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# Examining the Role of Inequality in Security Consumption

by

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# Abstract

A growing body of evidence suggests that economic inequality causes humans to take more risks and engage in aggressive behaviours. In a 'winner-takes-all' environment, risky activities such as gambling, lying, and crime can be a person's only means of keeping up with competitors and accessing contested resources. There is comparatively little research investigating whether people anticipate risk-taking and conflict from their neighbors in unequal environments. An informative context for examining the distrust-inducing effects of inequality is the security market, which offers goods that protect customers from the actions of malicious agents. For instance, barred windows are purchased because they (purportedly) protect consumers from break-ins. Given the costly nature of these security products, security consumption is generally only worthwhile if a consumer expects that other people have harmful intentions.

The proposed research will examine whether inequality will increase consumers' willingness to purchase security products through three studies: 1) experimental manipulations of inequality in an economic game context, 2) experimental manipulations of distributional fairness, and 3) multilevel analyses of the effects of nation-level inequality (k = 32) on individuals' consumption of security goods (n = 79,776).

# Preface

This thesis is original, unpublished, independent work by the author, D. Novakowski

# Acknowledgements

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The manuscript, including the figures and statistical analyses, was created using R (Version 4.1.1, R Core Team, 2021), RStudio (Version 1.4.1717, RStudio team, 2021), with major contributions from R-packages *tidyverse* (Version 1.3.1; Wickham et al., 2019), *ordinal* (Version 2019.12.10; Christensen, 2019), *lme4* (Version 1.1.27.1; Bates, Mächler, Bolker, & Walker, 2015), *afex* (Version 1.0, Singmann, Bolker, Westfall, Aust & Ben-Shachar, 2021), *stevemisc* (Version 1.3.0; Miller, 2021), *bookdown* (Version 0.24; Xie, 2021) and *Superpower* (Version 0.1.2; Lakens & Caldwell, 2021). Other useful packages can be found listed in the supplementary material, and in the shared code scripts.

# Dedication

Blah blah

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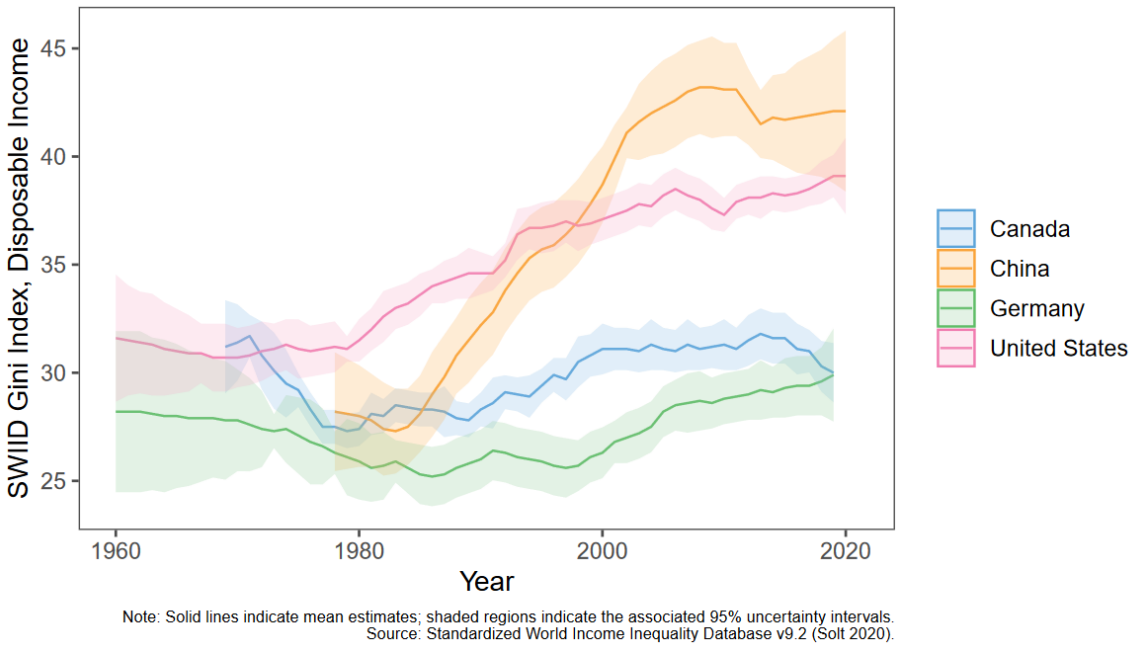
# Introduction

**Figure 1:** Income inequality trends for select countries.




Income and wealth inequalities can vary dramatically between countries, cities, and time periods. For instance, inequality has risen in the US over the past 30 years (Horowitz, Igielnik, & Kochhar, 2020), and even moreso in China. There is substantial debate whether inequality should be embraced as a byproduct of differences in productivity, or resisted as a corrosive force of injustice.

Inequality's moral and political significance is closely linked to its impacts on people's psychology and behaviour. At a societal level, inequality has been associated with higher homicide rates (Daly, 2016), consumer debt (Christen & Morgan, 2005), and reduced cooperation (Anderson, Mellor, & Milyo, 2008; Burton-Chellew, May, & West, 2013), while at an individual level, inequality causes people to be more likely to

resort to risky and hostile activities in desperate attempts to compete for otherwise inaccessible positions and resources (Krupp & Cook, 2018; Mishra, Hing, & Lalumière, 2015; Payne, Brown-Iannuzzi, & Hannay, 2017). This dissertation seeks to examine whether inequality causes a special case of risk aversion: social distrust, and increases individuals' willingness to consume security products.

## 0.1 Risk-Taking and Inequality

The decision of when and whether to take a risk can have substantial effects on individual and collective outcomes. For instance, individuals make choices to start businesses, gamble, participate in extreme sports, or engage in criminal activities. These activities vary to the extent that they are encouraged or prohibited, but they all share an element of risk - they can go very well, or very poorly. For this project, risk is defined as higher variability in outcomes (reviewed in Mishra, 2014). For instance, a 10% chance at winning $30 (versus getting nothing) compared to getting $3 guaranteed have the same expected value; repeating each choice 100 times yields about the same average payoff of $3. However, the 10% chance at $30 has a wider range of possible outcomes, and is thus more risky. Importantly, growing evidence indicates that risk-seeking does not occur arbitrarily, and can often be considered as an adaptive response to especially profitable or desperate circumstances.

The animal behaviour literature has highlighted the importance of thresholds in guiding risk-taking behaviours. For instance, the energy budget rule describes how organisms *should* accept more risky foraging strategies when low-risk alternatives fail to meet individuals' 24-hour energy requirements Stephens (1981). This energy budget rule is but one instance of the broader *risk-sensitivity theory* (aka risk-sensitivity foraging theory), which has been extended to studies of human risk-taking (e.g., Mishra & Lalumière, 2010). Using currencies beyond nutritional requirements (e.g., money, social status, survival decisions), studies have demonstrated that the inability to reach goal states continues to elicit increased risk-taking, consistent with risk-sensitivity theory (Island, Szalda-Petree, & Kucera, 2007; Pietras & Hackenberg, 2001; Rode, Cosmides, Hell, & Tooby, 1999; Wang, 2002).

Following from risk-sensitivity theory, two accounts have been proposed to explain how population-level inequality influences risk-taking in individuals. First, inequality increases risk-taking through *absolute deprivation*; unequal societies tend to have more poverty, leading many people to take risks to meet their basic needs (Pridemore, 2008). Second, risk-taking can be a response to *relative deprivation*, where even well-off

individuals feel disadvantaged when compared to other individuals or imagined states, taking risks in an attempt to keep up with peers and a subjective reference point (Mishra, Barclay, & Sparks, 2017).

The effect of relative deprivation on risk-taking is a fairly recent extension of risk-sensitivity theory, driven by the so-called "positional bias," whereby humans judge success relative to others, instead of in absolute terms (Hill & Buss, 2006). For instance, individuals are more likely to report lower life satisfaction if they made less money than their co-workers (Frank, 2001). Half of individuals prefer less purchasing power in order to have more money relative to peers (Solnick & Hemenway, 1998). Merely suggesting to participants that they have less money than "similar others" induces feelings of relative deprivation and gambling behaviours (Callan, Ellard, Will Shead, & Hodgins, 2008). Likewise, inequality leads to feelings of envy and spiteful behaviours (Wobker, 2015).

Hill & Buss (2006) suggest that the positional bias is an adaptive response to the inherently competitive nature of biological fitness. Since resource access is is necessary for survival, health, and reproduction, individuals who monopolize resources will pass a greater share of their genes onto future generations (Hamilton, 1964). In competitive environments, organisms that fail to notice or counter their own disadvantage (i.e., those that lack a positional bias) should see their genetic representation decline over time (Garay & Móri, 2011). Even at the proximate level, beyond the "ultimate" level of Darwinian "struggle for existence," relative deprivation or advantage are self-reinforcing. Consider two individuals applying for the same promotion, although both may be qualified from an absolute perspective, whoever is judged to be the best candidate (e.g., experience, aptitude) is likely to get the job. Then, whoever gets the contested job may benefit from increased prestige, salary, valued experiences, and a new professional network; if there is ever further competition between these two individuals (e.g., other jobs, grants, romantic partners, clients), those contests will become increasingly favored towards whoever is already privileged.

Together, inequality fosters risk-taking by increasing the distance between individuals and their goals. Inequality causes absolute deprivation because the skewed distribution of resources make it increasingly difficult for a person to meet a relatively stable level of need (e.g., paying rent). By contrast, inequality causes relative deprivation through increasingly extreme social comparisons, leading individuals to similarly skew their aspiration levels ever higher.

This dissertation is inspired by considering the social consequences of this inequality-risk relationship. If inequality causes people to take more risks and/or behave in

a hostile manner, would people in the same unequal environment come to expect harmful behaviours from their neighbors? Chapter one will elaborate on this envy anticipation hypothesis of distrust, which predicts that in the presence of inequality, concerns over partners' envy will drive people to pay to secure themselves against social threats. The following section will explore what it actually means to secure oneself.

## 0.2 Security - A Typology of Protection

The pursuit of security is fundamental to human life; we lock our car doors, purchase insurance, stockpile protective equipment, and flee unstable countries. Many markets are directly and indirectly influenced by this desire for security. In the United States, insurance premiums totaled to $1.2 trillion in 2017 (Insurance Information Institute, 2020). Likewise, people who are concerned for their safety are less likely to use of public transportation (Delbosc & Currie, 2012; Loukaitou-Sideris, 1997). The pursuit of security can require notable trade-offs for consumers; every dollar spent on a heavy-duty deadbolt or theft insurance cannot be spent on other goods such as savings, recreation, or healthcare. Individuals' purchasing and use of products that seek to increase one's security are of particular interest for this project, or what will be termed security consumption.

The idea of security is expansive, rooted in the Latin term *securitas*, meaning freedom from care or anxiety. So, (in)security can occur in many areas, ranging from health, employment, natural disasters, nutrition, as well as conflicts between individuals or nations. Given its potential breadth, a concept of security consumption must be clarified in terms of its relevant domains and forms. At its most comprehensive, security consumption could be considered as any prevention-focused activity, conducted by an agent seeking to minimize the expected costs from harms. With such a wide-ranging definition, security consumption can be carried out by individual consumers, organizations, as well as nation-states.

There has been substantial interest in the area of individual security, particularly within sociology. The pursuit of security can be found under many different names in the academic literature:

> *constrained behaviors, defensive behaviours, precautions, preventive measures, protective behaviours, routine activities, safety precautions, security behaviours, self-protection (avoidance, active security)*

In addition to complicating efforts to search the literature, this abundance of labels also obscures when articles are examining the same constructs and theories. However, throughout this literature, all of these terms seem to consistently share four aspects. They capture an expenditure of resources (money, time, effort, space, attention, social capital, opportunity) that helps the agent and/or their ingroup (e.g., family, neighbors, co-workers) to (a) recover from, (b) cope with, (c) avoid, and/or (d) deter hazards.

DuBow, McCabe, & Kaplan (1979) defined a similar suite of 'behavioural reactions to crime': a) avoidance ("increasing distance from situations [perceived to be risky]"), b) personal and home protection ("increasing resistance to victimization"), c) insurance ("minimizing costs without reducing exposure or resistance... keeping valuables in a safe deposit box to reduce the potential loss when victimized"), d) communicative behaviour ("sharing of information and emotions related to crime with others"), e) participatory behaviour ("action taken with others... may be informally or formally organized"). They also provide a detailed breakdown of each of these components (e.g., avoidance consisting of i) spatial/temporal, ii) situational, iii) activity-specific, iv) supervision of yoith, v) transportation choices, vi) relocation decisions).

Scholars in risk management classify any source of costs as a hazard, whereas threats are defined as warnings that something unpleasant may occur (Brunschot & Kennedy, 2007). Given the conceptual breadth of consumer behaviour (MacInnis & Folkes, 2010), security consumption can be defined as an expenditure of resources in the consumer context that helps the agent to (a) recover from, (b) cope with, (c) avoid, and/or (d) deter hazards. Notably, security behaviours do not always manifest so explicitly in the consumer context; for instance, someone might be concerned about being victimized by a burglary and develop habits of checking their locks, or limiting their exposure to high-risk areas. Although such vigilance might improve one's ability to cope with an intruder, or prevent victimization altogether, it may not always manifest in meaningful exchanges within the security market. Thus, security behaviours can also modify consumer's overall consumer context.

*Recovery* corresponds namely to purchasing insurance. For instance, for vehicles, some people purchase extra insurance to help recuperate costs from events like collisions or vandalism, above and beyond legally-required insurance packages. *Coping* refers to goods and services that help agents reduce the costs incurred when exposed to harms. For instance, a fire extinguisher helps in coping with flammable hazards, such that when it occurs, a fire can be smothered before it causes substantial harm. Likewise, individuals can expend resources to *avoid* certain hazards, for instance, people might invest substantial resources in the process of moving away from regions that suffer

from crime, air pollution, or natural disasters, or avoid certain activities or areas. Lastly, agents can consume security goods that are meant as *deterrents* for potentially antagonistic actors. For instance, someone might purchase a "beware of dog" sign for their property to deter intruders, when they in fact do not own a dog.

