19 and -.04, indirect effect = -.11) revealed no difference in the size of the indirect effect via these parallel pathways (BCa CI's of -.11 and .06, effect = .04). In Study 1b, contrasts of the indirect relation of income to attitudes to redistribution via derived social circle means (BCa CI's of -.07 and .0001, indirect effect = -.03), political ideology (BCa CI's of -.12 and -.003, indirect effect = -.06) and self-interest (BCa CI's of -.21 and -.09, indirect effect = -.16) revealed no difference in the size of the (non-significant) indirect path via social circles compared to that via political ideology (BCa CI's of -.04 and .09, effect = .03). The indirect path via self-interest, however, was significantly greater than the indirect path via both social circles (BCa CI's of .05 and .19, effect = .11) and political ideology (BCa CI's of .01 and .16, effect = .09). Repeating this analysis for the Study 1b directly estimated social circle

means produced the same pattern of results (effect via social circles = effect via political ideology; effect via self-interest > effect via political ideology and effect via social circles), with the exception that the indirect path via social circles attained significance (BCa CI's of -.12 and -.01, effect = -.06).

To summarise, these models show that, in Study 1a, income was negatively, indirectly related to support for redistribution to the same extent via both ideological and ecological pathways. Contrastingly, for Study 1b, derived social circle mean income was negatively, indirectly related to support for redistribution via ideological variables (political ideology and self-interest) only, and to a greater extent via self-interest specifically. Similarly, for Study 1b directly estimated social circle mean income, although both the ideological and ecological variables accounted for the negative relation between income and support for redistribution, the path via self-interest specifically was most influential.

Study 2

In Studies 1a and 1b, participants' subjective estimates of the income distribution across their social contacts were assumed to reflect the natural sample of incomes to which they are exposed in their day-to-day lives. This assumption is shared by other studies of social sampling effects, which have also relied on subjective estimates (e.g., Galesic et al., 2011). However, variance in these subjective estimates may be attributable to psychological factors as well as objective differences in social circles. For example, participants may anchor upon their own income to estimate social circle incomes (Krüger, 1999).

The present study examines whether the previous findings could be conceptually replicated using an objective indicator of social circle incomes. Specifically, using data

from the New Zealand Attitudes and Values Survey (NZAVS; 2009), we examined whether household income is indirectly related to perceived economic/social fairness via neighbourhood-level economic deprivation, independent of political ideology and other control variables.

2.4. Methods

Participants

Participants were 4634 registered voters in New Zealand, for whom complete data for the measures analyzed here were available (2681 women, 1953 men). Participants, of whom 79.2% were born in New Zealand and 79.2% were employed, had a mean age of 47.25 ($SD = 14.66$). Mean household income was \$85,552 ($SD = \$71,154$). The majority of missing data were due to non-reported household income.

Sampling Procedure

The full Time 1 (2009) NZAVS contained responses from 6518 participants sampled from the 2009 New Zealand electoral roll. The electoral roll is publicly available for scientific research and in 2009 contained 2,986,546 registered voters. This represented all citizens over 18 years of age who were eligible to vote regardless of whether they chose to vote, barring people who had their contact details removed due to specific case-by-case concerns about privacy. The sample frame was split into three parts. Sample Frame 1 constituted a random sample of 25,000 people from the electoral roll (4,060 respondents). Sample Frame 2 constituted a second random sample of a further 10,000 people from the electoral roll (1,609 respondents).

Sample Frame 3 constituted a booster sample of 5,500 people randomly selected from meshblock area units of the country with a high proportion of Māori, Pacific

Nations and Asian peoples (671 respondents). Statistics New Zealand (2014) define a meshblock as:

“...the smallest geographic unit for which statistical data is collected and processed by Statistics New Zealand. A meshblock is a defined geographic area, varying in size from part of a city block to large areas of rural land. Each meshblock abuts against another to form a network covering all of New Zealand including coasts and inlets, and extending out to the two hundred mile economic zone. Meshblocks are added together to ‘build up’ larger geographic areas such as area units and urban areas. They are also the principal unit used to draw-up and define electoral district and local authority boundaries.”

Meshblocks were selected using ethnic group proportions based on 2006 national census data. A further 178 people responded but did not provide contact details and so could not be matched to a sample frame.

In sum, postal questionnaires were sent to 40,500 registered voters or roughly 1.36% of all registered voters in New Zealand. The overall response rate (adjusting for the address accuracy of the electoral roll and including anonymous responses) was 16.6%.

Measures

Fairness

The individual-level fairness measure was composed from four items ($\alpha = .65$) available in the NZAVS that were most conceptually similar to the fairness/satisfaction items employed in Studies 1a and 1b. Two items were from General System Justification scale (Kay & Jost, 2003), specifically, “In general, the New Zealand political system

operates as it should” and “In general, I find New Zealand society to be fair” (1 = *Strongly Disagree*; 7 = *Strongly Agree*). Two further items were included from the National Wellbeing Index (Tilioune, Cummins & Davern, 2006). Respondents rated their satisfaction with “The economic situation in New Zealand” and “The social conditions in New Zealand” (1 = *Completely Dissatisfied*; 10 = *Completely Satisfied*). Items were standardised prior to averaging to account for differences in scaling.

Meshblock Deprivation

The NZDep2006 Scale of Deprivation (Salmond, Crampton & Atkinson, 2007) is a neighbourhood-level measure of relative socioeconomic deprivation based on national census data, combining weighted information on the proportion of people in a given meshblock (geographical unit) experiencing various dimensions of deprivation (e.g., the proportion of people in receipt of a means-tested benefit, not living in their own home, aged 16-24 and unemployed, with no access to a car; the proportion of equivalized households with income below an income threshold). The scale ranges from 1-10, dividing New Zealand into deciles according to the distribution of the principal component scores derived from these dimensions, with a score of 10 (1) indicating that a given area is in the most (least) deprived 10% of areas in New Zealand according to the NZDep2006 scores. The NZDep2006 scale was used in the present analysis as an objective proxy for participants’ social circle estimates, on the assumption that individuals living in more/less deprived areas will tend to have relatively poorer/wealthier social contacts. Insofar as geographic mobility and communication technologies allow for social ties with people from other regions, it should be acknowledged that the NZDep2006 may underestimate the variance in income levels to which people are exposed via their social contacts, and is hence by no means a perfect alternative to estimated social circle distributions. All else being equal, this may result

in underestimation of social sampling effects. Nevertheless, prior research and theory emphasises spatial proximity as a key defining feature of social networks (Reagans, 2011; McPherson et al., 2001; Wellman, 1996). Our sample contained 4226 unique meshblock area units, with 1.09 participants per unit ($SD = .33$, range 1-5). The geographic size of these meshblock units differs depending on population density, but each unit tends to cover a region containing a median of roughly 90 residents ($M = 103$, $SD = 72$, range = 3-1,431). In 2013, at the time of the latest census, there were a total of 46,637 meshblocks. Mean area-unit deprivation across meshblock units included in the sample was 4.91 ($SD = 2.82$).

Covariates

Political orientation was measured in the NZAVS on a 7-point scale (1 = *Extremely Conservative*; 7 = *Extremely Liberal*) and was included in the model. Other control variables were age, gender (0 = Male; 1 = Female), whether the respondent was born in New Zealand (0 = No; 1 = Yes) and whether the respondent was in paid employment (0 = No; 1 = Yes).

2.5. Results

As anticipated, the relationship between household income and fairness was mediated via meshblock deprivation score, after accounting for the aforementioned control variables (BCa CI's of .008 and .019, indirect effect = .013); wealthier respondents lived in less deprived neighbourhoods and consequently perceived New Zealand to be a more fair society. The direct effect of household income on fairness remained significant (BCa CI's of .044 and .086, direct effect = .065). The outcome of the model was the same whether we took the two General System Justification items only, the two National Wellbeing Index items only, or all four items as the criterion variable.

To ensure that these results did not depend on a particular operationalization of neighborhood wealth or economic attitudes, we tested a number of conceptually similar models, substituting different measures of each construct. For example, we found that significant indirect paths ran from household income through neighborhood median income, the proportion of poor relative to wealthy residents and the proportion of residents in receipt of state benefits. These indirect paths were significant whether we took fairness, General System Justification, National Wellbeing Index, or voting for the National party (the incumbent, economically conservative party) as outcome measures (these analyses appear in Table S1, Appendix III).

Similarly to Studies 1a and 1b, a parallel mediation model (PROCESS model 4, 10,000 resamples) was tested to compare the relative contribution of income to fairness via ecological (neighbourhood deprivation) and ideological (political ideology) pathways (age, gender, employment status and whether participants were born in New Zealand were included as controls). A contrast of the indirect relation of income to fairness via neighbourhood deprivation (BCa CI's of .008 and .019, indirect effect = -.013) and political ideology (BCa CI's of -.0001 and .002, indirect effect = .001) revealed that the indirect effect was only significant via neighbourhood deprivation, and that this relationship was significantly greater than the (non-significant) pathway via political ideology (BCa CI's of .007 and .019, effect = .013). In sum, this model suggests that the ecological variable alone, neighbourhood-level deprivation, accounts for the positive relation between income and fairness.

2.6. General Discussion

The present findings confirm that self-interest (Study 1b) and ideological motivations (Studies 1a and 1b) are important contributors to the differing economic attitudes of wealthier and poorer people (Hasenfeld & Rafferty, 1989; Meltzer & Richard, 1981).

The present findings also uncover another mechanism. Consistent with theory and research on social sampling effects, wealthier (relative to poorer) Americans reported moving in wealthier social circles, and extrapolated from them when estimating wealth levels across America as a whole (Studies 1a, 1b). In turn, consistent with theory on normative justice judgments, these estimates were associated with the perceived fairness of wealth distribution in America, and opposition to redistribution.

These results suggest that the rich and poor do not simply have different views about how wealth should be distributed across society. Rather, they subjectively experience living in subtly – but importantly – different societies. Thus, in the relatively affluent America inhabited by wealthier Americans, there is less need to distribute wealth more equally (Mitchell et al., 1993; Scott et al., 2001). Study 2, using data from New Zealand, shows that this is not unique to the USA. It also demonstrates that the relationship between people’s own income and their attitudes toward redistribution is mediated by *objective* metrics of wealth levels in their social circles. This provides new validation of the social sampling perspective, which assumes that cognition is determined by objective ecological conditions, but has been tested using participants’ subjective perceptions, rather than objective measures, of those conditions (Galesic et al., 2012).

On balance, the present data seem to suggest that sampling processes and ideology are perhaps equally important in *explaining the negative relation between income and economic attitudes*. Recall that both ideology and social samples mediated the relation between income and support for redistribution to a similar extent in Study 1a, and neighbourhood deprivation (but not political attitudes) mediated the relation between income and fairness in Study 2. The present findings, however, cannot be interpreted as suggesting that social sampling processes play a relatively more important

role in determining economic attitudes generally than either self-interest or ideology. As is underscored by the Study 1a and 1b zero-order correlations (see Table 1) between political attitudes/social circles on the one side, and support for redistribution/fairness on the other, political attitudes seemingly bear a stronger direct relationship to economic attitudes than do social samples. Furthermore, in Study 1b, self-interest emerged as the only significant mediator of the relation between income and support for redistribution. Hence, although social sampling processes help explain (i.e., mediate) the differing economic attitudes of wealthier and poorer individuals, self-interest perhaps plays a more powerful role in this regard, and both political attitudes and self-interest are more powerful direct determinants of economic attitudes than social sampling. It should, however, be appreciated that, insofar as attitudes to inequality and redistribution are in and of themselves components of political ideology, (e.g., Jost et al., 2003), strong relationships between political ideology and support for redistribution likely reflect some degree of redundancy between these variables (and similarly so for self-interest in, and support for, redistribution).

We suggest that the processes observed here are antagonistic to political efforts to reduce inequality. As inequality grows, wealth is becoming more spatially concentrated (Massey & Fischer, 2003). This may lead to increasingly dissociated enclaves of political perception and preference. Further, the disproportionate political power held by wealthier citizen's means that their (relatively less egalitarian) economic preferences will tend to hold sway (Bonica, McCarty, Poole & Rosenthal, 2013).

Social sampling may also be antagonistic to rational political thought. It assumes one's social circles are representative of wider society, and so can be seen as a manifestation of false consciousness (Jost, 1995; Pratto & Stewart, 2012). It is also a source of bias that may undermine people's ability to realistically appraise the economic

hierarchy and their position within it. This ability is prerequisite for rational decision making in models of political economy (Cruces, Perez Truglia & Tetaz, 2011; Meltzer and Richard, 1981).

In contrast, the present results do not support strong claims about the accuracy of economic perceptions by specific groups in society. Poorer (vs. richer) participants' explicit estimates were less accurate, underestimating mean US incomes to a greater extent (Study 1b). However, these explicit estimates diverged widely from derived estimates of mean incomes which were significantly above objective levels, and similarly so, for both poorer and richer participants. This method variance demands reticence in judging the overall accuracy of perceptions of economic efficiency. The same appears to be true of perceived economic inequality (Chambers, Swan & Heesacker, 2014; Norton & Ariely, 2011), although the present studies do not speak directly to that literature. As people's own wealth increases, their social circles become wealthier, but not necessarily more unequal. For this reason, we did not expect, nor observe, indirect paths from participants' own wealth via social circle inequality to national inequality.

Social sampling exemplifies how "cognition is situated – not isolated in inner representations and processes but causally interdependent with the current physical and social environment" (Smith & Semin, 2007, p. 132). The present results highlight the importance of ecological processes for understanding political behaviour, in addition to individuals' ideologies or perceived self-interest. Attitudes to redistribution and the economic status quo appear to be subject to (informational) biases in the environment as well as biases in the mind.

Chapter 3

3.1. Introduction

Chapter 2 examined how the distribution of incomes in individual's immediate social environment are systematically related to perceptions of the income distribution across wider society via social sampling processes (Galesic et al., 2012). Sampling processes apparently lead wealthier people to perceive greater levels of aggregate wealth across society, in turn leading them to be relatively more opposed to redistribution than poorer people. These findings suggest that divergence in the economic preferences of wealthier and poorer individuals cannot be explained entirely by differences in political attitudes and material interests. Rather, it is also important to consider how the informational structure of peoples' immediate social environment systematically moulds perceptions of the income distribution (e.g., Cruces et al., 2013).

The present chapter seeks experimental corroboration for the findings described in Chapter 2, and for the contention that sampling processes play a causal role in determining important economic attitudes. In Studies 3a and 3b, Mechanical Turk participants were serially presented with values that comprised either a low or high mean (i.e., low vs. high efficiency) sample of incomes, which were ostensibly representative of the true UK (Study 3a) or US (Study 3b) household income distribution. These studies sought to ascertain whether manipulating perceptions of the income distribution in this manner would impact upon perceptions of fairness and support for redistributive measures.

Given the correlational nature of Studies 1a through 2, it is not certain whether social sampling processes are indeed causally related to economic attitudes. For example, it is conceivable that participants' subjective income distributions are prone to

distortion via selective attention, processing or recall of information in the service of reaching a desired conclusion (e.g., Kunda, 1990; Kruglanski, 1996). The Study 2 findings do not lend themselves to this interpretation given that the analyses employed an objective proxy for social circles (i.e., neighbourhood deprivation) as opposed to participants' estimates. The correlational nature of Study 2 nevertheless leaves room for alternative explanations. For example, it is possible (if seemingly unlikely) that the true causal path from fairness to income via neighbourhood deprivation is reversed relative to the proposed model - people whom perceive New Zealand society as less fair may choose to live in poorer neighbourhoods, which renders them prone to poverty.

As discussed in Chapter 1, prior research and theorising emphasising processes of motivated social cognition highlights the tendency for people to distort information such that it aligns with prior political beliefs, attitudes, and epistemic and existential needs (for a review, see Jost, Glaser, Kruglanski & Sulloway, 2003). For example, wealthier people may be prone to strategically estimate more efficient social circle and population income distributions as means of justifying their relative economic advantage and legitimising opposition to redistributive efforts. Conversely, poorer people may estimate less efficient distributions to make their own position appear relatively more favourable and maintain positive self-esteem, consistent with a common interpretation of the better-than-average effect (Alicke, Klotz, Breitenbecher, Yurak & Vredenburg, 1995). Indeed, research and theory in political psychology suggests that members of advantaged and disadvantaged groups alike engage in motivated distortion of social and political information in order to preserve the perceived legitimacy of the status quo, and their position within it (Jost & Hunyady, 2002). Relatedly, prior research demonstrates that individuals can strategically misremember information in order to maintain the perception that the world is fair and just (Callan, Kay, Davidenko & Ellard, 2009).

A non-motivated alternative to the sampling account may reside in the self-anchoring phenomenon (e.g., Cadinu & Rothbart, 1996) or, relatedly, egocentric bias (e.g., Ross & Sicoly, 1979); people may employ knowledge of their own income in estimating the wider distribution, leading to relative overestimation of incomes similar to their own. Whether stemming from motivation or erroneous judgment processes, top-down biases of this kind can account for relationships between income, estimated social circle distributions, and estimated population distributions, and predict results similar to those obtained in Studies 1a and 1b.

These alternatives to the social sampling account all imply that systematic distortions in the subjective income distributions of wealthier and poorer individuals described in Chapter 2 result from subjective, top-down biases in the mind. In such accounts, cognitive or motivational biases exist in the mind prior to sampling, and serve to determine what information is sampled either internally (i.e., from memory) or externally (i.e., from the environment). For example, as implied by one common interpretation of confirmation bias (e.g., Jones & Sugden, 2001; Snyder & Swann, 1978), individuals that entertain a given hypothesis (e.g., “many Americans are wealthy”) may be biased toward sampling confirmatory information (e.g., wealthy Americans) from memory or environment. The sampling account, on the other hand, suggests that distortion results from interaction between informational biases already apparent in the environment itself and the sampling processes that people employ; biases often reside in samples prior to any distortion occurring in the mind. Hence the sampling account entails that biases in judgement result not from sub-optimally rational processes (e.g., heuristics, motivational influences), but from biased samples that provide sub-optimal representational input for generally rational cognitive processes. Although the resulting outcome, that is, biased or inaccurate judgement, is the same

irrespective of whether biasing processes or biased samples are diagnosed as the cause, the distinction is not trivial because either would require a different treatment.

The findings reported in Chapter 2 imply that, as a result of sampling processes, living standards across society as a whole appear more favourable from the perspective of increasingly wealthy individuals. Prior research and theory suggests that such increased perceptions of efficiency will reduce support for efforts aimed at reducing inequality - people become less concerned with inequality as efficiency increases, a phenomenon termed the equality-efficiency trade-off (Mitchell et al., 1993; Okun, 1975; Rawls, 1971; Scott et al., 2001). Presumably, sampling processes promote anti-egalitarian political attitudes amongst the wealthy in part because higher (perceived) efficiency entails aggregate material benefits for society as a whole, as eschewed for example in the doctrine of “trickle down” economics. In this manner, social sampling may lead wealthier people to perceive that the economic organisation of society is relatively fairer than would be the case given accurate perception, which in turn serves to justify anti-egalitarian political attitudes. Correspondingly, interventions that seek to increase support for redistributive efforts, for example by increasing empathy for the poor or changing political attitudes, may meet with limited success since chronic and pervasive sampling processes militate against such efforts via distorted perceptions.

In considering the role of sampling processes in judgement biases, some researchers have employed a metaphor of humans as “naïve” intuitive statisticians (Fiedler & Juslin, 2006; Juslin, Winman & Hanson, 2007). To quote Fiedler and Juslin (2006), this metaphor captures the notion that:

“[...] the processes operating on the given input information in general provide accurate descriptions of the samples and, as such, are not violating normative principles of logic and reasoning. Erroneous

judgements rather arise from the naiveté with which the mind takes the information input for granted, failing to correct for selectivity and constraints imposed on input, reflecting both environmental structures and strategies of sampling.” [p. 4]

As discussed in Chapter 1, there is an extensive body of evidence to suggest that people possess an ability to automatically monitor and store natural frequencies and provide reasonably accurate frequency judgements when required to do so (Gigerenzer & Murray, 1989; Hasher & Zacks, 1979; Lindskog, Winman & Juslin, 2013; Zacks & Hasher, 2002;). Hence, it seems a reasonable assumption that individuals are able to encode and reproduce, with some degree of accuracy, information concerning the distribution of incomes or other indicators of social status that are encountered in their day-to-day lives.

The notion of the intuitive statistician implies that, by and large, social sampling processes are rational, obey normative principles of reasoning and produce accurate descriptions of the samples encountered. Even if social samples of incomes are drawn from the environment in a relatively unbiased fashion (i.e., sampling *processes* are unbiased), individuals are arguably naive to the environmental constraints imposed on samples themselves (Fiedler, 2000; Juslin, Winman & Hanson, 2007). As discussed in the prior chapters, social environments are structured such that similar individuals (e.g. relatively poor or wealthy individuals) tend to live close together and move in similar social circles; that is, social networks have a tendency toward homophily (McPherson, Smith-Lovin & Cook, 2001). Social samples of incomes are thus non-random, varying systematically as a function of a person’s own position in the income distribution (Cruces et al., 2013; Galesic et al., 2012).

Further, as noted by Juslin et al. (2007), people are naive to the properties of sample statistics and tend to assume that the properties of samples accurately describe properties of populations – they evidence a belief in a “law of small numbers” (Tversky & Kahneman, 1971). For example, individuals appear to accurately judge sample variance but fail to correct for sample size in generalising to populations, producing relative underestimation of population variance (Kareev, Arnon & Horwitz-Zeliger, 2002). Similarly, individuals may underestimate the probability of rare events in the population (e.g., very wealthy individuals) because they do not appreciate that small samples are relatively less likely to include them (Hertwig, Barron, Weber & Erev, 2004). The systematic biases in population-level estimates observed in Studies 1a and 1b, then, potentially reflect a failure to correct (or a relative under-correction) for external biases in samples, in combination with naive assumptions about the statistical properties of samples in generalizing to populations.

The present chapter reports two further studies which seek to obtain experimental evidence for the notion that differences in available income samples impact upon fairness judgements and redistributive preferences independently of ideological or other top-down influences. Specifically, in Study 3a, US MTurk participants were presented with a series of incomes, ostensibly sampled representatively from the distribution of household incomes in the United Kingdom. Participants received either a low or high mean (i.e., low vs. high efficiency) distribution (inequality, i.e., the Gini index, was held constant). Similarly to Studies 1a and 1b, participants were then asked to estimate the population-level income distribution, judge the fairness of the distribution, and also the extent to which incomes should be redistributed. Study 3b was a direct replication of Study 3a, with the exception that participants were informed that incomes were sampled from the United States’ income distribution. In both Studies 3a and 3b, it was predicted that participants

would base subsequent population estimates upon the sample presented during the study. Hence participants exposed to a high (vs. low) mean sample were expected to estimate more efficient (i.e., higher mean) population income distributions. This, in turn, was expected to influence redistributive preferences and fairness judgements, such that participants exposed to a high mean sample would perceive the distribution to be relatively fairer, and would be less inclined to redistribute income.