Importantly, many security goods and services fill multiple roles simultaneously. For example, moving into a gated community may 1) help their residence avoid being near and visible to criminals, 2) have protective measures that help to cope with attempted thefts, and 3) deter criminals' willingness to attempt a theft, due to the perceived risk of getting caught or hurt. This definition of security consumption appears to be capable of incorporating and differentiating a wide variety of security behaviours.

The physical security industry has been projected to increase 29% between 2021 and 2025 (Markets and Markets, 2021), which likely is heavily impacted by organizations and nations. Understanding security consumption by institutions (i.e., business-to-business and business-to-government marketing) is important, as government and private organizations likewise seek to recover from, cope with, avoid, and deter harms. However, the contexts of such institutional decisions (e.g., institutional prestige, political leverage), the hazards (nuclear strikes by other countries), and the products available (armed drones), are distinct from the security decisions of everyday consumers. To avoid confusing organizations and individuals, this project will focus on security consumption amongst individual consumers, such limitation in scope will help to develop and explicate microfoundations of security consumption, which may eventually be generalized or modified to other security market contexts.

## 0.3 Social Conflict as a Security Hazard - and the Importance of (Dis)trust

Ensuring the safety of one's body and resources against hostile behaviours is an essential life function, creating a pressure for the development of vigilance and defensive strategies across the animal kingdom (e.g., Beauchamp, 2015). For instance, tadpoles are quick to abandon rich food sources when exposed to a chemical "alarm substance" secreted from other injured tadpoles (Petranka, 1989). Such defensive strategies can be seen in the tissue-regeneration of starfishes, aggressive behaviours in honey badgers, camouflage in stick insects, and the bright coloration of non-venomous animals such as the milk snake. Likewise, humans and our ancestors have historically faced

predation by other animals (Hart & Sussman, 2011), which have been argued to generate defensive adaptations in modern humans, such as leveraging social groups to collectively monitor for hazards (Kameda & Tamura, 2007).

While the risk of predation by other species seems to have a profound effect on security behaviours in humans, other people have proven to be a substantial hazard to our ancestors and contemporaries alike. Currently, other humans (via murder) are only outranked by mosquitoes as the deadliest animal to modern *Homo sapiens* (Gates, 2014). Given the costs of being victim to acts of hostility, and the benefits of cooperation (Henrich & Gil-White, 2001), the identification of friends versus enemies is a fundamental strategic problem in human interaction.

The classification of friends versus enemies is neatly captured the psychological process of trust/distrust; these polar opposites generally reflecting our belief whether another person's actions will be beneficial versus harmful to us (Kramer, 1999; Schul, Mayo, & Burnstein, 2008). Trust is important for navigating so-called 'commitment problems,' where individuals can benefit if they can convince their social partners that they are fully committed to a course of action (marriage, business contracts; Frank -Frank (1988)). By making an accurate judgment of who to trust and distrust, individuals can maintain longstanding and beneficial cooperative relationships, and likewise avoid leaving themselves vulnerable to harmful partners. Overall, trust can impact willingness to cooperate (Balliet & Van Lange, 2013) the politicians we vote for, the companies we patronize (Hong & Cha, 2013), and satisfaction reported after negotiations (Kong, Dirks, & Ferrin, 2014), so the processes and predictors of trust have understandably attracted a great deal of attention.

As mentioned earlier, hazards can come from a variety of sources (natural disasters, economic instability, pandemics). However, this project will focus on the threats posed by other people. This emphasis on social sources of harm is unique in requiring decision-makers to consider others' intentions when deciding whether to purchase security goods. Specifically, security behaviours are heavily influenced by the hypothetical imperative: *if I expect others to try to steal, my best response is to purchase security products*; (Hargreaves-Heap & Varoufakis, 2004). Given humans' characteristic reliance on markets to meet their needs, the remainder of this project will rest on the assumption that acts of security consumption (that protect against social hazards) are at least partly a consequence of consumer distrust of other agents.

Some behavioural evidence suggests that people can be overly distrustful. McEvily, Radzevick, & Weber (2012) found that in a behavioural distrust game, participants tended to spend too much money to prevent their partner from keeping large sums

of shared wealth. Paradoxically, excessive distrust, and the consequent security consumption, can increase one's vulnerability to victimizations. Surveying South African suburbs and security industry professionals, Marks & Overall (2015) found that households surrounded with high walls have obstructed lines of sight to and from the property, reducing the effectiveness of police patrols and "natural surveillance" by neighbors. Regardless of whether individuals are consuming too many or too few security products, understanding the determinants of security consumption may be informative both for industry practitioners, as well as a more general understanding of social trust.

This project set out to test an *envy anticipation model of distrust* and its ability to predict security consumption. Specifically, across the following chapters, this project will test the prediction that **H1)** economic inequality increases security consumption because **H2)** people expect disadvantaged peers to experience envy. Addressing the moderating effects of fairness on the effects of inequality, this project will also test the prediction that **H3)** the effect of inequality on security consumption will be lower when incomes appear to be earned, and that **H4)** Individuals will consume more security when inequality is the apparent result of group-based favoritism. This theory will be tested through a multimethod project of four experiments (Studies 1a&b, 2a&b) and a multilevel analysis of archival data using the International Crime Victimization Survey (ICVS; Study 3) (Van Kesteren, 2010) to test whether **H5)** nation-level inequality will be positively associated with consumption of security products See Table 1 for design table with a preliminary breakdown of hypotheses and designs. As a final note, the studies in this project have been pre-registered with the Open Science Framework, but are under private embargo in anticipation of blind peer review. As such, the pre-registration texts have been copied to the appendix verbatim.

**Table 1:** Design table

| Question | Hypothesis | Sampling plan | Analysis Plan | Interpretation given to different outcomes |
|---|---|---|---|---|
| Does inequality increase security consumption? | **H1)** Positive main effect of inequality | Study 1a; n = 978 crowdsourced. Power: cohen's f = 0.12. Study 1b: n determined by 1a effect. Exclusion: missing data, failed atn/comp checks | 2b*2w ANOVA and estimated marginal means, r package 'afex' | Task complexity (low comprehension of partner's income) |
| Does envy anticipation mediate the effect of inequality on security consumption? | **H2)** The indirect effect (ACME) will be sig. dif. from 0 | Study 1a & 1b, same samples as above | Mediation analysis; r package 'mediate' | Low comprehension |
| Does deservingness reduce inequality's effect on security consumption? | **H3)** Negative inequality*merit interaction. Pairwise: Hi-ineq /random > other conds. | Study 2a; n = 1,308 crowdsourced. Power: cohen's f = .1. Exclusion < 3 sliders; missing data, failed atn/comp checks | 2b*2b ANOVA and Pairwise comparisons, r package 'afex' | 1) Task complexity, 2)increased desire to keep funds (and thus purchase security) |
| Under inequality, does group-based favoritism increase security consumption? | **H4)** + outgroup partner*agentic interaction. pc: agentic/ outgroup > other conds. | Study 2b; n = 1,308 crowdsourced, following from power analysis in Study 2a. Exclusion: missing data, failed atn/comp checks | 2b*2b ANOVA and Pairwise comparisons, r package 'afex' | Task complexity (comprehending group membership, and allocation process) |
| Is country-level inequality associated with individual-level security consumption? | **H5)** Positive effect of nation gini on security consumption | Study 3; k = 32 countries, and n = 79,776 participants; Exclusion: missing data | Multi-level ordinal regression model, r package 'ordinal' | Country is too broad (stronger effects at province, city, neighborhood) |

# Chapter 1

# Experiments on Inequality and Security Consumption

Despite evidence for inequality's role in increasing risk-taking, there are data from simulation, cross-country, and panel studies suggesting that inequality also fosters a special case of risk aversion: low social trust (Berggren & Jordahl, 2006; Bjørnskov, 2007; De Courson & Nettle, 2021; Elgar, 2010; Knack & Keefer, 1997; Leigh, 2006; Wilkinson & Pickett, 2010; Zak & Knack, 2001). For instance, Barone & Mocetti (2016) found that among OECD countries, inequality is associated with lower country-level trust, as computed from the World Values Survey. Several theoretical streams have emerged to explain how inequality reduces trust.

## 1.1 Inequality and Distrust

Scholarly work on the relationship between inequality and trust remains in its early stages, although a few theoretical streams have emerged for different mechanisms by which inequality reduces trust (reviewed in Jordahl, 2007). The **social barriers** argument suggests that with inequality, income level increasingly becomes a source of dissimilarity between individuals, and that individuals begin to demonstrate a group membership bias towards individuals of their own income group, and exhibit less trust for the other income outgroup. The **social inferences** explanation argues that people interpret the presence of inequality as evidence of exploitation and injustice, and infer that other individuals (especially those at the top of the income distribution) are untrustworthy. The **resource conflict** explanation posits that inequality "magnifies the incentives for deceitful behaviour," which "will make people with more resources less inclined to trust [poor people]" (p. 5) (Jordahl, 2007). The **opportunity cost**

explanation lastly assumes that inequality causes poor people to spend more time and resources verifying whether social partners can be trusted, because poor people's opportunities tend to be less profitable (i.e., their opportunity costs of distrusting are lower) than for wealthy people.

Another account of how economic inequality leads to distrust is a so-called **psychosocial effect** (De Courson & Nettle, 2021), which is posited to occur after observing inequality-driven selfishness. Borrowing from risk-sensitivity theory, in an unequal environment, worse-off are more likely to be impoverished, and try to satisfy desperation thresholds (e.g., hospital bills) by exploiting cooperative groups. After observing those exploitative behaviours, others in a population become distrustful, eventually avoiding cooperation altogether. Thus, these proposed psychosocial effects of inequality focus on the downstream consequences of *absolute deprivation*. This project seeks to introduce considerations of *relative deprivation* (i.e., envy). In particular, this proposed theoretical extension argues that inequality will lead to reduced trust *without* individuals needing to observe the risky or criminal activities in the population, because people will try to limit their exposure to envious partners, out of fear of the partners' anticipated intentions and behaviours.

## 1.2 Inequality/Inequity Aversion

Concerns over relative deprivation have been extensively investigated in the field of behavioural economics, particularly in the study of inequality/inequity aversion. Individuals exhibit a strong urge to reduce inequality when they are the victim of an unequal distribution, but a weaker desire when they are the beneficiary (Loewenstein, Thompson, & Bazerman, 1989). Consider the two-player Ultimatum Game, where a Proposer offers a take-it-or-leave-it split of money, and the Responder either accepts the offer, or rejects it, with both parties getting nothing. About half of (western undergraduate) participants reject proposals that represent less than 20% of the total resource. Likewise, modal proposals in this game are generally 50% of the total wealth. (reviewed in Camerer, 2011).

The fact that Proposers often make generous offers is unexpected under classical economic models, which predict that a payoff-maximizing Responder would accept any non-zero sum. In these models, a rational Proposer would, in anticipation of these dispassionate preferences, offer the lowest amount possible without fear of rejection or reprisal. However, "generosity" in the Ultimatum game is also consistent with the Proposer's self-interest; if Responders are more likely to reject unequal offers, then a

Proposer's best response is to offer an equal split of the funds.

Proposers' surprisingly equal offers has sparked many active programs of research, with one of the key questions being whether this generosity is due to Proposers' tastes for fairness (they dislike producing inequality), or to strategic efforts to prevent costly rejections (reviewed in Cooper & Kagel, 2016). This latter strategy hypothesis has been supported by observations in the Dictator game. When participants cannot reject a proposed split; modal offers drop from 50% in the Ultimatum game to 0% of the available wealth (Forsythe, Horowitz, Savin, & Sefton, 1994).

The current state of the literature on other-regarding preferences suggests that fairness concerns generally outstrip raw inequality (aka "difference") aversion. For instance, a Responder's decision whether to reject a proposal often reciprocates the Proposer's good/bad behaviour, considering social norms, their partner's intentions, and available alternatives (reviewed in Cooper & Kagel, 2016). Importantly, raw inequality (difference) aversion seem to persist beyond fairness concerns. For example, Bereby-Meyer & Niederle (2005) found that in a three-player Ultimatum game, Responders reject unequal splits between them and another participant, even when the proposal holds no benefit to the Proposer. These rejections are unexpected according to fairness-based theories of other-regarding preferences, which generally predict that responders seek to punish selfish intentions (Bolton & Ockenfels, 2000; Fehr & Schmidt, 1999). Likewise, beyond the Ultimatum game, manipulations of inequality have been demonstrated to increase envy and spiteful behaviours, even amongst children (Dawes, Fowler, Johnson, McElreath, & Smirnov, 2007; Steinbeis & Singer, 2013; Wobker, 2015).

People also *perceive* that inequality is sufficient to elicit inequity aversion; Testing people's folk intuitions of whether inequity aversion is rooted in fairness, Shaw & Choshen-Hillel (2017) found that people are likely to think that advantageous inequity aversion (i.e., disliking being better-off) is a result of fairness concerns, compared to disadvantageous inequity aversion (disliking being worse-off). Both in an evolutionary and developmental sense, disadvantageous inequity aversion appears to be an old response, being observed in children, capuchins, macaques, chimpanzees, domestic dogs, crows, rats, and ravens (Brosnan & de Waal, 2014). Likewise, disadvantageous inequity aversion emerges four years earlier than advantageous inequity aversion in humans (reviewed in Shaw & Choshen-Hillel, 2017). While fairness is obviously important to elicit inequity aversion in humans, raw inequality appears to be sufficient in some cases to change social behaviours.The role of fairness in inequity aversion will be expanded upon in Chapter 2. This chapter instead focuses on "impartial"

inequalities produced by randomness, with no one to blame.

## 1.3 Envy anticipation

People seem to be wary of being envied. Data from small-scale preindustrial societies suggests that many individuals hold "evil eye beliefs," superstitions that a mere envious glance can cause real, material harm. These beliefs are most prevalent in societies with high wealth inequality, and especially where one's resources are easily visible and vulnerable to third parties (Gershman, 2015). Such patterns of envy avoidance seem adaptive when considering the risk- and conflict- inducing effects of inequality. If envious peers are more likely to be hostile and take risks, it may be helpful to anticipate when one's neighbors will feel relatively deprived, and to protect oneself from spiteful or desperate behaviours (Brams & King, 2005; Gershman, 2014).

Likewise, a major feature of scandinavian culture is the "Jante Law" (or Janteloven/Jantelagen), which explicitly discourages individual superiority, ambition, and nonconformity. (e.g., "You musn't think you are anything special;" Sandemose, 1936). Similarly, Anglo-Saxon societies refer to the 'cutting down' of 'tall poppies.' Like Gershman, this manuscript argues that these envy avoidance and status-reducing norms have propogated as a "useful rule of thumb approximating rational envy-avoidance behavior under conditions in which destructive envy is a real threat" [Gershman (2015); p.120]. These patterns of envy aversion seem adaptive when considering the risk- and conflict- inducing effects of inequality; if envious partners are likely to take risks and have malicious intent, being exposed and vulnerable to partners' behaviours may bring substantial costs. In fact, some existing theories already predict a "fear-of-envy" equilibrium, where concerns over partner's destructive envy can reduce people's likelihood to engage in wealth-creating behaviours (Brams & King, 2005; Gershman, 2014).

One of the proposed roles of human emotion is to help communicate and coordinate behaviours between social actors. Since emotions predispose individuals to certain actions (e.g., C. A. Smith & Lazarus, 1990; Sznycer, Sell, & Dumont, 2021), emotions such as jealousy, anger, and envy can serve as psychological commitment devices, steering decision-makers away from dispassionate assessments of alternatives (Frank, 1988). The human propensity for emotional reactions in the face of inequities create a credible threat: "if I am worse off, I will get envious and make life worse for you." Consider the Ultimatum Game described earlier, when deciding whether to split money unevenly. In anticipation of such emotional and antagonistic reactions, Proposers are

better off by anticipating and accommodating the Responder's preference for equality. Indeed, some researchers have suggested that the human sense of fairness evolved as a response to the threats of inequity aversion and spiteful behaviours (Brosnan & de Waal, 2014; Forber & Smead, 2014; Frank, 1988).

This project seeks to test an envy-anticipation theory of distrust, which generally predicts that *inequality reduces trust towards worse-off individuals among a reference group*. This theory posits that individuals anticipate envy from disadvantaged partners, expect a greater likelihood of harmful behaviours from these peers, and consequently, individuals will incur greater costs to protect themselves against hostile or risky behaviours. This envy-anticipation theory of distrust makes similar directional predictions as some existing inequality-distrust theories, but makes these predictions under more generalized circumstances. Even in the absence of absolute poverty, observations of antisocial behaviour, changes in intergroup contact, larger material incentives for antisocial behaviour, or different opportunity costs, this model would still predict that **H1)** economic inequality increases security consumption, and that **H2)** inequality's effect will be mediated by individuals' anticipation of their partners' envy.

## 1.4 Methods

Participants will be recruited through the crowdsourcing platform Prolific Academic. Sampled participants will be filtered for fluency in English, residing in the United States, balanced for an equal gender representation, and only eligible if they have not completed an earlier experiment in this series. All experiments except the pilot study also restricted eligible participants to those with a 97% approval rate. After data collection, participants will be excluded from analyses if they fail an attention or comprehension check items, or if they have erroneous or missing data for any of the model-relevant measures.

The Prolific-recruited participants will complete the studies on oTree (Chen, Schonger, & Wickens, 2016), an open-source platform for conducting single- and multi-player experiments and surveys. oTree studies are written largely in html, python, and javascript, which can be saved and shared in their raw code form, allowing for extensive sharing, reproduction, and customization of protocols. Protocols, materials, data, and cleaning scripts were pre-registered and posted on the Open Science Framework. However, in anticipation of blind peer review, the corresponding OSF project and pre-registrations have been placed under private embargo. Thus, the pre-registrations have been copied to the appendix. Experimental code for all experiments

can be found at `https://github.com/DallasNovakowski/security_game`; data and analysis/cleaning scripts are located at `https://github.com/DallasNovakowski/inequality_security`.

The experiments across both Chapters 1 and 2 will be conducted in the context of a two-player economic game, called the security game (Tambe, 2011). At its base form, the game is one-shot, extensive-form, and conducted with complete information. The original specification of the security game describes how a Defender should allocate limited security resources (e.g., personel at an airport) to protect against the threat of an Attacker, who benefits by carrying out a successful attack against a target. In this modification of the security game, participants are paired with an anonymous partner, with each being given an endowment of funds (e.g., \$300 each). Then, participants are told that their partner will have the opportunity to try to steal money from them (e.g., \$150 at a 50% chance of success, with failed thievery attempts instead making the partner lose \$150). Together, the incomes, purchasing of security, and decision to attempt a risky theft come together to determine the players' final payoffs ($\pi_i$)

After learning this information, participants can spend some of their money on security products (e.g., \$2.00 per unit), with each unit of security visibly reducing the probability of a successful thievery attempt (1% reduction per unit).

### The Security Game

As a more formal summary of the procedure: After being assigned roles and receiving initial incomes ($x_D$ = Defender's income, $x_A$ = Attacker's income), the Defender will be told that the Attacker will have the option to attempt to steal ($q$: 1 = steal, 0 = do not steal) amount $s$ ($0 < s \leq x_D$) from the Defender with a base probability of $p_g$ ($g$ : successful thievery, $0 < p_g < 1$). However, if the Attacker has an unsuccessful thievery attempt (with probability = 1 – $p_g$), they will lose the amount $k$ ($0 < k \leq x_A$). After learning this information, the Defender will have the option to purchase $z$ units of security; each unit of security costs $m$ ($0 < m$), and reduces the probability of a successful thievery attempt by $p_z$ ($0 < p_z < 1$). See Table 1.1 for glossary of terms for this game. The expected value functions for the Defender (Equation (1.1)) and Attacker (Equation (1.2)) can be expressed as:

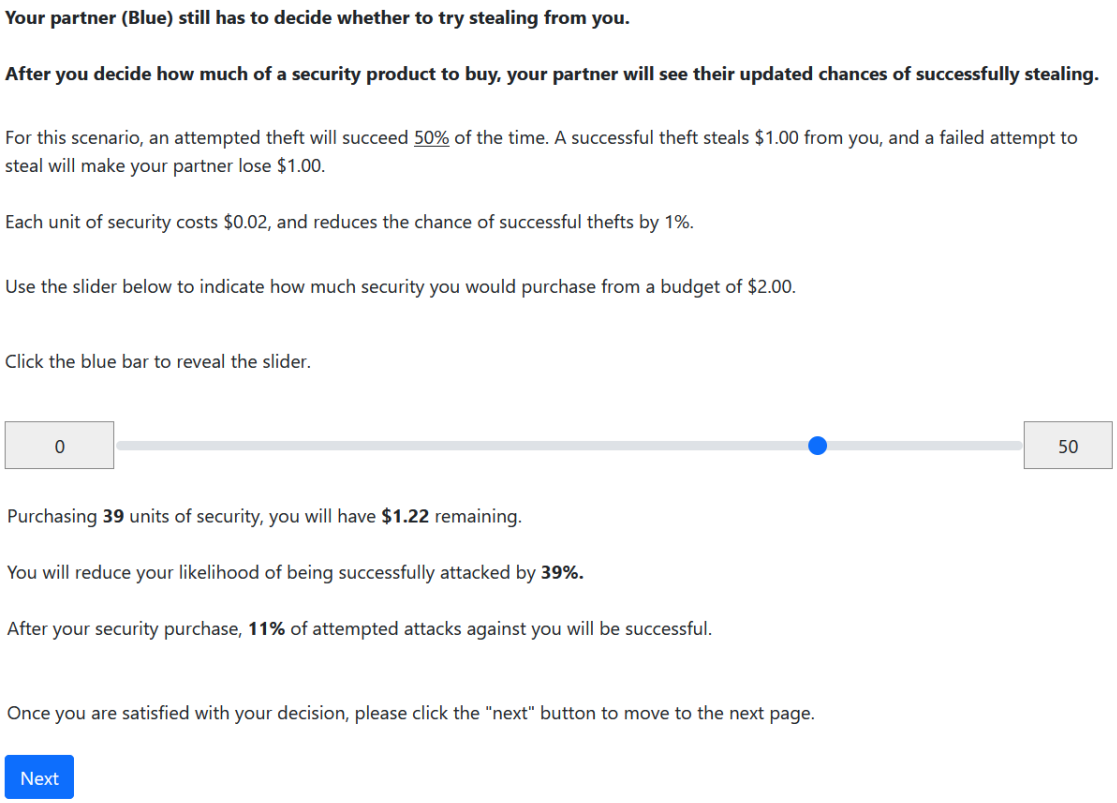
$$\pi_D = x_D - zm - q\{s(p_g - zp_z)\} \tag{1.1}$$

$$\pi_A = x_A + q\{s(p_g - zp_z) - k[1 - (p_g - zp_z)]\} \tag{1.2}$$

**Table 1.1:** Glossary of terms in the security game

| Notation | Label | Possible values | Value(s) in procedure |
|---|---|---|---|
| $i$ | Participant role | A, D (Attacker, Defender) | D |
| $\pi_i$ | Expected payoff of participant i | Given above | N/A |
| A | Attacker | N/A | N/A |
| D | Defender | N/A | N/A |
| $x_D$ | Defender's endowment | $0 < x_D$ | 2 (lo stake); 300 (hi stake) |
| $x_A$ | Attacker's endowment | $0 < x_A$ | lostak: {2(eq);1(ineq)}; histak: {300 (eq); 150 (ineq)} |
| $p_g$ | Base success rate of attack attempts | $0 < p_g < 1$ | 0.5; 0.6 |
| $z$ | Num units of security consumed | DV | DV |
| $m$ | Price per unit of Z | $0 < m$ | 0.02 (lostak); 3 (histak) |
| $p_z$ | Efficacy per unit of Z | $0 < p_z < 1$ | 0.01 |
| $k$ | Amount Attacker loses if unsuccessful | $0 < \mathrm{k} \leq x_A$ | 1 (lostak); 150 (histak) |
| $q$ | Attacker's decision to attempt attack | 1, 0 | 1 = attempt, 0 = no attempt |
| $s$ | Amount stolen by Attacker if successful | $0 < \mathrm{s} \leq x_D$ | 1 (lostak); 150 (histak) |
| $p_q$ | Probability of an attacker to attempt attack | $0 < p_q < 1$ | NA |

**Figure 1.1:** Example of participants' view in the security game.




*Notes:* Full experimental procedures available at https://security-h.herokuapp.com/demo

In written terms, a payoff-maximizing Attacker will seek to maximize their payoff through the combination of their initial endowment and the expected payoff of their decision to attempt an attack. Their ability to profit from an attack is determined by the base probability of attacks being successful, which may be reduced by the amount of security purchased by the Defender. Likewise, Attackers lose from unsuccessful attempts. Intuitively, a payoff-maximizing Attacker will choose to attempt an attack when the attack's expected value exceeds the payoff of not attacking.

Incentive compatibility

Attacker

Max. $\pi_A$

With respect to z, $p_q$

$$\pi_{\mathrm{A}} = x_A + E\left[y_A\right] \tag{1.3}$$

$$y_A = \begin{cases} +S, & (p_g - zp_z)\, p_q \\ -K, & 1 - (p_g - zp_z)\, p_q \\ 0, & (1 - p_q) \end{cases} \tag{1.4}$$

$$E\left[y_A\right] \geq 0 \Rightarrow \tag{1.5}$$

$$z \geq 0, \quad m \geq 0, \quad zm \leq x_D \tag{1.6}$$

$$max. \quad x_A + (p_q * E[y_A \mid q = 1]) + (1 - p_q)\, E[y_A \mid q = 0]) \tag{1.7}$$

Constrained by their budget, an expected value-maximizing Defender then seeks the optimal combination of their initial endowment, the price spent on security, and the expected loss of being targeted by the Attacker. As such, expensive security products need to be accompanied with substantial reductions in an attack's success rate or benefit/cost ratio in order to be worth the investment.

Defender Max. $\pi_D$ With respect to $m$, $p_q$, $p_z$

$$\pi_{\mathrm{D}} = \; x_D - zm - E\left[y_D\right] \tag{1.8}$$

$$y_D = \begin{cases} S, & (p_g - zp_z)\, p_q \\ 0, & 1 - (p_g - zp_z)\, p_q + \; (1 - p_q) \end{cases} \tag{1.9}$$

The security game experiments will take place across five studies, split across Chapters 1 and 2, testing the effects of inequality on security consumption (Study 1a), with uncertain probabilities and stakes of theft (1b), and with real potential monetary payoffs (1c). The last two experiments (in Chapter 2) will assess the effects of inequality when endowments are due to one's own efforts (versus random; 2a), and whether apparent group-based discrimination increases security consumption under constant of inequality (2b). Except for Study 2b and 1b, for each study, inequality will be randomly assigned as a between-subjects condition.