3.2. Study 3a Method

Participants

US participants were recruited online ($N = 203$, 50% male; $M_{\text{age}} = 35.12$ years; $SD_{\text{age}} = 12.04$) via Amazon's Mechanical Turk (MTurk; Buhrmester, Kwang, & Gosling, 2011) for a survey entitled "Estimating Social Distributions". In keeping with previous investigations of the representativeness of MTurk samples (Paolacci, Chandler & Ipeirotis, 2010), the incomes of the present sample tended to be somewhat lower, but similarly distributed, to the US population as a whole (based on estimates from the US Census Bureau, 2013). Thus, 8.12% of the sample reported household incomes placing them in the wealthiest 20% of the US population, and 15.08%, 25.1%, 27.6% and 24.1% reported household incomes in the 2nd, 3rd, 4th, and 5th wealthiest quintiles, respectively. Sample size was determined a priori based on budgetary considerations. For both studies reported herein, ethical approval was obtained from the institutional Ethics Committee, and the research was conducted in full accordance with British Psychological Society (BPS) ethical guidelines.

Materials

Distributions Stimuli

Two income samples were created, a low-wealth sample with a mean income of £41,000, and a high-wealth sample with a mean income of £72,000. These mean income values were selected to be approximately one standard deviation below/above the mean of the Study 1a (within SS) derived mean incomes calculated from participants' estimated population-level income distributions ($M = \$58,605$; $SD = \$17,231$)^{3.1}. Firstly, the low-mean sample was created by using a random number generator to produce a normal distribution of 100 values between 5000 and 200,000 with a mean of 41,000. A linear combination of weights (summing to zero) was then added to the distribution to produce positive skew (as is characteristic of income distributions) whilst holding the mean of the distribution at 41,000. An iterative process was used to solve for a further series of weights to further transform the low-wealth sample, creating a new distribution with a mean of 72,000, thus forming the high-wealth sample. Values were weighted such that both the range (5,824 – 182,041) and Gini index (Gini = 35) was the same for both low and high wealth samples.

Measures

Similarly to Study 1a, participants estimated the complete distribution of annual household income across the entire UK population by indicating the percentage earning incomes within each of eight £20,000 intervals (£0 – £20,000; £20,000 - £40,000... £140,000+), using a click-bar histogram. The final interval was open-ended (all incomes of \$£140,000 upward). Participants were also asked to directly estimate the mean UK household income (using single a click-bar ranging from £100 - £100,000).

Footnote 3.1

Due to an error in creating the distributions, the mean of the HMD is somewhat lower than +1SD from the Study 1a explicit population mean income estimate (72,000 vs. 75,000)

Two items assessing perceived inequality were included, specifically, “To what extent are household incomes equally – unequally distributed across the population of the UK” (1 = *Very Equally*; 6 = *Very Unequally*) and “To what extent is the difference in income between the poorest and wealthiest households in the UK small – large” (1 = *Very Small*; 6 = *Very Large*). The correlation between these two items ($r = .33$) was too low to warrant combining them into a single measure and hence these items were examined separately in reported analyses. Perceived fairness of the UK household income distribution was assessed using the same two fairness/satisfaction items ($r = .79$) used in Studies 1a and 1b (e.g., “To what extent do you feel that household incomes are fairly-unfairly distributed across the UK population”; 1 = *Extremely Fair*; 6 = *Extremely Unfair*). Attitudes toward redistribution were assessed using the same 4-item Gallup Poll scale ($\alpha = .75$) used in Studies 1a and 1b (e.g., “The UK government should redistribute wealth through taxes on the rich”; 1 = *Strongly Disagree*; 6 = *Strongly Agree*). Perceived self-interest in redistribution was measured using the same three-item scale ($\alpha = .79$) used in Study 1b (e.g., “To what extent do you feel that redistribution of wealth through tax and welfare is in agreement with your own financial interests”; 1 = *Strongly Disagree*; 6 = *Strongly Agree*). Political attitudes were assessed with the same three-item scale ($\alpha = .94$) used in Study 1b (“How would you describe your political attitudes”; 1 = *Very Liberal/Very Left-Wing/Strong Democrat*; 7 = *Very Conservative/Very Right-Wing/Strong Republican*). In a final section, participants provided basic demographic information (gender, age, education, household income), indicated whether or not they were born in the US, and if not, how long they had been resident in the US.

Procedure

At the very beginning of the survey, which was presented via Qualtrics survey software, participants were instructed that “[...] we are interested in peoples' accuracy in estimating how various attributes are distributed across a wider population on the basis of a representative subsample of the distribution. You will be shown a series of either IQ scores, reaction times or household incomes that are representative of the underlying population distribution from which they were drawn. You will then be asked to estimate what proportion of people fall in each of several consecutive intervals”. This instruction served to facilitate the cover story that the study was concerned with estimating distributions in general and not specifically income distributions (all participants saw an income distribution only). The purpose of this cover story was to reduce any suspicion that the study was interested in political attitudes and their relationship to perceived income distributions. On the following screen, participants were given a definition of household income (as per Studies 1a and 1b) and were informed that they would view a series of household incomes that were probabilistically sampled (a simple definition was provided) from UK census data, and were hence representative of the true UK household income distribution. Participants were further instructed that they were not expected to memorise individual incomes but to “try to get a sense of how they are distributed”. Participants were then randomly assigned to either the high or low wealth sample level and viewed a “slide show” presenting each of the 100 incomes, one income per page. Each income appeared on the screen for 2 seconds before the page automatically advanced. Both high and low wealth series were presented in a fixed-random order, such that incomes within each consecutive quartile (i.e., each consecutive 25 incomes) were fully-randomised across participants, but all participants viewed quartiles in increasing order (i.e., lowest through highest income quartile). Participants then completed the dependent measures in the order they are described above and were fully debriefed as to the true aims of the study.

3.3. Study 3a Results

Weighted-mean incomes and Gini indices of estimated income distributions were calculated using the same procedure used in Study 1a. Descriptive statistics and correlations appear in the upper panel of Table 3.1, and Figure 3.1 graphs mean estimated distributions for both the low and high sample wealth conditions against the values actually presented in either condition.

Firstly, independent t-tests were conducted to examine the effect of sample wealth level on the key dependent measures. As can be seen in the upper panel of Table 3.1, the sample wealth manipulation exerted a clear impact upon participants' population estimates; both derived and directly estimated means were significantly higher in the high versus low mean sample condition. Unexpectedly, Gini indices were also found to differ between sample wealth conditions; participants in the high sample wealth condition estimated less unequal distributions. Overtly measured perceptions of inequality did not differ between the low and high sample wealth conditions. Contrary to expectations, perceived fairness and support for redistribution did not differ significantly between the high and low sample wealth conditions.

Subsequent analyses sought to examine whether sample wealth level exerted any indirect effect on redistributive preferences sequentially via derived/estimated population mean incomes (Gini's/estimated inequality) and subsequently fairness. Similarly to Studies 1a and 1b, bootstrapped mediation analyses (10,000 resamples) examined the indirect effect of sample wealth level on redistributive preferences via the proposed mediators using the PROCESS macro for SPSS (model 6; see Hayes, 2012, 2013). Political preference and perceived self-interest in redistribution were included as covariates in all models. Due to the scaling of the incomes measures, all analyses were conducted on standardised data.

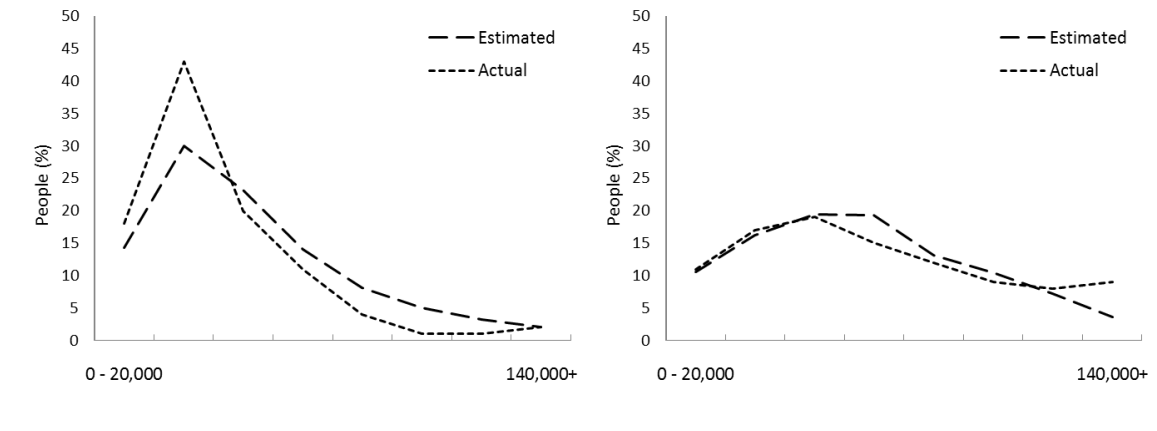
Table 3.1. Descriptive statistics and intercorrelations of the Study 3a (upper panel) and Study 3b (lower panel) variables.

Measures	Distribution Manipulation				Intercorrelations							
	Low Distribution	High Distribution	<i>t</i> (df)	<i>d</i>	1	2	3	4	5	6	7	8
Study 3a												
1. Derived mean income	£53,621 (11,306)	£69,104 (12,592)	9.05 (194)***	1.29	-							
2. Derived Gini index	32.90 (4.87)	31.49 (4.32)	2.13 (194)*	0.31	-.25**	-						
3. Estimated mean income	£39,730 (10,598)	£53,686 (13,649)	7.93 (192)***	1.14	.55***	-.39***	-					
4. Estimated inequality	4.20 (1.11)	4.16 (1.06)	.22 (194)	0.04	-.08	.17*	-.08	-				
5. Fairness/Satisfaction	3.22 (0.93)	3.23 (1.07)	.08 (194)	0.01	.07	-.19*	.11	-.44***	-			
6. Support for redistribution	3.93 (0.98)	3.83 (1.04)	.74 (194)	0.10	-.15*	.01	-.10	.21**	-.49***	-		
7. Household income	£55,388 (42,795)				.05	.02	.15*	-.001	.02	-.25***	-	
8. Political ideology	3.61 (1.36)				-.02	.23**	-.06	-.13	.19*	-.54***	.18*	-
9. Self-interest in redistribution	3.48 (1.08)				-.02	-.004	.001	-.02	-.16*	.44***	-.37***	-.36***
Study 3b												
1. Derived mean income	\$51,499 (11,375)	\$61,583 (10,441)	6.15 (175)***	0.92	-							
2. Derived Gini index	34.55 (4.68)	34.93 (5.17)	.51 (175)	0.00	-.06	-						
3. Estimated mean income	\$38,136 (12,036)	\$45,859 (10,219)	4.55 (171)***	0.69	.47***	-.27***	-					
4. Estimated inequality	4.78 (1.06)	4.60 (1.22)	1.10 (175)	0.16	-.19**	.16*	-.11	-				
5. Fairness/Satisfaction	2.63 (1.14)	2.69 (1.05)	.40 (175)	0.05	.15*	-.03	.08	-.38***	-			
6. Support for redistribution	4.26 (1.18)	4.17 (1.12)	.49 (175)	0.08	-.09	-.02	-.08	.22**	-.76***	-		
7. Household income	\$48,455 (\$59,786)				.17*	-.06	.05	-.06	.19*	-.12	-	
8. Political ideology	3.45 (1.48)				.08	.06	.05	-.08	.48***	-.54***	.05	-
9. Self-interest in redistribution	3.66 (1.01)				.04	-.01	-.07	.17*	-.38***	.45***	-.28***	-.31***

Note. Higher values indicate more of each construct (e.g., higher inequality). Higher values for political ideology indicate greater conservatism. Standard deviations are presented in parentheses. Statistics for income, ideology and self-interest are collapsed across conditions

* $p < .05$, ** $p < .01$, *** $p < .001$

Figure 3.1. Study 3a mean estimated population-level distributions in the low (left) and high (right) mean conditions against low and high mean distributions actually presented.

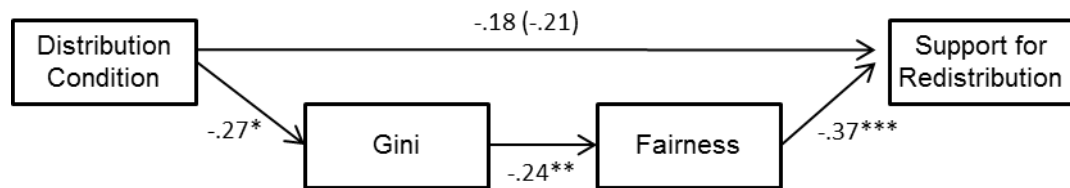


Study 3a Mediation Analyses

Contrary to expectations, sample wealth level was not found to exert any indirect effect on redistributive preferences via derived mean income and fairness, either with the relevant covariates included (BCa CI's of $-.12$ and $.04$, indirect effect = $-.03$), or without covariates (BCa CI's of $-.17$ and $.04$, indirect effect = $-.05$). No other indirect effects attained significance. Sample wealth level, however, was found to exert an indirect effect on redistributive preferences sequentially via Gini indices and fairness, both with the relevant covariates included in the model (BCa CI's of $-.07$ and $-.003$, indirect effect = $-.02$), and without covariates (BCa CI's of $-.09$ and $-.004$, indirect effect = $-.03$). No other indirect effects attained significance. As shown in Figure 3.2 participants in the high sample wealth condition estimated less unequal population income distributions, perceived the distribution to be fairer, and were consequently less supportive of redistributive efforts.

The above analyses were repeated for direct mean income and inequality estimates. Sample wealth level was not found to exert any indirect effect on

Figure 3.2. Study 3a indirect effect of distribution manipulation on support for redistribution via population Gini index and fairness. The total effect is given in parentheses.



redistributive preferences via directly estimated mean income and fairness, either with the relevant covariates included (BCa CI's of $-.14$ and $.002$, indirect effect = $-.06$), or without covariates (BCa CI's of $-.18$ and $.004$, indirect effect = $-.07$). No other indirect effects attained significance. Furthermore, sample wealth level was not found to exert any indirect effect on redistributive preferences sequentially via directly estimated inequality and fairness, both with the relevant covariates (i.e., political attitudes and self-interest in redistribution) included in the model (BCa CI's of $-.06$ and $.04$, indirect effect = $-.01$), or without covariates (BCa CI's of $-.07$ and $.06$, indirect effect = $-.01$). No other indirect effects attained significance.

3.4. Study 3a Discussion

As can be seen in Figure 3.1, mean estimated income distributions reasonably approximate the underlying shape of the low or high wealth samples that were presented in the learning task. Although participants in the high sample wealth condition estimated significantly less unequal distributions (recall that range and Gini were in fact held constant), overtly measured perceptions of inequality did not differ between conditions; participants in the high sample wealth condition estimated, but did not apparently perceive (quite correctly), lesser inequality. The sample wealth manipulation was, however, clearly successful; participants in the high sample wealth condition both

estimated significantly more efficient distributions and provided higher point-estimates of mean income, although, contrary to expectations, the manipulation did not translate into between-condition differences in either fairness or support for redistribution. Further, no indirect effect of the sample wealth manipulation on these attitudinal measures was observed via either derived or directly estimated mean incomes. Although the manipulation did not impact upon the attitudinal measures, either directly or indirectly, via levels of efficiency, the mediation analyses suggest that it did so indirectly via Gini indices; participants in the high sample wealth condition estimated (but did not overtly perceive) less unequal distributions, in turn leading to greater perceptions of fairness and, consequently, reduced support for redistribution. In contrast to the Study 1a and 1b findings, then, the redistributive concerns of participants in the present study were not sensitive to the efficiency of the income distribution, but were sensitive to inequality.

In hindsight, it seems possible that the sample wealth manipulation failed to directly impact upon fairness and redistributive concerns because US participants were presented, ostensibly, with a sample of UK incomes. Potentially, participants were not sufficiently invested in the task insofar as they are not subject to the distribution presented, or the information provided was too ambiguous to directly affect the attitudinal measures. For example, US participants may have difficulty in relating values of UK incomes to absolute standards of living, in which case efficiency is arguably an irrelevant criterion upon which to base fairness judgements (this issue is discussed more fully in the General Discussion section). We sought to investigate this possibility in Study 3b by instead presenting US participants with an ostensible sample of US incomes.

3.5. Study 3b Method

Participants

US participants were recruited online ($N = 178$, 51% male; $M_{\text{age}} = 34.97$ years; $SD_{\text{age}} = 12.92$) via Amazon's Mechanical Turk (MTurk; Buhrmester, Kwang, & Gosling, 2011) for a survey entitled "Estimating Social Distributions"^{3.2}. Similarly to Studies 1a, 1b and 3a, and previous investigations of the representativeness of MTurk samples, (Paolacci et al., 2010), the incomes of the present sample were lower, but similarly distributed, to the US population as a whole (based on estimates from the US Census Bureau, 2013). Thus, 5.65% of the sample reported household incomes placing them in the wealthiest 20% of the US population, and 9.04%, 22.03%, 26.55% and 36.72% reported household incomes in the 2nd, 3rd, 4th, and 5th wealthiest quintiles respectively.

Materials and Procedure

Study 3b was a full replication of Study 3a, and the design, materials (incomes samples and measures) and procedure were identical, with one minor exception – whereas in Study 3a participants were informed that the income series shown were probabilistically sampled from the true UK household income distribution, in Study 2b, participants were informed that incomes were sampled from the true US household income distribution. All instructions and measures were adapted appropriately, (e.g., \$ signs replaced £ signs).

Footnote 3.2

Although 200 participants were requested, due to a technical error, payment for 22 HITs was claimed without completion of the survey and hence the sample size was smaller than intended.

3.6. Study 3b Results

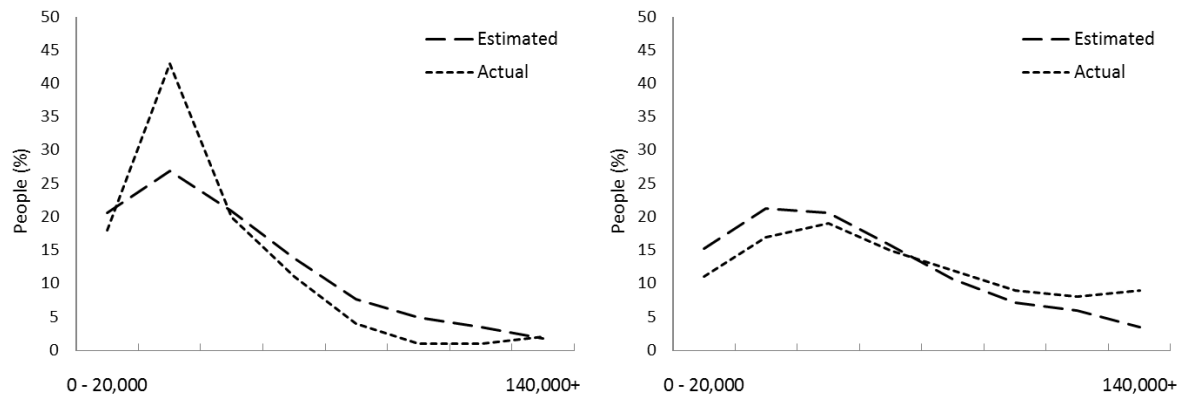
Weighted-mean incomes and Gini indices of estimated income distributions were calculated using the same procedure used in Studies 1a and 3a. Descriptive statistics and correlations appear in the lower panel of Table 3.1, and Figure 3.3 graphs mean estimated distributions for both the low and high sample wealth conditions against the values actually presented in either condition. Similarly to Study 3a, independent t-tests were conducted to examine the effect of sample wealth level on the key dependent measures. As can be seen in the lower panel of Table 3.1, the manipulation was again successful; derived and directly estimated means were significantly higher in the high sample wealth condition. In a departure from the Study 3a results, Gini indices did not differ between conditions. Consistent with the Study 3a findings, and contrary to expectations, however, perceived fairness and support for redistribution were not significantly different across the high and low sample wealth conditions.

The same bootstrapped mediation analyses employed in Study 3a were then repeated for the Study 3b data (10,000 resamples). Specifically, these analyses examined whether sample wealth level exerted an indirect effect on redistributive preferences sequentially via derived/estimated population mean incomes (Gini's/estimated inequality) and subsequently fairness. Political preference and perceived self-interest in redistribution were included as covariates. As previously, all data were standardised prior to analysis.

Study 3b Mediation Analyses

In a departure from the Study 3a findings, and as predicted, sample wealth level was found to exert an indirect effect on redistributive preferences via derived mean income and fairness, both with the relevant covariates (political attitudes, self-interest in

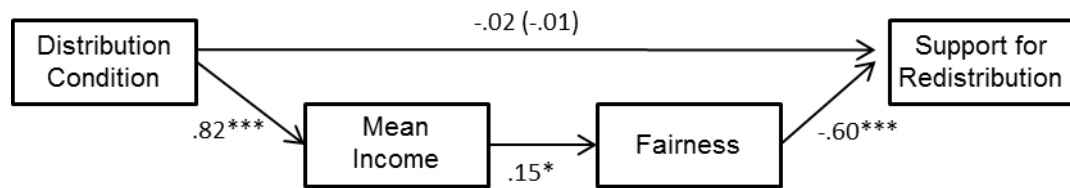
Figure 3.3. Study 3b mean estimated population-level distributions in the low (left) and high (right) mean conditions against low and high mean distributions actually presented.



redistribution) included in the model (BCa CI's of $-.18$ and $-.01$, indirect effect = $-.08$), and without covariates (BCa CI's of $-.23$ and $-.003$, indirect effect = $-.10$). As shown in Figure 3.4 participants in the high sample wealth condition estimated wealthier population income distributions, perceived the distribution to be fairer, and were consequently less supportive of redistributive efforts. No other indirect effects attained significance. Contrary to Study 3a, sample wealth level was not found to exert an indirect effect on redistributive preferences via Gini indices and fairness, either with the relevant covariates (political attitudes and self-interest in redistribution) included in the model (BCa CI's of $-.01$ and $.03$, indirect effect = $.003$), or without covariates (BCa CI's of $-.01$ and $.04$, indirect effect = $.002$).