Economic games examining self-protection are not new. Most notably, McEvily et al. (2012) devised a behavioural distrust game, and found that participants are very willing to pay money to guarantee that their partner splits a surplus income equally. Compared to the distrust game, the security game is explicitly probabilistic; partners have a risky choice to try stealing funds for themselves, and security expenditures reduce the probability of successful "attacks." In the behavioural distrust game, the potentially selfish behaviour (partner keeping shared funds), is risk-free, holding no

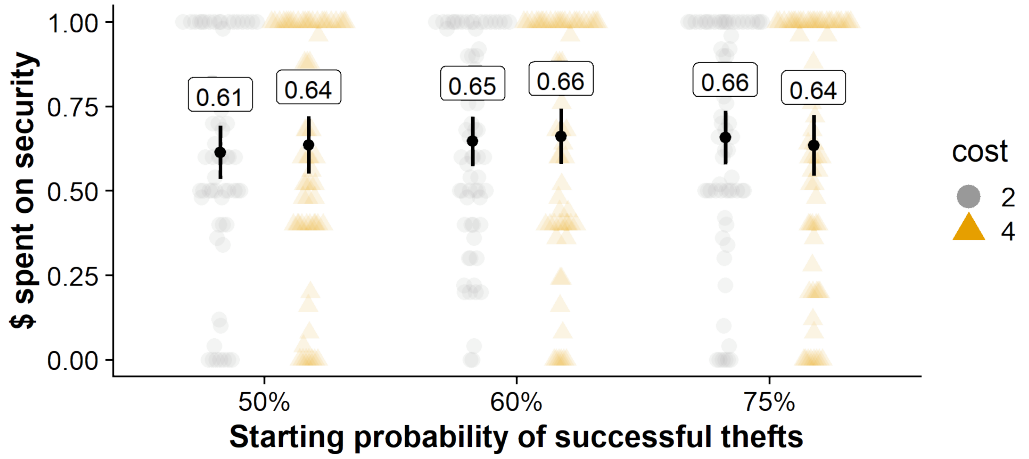
objective or immediate downside costs. The risky nature of the security game is particularly useful for this project because the proposed theory of envy anticipation will focus on the expectation of increased risk-taking in an envious partner.

### 1.4.1 Pilot data

A within-subjects pretest with an initial sample of 108 crowdsourced (Prolific) participants was conducted to calibrate the price of security ($0.02 vs. $0.04) and the baseline likelihood of successful thefts (50%, 60%, 75%). The pretest had two attention checks and implemented a relaxed comprehension check (selecting one of two options out of five; "Deciding whether to try stealing from my partner," or "Deciding whether to purchase a security product"), as only 24% of participants correctly answered a single-item comprehension check, leaving n = 66 retained participants. As described in the procedure above, participants were told they would be paired with a hypothetical partner, and each would be given $2. Then, participants were told that this partner would have the opportunity to steal $1 from them, at a baseline chance of success (50%, 60%, 75%), with unsuccessful thefts instead making the partner lose $1. Then, participants were asked to indicate how much of a security product they would like to purchase, with each unit of the product costing money ($.02, or $.04), and reducing the likelihood of a successful theft by 1%.

A 2w*3w within-subjects ANOVA failed to detect significant effects of baseline likelihood of successful thefts, Cohen's $f = 0.09$, CI(95%) = [0, 0.24], $F(1.81,117.68) = 0.54$, $p = 0.57$, nor security price, Cohen's $f = 0.02$ [0, 0.23], $p = 0.85$; see Figure 1.2 and Table 1.2). Study 1a used a baseline 50% chance of successful thefts, and each unit of security costing $0.02 (or equivalent proportional to stake size). This condition seemed to produce the lowest means and the least skewed data, at -0.49 (vs. skew = -0.62 when probability is 75% and price $0.02).

**Figure 1.2:** Study 1 pretest, effect of security cost and starting theft likelihood on security spending.




*Notes:* Means (95% CI) and jittered data, n=66. ANOVA in Table 1.2, $p$s $\geq$ 0.57.

**Table 1.2:** 2w*3w ANOVA results for Study 1 pretest

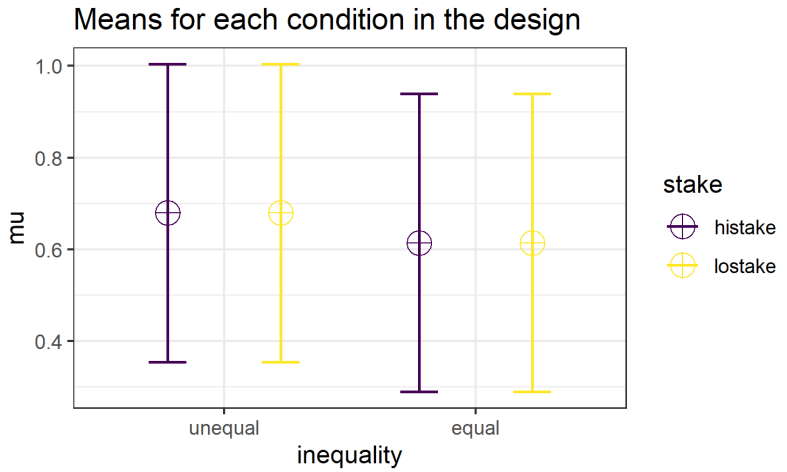
| | df | MSE | $F$ | $p$ | Cohen's $f$ | 95% CI |
|---|---|---|---|---|---|---|
| Prob | 1.81, 117.68 | 0.06 | 0.54 | 0.57 | 0.09 | [0, 0.24] |
| Cost | 1, 65 | 0.05 | 0.03 | 0.85 | 0.02 | [0, 0.23] |
| Prob*cost | 1.96, 127.61 | 0.05 | 0.42 | 0.66 | 0.08 | [0, 0.22] |

### 1.4.2 Measurement and variables (Study 1)

Throughout these experiments, participants were asked to choose how many units of a security product they wished to consume, ranging from 0 to 50. For the purposes of analysis and interpretation, security spending serves as the final dependent variable where possible. Security spending is computed by multiplying the number of security units consumed by the good's per-unit cost. Before participants made their security decisions, they were asked a battery of self-report questions about the game. For example, before their security decision, participants rated their agreement with the statements,

"My partner is probably going to try stealing from me"
"My partner probably feels envious of me"
"My partner probably feels jealous of me"
"My partner probably feels bitter"

**Figure 1.3:** Study 1a, values for power analysis, 2b*2w ANOVA.




*Notes:* Max possible value is $1.00; bars indicate +/-1 *SD*

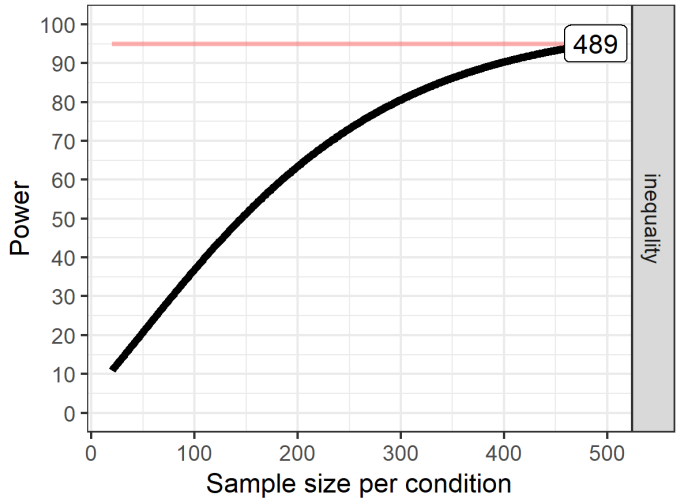
Given the bounded nature of the minimum and maximum values for the variables in the experimental designs, outliers were considered legitimate values and not deleted.

### 1.4.3 Study 1a: Inequality and security spending

The first experiment used a mixed (2b*2w) design in a hypothetical context, manipulating economic inequality (yes vs. no) between-subjects, and stake size ($2 vs. $300) within-subjects, to test whether **H1)** economic inequality increases security consumption (Table 1), versus **$H1_0$)** no significant effect of inequality. Using mean and standard deviation values of security expenditure from the pretest ($M$=0.61, $SD$=0.33), an a priori power analysis was conducted using r package SuperPower (Lakens & Caldwell, 2021) (see Figures 1.3 and 1.4 and and Tables 1.3 and A.1).

Given the unknown size of the hypothesized effect, this study targeted an effect size of Cohen's $f = 0.12$, which corresponds cohen's $d = .2$ for an independent-samples t-test; a small effect according to statistical convention (Cohen, 1988). To achieve 95% power with one tail, studies must have a minimum of n = 489 participants per between-subjects cell, or a study-wide total of n = 978.

**Figure 1.4:** Study 1a, sample size thresholds, 95% power 2b*2w ANOVA.




*Notes:* Specified sample size is per-between-condition for inequality's main effect.

**Table 1.3:** Study 1a, power for 2b*2w factorial ANOVA

| | Power | $\eta_p^2$ | Cohen's $f$ | Non-centrality |
|---|---|---|---|---|
| Inequality | 95.03 | 0.0132 | 0.1156 | 13.04 |
| Stake | 5.00 | 0.0000 | 0.0000 | 0.00 |
| Inequality*stake | 5.00 | 0.0000 | 0.0000 | 0.00 |

Following from the power analysis, 1087 participants were recruited from Prolific, in anticipation of a 10% attrition rate. After initial collection, 15 participants did not consent, and 53 either failed the comprehension check or attention check. After these exclusions, n = 1019 individuals were retained for analyses (48.87% female).

In the study, participants were asked to imagine that they are paired with another participant, where they would each be assigned to different roles (Blue or Yellow), and endowed with money. In the equal condition, participants were told that their partner received the same amount of funds as themselves (e.g., $2; $300 for large stakes). In the unequal condition, participants were told that their partner instead received only half their endowment (e.g. $1; $150 for large stakes). This procedure has a notable strength by controlling the participant's income across levels of inequality (as the partner's money is the only income that changes).

Before participants made their security decisions, they were asked four questions on whether they think their partner would be envious of them (e.g., "My partner

**Table 1.4:** Study 1a Descriptive Statistics

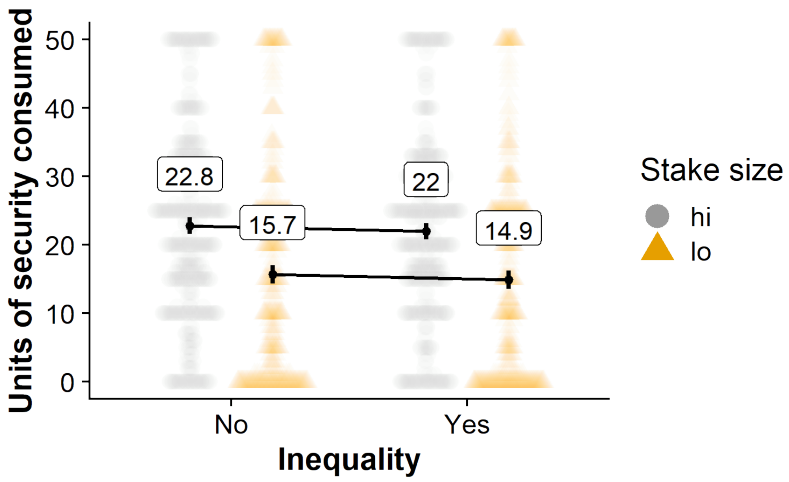
| Variable | Mean | SD | Cron. $\alpha$ 95% CI |
|---|---|---|---|
| Age | 33.8 | 11.9 | |
| Perceived envy | 3.6 | 1.5 | [0.94, 0.95] |
| Perceived attack likelihood | 4.9 | 1.4 | |
| Security consumed | 18.8 | 13.1 | |

probably feels envious of me"). Checked for internal consistency (Cronbach's $\alpha \geq .7$), these items were summed and used to test whether **H2)** The indirect effect (ACME) will be significantly different from 0 versus $\mathbf{H2_0)}$ no significant indirect effect of inequality on security consumption through anticipated partner envy. Since stake size was manipulated within-subjects, two sets of responses for game-related self-report items and security consumption decisions were available, so were analyzed using mixed models.

### Results

As seen in Figure 1.5 and Table 1.5, a 2b*2w mixed ANOVA did not detect any significant effect of inequality on security consumption, Cohen's $f = 0.03$, CI(95%) = [0, 0.09], $p = 0.35$, failing to reject $\mathbf{H1_0)}$. On an exploratory note, stake size had a significant and positive effect on security consumption, Cohen's $f = 0.55$ [0.48, 0.62], $p < .001$. The effect of stake size does not correspond to any theory-relevant hypotheses, but may reflect increased risk aversion for larger values - participants seemed more willing to risk having trivial quantities stolen from them.

**Figure 1.5:** Study 1a, effects of inequality and stake size on security consumption.



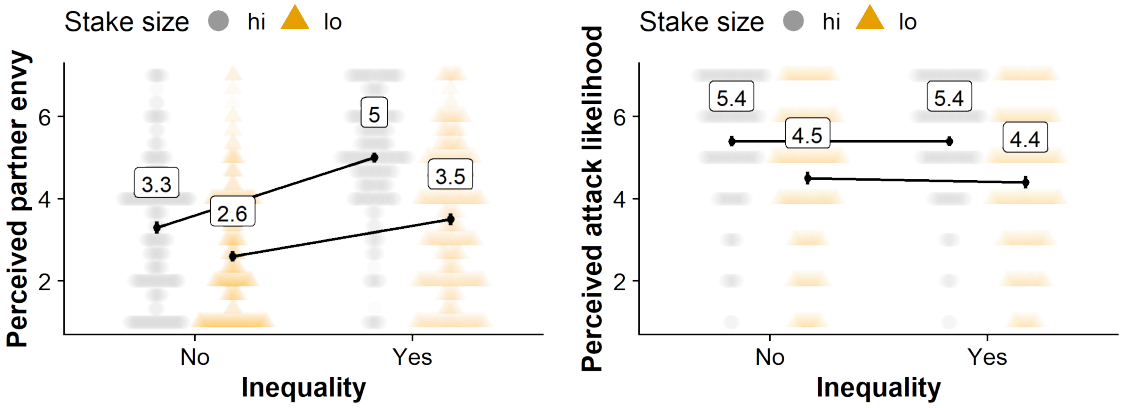

*Notes:* Means (95% CI) and jittered data, 2b*2w ANOVA in Table 1.5

**Table 1.5:** Study 1a, ANOVA; inequality*stake size on security consumption.

| | df | MSE | $F$ | $p$ | Cohen's $f$ | 95% CI |
|---|---|---|---|---|---|---|
| Inequality | 1, 1017 | 341.56 | 0.88 | .35 | 0.030 | [0, 0.09] |
| Stake size | 1, 1017 | 83.92 | 308.42 | <.001 | 0.550 | [0.48, 0.62] |
| Ineq*Stake | 1, 1017 | 83.92 | 0.00 | .98 | 0.001 | [0, 0] |

**Exploratory analyses** The manipulation check suggests that inequality does indeed increase perceived envy, as predicted, Cohen's $f = 0.46$ [0.39, 0.52], $p < .001$. In addition, a positive and significant interaction was detected between stake size and inequality; inequality led to more perceived envy when stake sizes were large, Cohen's $f = 0.27$ [0.2, 0.33], $p < .001$. The higher ratings of partner envy in the inequality condition suggest that inequality was noticeable by participants, and that they also believed that inequality could impact their partner. See Figure 1.6 and Table 1.6.

**Figure 1.6:** Study 1a, effects of inequality and stake size on perceived partner envy and perceived attack likelihood.




*Notes:* Means (95% CI) and jittered data, 2b*2w ANOVA in Table 1.6

**Table 1.6:** Study 1a, ANOVA; inequality*stake size on perceived envy.

| | MSE | $F$ | $p$ | Cohen's $f$ | 95% CI |
|---|---|---|---|---|---|
| Inequality | 3.94 | 214.05 | <.001 | 0.46 | [0.39, 0.52] |
| Stake size | 0.94 | 656.63 | <.001 | 0.80 | [0.73, 0.87] |
| Ineq*Stake | 0.94 | 72.48 | <.001 | 0.27 | [0.2, 0.33] |

Mirroring the effects on security consumption, a 2b*2w ANOVA failed to find an effect of inequality on perceived likelihood of the partner attacking, Cohen's $f$ = 0.004 [0, 0.05], $p$ = 0.9. However, stake size still had a significant and positive effect on perceived attack likelihood, Cohen's $f$ = 0.57 [0.51, 0.64], $p$ < .001. See Figure 1.6 and Table 1.7. Although inequality led to increased perceived envy, this perception did not cascade into expecting harmful actions from one's partner. The

**Table 1.7:** Study 1a, ANOVA; inequality*stake size on perceived attack likelihood.

| | MSE | $F$ | $p$ | Cohen's $f$ | 95% CI |
|---|---|---|---|---|---|
| Inequality | 4.08 | 0.01 | .90 | 0.004 | [0, 0.05] |
| Stake size | 1.39 | 335.11 | <.001 | 0.570 | [0.51, 0.64] |
| Ineq*Stake | 1.39 | 3.54 | .06 | 0.060 | [0, 0.12] |

results of Study 1a are discouraging for the proposed theory of envy anticipation. In this experiment, inequality did indeed cause participants to anticipate envy in their partner, but inequality did not increase the fear of any behavioural reactions from

this partner. The null results in Study 1a might be attributable to the complete-information nature of the security game. The presence of explicit probabilities and payoffs may have distracted participants from considering the larger social context of their decision. For instance, some of the comments from participants explicitly stated that they were attempting to maximize expected values. Such efforts to calculate optimal payoffs necessarily emphasize the numbers presented, with little consideration whether the surrounding context made their partner more or less likely to attack. As such, Study 1b made a number of methodological changes to decrease any potentially distracting or artificial features of the task, and to increase the salience of inequality.

### 1.4.4 Study 1b: Within-subjects inequality with uncertain decisions

Study 1b is a corrective effort after the null results in Study 1a, and seeks to refine the materials and procedure to see whether the predicted inequality-security relationship can be detected. The most notable change is that, the decisions made in Study 1b are made under uncertainty: participants only know that a successful theft has some chance of stealing some money, they do not know the exact values stolen nor the probabilities. Similarly, they do not know the effectiveness of the security product; they are simply told that the security product reduces the chances of a theft being successful. They are then told that the number of units of security consumed will be visible to the participants.

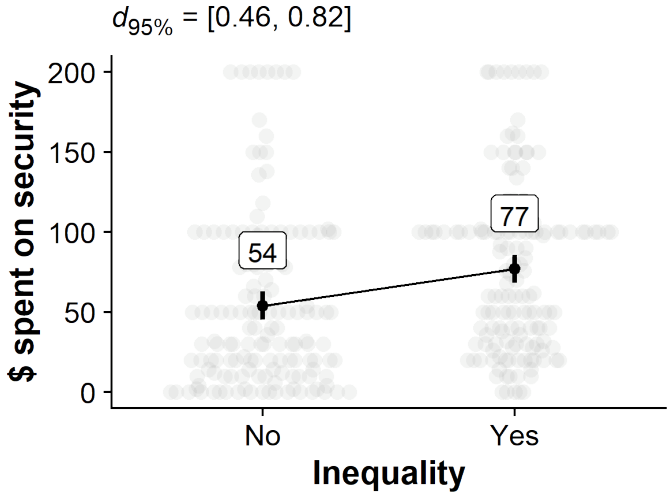
In addition to removing some potentially distracting stimuli, the introduction of uncertainty provides a better reflection of how security consumption decisions are likely made. Almost always, consumers do not know the exact stakes or probabilities of hazards. Instead, decision-makers are left to invest money in the vague hope that a security product will protect them from unknown possibilities. Another noteworthy change is that the inequality manipulation was made more salient by a within-subjects format (randomized order), and adding extra contrastive information (i.e., stating that the partner received **only $150**, emphasis in original).The security consumption variable also had its maximum range increased from 50 to 100, allowing for a larger range of responses. Likewise, since stake size was not manipulated, security spending could be used as the final dependent variable, at $2 per unit, yielding a maximum value of $200.

Lovakov, Andrey, and Elena R Agadullina. 2021. "Empirically Derived Guidelines for Effect Size Interpretation in Social Psychology." European Journal of Social

Psychology.

In order to examine the presence of effects on an exploratory basis, a pilot study was conducted without a pre-registered design. Recruiting from Prolific, this pilot study collected 154 participants, with n = 144 being retained for analysis (49.31% female). For this pilot data, significant positive effects of inequality were found for perceived partner envy, $d = 1.41$, CI(95%) = [1.19, 1.65], $t(143) = 16.97$, $p < .001$, and perceived likelihood of partner attacking, $d = 0.64$ [0.46, 0.82], $p < .001$ (Figure A.1). There was also a positive and significant effect of inequality on security spending, $d = 0.64$ [0.46, 0.82], $p < .001$ (Figure 1.7). Likewise, a mediation analysis using 5,000 bootstrapped samples found a significant indirect effect of inequality on security consumption through perceived partner envy, $b = 13.73$ [6.27, 21.53], $p <.001$ (Figure A.2). The pilot data found support for **H1)** and **H2)**, but a better-powered and pre-registered study will help to affirm the results' reliability.

**Figure 1.7:** Study 1b pilot, within-subjects effects of inequality on security spending.




*Notes:* Means (95% CI) and raw data, horizontally jittered according to kernel density.

Study 1b sought to strictly replicate its pilot study, with no changes except pre-registration (Section A.2.5) and a larger sample. Based on a pre-registered power analysis targeting $d = 0.2$ with 95% power for a paired t-test design, 310 participants were recruited, in anticipation of a 10% exclusion rate. After exclusions, n = 289 participants were retained for analyses (49.13% female).

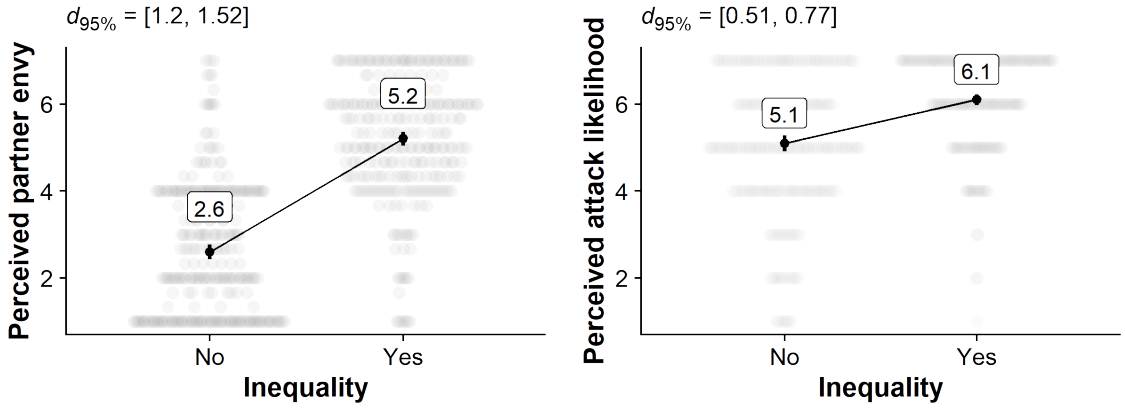
**Table 1.8:** Study 1b Descriptive Statistics

| Variable | Mean | SD | Cron. $\alpha$ 95% CI |
|---|---|---|---|
| Age | 33.7 | 11.0 | |
| Perceived envy | 3.9 | 1.1 | [0.95, 0.97] |
| Perceived attack likelihood | 5.6 | 1.1 | |
| Security consumed | 33.1 | 24.7 | |

### Results

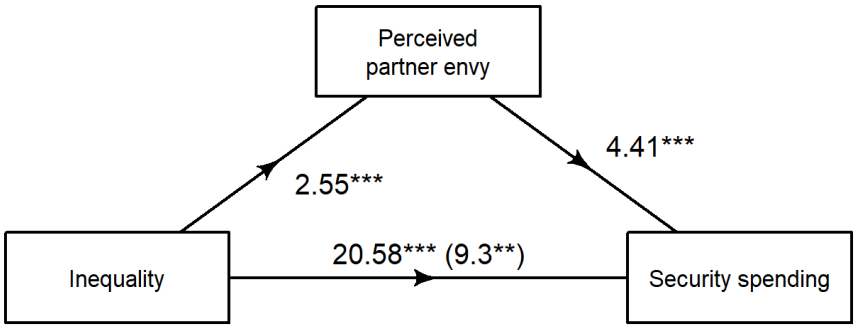
Study 1b found support for **H1)**: in the inequality condition, participants spent an average of $20.58 more on security products compared to the scenario with no inequality, $d = 0.59$ [0.46, 0.71], $p < .001$, corresponding to a medium effect size (Figure A.3). Demonstrating an effective manipulation check, there were significant positive effects of inequality on perceived partner envy and attack likelihood (Figure 1.8). Likewise, a mediation analysis detected a positive indirect effect of inequality on security spending through perceived partner envy, $b = 11.28$ [6.19, 16.46], $p <.001$, supporting **H2)** (see Figure 1.9 and Table 1.9). Consistent with trends observed in the pilot data, the mediation analysis indicated that the direct effect of inequality on security spending remained positive, $b = 9.3$ [2.96, 15.69], $p = .004$.

**Figure 1.8:** Study 1b, within-subjects effects of inequality on perceived partner envy and attack likelihood.




*Notes:* Means (95% CI) and jittered data.

**Figure 1.9:** Study 1b, mediation path diagram.




*Notes:* $^{***}p < .001$; $^{**}p < .01$; $^{*}p < .05$, Direct Effect in parentheses, Table 1.9

**Table 1.9:** Study 1b, full mediation model

| | *b* | 95% CI | *p* |
|---|---|---|---|
| Indirect Effect (ACME) | 11.28 | [6.19, 16.46] | <.001 |
| Direct Effect (ADE) | 9.30 | [2.96, 15.69] | .004 |
| Total Effect | 20.58 | [16.5, 24.61] | <.001 |
| Prop. Mediated | 0.55 | [0.3, 0.83] | <.001 |

### 1.4.5 Study 1c: Real payoffs

The use of hypothetical scenarios in Study 1a and 1b may limit the generalizability of any results. For instance, compared to hypothetical rewards, participants take fewer risks when incentivized by real payoffs and experiencing negative feedback (Hertwig & Ortmann, 2001; Xu et al., 2016). Replicating Study 1b, but with a between-subjects design, Study 1c tested **H1)** and **H2)** with participants believing that they are playing with a real partner, and that their decision could have real payoffs. Specifically, participants were entered in a lottery where one participant was selected to receive the payoff from one of their decisions. Since their partners were a sham, payoffs were calculated as though their partner did attempt an attack. In addition, Study 1c added an introductory scenario with lower payoffs and no inequality (i.e., each individual receiving $250). Given the use of a sham partner in Study 1c, participants were excluded if they refused to re-consent to their data being used. Based on a pre-registered power analysis targeting $d = 0.2$ with 80% power for an

independent-samples t-test, 704 participants were recruited. After exclusions, n = 665 participants were retained for analyses (49.32% female).

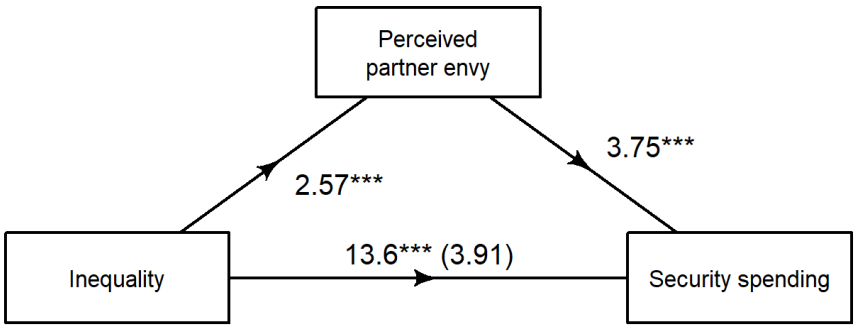
### Results

Supporting **H1)**, Study 1c found a significant and positive effect of inequality on security spending, Cohen's $f = 0.22$ [0.14, 0.3], $F(1,662) = 32.46$, $p < .001$ (Table 1.10). After controlling for security spending in the introductory scenario, participants spent an average of $13.59 more on the security product in the inequality condition (significant effects on perceived envy and attack likelihood; Tables A.5 and A.6, respectively). Replicating Study 1b, a mediation analysis found a significant and positive indirect effect of inequality on security spending through perceived partner envy, $b = 9.69$ [5.6, 13.97], $p <.001$, supporting **H2)** (Table 1.11). While Study 1b found a partial mediation of inequality through perceived partner envy, the model in Study 1c indicated a full mediation, with no significant direct effect of inequality on security spending, $b = 3.91$ [-2.32, 10.12], $p = .22$.