The above analyses were then repeated for direct mean income and inequality estimates. Sample wealth level was not found to exert any indirect effect on redistributive preferences via directly estimated mean income and fairness, either with the relevant covariates included (BCa CI's of $-.09$ and $.05$, indirect effect = $-.02$), or without covariates (BCa CI's of $-.13$ and $.05$, indirect effect = $-.04$). No other indirect effects attained significance. Furthermore, sample wealth level was not found to exert

Figure 3.4. Study 3a indirect effect of distribution manipulation on support for redistribution via derived mean-population income and fairness. The total effect is given in parentheses.



any indirect effect on redistributive preferences sequentially via directly estimated inequality and fairness, both with the relevant covariates (i.e., political attitudes and self-interest in redistribution) included in the model (BCa CI's of $-.10$ and $.02$, indirect effect = $-.04$), or without covariates (BCa CI's of $-.16$ and $.03$, indirect effect = $-.06$). No other indirect effects attained significance.

3.7. Study 3b Discussion

As was observed in Study 3a, mean estimated income distributions closely approximate the underlying shape of the income samples with which participants were presented prior to the estimation task (see Figure 3.3). Similarly to Study 3a, and in line with predictions, participants population estimates reflected the experimentally presented samples; as can be seen in the lower panel of Table 3.1, participants in the high sample wealth condition estimated significantly more efficient distributions and gave higher point-estimates of mean income. In a departure from the Study 3a findings, participants (quite correctly) both estimated and perceived similar levels of inequality in either condition; neither Gini indices nor directly measured perceptions of inequality were found to differ between sample wealth conditions. As in Study 3a, and contrary to our hypotheses, the manipulation was observed to have no direct effect upon the attitudinal measures; both fairness and support for redistribution were the same across the low and

high sample wealth conditions. Hence, the manipulation potentially did not fail to impact directly upon the attitudinal measures in Study 3a due to the dissociation between our US MTurk sample and the stimuli, ostensibly a sample of UK incomes.

Although the sample wealth manipulation did not translate directly into between-condition differences in either fairness or redistributive preferences, the mediation analyses indicate that the manipulation did exert an indirect effect on redistributive concerns via derived mean incomes and fairness. Echoing the correlational findings of Studies 1a and 1b, participants in the high sample wealth condition estimated wealthier population income distributions, perceived the distribution to be fairer and were consequently less supportive of redistributive efforts. In a departure from the Study 3a findings, the redistributive concerns of participants in Study 3b were not sensitive to levels of inequality in estimated distributions, but were sensitive, indirectly, to efficiency.

3.8. General Discussion

Contrary to expectations, manipulating efficiency via the presentation of low versus high wealth income samples did not impact directly upon the perceived fairness of the distribution, or support for redistribution. Although, in both Studies 3a and 3b, the sample wealth manipulation was clearly successful (participants in the high mean condition estimated higher mean income distributions and provided higher point-estimates of mean income), this did not translate into between-condition differences in either fairness or support for redistribution. Both Studies 3a and 3b, however, provide clear support for the proposal that people inductively extrapolate from small samples in estimating wider population distributions, as the social sampling model broadly suggests.

The correlational results from Studies 1a and 1b support the notion that fairness and redistributive concerns are sensitive to efficiency, such that higher perceived efficiency leads to greater perceptions of fairness and reduced support for redistributive measures. Studies 3a and 3b provide some support for this finding. Although manipulating efficiency did not directly affect fairness or support for redistribution in Studies 3a or 3b, in Study 3b, the efficiency manipulation did indirectly influence support for redistribution in a manner consonant with the models reported in Chapter 2. In Study 3b, the high wealth sample was associated, sequentially, with more efficient population estimates, greater perceived fairness and reduced support for redistribution, thus partially replicating the Study 1a and 1b models.

It is important to consider certain qualitative differences between social samples and the experimentally presented samples used in the present studies. These differences potentially render the experimental stimuli relatively less relevant to judgments of fairness and redistribution than social samples, and perhaps explain why stronger effects of the sample wealth manipulation on economic attitudes were not observed. For example, basic differences presumably exist between the memory structures utilised in the encoding and recall of information regarding social contacts compared to the experimental stimuli. A fuller discussion of memorial processes is beyond the scope of this chapter, however, the distinction between episodic and semantic memory is of basic relevance here (Tulving, 1972). Whereas the experimental stimuli likely depend exclusively upon episodic memory (i.e., explicit memory for specific, experienced events), memory for social contacts presumably involves semantic memorial processes also (i.e., knowledge of facts, meaning, concepts and associations). To elucidate this point, consider the difference between knowledge of the form “person X earns income Y” (an episodic memory) versus knowledge of the form “social contact X is poor and earns income Y” (episodic and semantic). Income *values* in and of themselves are

single, episodic items of knowledge, whereas poverty or wealth are semantically rich and socially significant concepts.

Measured social circle income distributions presumably capture not only episodic knowledge of how incomes values themselves are distributed in the social environment, but also chronic, meaningful experience of how others life circumstances and material living standards are related to incomes. Experimentally presented income samples, on the other hand, are presumably not semantically integrated in this manner, and may consequently fail to provoke a similar reaction. Relatedly, research shows that aggregate representations of groups evoke less emotionally charged responses than individual cases, a phenomenon termed the identifiable victim effect (Jenni & Lowenstein, 1997; Small & Lowenstein, 2003).

It seems unlikely that people are aware of the true values of the incomes of all but a few of their closest social contacts, insofar as such information is generally confidential and conversational norms proscribe discussing salaries (Edwards, 2005; Littman, 2001). Indeed, many US companies make use of “no-disclosure” contracts which expressly forbid employees from discussing salaries with their co-workers. Accordingly, values of incomes per se are unlikely to be sampled directly, but may instead be inferred post-hoc during estimation on the basis of known, salient cues to social status such as a person’s job, the neighbourhood they live in, the clothes they wear and various other indicators of material wealth and consumption (Belk, 1981). This echoes Brunswik’s (1952) lens model of perception, in which indirectly experienced, distal properties (e.g., a given persons’ income) are inferred on the basis of salient proximal cues (e.g., consumption behaviour or other cues to social status) to which they are probabilistically related. Through experience, individuals presumably

learn which cues have the greatest “ecological validity” as predictors of income, and also how different cues are related to different levels of income.

Research suggests that people are attuned to social status (e.g., Dalmaso, Pavan, Castelli & Galfano, 2011; Foulsham, Cheng, Tracy, Henrich & Kingstone, 2010) and that wealth is an important dimension upon which people engage in social comparisons (Clarke & Oswald, 1996; Clarke & Senik, 2010; Hagerty, 2000). Furthermore, people can provide relatively accurate descriptions of social distributions that are of personal or social significance to the self, for example because they reflect important dimensions for social comparison or help guide behaviour in unfamiliar situations (Galesic et al., 2012; Nisbett & Kunda, 1985). It seems likely that people possess relatively accurate knowledge regarding the distribution of status-related cues in their day-to-day environment, even if incomes themselves are not directly known. Hence, although individuals may not directly know the distribution of incomes across their social contacts, they may possess fairly accurate knowledge of the relevant proximal cues (e.g., social contacts’ job, appearance, possessions, lifestyle, neighborhood of residence, preferences) from which the income distribution may subsequently be inferred.

In order to arrive at an estimate of the income distribution across their social contacts, participants must necessarily consider the distribution of these overt cues, which in turn carry information about others material living standards and wellbeing. These cues in and of themselves could potentially be more relevant to considerations of fairness than the income values derived from them. Arguably, money in and of itself has no intrinsic value - rather, it is the satisfaction of needs and wants for which money allows (or, conversely, for which lack of money prevents), or “utility” in economic phraseology, that is of value. As such, it may not be the perceived distribution of income values per se that influences judgements of fairness and the proclivity to

redistribute income, but the rather more semantically rich, vivid and emotionally arousing content that underpins perceptions of the distribution – peoples' everyday experience of the affordances or constraints upon others living standards and wellbeing facilitated by different levels of income. Requiring participants to estimate an experimentally presented distribution of income values, however, does not necessitate any consideration of cues pertaining to standards of living that different levels of income allow for – income values can instead be drawn directly from memory.

A similar explanation may also underlie the divergence between the Study 3a and Study 3b findings in terms of the effects of equality (Gini) and efficiency (mean incomes); in Study 3a, participants' redistributive preferences showed, indirectly, some sensitivity to levels of inequality in estimated distributions, but were not sensitive to efficiency, and vice-versa in Study 3b. Consider that, in Study 3a, US participants ostensibly made judgements about the UK household income distribution, whereas in Study 3b, US participants made judgements about the US household income distribution (the absolute £ and \$ values presented were the same). Presumably, the majority of our US Study 3a participants possess limited experience with British currency and the cost of goods and services in the UK. Hence they are likely unable to associate different absolute living standards with different levels of income, or experience difficulty in doing so, insofar as they have no experience upon which to base their inferences. What constitutes a "low" or "high" income in a specific time and place is not determined by the absolute value of income itself, but by what income affords in a given geographical and temporal context. Where the relationship between monetary values and absolute living standards is ambiguous, efficiency might be considered irrelevant to judgements of fairness, and consequently redistributive concerns, because it carries no information about living standards across society. Inequality, on the other hand, is both a relative and normative construct, and monetary values need bear no

obvious relation to material living standards in order for inequality to be employed in judgements of distributive justice. Large inequalities may potentially be perceived as unfair irrespective of the relation between incomes and absolute living standards.

Indeed, where the relation between living standards and incomes is ambiguous, inequality in incomes provides the only criterion available upon which to base fairness judgements.

In some regards, the judgements of participants in Study 3a perhaps echo's Rawls' (1971) suggestion that participants behind a "veil of ignorance" (i.e., a hypothetical situation prior to random assignment of one's own social status) would employ a *maximin* principle, meaning they would seek to maximise the incomes of the poorest in society. These participants are in a similar situation insofar as they are a disinterested party (i.e., they are not subject to the distribution they are judging), and are perhaps uncertain as to the relation between incomes and absolute living standards in the sample presented. Irrespective of the relation between absolute living standards and income values, increments in equality entail perceived improvements in the fortunes of the least well-off, akin to Rawl's maximin principle. Increments in equality thus potentially hedge fairness concerns against the mere possibility that low incomes are associated with low absolute living standards.

Recall that the instructions provided to participants in both Studies 3a and 3b framed the task in terms of accuracy. Specifically, participants were informed that the aim of the study was to assess the accuracy of estimates relative to the true population-level distribution. Prior research on accuracy goals suggests ways in which this framing may have influenced participants' judgments (Chaiken, Giner-Sorolla & Chen, 1996; Chaiken, Liberman & Eagly, 1989; Chen, Schechter & Chaiken, 1996). It has been argued that accuracy-motivated judgment is characterised by impartiality and a

preference for objective, unambiguous information (Chaiken, 1980; Chen et al., 1996; Festinger, 1954), and promotes effortful, systematic (versus, effortless, heuristic) processing of stimuli (Eagly & Chaiken, 1993; Chaiken et al., 1996). Indeed, accuracy motivation has been shown to reduce primacy, salience and priming effects in impression formation (Borgida & Howard-Pitney, 1983; Tetlock, 1983; Martin, Seta & Crelia, 1990). Hence, if it is assumed that the task framing motivated participants to prioritise accuracy (i.e., relative to the population distribution), it may have encouraged more effortful processing of, and greater reliance upon, the experimentally presented samples insofar as these were construed as providing a faithful and unbiased estimate of the population.

It cannot, however, be ascertained from the present data whether the framing of the task did effectively promote an accuracy goal, or to the extent that it did so, whether this goal in turn facilitated more effortful processing of the stimuli. As noted by Neberg (1989, p.384-385) “accuracy goals may be less effective when competing with other goals and tasks for limited cognitive and behavioral resources”. Where cognitive capacity is constrained, for example due to time pressure, high task demands or competing priorities, or where motivation is low, accuracy-motivated individuals may fall back upon less cognitively demanding strategies that are deemed suitable for the task at hand (Chen et al., 1996). Social sampling perhaps represents a likely fall-back strategy in the present context given that it relies on well-reinforced and elaborated knowledge, perhaps requires little effort and processing capacity, and potentially represents the default strategy by which inferences about populations are drawn. Indeed, the requirement to monitor no less than one-hundred discrete items (individual incomes), and subsequently make probability judgments of their relative occurrence, presumably placed strong demands on participants’ memory and processing capacity. As such, although the experimental manipulation clearly influenced participants’

estimates in the intended manner, there is reason to suspect that population estimates remain contaminated to some degree by social sampling, even if participants are assumed to have prioritised accuracy.

In sum, the results of Studies 3a and 3b provide somewhat ambivalent support for the notion that manipulating perceptions of efficiency by presenting new information can meaningfully impact upon economic attitudes. As expected, providing novel samples was indeed sufficient to influence perceptions of the population-level income distribution, in line with the samples presented; participants in the high mean condition estimated more efficient population distributions in both Studies 3a and 3b. This did not, however, translate directly into between-condition differences in fairness or support for redistribution, in either Study 3a or 3b. On the other hand, the indirect effect of the sample wealth manipulation observed in Study 3b shows that the manipulation did exert some effect upon redistributive preferences in the direction predicted. Insofar as experimentally presented incomes are, unlike social samples, detached from real-world experience and content, potentially they are not sufficiently evocative to exert strong effects on economic attitudes. Similarly, if exposure to samples encountered in day-to-day life shapes economic attitudes in an ongoing and implicit manner, these attitudes potentially become fixed and rigid over time, and hence relatively impervious to a single, one-off exposure to new information.

Chapter 4

4.1. Introduction

The initial findings described in Chapter 2 provide reasonable evidence that social sampling processes influence important economic attitudes via selective exposure to systematically biased income samples. Studies 1a – 2 show that wealthier, relative to poorer individuals, are exposed to wealthier social samples of incomes in their day-to-day lives, and consequently perceive that the population as a whole is relatively more affluent. In turn, this leads wealthier individuals to perceive the distribution as relatively more fair and in turn reduces support for redistributive measures.

Chapter 3 provides some experimental support for the notion that sampling plays a causal role in determining economic attitudes in accord with the theoretical model described in Chapter 2. In Studies 3a and 3b, experimentally presenting participants with novel income samples clearly influenced estimates of the population level income distribution in line with the samples provided. Across both studies, participants presented with a high (vs. low) wealth sample estimated higher mean distributions, and provided higher point-estimates of mean income. Although the sample wealth manipulation did not directly impact upon economic attitudes in either Study 3a or 3b, it did so indirectly via population estimates (via Gini indices in Study 3a and derived means in Study 3b), conceptually replicating the models described in Chapter 2.

Cumulatively, Studies 1a – 3b clearly show that people draw upon readily available samples in estimating unknown population distributions, and hence the parameters of population estimates depend upon the parameters of the samples relied upon. The social sampling model described by Galesic et al. (2012) argues that, by default, people rely upon their immediate social circles in such estimation tasks in an

automatic, heuristic fashion. The studies reported in Chapter 3, however, suggest an important caveat regarding the automaticity of social sampling: when an ostensibly reliable alternative to social samples is available, people may instead base their estimates upon this novel information. Social sampling, then, may not be inevitable or immutable, insofar as people are perhaps able to forgo social sampling where alternative information is available. However, it cannot be ascertained with certainty from Studies 3a and 3b whether, or to what extent, participants rejected social samples and relied upon the experimentally presented samples, because social sampling was not measured in these studies.

In the present chapter, the focus of attention is shifted away from the effect of sampling processes upon economic attitudes toward considerations surrounding the mutability of social sampling. Specifically, the present chapter seeks to examine whether people are able to exert deliberate control over sampling processes such that population estimates no longer depend upon social samples. To this end, Chapter 4 presents two further studies that seek to ascertain whether providing an alternative sample, explicitly instructing participants to avoid social sampling or both interventions in combination, can serve to reduce or eliminate social sampling effects.

Prior research paints a somewhat pessimistic picture regarding human's ability to monitor and exert control over cognitive processes, a capacity termed *metacognition*. Research shows, for example, that experts are only marginally less prone to failures in statistical reasoning than lay persons, and that awareness of prominent judgement biases does little to prevent people falling foul of them (Kahneman & Tversky, 1979, 1982; Tversky & Kahneman, 1973). Repeated information can influence preferences even when people are fully aware of the repetition, and hence redundancy, of diagnostic information (Unkelbach et al., 2007). Attempts at suppressing stereotypic judgments

often rebound such that subsequent judgments are more stereotypic than in the absence of attempts at suppression (Macrae, Bodenhausen, Milne & Jetten, 1994). Relatedly, research shows that people are naïve in respect to the external constraints imposed on information by the environment, and also in respect to the sophisticated properties of samples such as sample size, variance and conditionality (Fiedler, 2012; Fiedler & Juslin, 2006; Juslin, Winman & Hansson, 2007).

Correspondingly, previous research in the domain of judgement and decision making presents a somewhat mixed picture in regards to people's ability to overcome prevalent biases in judgement. Further, different paradigms in the literature imply different strategies for improving judgment processes and outcomes. Much literature within the "heuristics and biases" paradigm, associated strongly with the work of Amos Tversky and Daniel Kahneman, employs a dual process model to parsimoniously account for both biased and normatively sound judgment. Under this view, the application of fast, effortless and intuitive "System 1" processes (i.e., heuristics) is assumed to underlie systematic biases in judgement (e.g., Kahneman, 2011; Tversky & Kahneman, 1983). Errors are shown to be both pervasive and difficult to overcome, even amongst trained experts (Kahneman & Tversky, 1979). In the view of Kahneman and Tversky (1979), judgement biases are like visual illusions; "[...] both types of errors remain compellingly attractive even when the person is fully aware of their nature" (p. 1). Overcoming such myopia in judgement thus requires the conscious suppression of intuitive processes in favour of slow, effortful and analytical "System 2" processes, for example via eliciting a metacognitive experience of difficulty or disfluency (Kahneman, 2011; Kahneman and Tversky, 1973; Oppenheimer, Epley & Eyre, 2007).

The "fast and frugal heuristics" paradigm, as its name implies, also invokes heuristics as the primary mechanism of human judgement and decision making

(Gigerenzer and Todd, 1999; Gigerenzer & Goldstein, 1995). As discussed in the introduction, from this perspective, heuristics are not considered a normatively sub-optimal means of judgement, but are instead *ecologically rational*, evolved adaptations to the natural environment. From this perspective, the relative accuracy of judgement is to be understood as a function of the relationship between heuristics and task demands. Poor fit between the evolutionary design and purpose of a heuristic and the structure of a task or environment leads to biased judgement. Theorising in this perspective is perhaps more optimistic about the possibility of correcting biases in judgement than the heuristics and biases literature. Adapting the representational properties of judgement tasks (e.g., presenting problems in frequencies rather than probabilities; Gigerenzer & Hoffrage, 1995) or training people to select the most reliable and informative cues from the environment (e.g., via decision trees; Martignon, Vitouch, Takezawa & Forster, 2003) can increase accuracy without placing strong demands on cognitive capacity.

The sampling approach (e.g., Fiedler, 2000; Fiedler & Juslin, 2006), contrastingly, places the explanatory burden not on cognitive processes per se in attempting to explain judgement biases, but upon the information input recruited by cognitive processes. Whereas the heuristics and biases and fast and frugal paradigms tacitly assume that input information provides an objective representation of the world, the sampling approach emphasises that constraints upon samples, be they drawn externally (from the environment) or internally (from memory), are sufficient to explain many apparent biases in judgement. Constraints are potentially imposed upon samples not only by the environment itself, but also by cognitive limitations upon judge's attention, capacity or processing goals. As such, sampling biases and resultant errors in judgement reflect processes of *cognitive-ecological interaction* (Fiedler, 2000; Fiedler & Wänke, 2009); the interaction of human minds with environmental structures.

The information samples to which people are exposed in their day-to-day lives, for example, are often not random but conditional upon another variable, often the criterion variable (i.e., the variable under prediction) itself. Samples are often not random, but quota samples because their composition is a function of another variable. The findings of Galesic et al. (2012) demonstrate how, due to the tendency toward homophily in social networks, this is the case for various social attributes (e.g., income, work stress, number of friends, health problems, education). In the case of income, the social samples people encounter are conditional upon the criterion of income itself; wealthier and poorer individuals are disproportionately exposed, respectively, to other wealthier and poorer individuals. In this manner, the information people receive about the distribution of various attributes, such as income, is related to people's own standing on the attribute concerned.

The metaphor of humans as naïve intuitive statisticians entails that people are naïve to constraints upon samples such as conditionality and as such do not correct for them; they suffer from “meta-cognitive myopia” (Fiedler, 2000, 2012). In sum, under the sampling approach, biases in judgement emerge due to a) biases inherent in samples and sampling processes that serve to bias samples, and b) the absence of appropriate metacognitive correction for such biases. Correspondingly, the sampling approach implies two possible routes to reducing bias in judgement; reducing bias in samples themselves, or enhancing metacognitive control over sampling processes.

Research on the *base-rate neglect phenomenon* - the tendency to overlook population base-rates in judging the conditional probability of rare events - provides a neat illustration of how sampling processes can serve to bias judgement. In Kahneman and Tversky's (1972) classic demonstration of the base rate fallacy, for example, participants were more likely to judge a person to be an engineer rather than a lawyer

when the target person ostensibly spent their free time on mathematical puzzles, despite being told that the base-rate of engineers in the population was considerably lower (30%) than lawyers (70%). The authors attribute this finding to reliance upon a “representativeness” heuristic (e.g., an assumption that behaviour x is more typical of person y) in judgements of conditional probability, at the expense of neglecting population base rates.

Gigerenzer and Hoffrage (1995) presented participants with an analogous task in which participants’ goal was to judge the conditional probability (i.e., $P(A|B)$) that a woman has breast cancer given a positive mammogram. Participants were told that the hit rate (i.e., probability of cancer given a positive test) was 80%, the false-alarm rate (probability of no cancer given a positive test) was 9.6% and the base-rate of cancer in the given population was 1%. Applying Bayes’ theorem, the actual posterior probability of cancer given a positive mammogram is no greater than 7.8%, although participants typically report inflated estimates of $P(A|B)$ in this and other analogous tasks. People are apparently misled by high hit rates and consequently overlook low base rates. The research further revealed that adapting the representational properties of the task, by substituting probabilities for frequencies, lead to a substantial increase in accuracy. The authors construe this finding in terms of the evolutionary adaptation of judgment mechanisms to ecological conditions. It is suggested that humans are attuned to natural frequencies, and judgment mechanisms have evolved to utilise frequencies specifically since information is typically encountered in this form. Probability, on the other hand, is a relatively modern invention and does not preserve information in the form upon which human’s mental calculus has evolved to function. In contrast to Kahneman and Tversky’s (1972) interpretation, then, inaccuracies in judgments of conditional probability do not result from the use of intuitive, suboptimal judgment mechanisms (such as a representativeness heuristic). Rather, they occur due to a poor fit between

judgment mechanisms and the representational format of information (i.e., probabilities) typically used in problems of this kind.

Fiedler, Brinkmann, Betsch and Wilde (2000) examined the same and other analogous judgement problems, although the authors' primary concern was with the active processes involved in acquiring information, that is, the sampling procedures people utilise. Some participants were presented with index cards organised by predictor category (e.g., positive vs. negative mammogram), which allowed participants to learn whether cancer was or was not present. Other participants received index cards organised by the criterion category (e.g., women with vs. without breast cancer), which informed participants whether a mammogram was positive or negative. The base rate of cancer and proportions of hits and false positives were preserved across conditions and could be learned by scanning through the files. Participants' task in both conditions was to estimate $P(A|B)$ on the basis of samples drawn freely from the box of index cards, such that participants could select any number of cards from either category (i.e., cancer/no cancer or positive/negative mammogram).

The authors found that judgements closely followed the proportions in the samples drawn. As such, the accuracy of participants in judging $P(A|B)$ depended upon how accuracy was normatively defined – either in relation to the sample of cards drawn by the participant, or in relation to the entire population of cards. Across both conditions, participants were remarkably accurate when accuracy was defined in relation to the sample drawn (i.e., $P(A|B)$ calculated for the cards actually sampled). Predictor sampling, however, produced considerably more accurate judgements in relation to the total population of index cards. The authors attributed this finding to a pervasive tendency of participants in the criterion (i.e., cancer/no cancer) sampling condition to oversample the infrequent (i.e., cancer present) category, thus vastly

inflating the base-rate in the sample, and consequently $P(A|B)$. To illustrate, consider a situation in which a participant draws equal observations of cancer and no cancer cases. The inflation in the base rate of cancer raises $P(A|B)$ to 89% in the sample, multiplying the conditional probability of cancer given a positive test by more than a factor of ten relative to the entire population of cards.

Fiedler (2000) suggests that, given the information actually sampled is judged accurately, base-rate neglect is something of a misnomer; “Serious biases might occur despite, or exactly because, judges are quite sensitive to the (inflated) base-rate in the sample” (p. 665). People *are* sensitive to base rates, but are seemingly unaware of the constraints imposed by oversampling the infrequent event. In sum, these findings suggest that a crucial factor in determining the accuracy of judgments of this kind is the extent to which the sampling process preserves the correct proportions via judges own selection strategies. Biased judgement appears to result from naïve assumptions in generating the sample and a consequent metacognitive failure to account for the effects of selection strategy on the information acquired.

Although estimating the population distribution of incomes does not entail a judgement of conditional probability, accurate judgement does require similar metacognitive abilities and understanding of sampling constraints. As emphasised previously, individuals’ income samples (i.e., social circle income distributions) are related to their own income; they are conditional upon judge’s own standing on this very attribute (i.e., people are overexposed to incomes relatively closer to their own income). Accurate generalisation from social samples to the population hence requires, first and foremost, an appreciation that the available sample is biased in this specific manner, and secondly, a process to correct population estimates for biases in samples resulting from this conditionality.

Under these ideal conditions, participants might be expected to engage in a type of *theory-based correction* (Wegner & Petty, 1997; Wegner, Silva, Petty & Garcia-Marques, 2012). That is, people acknowledge and make attempts to adjust for biases in accordance with their understanding of the nature of the bias. Research does suggest that people can exert at least some degree of control over various automatic processes (Fiedler, 2012; Fiedler, Bluemke & Unkelbach, 2011). For example, providing deliberate instructions can serve to eliminate priming effects (Degner, 2009) and selective attention strategies can be employed to avoid unwanted thoughts (Wenzlaff & Bates, 2000) or to devalue unattended stimuli (Fenske & Raymond, 2006). Research also shows that individuals can strategically disregard sampled information in the service of motivational goals (Doosje, Spears & Koomen, 1995).

Indeed, correction processes may not be entirely absent in social sampling. Galesic et al. (2012) suggest that “smoothing” evident in estimated population distributions might reflect adjustment to account for the biasing effect of homophily upon social samples. To the extent that such correction efforts are employed, however, they are clearly insufficient or misguided given that estimated populations still depend strongly upon social samples. Nevertheless, people do seemingly possess some tools they are able to recruit in order to exert control over unwanted, biasing stimulus influences under certain circumstances, and it remains an open question as to whether such control can be successfully applied over social sampling. As was highlighted previously in relation to naiveties in sampling processes, people may possess the requisite tools, but fail to recognise the circumstances under which such tools need be employed, or they apply them inappropriately. This is strikingly apparent from research demonstrating that experts are often equally prone to such “metacognitive myopia” (Fiedler, 2012) as lay persons, in spite of considerable statistical training and expertise

(Fiedler, Brinkmann et al., 2000; Kahneman & Tversky, 1979, 1982; Tversky & Kahneman, 1973).

An alternative means of reducing bias in population estimates, of course, is to draw a new and truly random sample of observations upon which to base estimates. Realistically, however, this may be largely impossible in the case of social attributes, samples of which are presumably constructed sequentially and updated over time through ongoing experience and interaction with the social environment. In many judgement situations, it will be impossible to draw a new sample directly because the relevant information is simply not available for immediate inspection in the given context (e.g., when estimating distributions in a psychology study). Because human beings do not possess the infinite knowledge and perceptual powers of Laplace's (1841) demon, the entire universe of information is never available for full inspection.

On the other hand, although generating a new sample on-line may not be possible, a person might have access to formal summary statistics (e.g., census data on household incomes) or an alternative sample of observations (e.g., an experimentally presented sample of incomes) which accurately represents the population. Where people are aware of, and motivated to avoid bias, they might discount biased information and rely instead upon such an alternative source of information (Schwarz & Clore, 1983). As opposed to simply adjusting responses to account for perceived bias in social circles, people may instead engage in a process of *recomputation* (Strack & Mussweiler, 2001), setting aside biased information (e.g., social samples) and basing judgements instead on remaining alternative information.

To summarise thus far, in estimating the population distribution of incomes (or other attributes), people may have little choice but to draw upon social samples insofar as they are potentially the only source of relevant information available, at least in the

absence of secondary knowledge such as official statistics. Conceivably, however, warning participants against social sampling may prompt theory-based correction of population estimates, or motivate a search for alternative information upon which estimates might reliably be based (Schwarz & Clore, 1983; Strack & Mussweiler, 2001). Introducing awareness of the specific nature of the bias in social samples (i.e., incomes relatively closer to one's own are overrepresented) may facilitate correction efforts by providing some basis for determining the necessary direction and magnitude of correction processes. This is in line with research and theory suggesting that people rely on their lay beliefs regarding the specific nature of biasing influences in correction attempts (Brekke & Wilson, 1994; Petty & Wegener, 1993; Strack, 1992). To effectively correct for biasing stimulus influences on judgment, people require accurate knowledge of the direction and magnitude of bias concerned (Petty & Wegener, 1993; Wegner & Petty, 1997; Wegner, Silva, Petty & Garcia-Marques, 2012).

As discussed, however, simply raising awareness of the biased nature of social samples may have a limited impact if practical constraints force reliance upon social sampling. This is perhaps hinted at by the observation of Galesic et al. (2012) that the smoothing of population estimates may reflect correction attempts aimed at reducing the bias in population estimates resulting from homophily in social circles. If smoothing does indeed reflect deliberate correction, it is clearly either insufficient in magnitude, or otherwise misguided, insofar as population estimates remain strongly related to social samples. In short, to the extent that such theory based correction already occurs in social sampling, but is apparently of limited effectiveness, explicitly prompting similar theory-based correction may do little to reduce the dependency of population estimates upon social circles. As such, prompting avoidance of social sampling *and* additionally providing alternative information upon which to base estimates may provide the most effective means of reducing social sampling. Under these conditions, participants may

be expected to engage in attempts at recomputation, basing population estimates on the novel, ostensibly reliable information (Strack & Mussweiler, 2001).

Accordingly, Studies 4a and 4b sought to ascertain whether providing an alternative sample of incomes, warning against social sampling or both interventions in combination, can serve to reduce or ameliorate the relationship between own income and estimated population distributions via social circle distributions. This approach rested on two key assumptions. Firstly, it was assumed that although individuals might produce relatively accurate descriptions of their social samples, they are naïve to constraints upon these samples that impoverish their generality to the wider population (Fiedler, 2012, 2000). Raising awareness of the biased nature of social samples may provoke attempts to correct population-level inferences accordingly (Wegner & Petty, 1997; Wegner, Silva, Petty & Garcia-Marques, 2012), or motivate a search for alternative, unbiased information (Schwarz & Clore, 1983). To investigate this possibility, half of all participants in Studies 4a and 4b were presented with a textual prompt that warned participants about the homophilous nature of social samples and instructed participants to avoid social sampling.

Secondly, it was assumed that in many judgement situations, people may have no choice but to draw upon social samples insofar as they are the only information available upon which inferences of the population-level distribution can be based. Hence, in isolation, warning against social sampling might exert a limited impact because practical constraints force reliance upon social sampling. Thus it was predicted that providing both a prompt *and* an alternative sample should provide the most effective means of reducing social sampling effects, by promoting attempts at recomputation of the population distribution on the basis of novel, ostensibly reliable information (Strack & Mussweiler, 2001). To investigate this possibility, half of all

participants in participants in Study 4a, and all participants in Study 4b, were presented with an alternative sample of incomes from which estimates of the population distribution could be derived.

4.2. Study 4a Method

Participants

US participants were recruited online ($N = 403$, 41.7% male; $M_{\text{age}} = 38.07$ years; $SD_{\text{age}} = 12.14$) via Amazon's Mechanical Turk (MTurk; Buhrmester, Kwang, & Gosling, 2011) for a survey entitled "Estimating Social Distributions". Consistent with our prior data, the incomes of the present sample tended to be somewhat lower, but similarly distributed, to the US population as a whole (based on estimates from the US Census Bureau, 2013). Thus, 8.2% of the sample reported household incomes placing them in the wealthiest 20% of the US population, and 22.8%, 20.6%, 25.8% and 22.6% reported household incomes in the 2nd, 3rd, 4th, and 5th wealthiest quintiles respectively. Sample size was determined a priori based on budgetary considerations. For both studies reported herein, ethical approval was obtained from the institutional Ethics Committee, and the research was conducted in full accordance with British Psychological Society (BPS) ethical guidelines.

Design & Materials

Approximately a quarter of participants ($N = 97$) were presented with a low-mean sample of incomes ($M = \$41,000$), a further quarter ($N = 101$) were presented with a high-mean sample of incomes ($M = \$72,000$), and all remaining participants ($N = 205$) were assigned to a control condition in which they completed a filler task involving simple arithmetic problems. Participants were randomly assigned to these conditions. The presented income samples were the same as used in Studies 2a and 2b. Within each

alternative sample condition and the control condition, half of all participants received a passage of text describing the phenomenon of homophily and emphasising the unrepresentative nature of social. The text also explicitly instructed participants not to base their estimates upon their social circles. The remaining half of participants received no additional information. Although the study employed a 3 (high-mean sample, low-mean sample, no alternative sample) x 2 (homophily information vs. no additional information) conditions, it is treated in key analyses as a nested 2 (alternative low or high-mean sample vs. no sample) x 2 (homophily information vs. no homophily information) fully between-subjects design. A nested design was adopted in order to facilitate a check as to whether participants were sensitive to the (low or high wealth) samples presented, and whether such sensitivity is itself a function of the homophily instruction. The crucial theoretical issue, however, was whether, and to what extent, participants based their population estimates upon social circles, versus an alternative sample of any character where one was available. The mean level of the alternative samples was not important given the central hypothesis under examination, and hence a nested design was both sufficient for the purposes of the study and economical in terms of the number of participants required.

Measures

Similarly to prior studies, participants estimated the complete distribution of annual household income across both their social contacts and the US population by indicating the percentage earning incomes within each of nine \$20,000 intervals (\$0 – \$20,000; \$20,000 - \$40,000... \$140,000+), using a click-bar histogram. The final interval was open-ended (all incomes of \$160,000 upward). Participants were also asked to directly estimate the mean household income for both their social contacts and the US population (using a single click-bar ranging from \$100 - \$150,000). For both social

circles and the US population, two items assessing perceived inequality were included, specifically, “To what extent are household incomes equally – unequally distributed across **your social contacts (the US population)**” (1 = *Very Equally*; 6 = *Very Unequally*) and “To what extent is income inequality across **your social contacts (the US population)** low-high” (1 = *Very Low*; 6 = *Very High*). The correlations between these two items ($r = .31$ and $r = .42$ for items pertaining to social circles and the US population, respectively) was too low to warrant combining them into single measures, so they were examined separately in reported analyses. Participants also completed a five-item scale designed to measure the perceived representativeness (i.e., of incomes in the wider US population) of participant’s own social circles, for example “My social contacts’ household incomes are representative of household incomes in the US as a whole” and “With regard to household incomes, my social contacts are like a microcosm of the US as a whole” (1 = *Strongly Disagree*; 6 = *Strongly Agree*). The full five items did not form a reliable scale and as such only items 1-3 were averaged to form a composite measure of perceived representativeness ($\alpha = .87$). A complete list of new items (those not appearing in prior studies) appears in Appendix IV.

Procedure

As in Studies 3a and 3b, participants received the cover story that “we are interested in people’s accuracy in estimating how various attributes are distributed across a wider population on the basis of a representative subsample of the distribution”. In an opening section, participants provided basic demographic information, including their household income, and indicated whether or not they were born in the US, and if not, how long they had been resident in the country. Participants then estimated their social circle income distribution and completed the other measures pertaining to their social circles as described above (e.g., direct estimate of mean social circle income). Participants were

then assigned to the low mean distribution, high mean distribution or arithmetic filler task. In both the low and high sample wealth conditions, participants were provided a definition of household income and were informed that they would view a series of household incomes that were probabilistically sampled (a simple definition was provided) from US census data, as per the procedure in Studies 3a and 3b. Participants were further instructed that they were not expected to memorise individual incomes but to “try to get a sense of how they are distributed”. Participants then viewed a slide show presenting each of the 100 incomes, one income per page, each displayed for 2 seconds. Both high and low mean income series were presented in a fixed-random order, such that incomes within each consecutive quartile (i.e., each consecutive 25 incomes) were fully-randomised across participants, but all participants viewed quartiles in increasing order (i.e., lowest through highest income quartile). Participants assigned to the control condition worked on a series of addition, subtraction and multiplication problems for a period of 220 seconds (equivalent to the total length of the sample slide shows). Half of all participants in each condition then received a brief description of the biased nature of social circles and were prompted not to base their estimates of the wider income distribution on their own social circles (see Appendix IV for full text). Participants were informed that “A large body of research has shown that social networks are homophilous. Simply put, people move in social circles of people who are similar to each other [...] If you are relatively well-off, your social contacts are probably wealthier than most Americans, on average; if you are relatively less well-off, your social contacts probably tend to be poorer than most Americans.” Participants were informed that “[...] levels of wealth among the people you know are probably not representative of those in America. As a result, you should try not to base your estimates on the people you know.” All participants then estimated the US population-level income distribution and completed other measures pertaining to the population (e.g., direct estimate of US

population mean income), and then completed the five-item perceived representativeness scale.

4.3. Study 4a Results & Discussion

As in previous studies, all variables were standardised prior to analysis for ease of interpretation, and weighted means and Gini indices for estimated distributions were derived using the same procedures used in prior studies. Descriptive statistics across each of the 2x3 conditions are provided in Table 4.1, and intercorrelations for the key variables are displayed in Table 4.2. For consistency with Studies 1a and 1b, 1 participant reporting a household income +4 *SD* above the sample mean was excluded from these analyses.

Firstly, a t-test was conducted to check whether perceived representativeness of social samples was successfully influenced by the homophily prompt manipulation. Social samples were perceived as marginally less representative in the prompt ($M = 3.29$, $SD = 1.05$) compared to the no-prompt condition ($M = 3.47$, $SD = 1.04$); $t(399) = 1.68$, $p = .09$. Hence the homophily prompt manipulation exerted only a minor and marginal effect on overt perceptions of the representativeness of social circles.

Analyses then sought to examine whether, in absolute terms, population estimates were influenced by either the low or high wealth samples presented, the homophily prompt, or both manipulations in combination. To this end, a 2 (homophily prompt; provided vs. not provided) x 3 (sample condition; low-wealth, high-wealth, no-sample control) fully-between subjects ANOVA was conducted to examine the absolute effect of the manipulations on derived population means. There was an unexpected main effect of homophily prompt, $F(1, 396) = 3.98$, $p = .04$, $\eta^2 = .01$, in which participants estimated wealthier population distributions when a prompt was provided

Tables 4.1 (top) and 4.2 (bottom). Study 4a descriptive statistics (4.1) and intercorrelations (4.2).

Measures	Condition					
	Control/No Homophily	Control/Homophily	Low/No Homophily	Low/Homophily	High/No Homophily	High/Homophily
SC mean income (derived)	\$54,577 (23,513)	\$57,843 (\$25,399)	\$53,679 (\$25,500)	\$51,950 (\$20,242)	\$55,214 (\$27,217)	\$55,591 (\$24,600)
SC mean income (estimated)	\$56,275 (27,740)	\$57,684 (\$25,633)	\$54,960 (\$27,176)	\$54,472 (\$25,713)	\$52,628 (\$28,578)	\$55,245 (\$28,083)
Population mean income (derived)	\$63,366 (20,943)	\$64,848 (\$17,320)	\$57,773 (\$14,870)	\$63,659 (\$21,464)	\$64,220 (\$13,220)	\$69,348 (\$28,083)
Population mean income (estimated)	\$50,784 (18,028)	\$54,069 (\$20,487)	\$47,324 (\$17,230)	\$50,031 (\$21,238)	\$51,750 (\$15,964)	\$52,708 (\$15,560)
SC Gini index	28.83 (8.43)	26.57 (8.63)	26.53 (8.75)	28.24 (6.29)	25.97 (6.29)	26.89 (7.20)
Population Gini index	35.37 (7.56)	36.11 (6.53)	35.91 (5.87)	35.13 (5.16)	34.66 (5.54)	33.83 (6.81)
SC perceived representativeness	3.52 (1.04)	3.30 (0.99)	3.33 (1.10)	3.29 (1.19)	3.52 (0.98)	3.29 (1.05)
Household income	\$59,917 (\$34,877)					

Note. Higher values indicate more of each construct (e.g., higher inequality). Standard deviations are presented in parantheses. Statistics for participant income are collapsed across conditions.

Measures	1	2	3	4	5	6	7
1. SC mean income (derived)	-						
2. SC mean income (estimated)	.58***	-					
3. Population mean income (derived)	.30***	.16**	-				
4. Population mean income (estimated)	.18***	.31***	.28***	-			
5. SC Gini index	-.5	-.23***	-.03	-.01	-		
6. Population Gini index	-.14**	-.11*	-.21***	-.35***	.21***	-	
7. SC perceived representativeness	-.09	-.09	-.05	.01	.09	-.10*	-
8. Household income	.48***	.48***	.12*	.14**	-.25***	-.06	-.06

* $p < .05$, ** $p < .01$, *** $p < .001$

(vs. not)^{4.1}. The main effect of sample wealth was marginally significant, $F(2, 396) = 2.34, p = .09, \eta^2 = .01$. Three planned comparisons were conducted comparing both the low and high sample wealth conditions vs. the control, and the low vs. high sample wealth conditions. The planned comparisons revealed no significant differences in derived population means between either the low ($M = \$60,746, SD = \$18,638$) or high sample wealth ($M = \$66,759, SD = \$21,919$) conditions compared to the no alternative sample control condition ($M = \$64,107, SD = \$19,132$), $t(299) = 1.44, p = .15$ and $t(300) = 1.08, p = .28$, respectively. Derived mean incomes were significantly higher in the high compared to low sample wealth condition, however; $t(196) = 2.1, p = .04$. Hence participants in the high, relative to low, sample wealth condition estimated wealthier population distributions, although estimates in neither the high or low condition differed significantly from those in the control condition. The interaction between homophily prompt and sample wealth was not significant; $F(2, 396) = 0.53, p = .59, \eta^2 = .003$.

Effects of Alternative Sample and Homophily Prompt on Social Sampling

Subsequent analyses sought to examine whether, in line with the hypotheses, presenting alternative (high or low mean) samples or providing information about the biased nature of social circles moderated the relationship between own income and estimated

Footnote 4.1

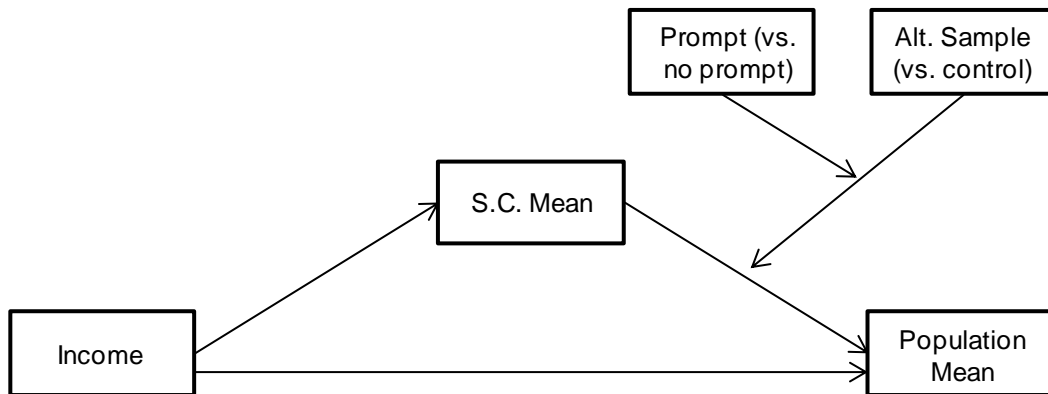
Additional analyses sought to examine whether the unexpected main-effect of the homophily prompt manipulation on derived population means was due to higher or lower income individuals responding differently (i.e., estimating more or less efficient distributions) to the prompt. To this end, three simple moderation models were generated (one within each distribution condition (PROCESS model 1, 10,000 resamples) testing the conditional effect of the prompt manipulation on derived population means as a function of income. The interaction terms were non-significant in all three models (all P 's > .05), indicating that poorer and wealthier participants responded similarly to the prompt manipulation.

population means via social circle means, either independently or in concert. To this end, bootstrapped moderated mediation analyses were conducted (10,000 resamples) using the PROCESS macro for SPSS (model 18; see Hayes, 2012, 2013). This model examines the conditional indirect effect of household income on derived population means via social circle means as a function of sample condition, prompt condition, and the three-way interaction between sample condition, prompt condition and social circle means on the b path. The theoretical model tested is shown in Figure 4.1. Note that this analysis treats high and low wealth sample participants as nested within a single alternative sample category (i.e., compares control participants with high and low-wealth sample participants simultaneously).