**Table 1.10:** Study 1c, ANCOVA; inequality on security spending.

| | df | MSE | $F$ | $p$ | Cohen's $f$ | 95% CI |
|---|---|---|---|---|---|---|
| Inequality | 1, 662 | 945.53 | 32.46 | <.001 | 0.22 | [0.14, 0.3] |
| Intro security spend. | 1, 662 | 945.53 | 1272.96 | <.001 | 1.39 | [1.28, 1.49] |

**Figure 1.10:** Study 1c, mediation path diagram.




*Notes:* ***$p < .001$; **$p < .01$; *$p < .05$, Direct Effect in parentheses, Table 1.11

**Table 1.11:** Study 1c, full mediation model.

| | *b* | 95% CI | *p* |
|---|---|---|---|
| Indirect Effect (ACME) | 9.69 | [5.6, 13.97] | <.001 |
| Direct Effect (ADE) | 3.91 | [-2.32, 10.12] | .22 |
| Total Effect | 13.60 | [8.96, 18.25] | <.001 |
| Prop. Mediated | 0.71 | [0.39, 1.22] | <.001 |

## 1.5 Discussion

Over three experiments, Study 1 provides evidence that inequality can indeed increase security consumption, and that this effect is mediated by participants' anticipation of their partner's envy. Study 1a failed to find support for **H1)** and **H2)**; although inequality increased perceived partner envy, this effect did not cascade into the expectation of partners attacking, nor security consumption. After introducing uncertain probabilities and quantities, and a within-subjects design, Study 1b found support both **H1)** and **H2)** across both a pilot and identical pre-registered procedure. Introducing the potential for real and large payoffs in a between-subjects design, Study 1c replicated Study 1b, supporting both hypotheses, and further supporting **H2)** with a full mediation of the effect of inequality through perceived partner envy.

A fairly high level of baseline distrust

The null effect in Study 1a raises the possibility that some decision features can distract participants from considering how inequality affects their peers' decision-making. The introduction of decision uncertainty in Studies 1b and 1c is a better reflection of security consumption in the day-to-day consumer context. When people make real security decisions, hazards will generally have unknown probabilities and consequences. In this case, modifying the experimental design to include uncertainty likely improves the generalizablity of the results.

A limitation to the studies in Chapter 1 is that inequality's effect on security consumption was only detected when another, low-inequality scenario was present (as in Studies 1b&c). The use of this contrasting scenario could introduce demand characteristics that influence participants' responses. When participants see both a scenario with no inequality, and another scenario with unequal wealth distributions, they may guess at how they are expected to behave, and adjust their behaviour accordingly; in this case, they might consume more security in the high-inequality condition. Concern over these demand characteristics may be mitigated by recognizing that there was still a significant effect of inequality in Study 1c, when participants had

an opportunity to win a real and large payoff. With real payoffs, participants have a larger incentive to act according to their true preferences, rather than try conform to a researcher's expectations.

In addition to potential demand characteristics, the fact that inequality's effect was only detected in the presence of a supplementary scenario raises the possibility that individuals must be made explicitly aware that inequality is present. If the effect of inequality is conditional on the presence of a contrastive scenario, the scope of the envy anticipation hypothesis would have to be substantially reduced. An informative future study would test the security game procedure with uncertainty, but without any introductory scenarios, or within-subjects designs.

Chapter 1 exclusively tested the impact of inequality in the context of a dyad of interacting partners. Consequently, its operationalization of income inequality is confounded with the participant either being better off or equally endowed compared to their partner. This confounding variable limits any strong inferences about inequality on individual behaviour. Instead, the results of Chapter 1 suggest that a person who is better-off than their peer is more likely to purchase security products. Any practical consideration of inequality should consider how those disparities manifesting in larger groups, rather than within a single pair of individuals.

How can we expect group-level inequality to impact individuals' envy anticipation and distrust when they might experience some of their own relative deprivation? An informative procedure may be a type of three-player (A,B,C) security game, where participants (A; the Defender) always get the same endowment as their potential attacker (B). Income inequality would be manipulated not between usual Defender-Attacker dyad (A-B), but rather at the larger group level, with players A and B always receiving identical endowments, and the third player (C) either receiving a larger or equal endowment compared to A and B. The context of this three-player game better resembles how inequality manifests in real-world groups, with a few individuals holding many resources, and many individuals holding few. This three-player context would be informative because it places participants in a position both to be envious, and to anticipate envy in their partner; both individuals would receive less money than the third person in the inequality condition.

If an unequal environment leaves participants just as "worse-off" as their partners, risk-sensitivity theory does not make a straightforward prediction of security consumption. At first impression, risk-sensitivity would seem to predict less security consumption with greater inequality, as individuals are worse-off than the third person, and might elicit greater risk-acceptance. However, risk-sensitivity theory predicts

greater risk-taking when a low-risk option cannot reach a desired goal state. In the case of security consumption, the high-risk option of not purchasing any security product only reduces the likelihood of losing more money; this risky choice does not offer any chance for the individual to reach the level of a lofty comparison standard (player C).

Conversely, in this three-player context, the Attacker's (B) risky option of stealing could offer them a chance of improving both their absolute and relative income. By attacking, player B can try to exploit their neighbor (player A), and if successful, they can potentially reach the income level of the relatively privileged player C. In this case, an envious player B remains a real threat to player A, and gives an incentive for player A to anticipate this envy, and purchase more security, even if player A is not the beneficiary of an unequal distribution.

An interesting test of these mechanisms would be to allow participants to occupy both the roles of Attacker and Defender; for instance, participants (player A) could have the option to attack the third party (player C), but they would also face the risk of being attacked by player B. In this case, Player A can try to improve their relative standing. It could be the case that Player A would exhibit an all-or-nothing risky decision, for instance choosing to try stealing, but purchasing no security; vice versa, they could purchase security, and choose not to steal. However, most participants in Chapter one display some degree of risk-intermediate preferences, purchasing some security, but not the maximum possible. Similarly, many participants may choose to purchase security products, and also try stealing from their partner. This latter possibility is reminiscent of the Hobbesian Trap, with a fear spiral leading to poorer individual and collective outcomes, as individuals are preoccupied by the desire and fear of stealing, leading to a life that "is solitary, poor, nasty, brutish and short."

Together, Chapter 1 provides initial support for the envy anticipation model of distrust. When they received more money than their peer, participants did indeed consume more of a security product that could protect them from the potential harmful actions of a partner. The results also suggest that the positive effect of inequality on security spending was accounted for by an increase in participants' beliefs that their partner was envious. These effects were replicated in a non-hypothetical scenario, when participants had an opportunity to get a large payoff due to their decision-making. The joint support of **H1)** and **H2)** indicates that inequality can make people expect the worst from their peers, and invest in costly measures to protect themselves from potentially antagonistic behaviours. However, some further research is still needed to explore the possible roles of supplementary scenarios in the effect, which could

introduce demand effects or reduce the generalizability of the effect of inequality on security spending. The next chapter will expand on the effects of economic inequality on distrust by exploring the role of distributional fairness, particularly through manipulations of equity and group-based discrimination.

# Chapter 2

# Effects of Fairness on Security Consumption

When considering how inequality affects individuals and groups, it is important to consider not just whether resources are being shared evenly, but also whether the distribution is fair. In particular, inequality coupled with unfairness should make individuals increasingly unaccepting of disparities, and heightens their changes of engaging in risky and hostile behaviours (van de Ven, Zeelenberg, & Pieters, 2009). Conversely, an unequal, but fair allocation of wealth should reduce partners' harmful potential. Extending Chapter One, the following chapter will explore whether distributional fairness has cascading effects on distrust and security consumption within individuals.

Why do people care about fairness? As discussed in Chapter one, a concern for fairness may have evolved (whether culturally or genetically) as a response to disadvantageous inequality aversion. By internalizing a preference to treat people fairly, an individual can better resist selfish temptations, and prevent themselves from eliciting envious feelings in their partners, and any destructive behavioural consequences. Similarly, classifying of an individual's behaviour as fair or unfair can help to apply other decision rules, such as generous tit-for-tat reciprocity (e.g., Rand, Ohtsuki, & Nowak, 2009), and forming reputation judgments. A reputation for being impartial (and fair) is rewarded by being preferred as a cooperation partner (Shaw, DeScioli, & Olson, 2012).

Beyond simple pairs of interacting individuals, fairness has also been argued to be a linchpin for allowing the sustainability of cooperative strategies, and the development of large-scale societies (Henrich et al., 2010). Some of the most common explanations for cooperative behaviours in humans, such as reciprocity (help those who have helped

you) and kin support (help your family) fall short of explaining the full scope of human cooperation. Even early human societies were characterized by a low proportion of genetic relations, and a high degree of intergroup migration. Especially with increasing group size, reciprocity and kin support would not provide a sufficient reason to cooperate with someone you do not know, and is not related to you by blood. While preferred cooperative partners might not always be available, reputations are comparatively persistent, allowing for individuals to selectively cooperate with those known for fair dealing. Having a shared idea of how a person should behave (i.e., common fairness norms) has proved to be a potent force in coordinating large groups of otherwise unrelated individuals towards a shared purpose. In the context of inequality, a distribution's adherence to fairness norms can help individuals to decide whether a disparity should be tolerated, and whether some type of corrective action can be expected.

## 2.1 Envy and Fairness

The main proposition of Chapter one is that inequality leads to individuals anticipate envy, and subsequently malicious intentions and risk-taking behaviours from their social partners. These expectations in turn lead to reduced trust and an increased willingness to consume security products. However, some research suggests that not all envious reactions are equal, and that these experiences of envy are shaped by the perceived fairness of an observed disparity.

A recent perspective on individuals' social comparisons has posited a "benign" as well as a "malicious" envy. Benign envy is associated with a sense of control and motivation to improve oneself, in attempt to "reach up" to the level of a better-off other. By contrast, malicious envy is characterized by a desire to harm the comparison target, and "dragging them down" to their level. Benign envy is argued to occur when the person believes that the envied person's advantage is deserved, while malicious envy is elicited when the individual believes that their environment is unfair (van de Ven, Zeelenberg, & Pieters, 2012).

Echoing the fairness-driven concepts of benign and malicious envy, some authors have suggested that people are not actually inequality-averse, but instead inequity-averse, predicting that people can actually be very tolerant of an unequal, but fair outcome. For instance, rejection rates in the Ultimatum game (and offer sizes) decrease when roles are earned through task performance (Fleiß, 2015). Meanwhile, Responders reject offers when inequalities are due to an intentional and clear decision to produce

disparities (Blount, 1995; Falk, Fehr, & Fischbacher, 2003).

Behavioural economists have found that participants who earned an advantaged position (versus random assignment) are less generous towards their fellow participants (Hoffman, McCabe, Shachat, & Smith, 1994). Similarly, (van de Ven, Zeelenberg, & Pieters, 2010) found that under conditions of inequality, those who are better off are more likely to engage in helping behaviours when they believe that their disadvantaged partner is frustrated by the disparity and harboring antagonistic motivations [i.e., "malicious envy"; Crusius & Lange, -Crusius & Lange (2014)]. By contrast, "winners" are actually less likely to help if they believe that the observed disparity has motivated their partner to improve themselves (i.e., the partner experiences "benign envy"). While inequality generally causes envy, the emotional and motivational content of these experiences appears to be heavily influenced by the perceived fairness and deservingness of these distributions.

When participants believe that their partner is experiencing malicious envy, they are more likely to engage in helping behaviours (van de Ven et al., 2010). In contrast, participants who earned an advantaged position (which is associated with benign envy) tend to be less generous (Hoffman et al., 1994). Together, the existing research suggests that people are not just sensitive to inequality and fairness, but that their decision-making also considers and anticipates others' psychological reactions to disparities. To this end, an additional proposition can be added to the *envy anticipation model of distrust*: the positive effect of inequality on distrust and security consumption will be negatively moderated by distributional fairness. Chapter 2 will examine whether merit-based (Study 2a) and apparently group-based allocations of wealth (Study 2b) impact security spending in the context of inequality.

## 2.2 Merit as a Fairness Norm

Distributional fairness may impact individuals' anticipation of envy from their partners, and their subsequent willingness to consume security products. When evaluating fair distributions, the best rule is often one that everyone follows. At least among western industrialized societies, fairness norms appear to be heavily influenced by meritocratic concerns. In a sample of Norwegian children (grades 5 to 13), Almås, Cappelen, Sørensen, & Tungodden (2010) found that younger participants were strict egalitarians (averse to any inequality). However, as the participants got older, they increasingly accepted inequality when the disparities came from differences in production. While inequality may be an important antecedent to conflict, Study 2a will test whether

a meritocratic distribution of wealth will attenuate inequality's effect on security consumption.

Like most theories of other-regarding preferences, this envy-anticipation theory of distrust and security seeks to incorporate the role of fairness into individuals' expectations of discrete envy. If the first chapter is predicated on the expectation of envy in response to inequality, it seems reasonable that individuals will become more distrustful in unfair environments. This chapter will test the proposition that *the effect of inequality on security consumption will be negatively moderated by greater distributional fairness.* This proposition takes on hypotheses to be tested in Studies 2a and 2b, respectively: **H3)** In a 2b*2b ANOVA, the inequality-merit interaction will have a significantly negative effect on security spending., and as planned comparisons, **H3a)** hi-inequality/random income condition will have significantly higher rates of security consumption than all other conditions (i.e., **.1)** hi-inequality/merit income, **.2)** no-inequality/random income, & **.3)** no-inequality/merit income).