This analysis revealed a significant three-way interaction between derived social circle means, sample wealth (no alternative sample control vs. high and low wealth sample) and homophily prompt (provided vs. not) on derived population means; $b = .49$, $SE = .19$, $p = .01$. No other interactions attained significance (all p 's $> .05$). For control (no alternative sample) participants, the conditional indirect relationship between income and derived population means via social circle means was similar across the prompt (BCa CI's of .05 and .26, indirect effect = .13) and no prompt conditions (BCa CI's of .06 and .34, indirect effect = .19). Amongst alternative sample participants, however, the conditional indirect relationship between income and population means via social circle means was not significant in the no prompt condition (BCa CI's of -.01 and .17, indirect effect = .08), but was significant in the prompt condition (BCa CI's of .12 and .43, indirect effect = .25). To summarise, when not provided with an alternative sample, mean estimated population incomes were related to participants' own income irrespective of whether or not participants were warned against social sampling. In other words, participants in the control condition appeared to social sample regardless of whether they were made aware of bias in social samples and explicitly prompted to

Figure 4.1.

Theoretical model of Study 4a conditional indirect effect of income on population means via social circle means. The indirect effect is moderated by sample condition (alternative vs. no alternative control), homophily prompt (provided vs. not) and the three-way interaction between sample condition, homophily prompt and social circle mean.



avoid social sampling. In contrast, when provided with an alternative sample, participants' estimates of mean population incomes were unrelated to their income in the absence of a warning against social sampling, but were related when the warning was provided in addition. Hence although providing an alternative (high or low wealth) sample appeared to reduce social sampling (population estimates were indirectly related to own income amongst all no-sample control participants, but not amongst alternative sample/no homophily prompt participants) providing both an alternative sample *and* warning against social sampling lead to an apparent increase in social sampling relative to the no-sample control^{4.2}.

Footnote 4.2

An alternative model including self-reported judgments of the representativeness of social circles as a covariate obtained similar results.

The Study 4a findings, then, were somewhat mixed. On the one hand, providing an alternative sample of incomes was sufficient to ameliorate the indirect link between own income and derived population means via social circle means. This was evident in the conditions in which no homophily prompt was provided. Here, the social sampling effect was not significant when an alternative sample of incomes was provided to participants, but was significant in the absence of an alternative sample. Simply providing information about the biased nature of social samples, however, did not serve to reduce social sampling; amongst control (no alternative sample) participants', the indirect relationship between own income and derived population means via social circles was the same irrespective of whether participants were warned against social sampling or not. Further, although it was expected that providing an alternative sample in addition to highlighting social sample bias would be the most successful means of reducing social sampling, the opposite tendency was in fact observed. Social sampling was strongest under these circumstances.

One possible explanation for this unexpected and rather striking finding is that the manipulation of awareness of homophily was not fully successful. Social circles were perceived as only marginally less representative in the prompt versus no-prompt condition. Hence participants may have simply disregarded the textual prompt highlighting the biased nature of social samples. Although the prompt manipulation did not effectively undermine explicit judgments of the representativeness of social circles, however, it clearly did exert an impact upon participants' judgement processes - when an alternative distribution was provided in addition to this information, social sampling apparently increased. Study 4b sought to examine whether these unexpected results represent a fluke peculiar to the present data or could be replicated under similar conditions.

4.4. Study 4b Method

Participants

US participants were recruited online ($N = 410$, 42.4% male; $M_{\text{age}} = 38.3$ years; $SD_{\text{age}} = 12.0$) via MTurk for a survey entitled “Estimating Social Distributions”.

Similarly to Studies 1a through 4a, the incomes of the present sample were lower, but similarly distributed, to the US population as a whole (based on estimates from the US Census Bureau, 2013). Thus, 7.7% of the sample reported household incomes placing them in the wealthiest 20% of the US population, and 21.5%, 28.4%, 25.4% and 17% reported household incomes in the 2nd, 3rd, 4th, and 5th wealthiest quintiles respectively.

Materials and Procedure

Experiment 4b sought to partially replicate the alternative sample manipulation used in Experiment 4a. Sample condition was not manipulated as a factor in the design, although all participants were presented with the high wealth sample previously used in Study 4a. As in Study 4a, the precise character of the alternative sample (i.e., relatively low or high wealth) was immaterial to the aims of the study, and hence the high-wealth sample was selected at random via a coin toss. As previously, half of all participants received a brief description of the homophilous nature of social circles and were prompted not to base their estimates of the wider income distribution on their own social circles. The materials and procedure were identical to the high mean condition in Study 4a. Specifically, participants provided demographic information (e.g., household income), completed items pertaining to their social circles (e.g., estimated the income distribution), viewed the incomes slide show, received a description of homophily or received no additional information, completed measures pertaining to the population

(e.g., estimated the population-level income distribution) and then completed the perceived representativeness of social circles items (as in Study 4a, the full five items did not form a reliable scale and as such only items 1-3 were included in the scale; $\alpha = .85$). Participants also responded to three additional items ($\alpha = .91$; Appendix IV) which were designed to measure the perceived credibility and representativeness of the alternative incomes sample, e.g., “To what extent do you feel that the sample of incomes you saw accurately reflects the actual distribution of household incomes in the US” (1 = *Strongly Disagree*; 6 = *Strongly Agree*).

4.5. Study 4b Results & Discussion

All variables were standardised prior to analysis for ease of interpretation, and weighted means and Gini indices for estimated distributions were derived using the same procedures used in prior studies. Descriptive statistics and intercorrelations are displayed in Table 4.3. For consistency with Studies 1a, 1b and 4a, 4 participants reporting a household income +4 *SD* above the sample mean were excluded from these analyses. A further participant failed to report their household income and is hence also missing from the following analyses.

Analyses first sought to examine the absolute effects of the homophily prompt manipulation on population estimates, and upon judgments of the representativeness of social circles and of the alternative sample. As can be seen in Table 4.3, the prompt manipulation had no effect on estimated population distributions as derived population means and Gini indices were the same in both conditions; $t(403) = 1.62, p = .11$ and $t(403) = 0.34, p = .91$, for derived population means and Gini indices respectively. Self-reported judgements of the representativeness of social circles (hereafter referred to as SC representativeness) were not observed to differ across prompt conditions, $t(400) = 1.48, p = .14$. Nor did the prompt manipulation affect the perceived representativeness

Table 4.3. Study 4b descriptive statistics and intercorrelations.

Measures	Prompt Manipulation		Intercorrelations							
	No prompt	Prompt	1	2	3	4	5	6	7	
1. Derived SC mean income	£55,995 (25,623)	£55,305 (24,619)	-							
2. Derived SC Gini index	26.50 (8.13)	26.93 (7.65)	.07	-						
3. Derived Pop. mean income	£62,618 (17,377)	£60,052 (14,311)	.24***	-.01	-					
4. Derived Pop. Gini index	36.06 (6.05)	35.86 (5.70)	-.11*	.15**	-.17**	-				
5. SC representativeness	3.61 (0.98)	3.46 (0.95)	-.20***	.06	-.04	-.05	-			
6. Alt. sample representativeness	4.09 (0.98)	4.15 (0.88)	-.02	-.08	-.01	.01	.19***	-		
7. Household income	£55,388 (42,795)		.55***	-.15**	.12*	-.12*	-.15**	.06	-	

Note. Higher values indicate more of each construct. Standard deviations are presented in parentheses. Statistics for income are collapsed across conditions

* $p < .05$, ** $p < .01$, *** $p < .001$

of the alternative sample (hereafter referred to as alternative sample representativeness), $t(400) = 0.58, p = .56$.

Effects of Homophily Prompt on Social Sampling

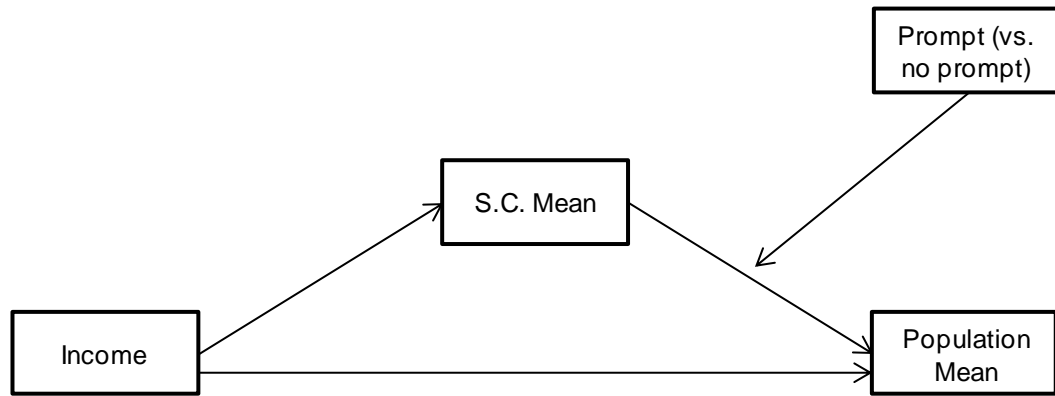
Subsequent analyses sought to examine whether providing a prompt about the biased nature of social samples influenced the indirect relationship between own income and derived population means via social circle means (i.e., moderated mediation). To this end, bootstrapped moderated mediation analyses were conducted (10,000 resamples) using the PROCESS macro for SPSS (model 14; see Hayes, 2012, 2013). This analysis examines the conditional indirect relationship of own income to derived population means via social circle means as a function of prompt condition (no prompt vs. prompt). The theoretical model tested is shown in Figure 4.2.

This analysis revealed a significant interaction between social circle means and homophily prompt on derived population means; $b = .35, SE = .10, p < .001$. Specifically, the indirect relationship between own income and population means via social circle means was significant when participants received a prompt (BCa CI's of .16 and .34, indirect effect = .24) and non-significant when they did not (BCa CI's of -.05 and .14, indirect effect = .05). Similarly to Study 4a, derived population means were positively and indirectly related to own income via social circles only when a prompt highlighting the biased nature of social circles was given (vs. not).

Additional analyses sought to examine whether the effect of the prompt manipulation on explicit judgments of social circle representativeness depended upon judgments of alternative sample representativeness. Across conditions, mean ratings of alternative sample representativeness ($M = 4.12, SD = 0.93$) were above the scale midpoint of 3.5; $t(401) = 13.35, p < .001$. Hence in absolute terms, on average, participants

Figure 4.2.

Theoretical model of Study 4b conditional indirect effect of income on population means via social circle means. The indirect effect is moderated by homophily prompt (provided vs. not) and the two-way interaction between homophily prompt and social circle mean.



tended not to doubt the authenticity or representativeness of the alternative sample.

Alternative sample representativeness was found to moderate the effect of the prompt manipulation on judgments of SC representativeness, $b = -.24$, $SE = .10$, $p = .02$.

Whereas the prompt manipulation had no effect upon SC representativeness when alternative sample representativeness was perceived to be low ($-1 SD$; BCa CI's of $-.19$ and $.33$, indirect effect = $.07$), the homophily prompt manipulation was negatively related to SC representativeness when alternative sample representativeness was high ($+1 SD$; BCa CI's of $-.63$ and $-.11$, indirect effect = $-.37$). Hence the prompt manipulation was only successful in shifting explicit perceptions of the representativeness of social samples when the alternative sample provided was itself perceived to be a reliable alternative.

This pattern of results amongst participants' explicit judgments of social circle and alternative sample representativeness perhaps suggests that raising awareness of social sample bias may successfully reduce the *proclivity* to engage in social sampling

to the extent that alternative information is both available and perceived to be reliable. Participants' explicit judgments of their social circles shifted in line with the manipulation only to the extent that the alternative sample was judged to be reliable. Nevertheless, explicit judgments of either social circle or the alternative sample representativeness apparently had no bearing on the extent to which participants actually engaged in social sampling. Including either or both variables as covariates or additional moderators in the key moderated-mediation analysis (i.e., of income on population means via social circle means, moderated by prompt condition) had no substantive influence on the outcome of the model. Hence, irrespective of its effect upon the perceived efficacy of either social samples or the alternative sample as an estimator of the population, explicitly warning participants about bias in social samples served to increase social sampling.

In combination with the findings from Study 4a, the results from Study 4b imply that the ironic rebound in social sampling observed in the presence of both a prompt not to rely on social sampling and an alternative sample is not a fluke, but a genuine effect of the combination of these conditions. In both studies, providing a prompt did not straightforwardly undermine the perceived representatives of social circles, but it did impact upon sampling processes - in the opposite direction than expected. Further, only when participants perceived the alternative sample to be a highly reliable estimator of the population was the manipulation successful in undermining explicit judgments of the representativeness of social circles. This change in self-reported representativeness of social circles, however, did not translate into a reduction in social sampling. Irrespective of the perceived representativeness of either social samples or the alternative sample, explicitly warning participants against social sampling only served to increase reliance upon this very strategy.

4.6. General Discussion

Studies 4a and 4b sought to examine whether social sampling effects might be reduced or eliminated by either explicitly highlighting the systematic bias in social samples via a textual prompt, providing participants with an alternative sample, or both interventions in combination. It was suggested that warning against social sampling may provoke attempts to correct population estimates accordingly, or motivate a search for alternative, unbiased information upon which to base population estimates. It was further suggested that warning against social sampling in isolation might exert only a limited impact because practical constraints force reliance upon social samples. Thus it was predicted that providing both a prompt *and* an alternative sample should provide the most effective means of reducing social sampling effects.

The results were, however, somewhat mixed. In Study 4a, in line with expectations, providing participants with a textual prompt highlighting the biased nature of social samples alone was not sufficient to undermine social sampling. In the absence of an alternative sample, participants' own income was indirectly linked to population estimates to a similar extent irrespective of whether or not participants were informed of the pitfalls of social sampling and instructed to avoid doing so. Apparently, participants in this condition continued to social sample, or reduced their reliance on social samples to only a minimal extent, such that population estimates remained contingent, via social samples, upon their own income. Several explanations for this apparent failure to respond in line with the homophily prompt are possible. Firstly, it may simply be the case that the manipulation was not sufficient to undermine the perceived reliability of social samples as an estimator of the population; self-reported perceptions of the representativeness of social circles differed only marginally between the prompt and no prompt conditions in Study 4a, and did not differ in Study 4b. Hence participants may

have explicitly rejected the prompt and continued to place faith in their social circles as an unbiased estimator of the population.

Recall that in Study 4b, however, judgements of the representativeness of the alternative sample moderated the effect of the same prompt manipulation on the perceived representativeness of social samples; when the representativeness of the alternative sample was judged to be high, the prompt (relative to no prompt) did cause participants to deemphasise the representativeness of their social circles, although this did not translate into a reduction in social sampling. This observation does imply, however, that it is not inevitable that people will reject information that casts doubt upon the representativeness of their social circles. Where an alternative sample is both available *and* is perceived to be credible and representative, faith in the apparent representativeness of social circles does indeed diminish in line with a warning about their biased nature. A seemingly more probable alternative, then, is that participants were unable to avoid drawing on social samples in spite of the prompt, either because alternative information was absent (i.e., in the Study 4a no-sample control conditions), or because overt attempts to exert control over sampling failed. Even to the extent the prompt was successful in alerting participants to the pitfalls of social sampling and motivating them to avoid doing so, the direction and magnitude of bias in social samples may not have been apparent, and consequently, participants may not have known how to appropriately alter their responses (Wilson & Brekke, 1994).

An alternative possibility is that the prompt did indeed provoke attempts at correction, but that the direction and magnitude of this correction process is itself determined systematically by social samples, such that the indirect link between population estimates and participants' own income via social samples remained intact. That is, participants may have tried to correct their inferences, but correction processes

relied upon pre-experimental knowledge of the income distribution stemming from social samples. It is noteworthy that, in Study 4a, although derived population means were indirectly related to income irrespective of whether or not a prompt was provided (i.e., social sampling influenced population-level estimates), a main-effect of prompt condition was observed such that estimated population means were higher in the prompt (vs. no prompt) condition. This potentially suggests that, although the prompt did not break the contingency of population estimates on own income, participants did attempt to engage in some form of correction in response to the prompt.

The information participants in the homophily prompt-only condition were exposed to entails that population estimates should be corrected for bias in social samples, but did not provide any tools or additional information (e.g., an algorithm, rule-of-thumb, population parameters or an alternative sample) from which the necessary direction and relative magnitude of correction might be reliably approximated. Participants were simply informed that “*If you are relatively well-off, your social contacts are probably wealthier than most Americans, on average; if you are relatively less well-off, your social contacts probably tend to be poorer than most Americans*”. Participants, then, were at the very least aware that to determine the approximate magnitude of correction across levels of income, they must consider their own position in the distribution (i.e., whether they are wealthy or poor relative to the average American).

The social sampling model data supplied by Galesic et al. (2012) imply that this very judgement as to one’s rank in the population is itself determined by social sampling processes. Specifically, due to homophily in social samples, when the underlying population distribution of a given characteristic is positively skewed (as in the case of income), better-off people will underestimate the frequency of worse-off

relative to better-off people in the population, making their own position appear relatively lower. Research shows that this can result in depreciation in *subjective* income rank such that wealthier individuals feel relatively poorer than they really are (Cruces et al., 2011). In turn, such depreciation in perceived rank might serve to systematically bias the correction process insofar as the direction and magnitude of any correction must account for one's perceived position in the distribution, and the manner in which social samples are consequently biased. It follows that relatively wealthier individuals might under-correct their social samples at the bottom and over-correct them at the top end of the distribution, insofar as it is implied in the prompt that a lower rank entails over-sampling of low, and under-sampling of high, incomes. This process could lead to relatively more efficient estimated population distributions than when social samples remain uncorrected, consistent with the main-effect of prompt condition on population means observed in Study 4a. Research on the flexible correction model (FCM; Wegner & Petty, 1997, 1995; Petty & Wegner, 1993) does indeed suggest that people correct judgements in different directions when they hold opposing naive theories as to the direction of a bias (Wegner, Petty & Dunn, 1998; Wegner & Petty, 1995). In short, then, attempts to correct for perceived bias in estimating population-level distributions from social samples may be subject to a kind of feedback loop; ironically, the self-same bias that is targeted by correction processes might conceivably in turn produce systematic biases in the correction process itself.

It was, however, observed in Study 4a that providing an alternative sample of incomes alone was in fact sufficient to ameliorate the indirect link between participants own income and their derived population means via social circle means. In both Studies 4a and 4b, in the absence of a prompt to do so, participants automatically disregarded their social samples in estimating the population distribution and presumably relied instead upon the alternative sample shown to them prior to the population distribution

estimation task. Recall that the experimental samples were ostensibly drawn from US Census Bureau data and participants were explicitly told that they were representative of the population income distribution. When participants were provided with novel, ostensibly reliable information, they automatically opted to base their judgements upon this new information as opposed to social samples. Interestingly, though, the perceived reliability of either social samples (Studies 4a and 4b) or alternative samples (Study 4b) had no impact upon the tendency to rely on social samples. Including either or both measures as covariates (or as an additional moderator in Study 4b) in the moderated-mediation analyses made no substantive difference to the outcome of the models tested in either study. To the extent that participants relied on social sampling, they did so irrespective of the perceived efficacy of either social samples or an alternative sample as an estimator of the population distribution. Evidently, then, participants found it difficult to overcome the tendency to draw on social samples even when they were ostensibly aware of the pitfalls of doing so.

Most strikingly, although it was predicted that the combination of both a prompt and an alternative sample would provide the most effective means of reducing social sampling, the data from both Studies 4a and 4b in fact suggest the opposite. Population means were most strongly related to own income via social sample means in the presence of both a prompt and an alternative sample. It was assumed that alerting participants to bias in social samples would serve to motivate attempts at bias correction. Providing an ostensibly reliable alternative sample would provide a means by which the (ostensibly) necessary direction and magnitude of correction could be learned by participants. This should reduce or eliminate the contingency of population estimates on own income via social circles because participants may engage in recomputation of the population distribution on the basis of the new sample. Ironically,

however, social sampling effects were in fact strongest under these conditions, in both Studies 4a and 4b.

This finding is reminiscent of ironic rebound effects of effortful control in other domains such as stereotyping (Ko, Muller, Judd & Stapel, 2008; McCrae, Bodenhausen, Milne & Jetten, 1994), self-regulation (Vohs, Baumeister & Ciarocco, 2005; Baumeister, Muraven & Tice, 2000) and thought suppression (Wenzlaff & Wegner, 2000; Wegner, Schneider, Carter & White, 1987). For example, research demonstrates that overt attempts to suppress stereotypes often rebound such that perceivers subsequently make more stereotypical judgements (McCrae et al., 1994), and similarly, attempts to exert self-control (e.g., resisting temptation, regulating emotional responses) can lead to diminishing performance over time (e.g., Muraven, Tice & Baumeister, 1998) or self-control failure in a subsequent task (e.g., Baumeister, Bratslavsky, Muraven & Tice, 1998). Such rebound effects have been explained by some researchers in terms of the depletion of mental resources (e.g., Govorun & Payne, 2006; Baumeister, 2002). It is assumed that suppressing unwanted thoughts or behaviours depletes limited resources available for self-regulation, and that successful suppression is dependent upon the availability of such resources (e.g., Baumeister, 2002).

Under many circumstances, social samples may provide the only information upon which inferences about a population can be drawn, and as such it might be expected that social sampling, like stereotyping, is a default strategy upon which people automatically rely in certain judgement situations. Suppressing a default tendency to draw on social samples may thus place a strain upon limited mental resources, in turn leading to an ironic rebound in this very tendency; attempting to suppress biased judgement may actually lead to greater bias due to depletion of mental resources. Given the procedure employed in the present studies, however, this explanation is somewhat

doubtful. There is little reason to assume that participants in Studies 3a and 3b were mentally depleted when estimating the population-level distribution, insofar as the prompt manipulation was delivered immediately prior to the population estimation task. Although avoiding bias in estimation, and of course the task itself, may place demands upon limited resources, such potentially depleting control efforts were not called upon until participants commenced the task in which biased judgement was observed. Rebound phenomena in stereotyping (e.g., McCrae et al., 1994) or behavioural tendencies (e.g., Denzler, Förster, Liberman & Rozenman, 2010) is typically *post-suppressional*, that is, it occurs in a new context after depleting control efforts are relaxed. The rebound effect observed in the present studies, however, occurred in the very same task in which participants were required to exert control, rather than subsequent to presumably depleting control efforts.

A more viable explanation for the observed rebound in social sampling may reside in the literature on thought suppression (for a review see Wenzlaff & Wegner, 2000). Wegner's (1992, 1994) theory of ironic processes in mental control states that thought suppression involves two mechanisms: an intentional process that searches memory or the environment for distracting (i.e., from the suppressed thought) information and an automatic monitoring process that checks for failures in suppression. The monitoring process is required to keep the suppressed thought at some level of activation in memory, although below the level of consciousness, in order to keep track of it. This ironically renders the thought hyperaccessible (Wegner & Erber, 1992), leading to its resurgence when control is relaxed, or when cognitive resources are limited. In contrast with ego-depletion explanations of rebound phenomena, this account allows for enhanced accessibility during as well as after control attempts. Attempts at thought suppression actually increase the accessibility of the target (Higgins, 1989), producing an increase in targeted thoughts during suppression

attempts. Indeed, a few studies have found evidence of enhanced accessibility during suppression in the absence of additional cognitive demands (Salkovskis & Campbell, 1994; Lavy & van den Hout, 1990, 1994), and imposing additional cognitive demands increases the frequency with which such enhancement is found (Wenzlaff & Bates, 1999, 1998; Wegner & Erber, 1992).

A similar ironic process may conceivably underlie the apparent rebound in social sampling observed in the present studies. Where participants seek to avoid bias by re-computing the distribution on the basis of the novel sample, the estimation task requires an intentional search in memory for incomes included in the alternative sample at each given level of the distribution. Suppressing a default tendency to draw upon social samples instead might require a simultaneous, automatic monitoring process to check for intrusions of socially sampled incomes, or to monitor the source (i.e., social vs. alternative sample) of data used in judgement. This monitoring process may ironically result in the intrusion of social sample data into the judgement process, due to heightened accessibility, as implied by Wegner's (1992, 1994) ironic processes account.

Furthermore, the estimation task under these conditions required participants not only to suppress social sampling, but also to intentionally search memory for alternative sample data and to compute the relevant proportions at each income interval. In short, high demands were presumably placed upon participants' cognitive capacity under these conditions. Insofar as suppression is an effortful process (Muraven et al., 1998; Wegner, 1992), it seems reasonable to assume that failures of suppression, and consequently rebound, are especially likely under these circumstances, as has been observed in prior research employing cognitive load manipulations (Wenzlaff & Bates, 1999, 1998; Wegner & Erber, 1992).

To summarise, the persistence of social sampling in the presence of a prompt and in the absence of an alternative sample, as observed in Study 4a, might suggest that participants either disregarded the prompt or were unable to avoid social sampling in the absence of alternative information upon which to base their judgements. Alternatively, it is conceivable that participants' did in fact attempt to engage in a theory-based correction (Wegner, Silva, Petty & Garcia-Marques, 2012; Wegner & Petty, 1997) of population estimates in response to the prompt. Participants were made aware of the source of bias in population estimates (i.e., conditionality of social samples upon own income) and, given the absence of relevant, alternative information, may have attempted to correct their inferences accordingly. However, if the nature of the correction process itself is systematically determined by social sampling processes, as the research of Galesic et al. (2012) could suggest, the contingency of population estimates on own income via social circles may remain intact, as was indeed observed in Study 4a. Furthermore, recall that in Study 4a, a main-effect of prompt condition on derived population means emerged, such that mean income was relatively higher in the prompt condition. This perhaps suggests that participants did attempt some degree of correction in response to the prompt, even though the indirect relationship between own income and population estimates remained intact.

In both Studies 4a and 4b, social sampling did not occur in the presence of an alternative sample only. Presumably, participants under these circumstances attempted to recompute the population distribution on the basis of the novel sample, and were relatively successful in disregarding their social samples. Only under these conditions was a relative reduction in social sampling observed. In the presence of both a prompt and an alternative sample, however, a relative increase in social sampling was evident. Potentially, attempts to actively suppress social sampling in favour of relying on the alternative sample lead to heightened accessibility of social samples, in turn producing

an ironic rebound in social sampling. This implies that, on the one hand, providing ostensibly reliable, novel information about the population alone can serve to reduce social sampling. Apparently, people will automatically employ this new information in their judgements in the absence of any explicit instruction to do so. On the other hand, when such a recomputation strategy is accompanied by active, controlled attempts at suppressing the tendency to social sample, an ironic rebound in social sampling occurs – the very act of suppressing the tendency to draw on social samples results in an increase in the influence of social samples on population estimates.

In conclusion, the present results suggest that social sampling is difficult to avoid, and that deliberate attempts at avoiding social sampling are potentially prone to backfire. It seems reasonable to assume that participants were not motivated to defy the instruction to avoid social sampling. For example, judgments of the representativeness and authenticity of the alternative sample had no bearing on the extent to which participants engaged in social sampling. Failure to follow the instruction seemingly suggests that participants were not unwilling to forgo social sampling, but that they were unable to do so. As such, social sampling does not seem to be amenable to volitional control. On the other hand, simply providing alternative samples may be sufficient to reduce or even eliminate social sampling. Hence altering the information people have at their disposal, and not their motivation, apparently represents the most effective means of reducing social sampling.

Chapter 5

5.1. Introduction

This final chapter comprises a general discussion of the present findings and their wider theoretical implications, and highlights unresolved issues and potential avenues of future research. The next section provides a review and recap of Chapters 1 – 4. Following sections discuss the broader implications of social sampling phenomena for attitudes toward inequality and redistributive preferences, and political polarisation. The potential effects of sampling processes on subjective judgments of socioeconomic status, and implications for redistributive attitudes, are also explored. Further discussion of an important issue touched upon in Chapter 3 – the content of social samples and the role of alternative sources of knowledge (e.g., the media) about the wealth distribution – is provided under “The Content of Social Samples”. Following this, “Preventing (or not) Social Sampling” addresses the mutability of social sampling in light of the findings described in Chapter 4. The penultimate section discusses the present findings in the broader context of cognitive-ecological models of judgement. The final section highlights limitations of the present research and unresolved questions, and suggests important directions for future research.

5.2. Summary of Chapters 1-4

Chapter 1 outlined how much current theory in political psychology emphasises the top-down, intra-psychic underpinnings of attitudes toward inequality, and political beliefs more generally. Such accounts broadly argue that political beliefs reflect opaque existential, epistemic and group-based motivations, and are adopted in order to satisfy these needs and motives. In this view, anti-egalitarian political attitudes reflect motivated attempts to manage uncertainty or fear in the face of threat (Jost et al., 2003),

rationalise current social arrangements (Jost, Pelham, Sheldon & Sullivan, 2003), or legitimise the hegemony of specific groups over others (Sidanius & Pratto, 1999). Importantly, the locus of these tendencies is inside the mind – political attitudes are assumed to stem from the top-down operation of psychological processes, such that political beliefs reflect reasoning in service of reaching a desired, goal-driven conclusion.

These accounts do, of course, acknowledge that normative and ideological social influences shape individuals' goals and motivations, which in turn leads to biased processing of information. The present account differs, however, in terms of how it models the interaction between social and psychological processes in determining political beliefs and attitudes. The social sampling phenomena investigated presently demonstrate how features of social structure, such as homophily, serve to organise information in a selective, non-random and systematic fashion (Fiedler, 2000; Simon, 1982). As a result, biased judgment can emerge even amongst “unbiased minds” (Galesic et al., 2012, p. 7), with no other motivation than to reach accurate conclusions about the social and political world.

Drawing upon research emphasising the role of social sampling in shaping knowledge of social distributions (Galesic et al., 2012), it was argued that sampling processes may play an important role in determining perceptions of how wealth is distributed across society, and consequently, attitudes toward the distribution. Specifically, because wealthier, relative to poorer people, are overexposed via sampling to similarly wealthy others in their day-to-day lives, they will tend to perceive higher aggregate levels of wealth across society as a whole. This has implications for attitudes toward inequality, because higher perceived efficiency may undermine support for measures aimed at reducing inequality (Deutsch, 1972; Okun, 1975; Rawls, 1971).

Further, such sampling processes may contribute toward divergence in the economic attitudes of wealthy and poor via divergence in perceptions of prevailing economic circumstances across society.

Chapter 2 reports 3 studies supporting the contention that the distribution of incomes in individuals' immediate social circles systematically influences perceptions of the income distribution across wider society via social sampling (Galesic et al., 2012). The results of Studies 1a and 1b suggest that sampling processes partly explain the divergent economic attitudes of relatively wealthier and poorer individuals. Since wealthier individuals move in wealthier social circles, they are prone to estimate that the distribution of incomes across society as a whole is more efficient (i.e., the distribution has a higher mean income), and consequently, fairer. This, in turn, was associated with greater opposition to redistributive measures amongst wealthier people. Importantly, this finding held whilst accounting for ideology (Studies 1a and 1b) and perceived self-interest in redistributive measures (Study 1b). These results support the contention that divergence in the economic preferences of wealthier and poorer individuals cannot be explained entirely by differences in the political preferences and material interests of wealthier and poorer people. Rather, consideration of how the informational structure of immediate social environments moulds perception of the income distribution via sampling processes is also necessary (e.g., Cruces et al., 2013).

Additional support for our theoretical model was obtained in Study 2, which conceptually replicated the initial findings using data drawn from a large scale, nationally representative survey conducted in New Zealand. Specifically, the relationship between household income and attitudes toward the economic status quo in New Zealand was mediated via neighbourhood-level deprivation (a proxy for social circle wealth) whilst controlling for political ideology and other relevant control

variables. Wealthier, relative to poorer people, resided in wealthier neighbourhoods (presumably exposing them to wealthier social samples) and in turn rated New Zealand society as more fair. Importantly, these results conceptually replicate the findings of Studies 1a and 1b using an objective indicator of social circle wealth, as opposed to subjective, self-reported estimates. This strengthens confidence in the proposal that objective ecological conditions serve to influence political and economic attitudes by directly assessing the role of these conditions, as opposed to relying on potentially biased or inaccurate estimates of social circle incomes.

Nevertheless, given the correlational nature of Studies 1a through 2 it is not certain that sampling processes play a *causal* role in determining such attitudes. As discussed in Chapter 3, motivational processes or other “top-down” influences on judgement might conceivably account for the relationships observed between individuals’ own wealth, estimated distributions and economic attitudes, for example by biasing social circle and population estimates in line with political preferences. Study 2 goes some way toward assuaging this concern by utilising an objective proxy for social circle wealth. Nonetheless, reverse causality (i.e., economic and political attitudes determine wealth, which in turn determines the neighbourhood in which individuals live) or spurious correlation due to unaccounted for, confounding variables cannot be entirely ruled out.

Chapter 3 sought to address these concerns by employing experimental designs. Studies 3a and 3b attempted to directly manipulate perceptions of the income distribution via experimentally presented (low or high mean) income samples. Since more efficient distributions are perceived as more fair (Scott et al., 2001; Mitchell et al., 2003), it was expected that participants presented with a high, compared to a low mean

distribution, would rate the distribution as more fair and show less support for redistribution.

The results of Studies 3a and 3b, however, were somewhat equivocal. It is clear that participants made use of the novel samples provided, because estimates of the population-level income distribution differed in line with the (low vs. high wealth) samples presented. That is, in both Studies 3a and 3b, participants in the high-mean distribution condition estimated higher mean income distributions than those in the low-mean condition, and provided higher point-estimates of mean income. Furthermore, mean-estimated distributions across participants, although imperfect, qualitatively resembled the high or low mean distributions with which participants were presented in the learning phase of the experiments. Providing participants with novel, experimentally induced samples was indeed sufficient to influence perceptions of the population-level income distribution. In agreement with research emphasising the accuracy of frequency learning and accurate assessment of samples (e.g., Zacks & Hasher, 2002; Fiedler, 2000), participants evidently learned and recalled the incomes presented with a relative degree of accuracy, and subsequently used the novel sample information to inform their population-level estimates.

This effect upon estimated population distributions did not translate directly into between-condition differences in fairness or support for redistribution, in either Study 3a or 3b. It cannot be ascertained from the present data why stronger effects of the manipulation were not observed, but several possibilities were speculatively considered in Chapter 2. For example, insofar as experimentally presented incomes are, unlike social samples, detached from real-world experience and content, it is possible they are not sufficiently evocative to exert strong effects upon economic attitudes. Similarly, if exposure to income samples encountered in everyday life shapes economic attitudes in a

chronic and ubiquitous manner, such attitudes potentially become fixed and rigid over time, and hence impervious to a single, one-off exposure to new information.

Nevertheless, indirect effects of the distribution manipulation did emerge, via inequality (i.e., Gini indices of estimated population distributions) in Study 3a and via efficiency (i.e., mean income of estimated population distributions) in Study 3b. In Study 3a, participants in the high, relative to low mean condition, estimated less unequal population distributions, perceived the distribution as more fair, and were in turn less supportive of redistributive efforts. In Study 3b, participants in the high, relative to low mean condition, estimated more efficient population distributions, perceived the distribution as more fair, and were in turn less supportive of redistributive efforts. Study 3b thus conceptually replicated the Study 1a and 1b models using an experimental manipulation of available samples, as opposed to measured social circle distributions.

In Chapter 4, Studies 4a and 4b sought to examine whether social sampling could be reduced or attenuated by promoting awareness of systematic bias in social samples, providing an alternative sample, or both interventions in combination. It was assumed that, although individuals may be able to produce relatively accurate descriptions of their social samples, they are naïve to the systematic bias that renders social samples unrepresentative of the wider population (Fiedler, 2000, 2012; Fiedler & Juslin, 2006). Further, it was assumed that in many judgement situations, people may have no choice but to draw upon social samples insofar as no alternative information is available upon which inferences of population-level distributions can be based. Warning against social sampling may thus exert little or no impact on social sampling when the absence of alternative information necessitates drawing upon social samples. Hence it was assumed that highlighting bias in social samples in addition to providing an

alternative sample upon which to base judgement would provide the most effective means of reducing social sampling. In Study 3a, it was anticipated that the indirect relation between own income and estimated population distributions via social samples would be weakest or non-significant when participants were made aware of constraints on social samples, and were additionally provided with an alternative (low or high mean) sample of incomes.

Contrary to expectations, the indirect relationship between own income and estimated population distributions via mean social circle income was strongest when participants were both made aware of social circle bias *and* provided with an alternative sample. For participants who were not presented with an alternative distribution of incomes, social sampling effects occurred and were similar irrespective of whether participants were warned against social sampling or not (i.e., the indirect relationship between own income and estimated population mean income via mean social circle income was significant and of similar size for prompt and no-prompt groups). Providing an alternative sample of incomes in isolation, however, was indeed sufficient to eliminate social sampling (the indirect relationship between own income and estimated population mean income via social circle mean income was not significant). Study 4b, a partial replication of Study 4a, replicated the key findings. Providing an alternative sample alone was sufficient to eliminate the indirect relationship between own income and estimated population mean income via social circles. However, this relationship, indicative of social sampling, was significant when participants were additionally warned against social sampling.

The cause of the unexpected and ironic increase in social sampling observed when bias was made salient in the presence of a reliable alternative sample cannot be reliably ascertained from the present studies, although several possibilities were

considered in Chapter 4. One possibility is that the direction of the correction process itself is systematically determined by social sampling processes, as the research of Galesic et al. (2012) could suggest, thus preserving the relation of population estimates to own income via social circles. An alternative explanation may reside in the literature on thought suppression (e.g., Wenzlaff & Wegner, 2000). Overt attempts to suppress social sampling might ironically increase the accessibility of social samples, producing an increase in socially sampled incomes as a result of suppression attempts (e.g., Higgins, 1989). Regardless of the underlying mechanism, the results of Studies 4a and 4b imply that social sampling is difficult to avoid, and that explicit attempts to avoid social sampling are likely to fail. Consequently, providing alternative information alone appears to be the most effective means of reducing social sampling

5.3. Implications of Social Sampling for Attitudes toward Inequality

Studies 1a-2, 4a and 4b, then, provide support for the notion that people do indeed sample from their social circles in order to estimate the population-level income distribution. The homophilous nature of social circles means that people are disproportionately exposed to others of similar incomes, relative to the population level income distribution, leading to systematic differences in the perceptions of poorer and wealthier individuals. Wealthier, relative to poorer people, perceive generally higher levels of affluence across society as a whole, estimating a more efficient (i.e., higher mean income) distribution across the population. Studies 1a-2 further demonstrated that the systematic influence of social sampling on perceptions of efficiency has an effect upon economic attitudes, partly explaining the divergence in judgements of fairness (Studies 1-2) between wealthier and poorer individuals, and in turn, attitudes toward redistributive measures (Studies 1a and 1b). Differences in political ideology and self-interest explain, directly, a larger amount of variation in such attitudes, but relatively

mundane and innocent sampling processes clearly also play an important role in that they account for the relation between wealth and economic attitudes.

These effects of social sampling arguably reflect the “operations of an unbiased mind acting in a particular social structure” (Galesic et al., 2012, p. 7), that is, they do not result from differences in self-interest, ideology or other motivational forces but from differences in environment. Nonetheless, social sampling does work in tandem with these top-down processes. For example, in Studies 1a and 1b, the effect of own income on attitudes toward redistribution was also mediated via political attitudes (Studies 1a and 1b) and perceived self-interest in redistributive measures (Study 1b). This suggests that individuals’ own wealth serves to shape economic attitudes via distinct, but parallel and complementary, ecological and attitudinal processes. Income systematically structures information about distributive outcomes via social samples, producing divergence in the perceptions (e.g., of the distribution of wealth across society) of wealthier and poorer people. In parallel, income also produces divergence in attitudes and motivations - wealthier people report more conservative political attitudes and less self-interest in redistributive measures. These differing processes work in the same direction, reducing support for redistributive measures as wealth increases.

Further, it seems likely that political attitudes and perceptions of self-interest also share some degree of interdependence with sampling processes. Sampling processes presumably serve to undergird or indirectly legitimise political attitudes by shaping perceptions in a manner that accords with and supports such attitudes. Insofar as sampling processes lead wealthier (and hence more conservative) individuals to perceive that the income distribution is relatively more efficient, the opposition to egalitarianism that is an inherent feature of political conservatism is perhaps more easily justified by wealthier individuals. Efficiency is often considered to justify inequalities

(Hayek, 1976; Okun, 1975), and as the present and past research demonstrates, is employed as a normative principle by lay persons in judging the fairness of distributive outcomes (Mitchell et al., 1993; Scott et al., 2001). Both equality and efficiency are treated as “normal goods” in judging distributive outcomes (i.e., more of either is preferred to less), and individuals make trade-offs between these properties; people tolerate greater inequality as efficiency increases (Mitchell et al., 1993; Rawls, 1971; Scott et al., 2001). As such, the relatively greater efficiency perceived by wealthier individuals due to social sampling presumably serves to legitimise anti-egalitarian attitudes which are associated with conservatism.

Although the present research only specifically examined the effect of efficiency perceptions on attitudes toward redistribution, it seems probable that the same processes also affect other facets of economic attitudes. For example, greater perceptions of efficiency might also be associated with endorsement of “trickle down” economics – the notion that reducing the tax burden of businesses and the wealthy indirectly benefits society as a whole by stimulating production – and may militate against any proposed reforms to the economic status quo which entail perceived costs to efficiency, such as progressive taxation and increased social spending. More generally, because efficiency is treated as a normal good, wealthier individual’s perceptions are more closely aligned with abstract, normative ideals of distributive justice. Hence the wealthier subjectively live in a relatively fairer society, irrespective of their self-interest or political ideology.

The present research focused exclusively on the social sampling of incomes and the role own income plays in systematically structuring social samples of income specifically. A vast amount of other information, however, can also be sampled via the same process (i.e., from social contacts) and is subject to the constraints of homophily in social networks –people are relatively similar to their social contacts across a range of

attributes (Galesic et al., 2012; McPherson, Smith-Lovin & Cook, 2001). In addition to sociodemographic attributes, theory and research also suggests that homophily may extend to psychological attributes, such as beliefs, attitudes and preferences, either because like-minded individuals selectively associate with each other (Festinger, 1957) or due to conformity bred via social influence (Asch, 1954; Cialdini & Goldstein, 2004).

Researchers have indeed asserted that the self-selection of individuals into attitudinally homophilous social networks creates “echo chambers” which contribute to polarisation in political attitudes (Bishop, 2009; Sunstein, 2009), and evidence also suggests that “people who talk together vote together” (Pattie & Johnston, 2000). Similarly, as mentioned in Chapter 1, individuals choose to live in communities in which their political ideology is widely shared, and members of local political minorities are more inclined to migrate compared to members of local political majorities (Motyl, Iyer, Oishi, Trawalter & Nosek, 2014). It is well established that social networks are an important source of social and political information and serve to influence political attitudes and behaviour (Klofstad, Sokhey & McClurg, 2013; Mutz, 2002; Newman, 2013). Presumably, social sampling processes serve to reinforce polarisation in political attitudes and behaviour insofar as demographic homophily in social groups may also lead members to share similar perceptions of prevailing social, economic and political circumstances. Such shared perceptions might serve to reinforce political attitudes because interaction with similar social group members presumably provides validation for these perceptions. Further, both attitudinal and demographic homophily may also breed false consensus. Irrespective of any motivation to do so, people may come to overestimate the extent to which their own political perceptions and attitudes are shared by others in society because similar perceptions and attitudes are relatively overrepresented in their social networks, and hence in their social samples.

Research has uncovered large asymmetries in the social mobility of wealthy and poor individuals; the children of poor parents are disproportionately likely to remain poor in adult life, whereas the children of wealthy parents are disproportionately likely to remain wealthy (Pew Charitable Trust, 2012). In a pure meritocracy where there is substantial equality of opportunity across social groups, there should be little or no correlation between the socioeconomic status of parents and children, at least if it is assumed that innate ability and effort are also equally distributed across groups. Indeed, research demonstrates that, at the earliest stages of life, socioeconomic status is unrelated to cognitive ability (Fryer & Levitt, 2013). Differences in ability between children in low and high status families instead emerge in early childhood, increasing over time, and are related to levels of material and social investment of parents (Duncan & Murnane, 2011; Guryan, Hurst & Kearney, 2011).