## 2.3 Methods

### 2.3.1 A priori power analysis

Given the unknown size of the hypothesized inequality*merit interaction, this study targeted an effect size of cohen's f = .1. To achieve 95% power, studies must have a minimum of 327 participants per cell, or a study-wide total of n = 1,308. This large sample size is necessary given the possibility of a "no-way" interaction, where only one condition is significantly different from the remaining conditions (Simonsohn, 2015). In a no-way interaction, the simple effect of inequality is nullified depending on the moderator, and thus requires at least twice as many subjects per cell to achieve as much power as the simple effect. Figures A.5 and A.6 display the pattern of means and per-condition sample size thresholds to obtain 95% power. Additionally, see Tables 2.1 and A.7 for the power and effect sizes of the ANOVA and pairwise comparisons, respectively.

### 2.3.2 Study 2a: Meritocracy and Inequality

Groups will always be characterized by inequality through some type of outcome, whether wealth, income, or status. Indeed, a popular idea is that inequality propels individuals towards productive behaviours (Stearns, 1999). In this line of reasoning, the presence of inequality may be less of an issue compared to how the inequal-

**Table 2.1:** A priori power of main and interaction effects for 2b*2b factorial designs (Studies 2a,b)

| | power | partial_eta_squared | cohen_f | non_centrality |
|---|---|---|---|---|
| inequality | 95.05 | 0.0099 | 0.1 | 13.05 |
| undeserved | 95.05 | 0.0099 | 0.1 | 13.05 |
| inequality:undeserved | 95.05 | 0.0099 | 0.1 | 13.05 |

ity was produced. As planned comparisons, Study 2a will examine whether **H3a)** hi-inequality/random income condition will have significantly higher rates of security consumption than all other conditions (i.e., **.1)** hi-inequality/merit income, **.2)** no-inequality/random income, & **.3)** no-inequality/merit income. Study 2a again varies inequality (yes vs. no) as the previous studies, but also manipulates whether one's income is seemingly allocated *randomly or based on task performance* (random vs. merit).

Participants will be filtered for participation through computer (vs. smartphone/tablet). For this random vs. merit manipulation, all participants will complete a slider task, where they have one minute to correctly place 40 sliders at the midpoint of various lines that appear on their screens. Participants will then either be told that their incomes were allocated randomly, or because they correctly placed more sliders than their partner. The slider task has been used in several studies to examine the effects of effort in distributional preferences, and has demonstrated that individuals that win their income through performance are less likely to support redistribution of effort-driven income allocations (Cassar & Klein, 2019).

As a manipulation check (and in addition to the envy items in Study 1), before making their consumption decision, participants will be asked to report whether they believed the incomes were disbursed fairly:

"My partner's assigned income is fair"
"My assigned income is fair"
"The way incomes were given for this game is fair"

Participants were excluded from analyses if they completed fewer than five sliders, which would both indicate low engagement in the study and likely undermine the credibility of the manipulation. Results were analyzed using a 2b*2b between-subjects ANOVA.

### 2.3.3 Study 2b: Inequality as a Product of Group-Based Discrimination

Fairness appears to have its roots in avoiding perceptions of partiality. For instance, young children will discard a resource in order to avoid sharing it unequally (Shaw & Olson, 2012). Likewise, individuals are more willing to behave unfairly towards others if they could still appear to be impartial (Shaw et al., 2014); for instance, in the Dictator game, individuals will accept a smaller payoff if they can skip the game and avoid informing partners of their selfish choice (Dana, Cain, & Dawes, 2006). Acts of partiality often signify the formation of alliances, which inherently exclude others (DeScioli & Kurzban, 2009). Since substantial interpersonal and intergroup conflict is rooted in the perception of unfairness and discrimination (Tajfel, Turner, Austin, & Worchel, 1979), it seems important to understand how fairness influences willingness to consume security goods.

Since Study 2a directly compares meritocratic versus random allocation of resources, it does not answer the more fundamental question: do people become distrustful and consume more security goods when inequality appears to be more discriminatory and unfair? Study 2b will keep a constant presence of inequality, but manipulate whether the partner is a member of an ingroup or outgroup, and whether the incomes are distributed randomly, or by a separate *ingroup individual*. Study 2b will use a 2b*2b between subjects ANOVA to test whether **H4)** the effect of partner's outgroup membership on security consumption will be more positive when inequality is produced by the choice of an ingroup member (versus $\mathbf{H4_0}$**)** no significant outgroup*agentic-distribution interaction). As planned comparisons, Study 2b will examine whether **H4a)** the outgroup partner/agentic-distribution condition will have significantly higher rates of security consumption than all other conditions (i.e., **.1)** outgroup/random, **.2)** ingroup/agentic, & **.3)** ingroup/random). Participants will complete the same fairness manipulation check as and be powered for the same effect as in Study 2a.

Particularly in the presence of increasingly diverse societies, an understanding of how people respond to perceived threats posed by "others" may be instrumental in engineering social infrastructure and policies to improve people's real and felt security.

# Chapter 3

# Nation-Level Inequality and Individual-Level Security Consumption

Security consumption is a particularly interesting phenomenon because it often reflects an individual-level effort to address a collective problem. A consumer's efforts to improve security generally attempt to decouple oneself from the shared risks of victimization. These consumption decisions allow individuals to tailor their protection to their unique perceived need, but they may also introduce inefficiencies into the collective pursuit of security.

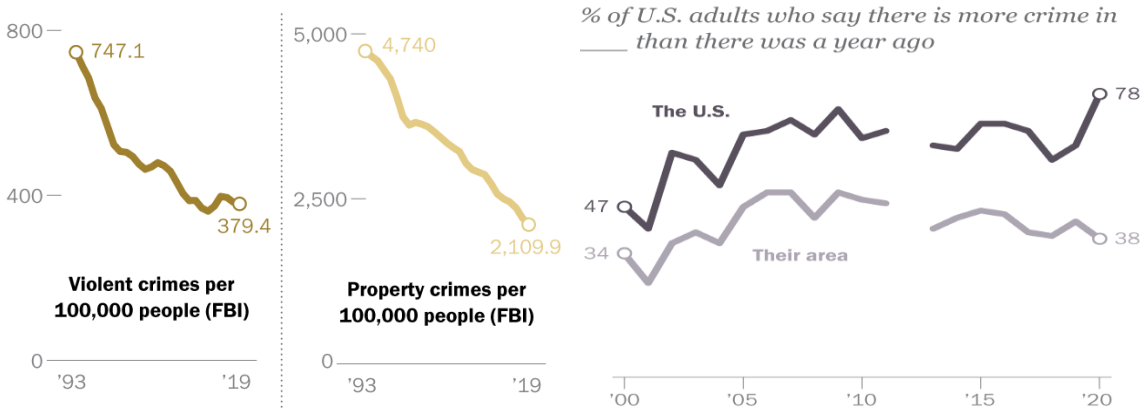
For instance, in some cases, security consumption is clearly an overreaction to perceived threats. Consider the case of building high walls around one's home, as seen extensively in high-inequality regions such as South Africa. Paradoxically, costly fortifications can actually increase the vulnerability of one's home and community, as reduced visibility within the neighborhood allows for criminals to enter properties without detection (Marks & Overall, 2015). This chapter seeks to elaborate on the collective (nation-level) effects of inequality on individual-level security consumption.

The highly-controlled nature of the experiments in Chapters 1 and 2 allow for precise tests of the hypothesized inequality-security consumption relationship. Although one experiment offered real consequences for participants' choices, economic games are highly artificial by design. Insights generated from the security game might not generalize to consumers' daily consumption context. To provide a test of the envy anticipation model with greater external validity, this project supplements these previous experimental results with a multilevel analysis of archival data on respondents' use of security products, to test whether **H5)** nation-level inequality will be positively

associated with consumption of security products (versus **H5$_0$**) no significant effect of nation-level inequality). A significant relationship between nation-level inequality and individual security measures does not serve as an an exhaustive test of the envy anticipation hypothesis; a positive effect would likewise be expected under the other models of inequality-distrust discussed in Chapter 1 (social barriers, social inferences, resource conflict, opportunity cost, psychosocial effect). As such, the earlier experiments and this multilevel analysis shall serve as complements to each other, testing the envy anticipation model of distrust with high internal and external validity, respectively.

## 3.1 Fear and Security in the Real World

**Figure 3.1:** Actual and perceived U.S. crime trends (Gramlich, 2020)




Just as a multilevel analysis examines the effects of inequality on security measures beyond a tightly-controlled experiment, it is also important to consider how individual's psychological and behavioural responses to the threat of crime manifest in their day-to-day lives. Individuals' experiences of crime, and their efforts to protect themselves against it, have been extensive subjects of study in subfields of sociology: criminology and victimology. Perhaps one of the most striking findings from these inquiries is that people's perception of crime rates, as well as their fear of crime, have tended to remain high despite objective crime rate estimates dropping (Figure 3.1).

Official crime estimates declined in the U.S. between 1980 and 2013 (for instance, recorded homicides dropping from 20,000 cases in 1980 to 12,000 in 2013). However, during this same period, surveyed perceptions and fear of crime remained essentially

the same; in 1980, 37% of respondents reported a perceived in increase in crime since the last year, in 2013, 41% reported an increase (reviewed in Rader, 2017). This gap between real and perceived crime rates suggests that people's thinking on crime emerges from something more than simply reacting to observed criminality. These perceptions of crime seem to have a meaningful impact on people's attitudes and behaviours. Forde (1993) examined survey data throughout the 80s and 90s in Winnipeg, Manitoba, and found that individuals who perceived crime as increasing generally reported feeling less safe, and had generally negative attitudes towards walking alone at nighttime.

Borrowing from methods used in sociology, a multilevel analysis allows for participants to be compared both as individuals, as well as members of higher-level groups (e.g., countries, neighborhoods). Similarly, a multilevel analysis of survey data allows for the assessment of community members' real-life experiences, instead of the fabricated circumstances and measures used in experimental designs. Accordingly, it is important to consider the macro (societal), meso (group), and and micro (individual) factors that lead to the adoption of security measures.

### 3.1.1 Micro predictors of fear and security

Some of the most obvious documented predictors of security measures are individuals' experiences and fears of criminal victimization (Barnes & Ephross, 1994; Frieze, Hymer, & Greenberg, 1987; Myers Jr & Chung, 1998; San-Juan, Vozmediano, & Vergara, 2012; D. A. Smith & Uchida, 1988; Weinstein, 1989). For instance, Guedes, Domingos, & Cardoso (2018) found that perceived victimization risk is associated with fear of crime ($r = .49$), and each of these psychological phenomena are associated with the adoption of security behaviours ($r$s = .22, .23, respectively). Moreover, the relationship between victimization and security measures is mediated by increased fear of crime and perceived risk of victimization (Giblin, Burruss, Corsaro, & Schafer, 2012). This pathway of victimization to security measures is predicted by Ferraro (1995)'s risk interpretation model, which posits that individuals' perceived need to protect themselves from their environment is informed by their own personal experiences. Similarly, experiencing more severe crimes leads to greater adoption of security measures, which has also been found to be mediated by distress, perceived victimization risk, and fear of crime (Chadee, Williams, & Bachew, 2020).

Other studies document positive relationships between fear of crime and perceived victimization risk (LaGrange, Ferraro, & Supancic, 1992; Ortega & Myles, 1987; Reid,

Roberts, & Hilliard, 1998), and more have linked fear of crime to greater security measures (Jackson & Gray, 2010; Keane, 1998; Mesch & Fishman, 1998; Ortega & Myles, 1987; Reid et al., 1998; Riger, Gordon, & LeBailly, 1982; Rountree & Land, 1996; Williams & Singh, 1994). Kleck, Kovandzic, Saber, & Hauser (2011) found that perceived risk of criminal victimization, increases gun purchasing intentions amongst non-owners (and increases the likelihood of owning a gun for defensive reasons). Although there are notable variations in some of these results (e.g., a stronger relationship between fear of crime and household (vs. personal) security measures, Mesch & Fishman, 1998), the general consensus is that these victimization variables are positively associated with one another.

Fear of crime, and security measures by extension, have been of substantial interest to researchers, seeming to act as a "sponge" that also expresses vague social concerns, disconnected from any real threat of victimization (Jackson, 2004). Taylor & Hale (1986) summarized three models of how fear of crime originates: 1) indirect victimization, with those more likely to be exposed to crime information, and/or those least able to cope with criminal attempts, 2) perceived disorder, caused by increased exposure to 'incivilities', and 3) community concern, with an interaction of increasing crime and lack of structural ties causing increased concerns and fear.

Since the majority of studies in victimology are cross-sectional, there has been limited ability to assess the causal order of the the relationships between fear of crime, security measures, and perceived victimization risk. However, some evidence combining archival individual victimization data and simultaneous equation techniques points to a reciprocal and reinforcing relationship between people's fear of crime, perceived risk, and their adopted security measures. Liska, Sanchirico, & Reed (1988) found that 'constrained behaviors' and fear of crime were reciprocal, with a feedback loop occurring between these two variables. They claimed these behaviours constrain "social behavior and thus results in avoiding such situations, which in turn accentuates fear" (p. 835) (see also Ferraro, 1995; Rountree, 1998; W. R. Smith & Torstensson, 1997). Similarly, Rader (2004) suggests, "it may be the case that people remain afraid even after buying a security system because the security system is a constant reminder of the potential threat of victimization that might occur if they do not continue to engage in such behaviors" (p. 699-700). These results should be discouraging for potential consumers of security products; they may actually feel less safe after their purchases.

Other authors have highlighted some important distinctions between fear of crimes against property versus crimes against persons. In an survey of Israeli community

members, Mesch & Fishman (1998) found that fear of crime was more often reported for one's family than for oneself. They also found that protective measures were more likely to be adopted for property offenses than personal crimes. Likewise, only the fear of burglary significantly affected the likelihood of household protective action, and in a full regression, fear of sexual assault, fear of being home at night, fear of family being assaulted, and fear of family being sexual assaulted, were all predictive of personal security measures. Thus, these extra considerations in the conceptualization of ear of crime do indeed to improve the concept's ability to predict the adoption of security measures.