These findings imply that the wealthy are afforded greater opportunity to preserve, than the poor are to improve, their social status. Inequalities, for example in education, healthcare and job opportunities between wealthier and poorer individuals, also serve to diminish social mobility and entrench wealth and income inequality (Breen & Jonsson, 2005; Corak, 2013; Wilkinson & Pickett, 2011). This is an important observation from the perspective of social sampling insofar as it suggests that, not only do wealthy and poor differ in terms of their perceptions of distributive *outcomes* such as efficiency, but perhaps also in their perceptions of the distribution of *opportunities* which allow people to maintain or improve their social status. Insofar as wealthy individuals and their social contacts are afforded greater opportunities, and their endeavours are more often met with success, they may perceive that prevailing social and economic conditions offer relatively more opportunity for self-improvement given investment and effort. In short, social sampling processes might lead wealthier people to perceive that society is relatively more meritocratic than poorer people, and as such,

they may be more likely to attribute low social status to individual failures (e.g., lack of effort) as opposed to external and uncontrollable social constraints.

Research on distributive justice also demonstrates that meritocracy serves to justify inequality (McCoy & Major, 2007), and that people prioritise efficiency over equality to a greater degree at higher levels of meritocracy (Mitchell, Tetlock, Newman & Lerner, 2003; Mitchell et al., 1993). Greater perceptions of meritocracy may thus be an additional factor in determining wealthier individuals' relatively higher opposition to redistribution, and might also lead wealthier individuals to prioritise the maximisation of net wealth and economic growth (i.e., efficiency) over reducing inequality to a relatively greater extent.

5.4. Social Sampling and Political Polarisation

The present findings build on previous research suggesting that wealth inequality is of a potentially self-reinforcing nature (Kelly & Enns, 2010; Volscho & Kelley, 2012).

Insofar as rising inequality serves to increase social distance between wealthy and poor individuals, social sampling processes will produce greater divergence in perceptions of prevailing social and economic circumstances, which may in turn manifest in increasingly polarised political attitudes between wealthy and poor. Indeed, research has documented an association between rising inequality and increased polarisation amongst party policy positions, and greater stratification of partisanship by income levels over time in the US (McCarty, Poole & Rosenthal, 2003, 2006), as well as greater polarisation amongst the electorate in US states with higher income inequality (Garand, 2010). The present findings suggest that social sampling processes may partly explain the apparent relationship between inequality and political polarisation.

Inequality also serves to increase physical distance, because rising inequality is associated with increased spatial segregation of wealthy and poor, resulting in reduced interaction between people of differing socioeconomic status (Massey & Fischer, 2003). Rising spatial segregation, then, presumably serves to exacerbate demographic homophily within social networks, insofar as wealthy individuals live in communities disproportionately populated by other similarly wealthy individuals, and vice versa for poor individuals. Study 2 suggests that such segregation, in turn, influences political attitudes; wealthy, relative to poorer respondents, reported living in more affluent neighbourhoods and consequently judged society to be fairer. This finding parallels prior research demonstrating that wealthier individuals develop greater support for leftist parties when they live in neighbourhoods with a relatively higher proportion of low-income individuals (Huckfeldt, 1983).

Ironically, however, policies that are ostensibly designed to promote mixing across different social groups may inadvertently breed greater segregation. Research consistently reveals, for example, that gentrifying, “urban renewal” projects result in the displacement of low-income families from inner-city areas due to inflation in property values and rents, thus increasing spatial segregation between socioeconomic groups (Davidson & Lees, 2005; Slater, 2004; Walks & Maaranen, 2008). Such perverse effects of gentrification may also serve to breed hostility and conflict between poorer and wealthier individuals, as exemplified by the recent “Reclaim Brixton” protest against gentrification in south London (McKie, 2015). Presumably, intergroup conflict of this kind serves to further discourage mixing across socioeconomic groups.

Insofar as rising inequality is associated with increased polarisation in the political attitudes of wealthy and poor voters (Garand, 2010; McCarty, Poole &

Rosenthal, 2003, 2006), an outstanding question remains as to why such polarisation has not in turn resulted in redistributive measures that serve to limit or reduce inequality. Models in political economy have suggested that the democratic process should serve to limit inequality, because an increasing proportion of the electorate will vote for parties proposing redistributive measures as inequality increases (Meltzer & Richard, 1981). Inequality in OECD countries, however, has risen steadily since the 1980s and stands at its highest level in 30 years (Cingano, 2014). Bonica, McCarty, Poole and Rosenthal (2013) point to the role of politics and public policy in perpetuating inequality. Polarisation leads to gridlock in political legislatures that immobilises reform efforts. Further, relatively lower electoral participation amongst lower income groups, combined with large political donations and lobbying efforts by wealthy individuals and business interests, ensure that wealthier people exert a disproportionate influence over public policy (Bonica, McCarty, Poole & Rosenthal, 2013; Volscho & Kelley, 2012).

5.5. Social Sampling and Bias in Subjective Rank

Political factors notwithstanding, and echoing the present findings, research implies that biased perceptions of the wealth distribution also play a direct role in maintaining inequality by militating against redistributive efforts. Across demographic and political groups, people are prone to underestimate the extent of wealth inequality, and estimate idealised distributions that are significantly more equitable than the status quo (Norton & Ariely, 2011). To the extent that inequality is widely underestimated, baseline demand for redistributive efforts is potentially lower than it would be given accurate perception of the distribution. Further, biased perceptions of the distribution in turn result in biases in perceived rank, and consequently, materially irrational redistributive preferences (Cruces, Perez-Truglia & Tetaz, 2011).

Paralleling the social sampling model, Cruces et al. (2011) suggest that individuals estimate the population-level income distribution by drawing on immediate reference groups. Estimates of rank hence reflect position within (socioeconomically homophilous) reference groups and are therefore biased relative to objective rank (i.e., in the true population). The authors found that, whilst poor people often overestimated their income rank, wealthy people often underestimated it, and bias in estimated rank was related to relative rank within reference groups; poorer people overestimated their rank, and to a greater extent, as position within the reference group increased (and vice-versa for wealthy people). Notably, individuals with more heterogeneous social contacts were less prone to bias.

Importantly, bias in perceived rank apparently exerted a causal effect on redistributive attitudes amongst poorer individuals; correcting for upward biases by providing information on the true distribution increased support for redistribution amongst poorer individuals, although the converse effect did not occur for wealthier individuals. These findings suggest that social sampling processes may exert an asymmetrical effect on the redistributive preferences of wealthy and poor via judgments of rank. Specifically, upward bias in perceived rank serves to reduce demand for redistribution amongst poorer individuals, but downward bias in perceived rank does not increase such demand amongst the wealthy. Paradoxically, then, although social sampling processes apparently lead to relatively greater demand for redistribution amongst the poor, the same processes might simultaneously serve to dampen aggregate demand by simultaneously distorting perceptions of self-interest in redistribution downward amongst some poor individuals.

5.6. The Content of Social Samples

Social sampling is not the only channel by which people, wealthy and poor, can learn about levels of inequality, affluence and poverty across society. People are indirectly exposed to information concerning the distribution of wealth through various media, such as TV and print news, political messages and campaigns, as well as via interaction with other people. Undoubtedly, wealthy individuals are aware of the existence of poor individuals, and vice versa, irrespective of homophily in incomes within social networks, and each group have some awareness of the lifestyles and living circumstances of the other. Such indirect exposure might be assumed to engender some degree of convergence in perceptions, and perhaps decrease judgements of fairness, and increase support for redistributive measures, amongst the wealthy. The present research demonstrates that social sampling effects nevertheless account for differences in perceptions and preferences, and hence are detectable in spite of any potential convergence that might be produced through vicarious exposure (e.g., via the media) to information on distributive outcomes.

Arguably, important qualitative differences exist between social samples of income and similar information learned indirectly, for example via the media. Information about social contacts is learned in a chronic and unintentional manner, absorbed, updated and integrated over time during every day social encounters (Galesic et al., 2012; Nisbett & Kunda, 1985). Repeated exposure, as well as semantic richness and deeper integration presumably convey memorial advantages of social samples over information about others outcomes learned indirectly, for example via the media.

Furthermore, as argued in Chapter 3, it seems probable that the majority of social contacts' incomes are not known directly, but are inferred during estimation on the basis of relevant, proximal cues to socioeconomic status such as employment, lifestyle and material possessions (Belk, 1981). For this reason, estimated social circle

distributions potentially capture not only abstract perceptions of the distribution of income per se, but concrete experience of the distribution of living standards, and the constraints and affordances upon living standards associated with different levels of income. This richer knowledge and experience is perhaps what actually informs judgments of fairness via social sampling, rather than the abstracted income values attached to social contacts. Estimated social circle income distributions might serve as a proxy for more vivid, arousing and concrete knowledge gleaned via direct contact and interaction with social contacts. Indirect exposure to abstract information about distributive outcomes (e.g., statistics on the distribution or verbal messages about the extent of inequality and poverty in the media) may therefore exert a relatively less powerful impact on attitudes because such information is not associated with meaningful experience of those outcomes, and is not subject to ongoing repetition and elaboration.

This may partly explain why, in Studies 3a and 3b, although presenting participants with (ostensibly real) novel, low or high efficiency income samples was sufficient to influence perceptions of the income distribution accordingly, no direct effects of the manipulation were observed upon judgments of fairness and support for redistribution. Potentially, insofar as income values can be drawn directly from memory in this context, rather than inferred on the basis of social contacts attributes, they entail little or no consideration of the more vivid and arousing information that perhaps underlies the relationship between social circle income distributions and economic attitudes. Further, ecologically situated sampling processes presumably contribute to the formation of political attitudes continuously over time, and attitudes may become relatively rigid and impervious to a one-off exposure to new information.

5.7. Preventing (or not) Social Sampling

Generally, the present findings are somewhat ambivalent as to whether presenting novel information can serve to reduce social sampling, correct perceptions and in turn produce change in political attitudes. As mentioned, in Studies 3a and 3b, although participants presented with a more (vs. less) efficient sample of incomes subsequently estimated more efficient population distributions, the manipulation had no direct effect upon either fairness or support for redistribution. An indirect effect of the manipulation on redistributive attitudes sequentially via estimated population mean income and fairness was, however, observed in Study 3b. This shows that the manipulation did exert an influence on redistributive attitudes by changing perceptions of efficiency, but that redistributive attitudes were only affected to the extent that the manipulation successfully produced changes in perceptions of the distribution. The indirect path from the distribution manipulation to redistributive attitudes via fairness only was not significant – the manipulation had no effect when resulting variance in estimated population means was unaccounted for in the model. As such, providing novel information on the distribution can apparently produce some minor change in attitudes to the extent that perceptions of the distribution, notably levels of efficiency, are successfully modified by the new information.

Further, in Studies 4a and 4b, providing an alternative sample of incomes was sufficient to eliminate social sampling insofar as, under these conditions, estimates of the distribution no longer depended, indirectly, upon own income as a result of differences in levels of social circle income. Studies 4a and 4b suggest that, where a new, ostensibly reliable sample is available, people will automatically disregard social samples and base population estimates upon new information, although Studies 3a and 3b perhaps suggest that such novel information exerts only a small effect on attitudes. Further, social sampling processes will presumably inhibit ongoing change in both perceptions and attitudes in response to novel information. The ongoing embeddedness

of people within their social circles potentially renders both perceptions of the distribution and attitudes rigid over time; changes in response to new information on the distribution may therefore be both small and temporary.

Studies 4a and 4b also revealed that making bias in social samples salient is ineffective in reducing social sampling. This is perhaps unsurprising given that, lacking any alternative information upon which to base population estimates, participants were forced to rely upon social samples irrespective of awareness of the potential for bias in resulting population estimates. In both Studies 4a and 4b, however, inducing awareness of social sample bias, in addition to providing an alternative sample, actually lead to a relative increase in social sampling. As such, the conditions under which participants were expected to be motivated to avoid social sampling (when the biasing effect of social sampling was salient) and also most able to avoid doing so (where alternative information was available upon which to base population estimates) ironically produced the highest levels of social sampling. To the extent that participants did attempt to engage in deliberate correction of their population estimates in response to perceived bias in social samples, then, such attempts clearly backfired. As discussed in Chapter 4, this finding parallels rebound phenomena observed in other domains such as thought suppression (Higgins, 1989; Wegener, 1992, 1994), and potentially involves a similar mechanism. Suppressing a default tendency to draw upon social samples may require a simultaneous, automatic monitoring process to check for intrusions of social sample data upon the judgement process, or to monitor the source (i.e., social sample vs. alternative sample) of data sampled. Such a monitoring process may ironically result in the intrusion of social sample data into judgement due to heightened accessibility of social sample data, as implied by Wegner's (1992, 1994) ironic processes account. It cannot be ascertained from the present studies precisely why rebound effects occurred in Studies 4a and 4b, and an interpretation in terms of ironic processes is hence purely

speculative. Nevertheless, Studies 4a and 4b clearly imply that attempts to reduce social sampling by raising awareness of bias in social samples are likely to be ineffective, and that deliberate attempts at suppressing a default tendency to social sample may ironically increase social sampling.

5.8. The Role of Cognitive-Ecological Processes in Political Cognition

The present results underscore the importance of ecological processes for understanding political attitudes and behaviour generally, in addition to individuals' ideologies, interests and motivations. Theory and research emphasising the role of sampling processes shows that biased judgment can emerge in the absence of motivational biases or cognitive shortcomings insofar the environment determines what information is available for inclusion in the judgement process (Fiedler, 2000). Space, time, density of information, social distance and cultural and economic restrictions serve to shape and limit the information samples to which people are exposed. As such, the information people can potentially acquire about important social and political circumstances and outcomes via sampling, such as the distribution of wealth across society, is constrained by the environment and a person's location within it.

Prior research implies that people can provide normatively accurate descriptions of samples encountered (Fiedler, 2000; Gigerenzer & Murray, 1987; Peterson & Beach, 1967; Zacks & Hasher, 2002), but as a consequence, pre-existing biases in samples will carry over into judgements of populations (Fiedler, 2000; Fiedler, Brinkmann, Betsch & Wilde, 2000; Juslin, Winman & Hanson, 2007). Although there is no means of assessing the accuracy of participants' descriptions of their social samples in the present studies, this prior research suggests that they are likely to be reasonably accurate. Social sampling, in tandem with the homophilous nature of social networks, ensures that samples of income to which people are exposed are non-random because they are

conditioned upon people's own standing on this very attribute. This ensures that systematic biases will exist in perceptions of the population-level income distribution irrespective of ideological or self-serving biases in judgement, even when, and perhaps because, social samples are processed accurately and without bias (Fiedler, 2000; Galesic et al., 2012).

The present findings resonate with prior research and theory suggesting that people are naïve to the constraints of samples and sampling processes, and consequently fail to account for these constraints in judgment (Fiedler, 2012; Fiedler & Juslin, 2006; Juslin, Winman & Hansson, 2007). This work suggests that people assume that samples are representative of relevant populations, failing to account for selectivity imposed by either the environment itself or by the sampling processes employed to extract information from it. Social sampling encapsulates precisely this tendency; people assume that their social circles are representative of the wider population, failing to account for the conditionality of social samples upon their own ranking on the attribute under judgement. In both Studies 4a and 4b, participants tended to believe that incomes across their social circles were representative of incomes in the wider population (average judgements were at the scale midpoint in all conditions), and providing explicit information to the contrary had no effect on judgements of social circle representativeness. Further, raising awareness of bias in social samples did not reduce, and under certain conditions ironically increased, social sampling in Studies 4a and 4b.

Hence people are apparently unable or reluctant to correct for biases in social sampling even to the extent that they are made aware of them. Previous research has construed similar effects, for example the ability to discount redundant information or account for the sample size effects, in terms of metacognitive shortcomings (Fiedler, 2000; Fiedler, 2012; Unkelback et al., 2007). Sampled information is processed

accurately but uncritically, and people lack the necessary metacognitive facilities to account for properties of samples and sampling strategies, and apply appropriate correction to judgements where necessary. As such, and in agreement with the present findings, it may be necessary to change samples themselves in order to prevent biases manifesting in judgment.

The present findings, and prior research and theorising on sampling processes more generally, is also informative for the socioecological model (e.g., Oishi, 2014). As described in Chapter 1, the socioecological approach emphasises the role played by objective features of the environment in shaping cognition, emotion and behaviour, and vice-versa. The present findings are one example of such interactivity between persons and environments, linking individuals' characteristics (income) to the immediate social environment to which they are exposed (their social circles), and in turn to political attitudes. Furthermore, the role of sampling processes in mediating between the macro-environment, and individuals' psychological responses toward it, potentially suggest an important nuance to the socioecological model. Specifically, the present findings highlight that the way in which individuals experience and respond to objective properties of macro-environments (e.g., national levels of wealth) is partly determined, and constrained by, the informational structure of micro-environments (e.g., social circle wealth) via which the wider world is experienced. As such, apparent effects of macro-level environmental variables (e.g., GDP, inequality, demographics, institutions) on individual-level psychological outcomes (e.g., political attitudes, voting preferences, happiness,) may not always result, straightforwardly and strictly from, objective variation in environmental structures per se, but from variation in how those properties are experienced and perceived across different people. Where inferences about macro-level properties (e.g., wealth, inequality, demographics) are drawn on the basis of small, immediate and systematically determined samples, individual-level perception need not

coincide with macro-level reality, even in the absence of motivational, ideological or cognitive biases.

Such insights from the sampling approach may be fruitfully incorporated into the socioecological model. For example, sampling processes may help explain paradoxical findings in which correlations between the same variables occur only at the macro (e.g., between-country) or individual level of analysis, or take different signs at either level. Gelman et al. (2009), for example, show that although individual-level wealth in the US is positively correlated with a tendency to vote Republican, wealthier states tend to vote for the Democrats. This paradox is potentially explained by individuals' perception of their own wealth relative to others in their immediate reference group. Irrespective of absolute wealth, people living in wealthier states may perceive themselves as *relatively* less well-off (because there are proportionally more high-earners in wealthy states) and are perhaps more likely to vote for the Democrats as a result (and vice-versa for Republican voters).

An alternative explanation is also possible, however, because higher (lower) mean wealth at the state-level need not result from higher (lower) proportions of wealthy individuals residing in particular states. Instead, higher state-level wealth might reflect heavy skewing of the wealth distribution – some states may be wealthier because they contain a small number of extremely wealthy individuals. This underscores an important caveat that must be borne in mind when individual-level outcomes (e.g., voting tendencies) are explained in terms of ecological differences (e.g., differing proportions of wealthy individuals across states). Because differences between environments do not always translate straightforwardly to differences between individuals within those environments, correlations between ecological and individual-level variables are potentially spurious. If wealthier states are wealthier simply due to

skewing of the distribution by a few very rich residents, then there is no paradox between the (apparently) contradictory relations of state-level and individual-level wealth to voting tendencies – income alone can explain voting tendencies, and the relationship of state-level wealth to voting tendencies is in fact misleading. This would render the relativity explanation an example of the ecological fallacy, in which erroneous inferences about individual-level outcomes are drawn on the basis of observed differences at the aggregate (i.e., ecological) level.

A related issue, of direct relevance to the present findings, concerns the direction of causality in interactions between persons and the environment – the extent to which variation in individual-level cognition, attitudes and behaviour result from differences in environment, versus the extent to which variations in environment result from differences in cognition, attitudes and behaviour. As discussed elsewhere, the environments to which people are exposed are not entirely static or arbitrary, but are partially shaped by individuals' own behaviour, for example via processes of self-selection (Winkel, Saegert & Evans, 2009). The research by Motyl et al. (2014) showing that individuals choose to live in communities where their political attitudes are widely shared is a good example of how individuals' attitudes and behaviour play a role in determining the day-to-day political ecology to which they are exposed.

Recall that the social sampling model proposed and tested in Chapter 1 assumes that a person's income determines the wealth of their social contacts, such that wealthier people are exposed to wealthier social contacts *because* they are wealthier. Although this is conceptually similar to self-selection (it involves properties of persons determining their environment), it is also importantly different. Namely, the social sampling account neither assumes nor rejects any motivation on the behalf of individuals' to associate with others who are similarly wealthy to themselves. The

relation between individual and social circle wealth might reflect either an active process in which people choose to associate with similarly wealthy others (i.e., self-selection), a passive process in which social structure ensures relative overexposure to similarly wealthy others, or some combination of both. Similarly, although the present model assumes that income influences political attitudes via social sampling processes, the findings of Motyl et al. (2014) suggest a potential reversal of the proposed causal chain. Specifically, if it assumed that a) individuals self-select into communities where their political attitudes are widely shared and b) poorer (wealthier) communities provide poorer (better) earning opportunities for individuals belonging to them, then it is possible that political attitudes causally affect individuals' income. In short, pro-redistributionist individuals may *choose* to live in areas where such attitudes are common, and because these areas tend to provide low paying jobs, they and their social contacts are relatively less well off (and vice-versa).

5.9. Limitations and Future Directions

The present research represents an important first step in investigating the role that social sampling plays in shaping individuals' perceptions of the social world around them, and how these perceptions in turn serve to shape political attitudes. Nevertheless, many important questions remain unaddressed. This final section will briefly highlight outstanding questions regarding social sampling phenomena, suggest potential avenues of future research, and discuss limitations of the present studies. The following discussion is not exhaustive, but aims to focus upon the key questions and issues raised by the present research.

One important question unaddressed by the present studies concerns whether, and to what extent, people are aware of social sampling. It remains an open question as to whether social sampling represents a deliberate strategy upon which people rely in

estimating populations, or whether, as suggested by Galesic et al. (2012), it is an implicit and automatic tendency, akin to a heuristic. Although Studies 4a and 4b demonstrate that it is difficult for people to avoid social sampling, these studies provide no insight into people's pre-existing *awareness* of the means by which they make population estimates. Future research should seek to address this issue, perhaps by openly questioning people about the strategies they use in making inferences about populations.

Furthermore, it is unclear to what extent people are aware of the tendency toward homophily in social circles and, correspondingly, that they are hence a biased estimator of the population. Prior research and theory would suggest that this is not likely the case – as discussed, people are “metacognitively myopic”, demonstrating poor understanding of the properties of samples and employing them in an uncritical fashion in judging populations (Fiedler, 2012, 2000; Fiedler, Brinkmann et al., 2000; Fiedler & Juslin, 2006). Nevertheless, it is possible to imagine circumstances in which it is difficult for people to avoid acknowledging the unrepresentative nature of their social circles. For example, people who are at the extremes of the socioeconomic spectrum (i.e., very wealthy or very poor) conceivably possess some explicit understanding that their social circles are not representative.

This raises the question of whether such individuals rely on social sampling in spite of understanding that their social circles are unrepresentative. Future research might seek to address this issue by investigating social sampling amongst such individuals, such as the very wealthy. Evidence, for example, that very wealthy people are prone to social sampling, in spite of acknowledging the unrepresentative nature of their social circles, would support the contention that social sampling is an automatic and unavoidable tendency.

Further questions arise as to the kind of information that is contained in social samples. Although the present studies investigated social sampling via estimates of income distributions, as suggested earlier, it is perhaps not the distribution of income values per se that matters for judgments of fairness and redistribution. Estimated social circle income distributions potentially also capture concrete experience of others living standards, and the constraints and affordances associated with different levels of income. Potentially, it is this richer knowledge and experience that informs judgments of fairness and preferences for redistribution. Estimated social circle distributions might serve simply as a proxy for more vivid, arousing and concrete knowledge about others wellbeing which is more relevant to such judgments.

Future research should seek to examine this issue directly, perhaps by simultaneously manipulating both sample wealth levels and the qualitative properties of the information provided. For example, participants could be presented with a low versus high wealth sample, where wealth levels are conveyed by either numerical income values, qualitative information (e.g., via text vignettes) pertaining to wellbeing, lifestyle and consumption, or both kinds of information in combination.

Relatedly, it is likely the case that it is not only information pertaining to peoples' wealth that is learned via social sampling and employed in judgments of fairness and redistributive preferences. As mentioned earlier in the chapter, inequalities exist not only in distributive outcomes such as wealth, but also in opportunities, and hence social mobility (Corak, 2013; Wilkinson & Pickett, 2011; Breen & Jonsson, 2005). The social sampling model implies that such inequality in opportunity will become manifest in peoples' perceptions. As a result, wealthier people perhaps perceive that society provides greater opportunity for improving ones social position, and is more meritocratic, than do poorer people.

This has several important implications. Firstly, to the extent that wealthier people perceive higher levels of meritocracy, they are potentially more likely to attribute poverty to personal failings as opposed to inequitable social arrangements (McCoy & Major, 2007; Mitchell et al., 1993). As such, higher perceptions of meritocracy may be an additional factor leading to lower support for redistribution amongst wealthier, relative to poorer individuals.

Secondly, to the extent that poorer (relative to wealthier) people perceive that social contacts' attempts at improvement (e.g., via education, seeking better paid employment) often go unrewarded, and that society is relatively less meritocratic, they are perhaps discouraged from engaging in similar attempts at improving their own circumstances. In this manner, inequalities in social mobility, and consequently in perceptions of opportunity and success, might act as a vicarious driver of learned helplessness amongst poorer individuals (Brown & Inouye, 1978; DeVellis, DeVellis & McCauley, 1978).

Future research might seek to address these important questions by examining whether perceptions of social mobility or success amongst social contacts are related to a person's income, and in turn, how such perceptions influence belief in meritocracy, sense of control over life circumstances and personal efficacy.

The present research emphasised the role of social sampling specifically, involving information samples drawn from individuals' social networks, in determining judgments of the population income distribution. Of course, social contacts are not the sole source of information of relevance to such judgments. People are also exposed to information about levels of inequality, and extremes of poverty and affluence, through other sources such as TV and print media. It is not clear from the present research what

role such vicarious sources of information play in judging population distributions, and whether such forms of information have any bearing on social sampling processes.

Future research should seek to clarify the role of such vicarious information, for example by presenting participants with news articles addressing poverty or inequality, in addition to measuring social circle and population distributions. Such information may undermine social sampling, as perhaps suggested by the findings of Studies 4a and 4b where participants automatically relied on an alternative sample where available. Alternatively, such information is perhaps combined with social sampling, such that population estimates are adjusted accordingly but continue to be largely based upon social samples.

Although the present research shows that social sampling leads wealthier people to be relatively more opposed to redistribution, and vice versa, it is not straightforwardly the case that wealthy people adopt conservative, and poorer people liberal, ideological positions (e.g., Jost et al., 2004). Poor people often adopt conservative, anti-egalitarian political ideologies, and wealthy people often adopt liberal ideologies. Social sampling implies a dissonance between the ideological preferences and perceptions of such poor conservatives and wealthy liberals. Why, for example, would poor individuals adopt anti-egalitarian political ideologies, despite the fact that their social samples expose them to the damaging consequences of poverty and inequality? It is important for future research to establish how people resolve tensions between ideological motivations, on the one hand, and their perceptions of prevailing economic circumstances, on the other. A clear shortcoming of the present theoretical model is that it struggles to account for those cases in which individuals' political beliefs run counter to their personal and group interests.

A related issue concerns the relative influence of ecological versus ideological processes in determining economic attitudes. As discussed in Chapter 2, although sampling processes appear equally important as political attitudes in explaining (by mediating) the negative relation between income and support for redistribution, political attitudes (and self-interest) bear a stronger direct relationship to such attitudes than do social samples. Furthermore, the results from Study 1b suggest that self-interest, but not either social samples or political ideology, account for the relation between income and attitudes toward redistribution. As such, ecological processes alone cannot fully explain attitudes to redistribution. Ideological processes, and self-interest, potentially play a more important role in shaping such attitudes than do social sampling processes. On the other hand, as noted in Chapter 2, it should also be borne in mind that, insofar as attitudes to inequality and redistribution are in and of themselves components of political ideology, (e.g., Jost et al., 2003), there is potentially some degree of redundancy between these variables, and similarly so for self-interest in, and support for, redistribution. Future research should seek to more fairly and directly examine the relative contributions of sampling versus ideological processes or self-interest, perhaps by examining dependent variables that are less proximal to ideology, or operationalising variables in such a way that is less likely to elicit ideological thinking. For example, rather than directly measuring attitudes to redistribution, which is transparently politically loaded, future research might assess preferences for inequality using hypothetical salary allocations (e.g., Jasso, 1983).

An additional shortcoming of the present research is its strict reliance on MTurk samples. Research has criticised over-reliance on crowd-sourced samples, and it has been shown for example that MTurk workers are not fully representative of the wider population. MTurk workers are on average younger, better educated, underemployed and more liberal compared to the general population (Berinsky, Huber, & Lenz, 2012;

Paolacci & Chandler, 2014; Paolacci, Chandler & Ipeirotis, 2010). Nevertheless, MTurk samples are more representative than traditional student samples, and were more suitable for the present research given that it was necessary to recruit members of earning households across a range of incomes.

Research further suggests that MTurkers, although highly motivated, are keen to please requesters and are potentially more prone to demand effects. MTurkers score higher on measures of social desirability (Behrend, Sharek, Meade, & Wiebe, 2011), and may use the Internet to find answers to factual questions (Goodman, Cryder & Cheema, 2013). This represents a potential problem for the present studies – conceivably, participants may have searched the Internet for information on the income distribution rather than basing estimates solely on pre-existing knowledge.

The importance of sampling phenomena in political cognition is underscored by recent experimental research showing that search engine rankings can exert powerful effects on the preferences of undecided voters (Epstein & Robertson, 2015). These data suggest that, where elections are won by small margins, such “search engine manipulation” is potentially sufficient to determine electoral outcomes. Alongside the present findings, this research highlights how even seemingly trivial sampling processes can have important consequences for political attitudes and behaviour, and hence political outcomes, in the real world.

Correspondingly, future research should examine the broader role that sampling processes play in political cognition and attitudes, other than in perceptions of the income distribution and redistributive preferences. It was suggested in Chapter 1, for example, that sampling processes may contribute to widespread biases in factual political knowledge (e.g., concerning the division of government spending, levels of immigration, the prevalence of benefit fraud). What role might structural (i.e.,

environmental) availability biases play in the genesis of common misperceptions surrounding social, political and economic realities? How might peoples' search strategies vis-a-vis the environment contribute to biases in political knowledge, even in the absence of biased processing in the mind?

Questions of this kind require examination of how environmental structures interact with the sampling processes by which people acquire information from the external world. To paraphrase Simon (1990, p.7), the present research underscores the importance of considering how both "scissor blades", the environmental and the psychological, interact in shaping political thought, attitudes and behaviour.

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APPENDIX I: STUDY 1A MEASURES

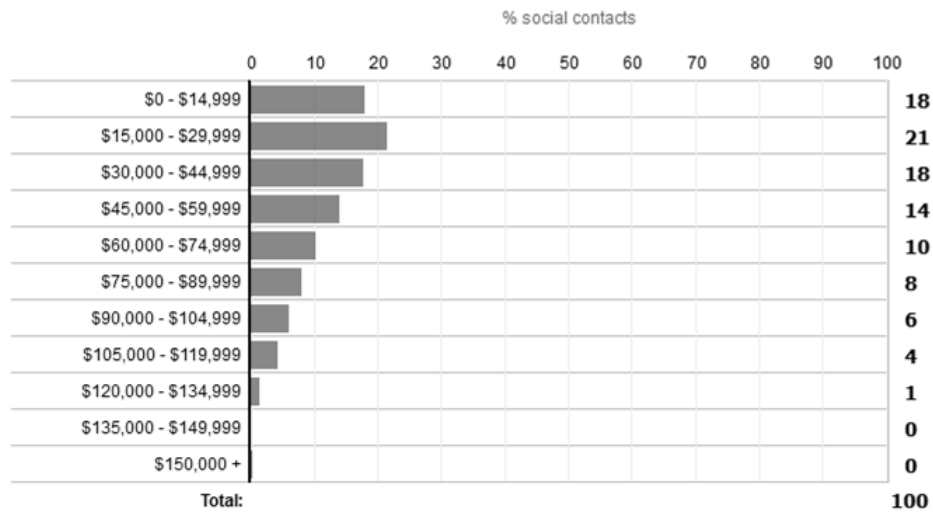
The following are examples of the Study 1a social circle (a) and population distribution (b) estimation tasks. The social circle task example demonstrates a hypothetical response (in the surveys, distributions were always presented with 0% allocated initially). Studies 3a-4b used the same procedure, although income intervals varied as described in the relevant method sections.

a)

In this question we are interested in your estimates of how different household incomes are distributed across your social contacts.

Social contacts are defined as adults you have been in personal, face-to-face contact with at least twice this year, for example friends, family, colleagues and other acquaintances.

Please estimate what percentage of your social contacts are from households earning each of the below levels of annual household income. A running total is displayed at the bottom-right of the matrix and must total 100 (i.e., 100% of social contacts) before you may progress.



b)

For this question, we are interested in your estimates of how household incomes are distributed across the entire population of the United States.

Please use the draggable bars to estimate what percentage of all US households earn each of the below levels of annual household income. A running total is displayed at the bottom-right of the matrix and must total exactly 100 (i.e., 100% of US households) before you may progress.

	% US Population											
	0	10	20	30	40	50	60	70	80	90	100	
\$0 - \$14,999												0
\$15,000 - \$29,999												0
\$30,000 - \$44,999												0
\$45,000 - \$59,999												0
\$60,000 - \$74,999												0
\$75,000 - \$89,999												0
\$90,000 - \$104,999												0
\$105,000 - \$119,999												0
\$120,000 - \$134,999												0
\$135,000 - \$149,999												0
\$150,000 +												0
Total:												0

FAIRNESS

Perceived fairness of the income distribution was measured using the following two items (identical items were used in Studies 1b, 3a and 3b, although scaling varied as indicated in the relevant method sections):

1. To what extent do you feel that household incomes are fairly-unfairly distributed across the US population (R)?
2. How satisfied-dissatisfied are you with the way in which household incomes are distributed across the US population (R)?

1 = Extremely fair/satisfied; 9 = Extremely unfair/dissatisfied

SUPPORT FOR REDISTRIBUTION

Preferences for redistribution were measured using the following four items taken from the 1998 Gallup Poll Social Audit Survey (identical items were used in Studies 1b, 3a and 3b):

1. The government should redistribute wealth through heavy taxes on the rich.
2. The government should not make any special effort to help the poor, because they should help themselves (R).
3. Money and wealth in this country should be more evenly distributed among a larger percentage of people.
4. The fact that some people in the US are rich and others are poor is an acceptable part of our economic system (R).

1 = Strongly disagree; 6 = Strongly agree

POLITICAL ORIENTATION

1. How would you describe your political attitudes?

1 = Extremely Liberal; 9 = Extremely Conservative

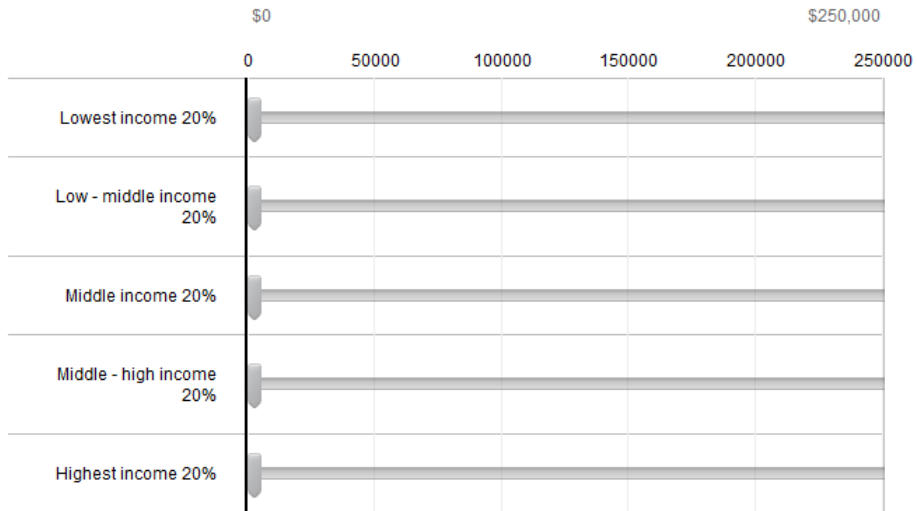
APPENDIX II: STUDY 1B MEASURES

Examples of Study 1b social circle (top) and population distribution (bottom) estimation tasks.

Imagine that all your social contacts stood in a line in order of income, from the person with the very lowest to the person with the very highest household income.

They are then split into five equally sized groups; the 20% with the very lowest incomes, the 20% with the next lowest incomes, and so on up to the 20% with the very highest incomes.

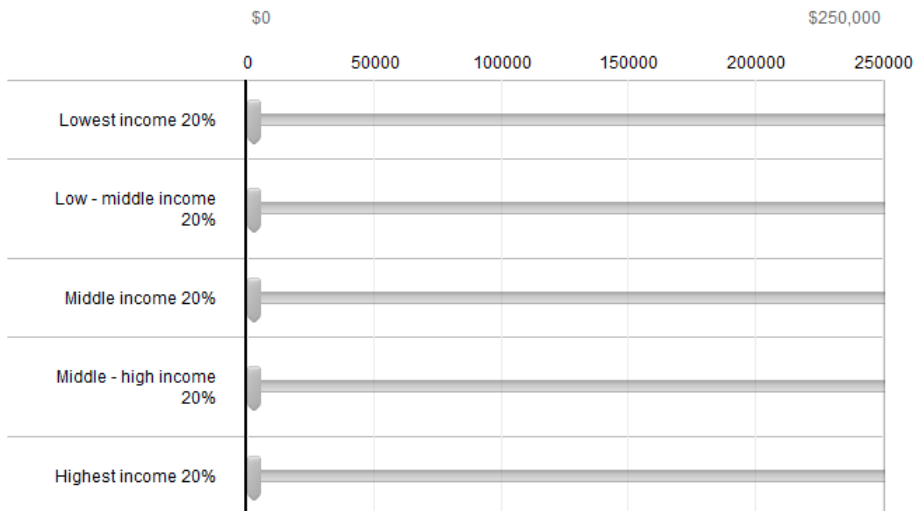
What is the average annual household income within each of the five groups?



Imagine that the entire population of the United States stood in a line in order of income, from the person with the very lowest to the person with the very highest household income.

The population is then split into five equally sized groups; the 20% with the very lowest incomes, the 20% with the next lowest incomes, and so on up to the 20% with the very highest incomes.

What is the average annual household income within each of the five groups?



DIRECT MEAN INCOME ESTIMATES

In Study 1b, direct estimates of mean social circle (top) and US population (bottom) income were measured using the following scales. Studies 3a – 4b used the same procedure, although the maximum value was increased to \$150,000 as indicated in the relevant method sections.

Please use the slider below to indicate the average annual household income across your social contacts...



Please use the slider below to indicate the average annual household income in the US...



PERCEIVED INEQUALITY MEASURE

Direct perceptions of social circle and population inequality were measured using the following two items (the same items were also used in Studies 3a – 4b).

1. To what extent are household incomes equally - unequally distributed across your **social contacts (the US population)**?
2. To what extent is the difference in income between your poorest and wealthiest **social contacts (the US population)** small - large?

1 = *Very equally/small*; 6 = *Very unequally/large*

SELF INTEREST IN REDISTRIBUTION

In Study 1b, perceived self-interest in redistribution was measured using the following three items (the same scale was also used in studies 3a and 3b).

1. To what extent do you personally gain or lose financially from government tax and welfare policies aimed at redistributing wealth from richer to poorer citizens? (R)

1 = Gain strongly; 6 = Lose strongly

2. To what extent do you feel that redistribution of wealth through tax and welfare is in agreement with your own financial interests?
3. To what extent do you feel that redistribution of wealth through tax and welfare is financially beneficial to you personally?

1 = Strongly disagree; 6 = Strongly agree

3-ITEM POLITICAL ORIENTATION SCALE

In Study 1b, political attitudes were assessed using the following 3-item scale.

“How would you describe your political attitudes?”

1. 1 = *Very liberal*; 9 = *Very conservative*
2. 1 = *Very left-wing*; 9 = *Very right-wing*
3. 1 = *Strong Democrat*; 9 = *Strong Republican*

APPENDIX III: STUDY 2 SUPPLEMENTARY ANALYSES

Supplementary Table S1. Additional mediation analyses of the effect of income on alternative political attitudes via varying proxies for neighbourhood wealth levels.

Unless specified otherwise, the analyses reported below are based on Census Area Units (CAU's) rather than meshblock units (MBU's), as the mediators examined are not available at the finer-grained meshblock level in the NZAVS data (with the exception of the NZdep2006; MBU deprivation). The sample contained 1373 unique CAU's, with 3.38 participants per unit ($SD = 2.35$, range 1-16). The geographic size of CAU's differs depending on population density, but each unit tends to cover a region containing a median of roughly 1977 residents ($M = 2210$, $SD = 1673$).

Table S1 Study 2 indirect effects of household income

Mediator	<i>b</i>	<i>SE</i>	LLCI	ULCI
Outcome Variable: Fairness				
CAU Deprivation	.009	.003	.004	.014
CAU Median Income	.007	.003	.002	.013
CAU Proportion of poor relative to wealthy residents	.007	.003	.002	.013
CAU Proportion of residents in receipt of state benefits	.01	.003	.005	.016
Outcome Variable: General System Justification				
MBU Deprivation				
CAU Deprivation	.011	.004	.004	.018
CAU Median Income	.011	.004	.004	.019
CAU Proportion of poor relative to wealthy residents	.009	.004	.002	.018
CAU Proportion of residents in receipt of state benefits	.013	.004	.007	.022
Outcome Variable: National Wellbeing Index				
MBU Deprivation				
CAU Deprivation	.01	.004	.003	.017
CAU Median Income	.006	.004	-.002	.014
CAU Proportion of poor relative to wealthy residents	.008	.004	.001	.016
CAU Proportion of residents in receipt of state benefits	.011	.004	.005	.019
Outcome Variable: Vote for National Party (0 = No, 1 = Yes)				
MBU Deprivation	.087	.013	.064	.115
CAU Deprivation	.085	.013	.062	.112
CAU Median Income	.039	.011	.02	.062
CAU Proportion of poor relative to wealthy residents	.048	.011	.029	.071
CAU Proportion of residents in receipt of state benefits	.079	.013	.055	.107

Note. All scale variables were standardised prior to analysis. Political ideology, age, gender, whether the respondent was born in New Zealand vs. not and whether the respondent was in paid employment vs. not, were included as covariates in all reported analyses.

APPENDIX IV: STUDY 4A AND 4B MATERIALS

HOMOPHILY PROMPT:

Half of all participants in Studies 4a and 4b received the following instruction.

“Thanks for your attention. Before moving on to the final phase of this study, please read and consider the information on this page carefully.

A large body of research has shown that social networks are homophilous. Simply put, people move in social circles of people who are similar to each other. These social circles are like "bubbles" in which "birds of a feather flock together".

So, for example, wealthier individuals tend to live near, and associate with, wealthier people. The converse is true of poorer people, who tend to have more contact with other relatively poor people.

Thus, a person's social contacts are generally not representative of the wider society in which they live. When you think about the people you know, there's a good chance that they don't represent the extremes of rich and poor that exist in America. If you are relatively well-off, your social contacts are probably wealthier than most Americans, on average; if you are relatively less well-off, your social contacts probably tend to be poorer than most Americans.

In the next task you will be asked to estimate how household incomes are distributed across America. As you work on this task, please keep in mind that since "birds of a feather flock together", **levels of wealth among the people you know are probably not representative of those in America. As a result, you should try not to base your estimates on the people you know.**”

SOCIAL CIRCLE REPRESENTATIVENESS

In Studies 4a and 4b, perceived representativeness of social circles was measured using the following 5 items (items 4 and 5 were not included in the final scale).

1. My social contacts' household incomes are representative of household incomes in the US as a whole.

2. My social contacts' household incomes are typical of household incomes in the US as a whole.
3. With regard to household incomes, my social contacts are like a microcosm of the US as a whole.
4. My social contacts tend to have incomes rather like mine, which do not reflect the extremes of wealth and poverty in the US. (R)
5. In general, people tend to mix in social circles of people whose incomes are like theirs, rather than a representative sample of incomes across the country (R)

1 = *Strongly disagree*; 6 = *Strongly agree*

ALTERNATIVE SAMPLE RELIABILITY

In Study 4b, perceived reliability of the alternative sample was measured using the following 3 items.

1. To what extent do you feel that the sample of incomes you saw accurately reflects the actual distribution of household income in the US?
2. To what extent do you feel that the sample of incomes you saw provides a believable representation of the actual distribution of household income in the US?
3. To what extent do you feel that the sample of incomes you saw provides a plausible representation of the actual distribution of household income in the US?

1 = *Strongly disagree*; 6 = *Strongly agree*