### 3.1.2 Meso predictors of fear and security

Meso systems are probably the most flexible 'level' of society to define. Meso could by used to describe group of individuals playing pick-up sports, a neighborhood, or a powerful multi-national corporation. Perhaps surprisingly then, meso-level predictors of security measures seem to have been scarcely studied. For instance, San-Juan, Vozmediano, & Vergara (2010) examined a city in northern Spain, and found that residential districts had significantly different adoption rates of security measures. However, they did not carry out any other analyses to further examine the source of the variation (which is probably for the best since they only examined eight districts). D. A. Smith & Uchida (1988) analyzed some aggregated neighborhood-level data (but without a multilevel model), and found that individuals in some areas were more likely to have security measures, for instance, those neighborhoods with more single-parent households. Those few meso level studies generally have substantial limitations, likely due to the difficulty of surveying a sufficient number of meso-level and individual-level units. A good example of a meso-level study examined fear of crime. In a study across 5,196 british neighborhoods and 102,133 individuals, Brunton-Smith & Sturgis (2011) found a number of regional predictors of fear of crime: greater economic disadvantage, urbanization, younger population, housing supply, ethnic heterogeneity, local crime, and disorder. Additionally, they found that individual predictors included being: female, younger, uneducated, nonwhite, a newspaper reader, crime victim, not married, ill, and a long-time resident. While Study 3 will only examine micro- and macro-level predictors of security, there is defnitely predictive value to incorporating predictors at the intermediate meso level/

### 3.1.3 Macro predictors of fear and security

Lastly, macro-level factors reflect the largest-scale influences in a country. These can range from economic, cultural, legal, political, and ecological factors. Using the European Social Survey, Vauclair & Bratanova (2017) found that individuals in more unequal countries reported a greater fear of crime. The authors also report that income inequality's effect on fear of crime is strongest among ethnic *majority* members. This inequality*ingroup interaction was in direct contradiction to a key hypothesis in the fear of crime literature: the sense of vulnerability, which predicts that the groups most objectively vulnerable to crime [e.g., older individuals, women, minority groups\; Hale (1996)] should be the most afraid. One sobering interpretation of this fear amongst majority groups is the notion of fear of crime as a "sponge," suggesting that 'people are not as "fearful" of personally being victimized as often we think; rather, they are expressing their social concerns through the symbolically dense concept of crime' (Jackson, 2004, p. 962). However, this interaction effect must be interpreted with caution, as estimates of cross-level interactions are often unstable when cluster sizes are fewer than 50 (Hox, Moerbeek, & Van de Schoot, 2018).

Other studies provide mixed evidence of the inequality-security/fear relationship. After controlling for many country-level factors (e.g., Benefits in kind for families/children; unemployment rate; registered crimes), Hummelsheim, Hirtenlehner, Jackson, & Oberwittler (2011) found that across 23 European countries, nation-level Gini had no incremental effect when predicting fear of crime. Corvalan & Pazzona (2022) reports a significant relationship between nation-level inequality and aggregated "private security" measures. However, the authors do not report any methodological details for their analysis, and only report on effects at the country-level. One must be cautious in such a case to avoid the ecological fallacy, where observations and inferences of groups are erroneously applied to the group members. In this case, while an aggregate inequality-security correlation may be observed, this relationship may not persist when making predictions for individuals. Likewise, they present a model whereby inequality increases crime and security consumption, through the increased material incentives of (preventing) criminality.

## 3.2 Methods

The relationship between country-level inequality and the consumption of security products was tested in Study 3 using multilevel regressions. These analyses were

accomplished using a combination of three archival datasets: the ICVS (International Crime Victimization Survey; for individual-level measures, including security; Van Kesteren 2010), the Standardized World Income Inequality Database (SWIID; for nation inequality; Solt, 2020), and the Penn World Tables (PWT, version 10.0; for nation GDP; Feenstra, Inklaar, & Timmer, 2015). These databases were accessed as follows: ICVS, September 1, 2020; SWIID, October 21, 2020, and PWT, March 17, 2021. After a period of data exploration, cleaning, and pilot-testing, the procedure for testing **H5)** has been pre-registered (Section A.5) prior to any hypothesis-relevant analyses. Pre-registration is beneficial when analyzing archival data, as such data-dependent analysis, the so-called "garden of forking paths" (Gelman & Loken, 2014) allows a researcher to pick and choose those data and analyses that best suit their preferred conclusions (Simmons, Nelson, & Simonsohn, 2011). Hopefully, a pre-commitment to preprocessing and analysis plans before model estimation reduces these researcher degrees of freedom, and leads to the transparent selection of a more procedurally-sound model.

### 3.2.1 Data Inclusion and Exclusion

Indicators of individual security consumption and other person-level measures have been accessed from the ICVS (Van Kesteren, 2010), which has surveyed over 300,000 people across 75 different countries since 1989 on householders' experiences with crime, policing, crime prevention, and feelings of unsafety. Data screening commenced with the ICVS, to find those years and countries that had data to the research aims, particularly participants' reported usage of security measures. The ICVS has been collected across eight different sweeps (1: 1989 [13 countries], 2: 1992-1994 [29], 3: 1995-1998 [44], 4: 1999-2003 [46], 5:2004-2006 [18], 6: 2010 [6], 7: 2014 [5], 8: 2015 [1]; with an additional EU-specific sweep in 2005 [16]).

The ICVS is not a fully standardized survey, and thus varies across countries in its procedure. For instance, sampling methods include 3 stage stratified samples, random digit dialing, and random sample stratified by administrative regions. Likewise, interviews were conducted both with face-to-face and computer-assisted-telephone-interviewing.

Importantly, in each of the sweeps in the ICVS, there is substantial variation in which countries are surveyed, and the questionnaires used. There have been over a dozen different questionnaires that have been implemented across the ICVS's lifespan. With so much variability, one cannot simply compute and compare variables without

screening and cleaning. Otherwise, unfair comparisons will be made, such as comparing Norway in 2005 to Indonesia in 1989, with respondents in Norway not even being asked any questions on their security consumption. Some extensive screening was conducted to guide the selection of countries and time periods for analysis, to ensure the comparability of responses. Personal correspondence with an author and maintainer of the ICVS has revealed that the items used in questionnaires were screened for comparability before combination into the full dataset.

The main analysis for this study opts to combine the 2004-2006 survey sweep with the 2005 EU-specific sweep, yielding an initial count of 34 countries. These sweeps are perfectly complementary, with no countries being measured twice. Just under half of all ICVS-surveyed countries were included between 2004-2006. This 2004-2006 window offers some notable benefits: relatively recent data, a small time window encompassing each observation, and a sufficient number of level 2 clustering variables (30+ clusters are recommended for multilevel models; Snijders & Bosker, 2011). As will be described later, another sweep from 1999-2003 was also analyzed as part of a pre-registered robustness check for the 2004-06 results.

After selecting a time period (2004-06) and cleaning the nation-level identifiers, further screening was guided by attempting to choose the greatest number of relevant variables for analysis. For instance, Hong Kong was dropped due to complete missingness for participant age. Similarly, individual observations were dropped if they were missing responses on age, gender, or employment status. Multiple security measures are also surveyed in the ICVS. These items were screened to assess which variables would be used to construct the dependent variable of security consumption: watch dog, high fence, neighborhood watch scheme, burglar alarm, special door locks, special grills, caretaker security, and gun ownership.

Some of the data collection in the ICVS was ordered and financed by specific governments. As a result, there can be some inconsistencies in the data. For instance, the 2005 period has duplicated data for the UK, due to some separate contracting between the EU and UK governments. These duplicated data have some greater granularity, including Scotland and Northern Ireland as separate countries. Unfortunately, these smaller regions are less likely to systematically collect country-level data, particularly not collecting inequality data regularly, so are only represented at the level of the UK.

Approximately 90 participants were dropped because they reported living in an institution, so were not asked any security questions. A reporting feature in the ICVS bears mention. For binary items, such as these above prevention measures, “yes” responses are coded as a “1,” whereas “no” answers are coded as missing, effectively

not checking a box. As such, it leaves ambiguity as to whether a blank response is a "no," or a true missing value. Fortunately, there is an item for participants to indicate a refusal to respond, so participants were excluded if they refused response (Similarly, participants were excluded if they reported "do not know" when asked about their security measures). As an additional check, each country was screened to see whether any of them had *only* missing values for any of the measures. Switzerland had missing responses for all security items, this country and all its respondents were removed.

Observations were likewise excluded from the final 2004-6 dataset if they were missing on age, gender, employment status, and number of victimization experiences. After excluding for missing data on security and key independent variables, k = 32 countries, and n = 79,776 participants were retained for primary analyses (average 2,523 respondents per country). Those retained countries were as follows:

> Argentina, Australia, Austria, Belgium, Bulgaria, Canada, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Italy, Japan, Lithuania, Luxembourg, Mexico, Netherlands, New Zealand, Norway, Peru, Poland, Portugal, South Africa, Spain, Sweden, Turkey, United Kingdom, and the United States

A notable limitation to this 2004-06 ICVS data is the over-representation of WEIRD countries (Western, Educated, Industrialized, Rich, Democratic). Other, less WEIRD, and more unequal countries (e.g., phillipines, nigeria, india) are better represented throughout sweeps 2 to 4. Further screening information can be found in the supplementary materials (Section A.5), but as a summary, this 2004-06 period provides a balance of relative data recency, larger sample size, and a larger range of values on key variables such as inequality and victimization experiences.

### 3.2.2 Measurement and Variables

#### Outcome variable

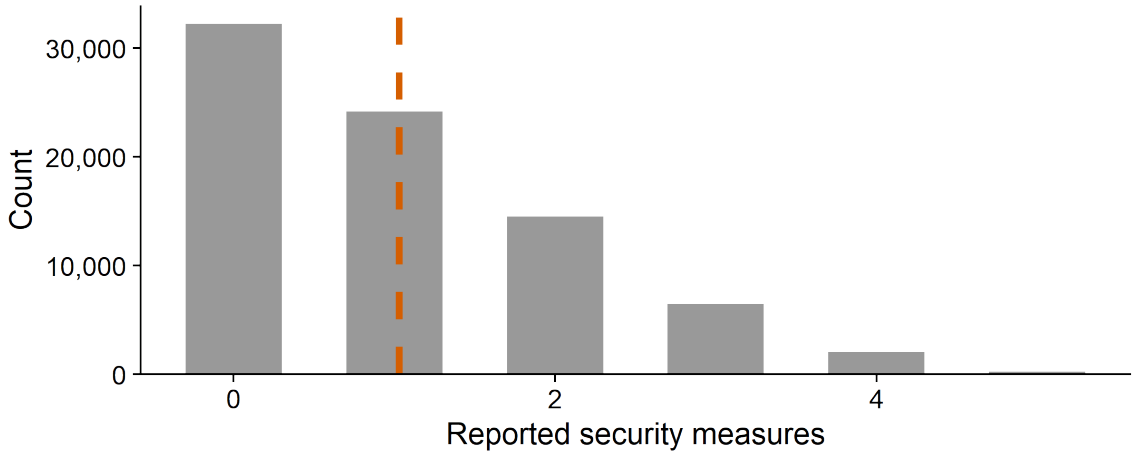
A reduced group of prevention measures included burglar alarms, special door locks, special grills, high fence, caretaker security, and gun ownership. However, gun ownership was missing for South Africa, Peru, and Japan. Each of these countries are valuable in making the data more representative of non-WEIRD countries; additionally, South Africa and Peru are some of the highest-inequality countries in this dataset. Excluding these countries and restricting the range of observations for the sake of one additional security measure did not seem to be an acceptable trade-off, so gun ownership was dropped from the computation of this study's dependent variable.

Security consumption ($M = 1.03$, $SD = 1.09$) has been computed by summing respondents' self-reported ownership of 5 different preventative measures:

> burglar alarm, special door locks, window grills, high perimeter fence, and presence of a caretaker who provides security.

This security consumption variable excludes several security items in the ICVS due to missing data, such as owning a watch dog, having surveillance arrangements with neighbors, and purchasing insurance against criminal activities.

**Figure 3.2:** Count of security measures in the ICVS (Study 3).




*Notes:* Dotted line indicates mean ($M$ =1.03)

Security consumption may hold some methodological benefits from psychological self-report measures of fear of crime and perceived victimization risk. For instance, a reported sense of fear or vulnerability may be viewed as unfavorable attitudes to report, especially for men. "Sutton and Farral (2005) even argue that, in reality, men might have a greater fear of crime than women but it is a private experience" (San-Juan et al., 2012, p. 663). In order to avoid reporting their fear, individuals might under-report out of social desirability. By comparison, the existence of a security measure is a far more concrete, and less subjective question; rather than having to gauge a potentially uncomfortable belief or feeling, individuals just have to make a binary observation about a feature of their living space, arguably leaving less room for biased responses.

### Predictor variables

**Individual measures.** Similar to security measures, self-reported crime victimization was computed as a count variable for the following items: car thefts, bicycle

thefts, burglaries, attempted burglaries, robberies, and thefts, summing six binary items on respondent's experiences of victimization. Similar to security measures, not all items appeared to have been administered in all surveys. For instance, Peru is missing responses on assaults, and Australia is missing sexual offences. Reported victimizations will be represented twice in the analyses - once as a nation-average value, and again for the individual respondent, capturing both the contextual effects of a high-crime country, and individuals' own experiences of victimization. Self-reported victimization has been suggested to be a superior way to measure a country's crime rates, as official crime statistics can face many data integrity issues, such as systematic under-reporting (Dijk, Kesteren, & Smit, 2007). For instance, Soares (2004) suggests that poor countries tend to under-report crime more than rich countries. Under-reporting or under-recording might also be a problem in autocratic regimes.

Age was reported in five-year bins, ranging from 16 to 70+. Individuals were coded as employed if they reported working or being in the army, and not employed if they were "looking for work," "keeping home," "retired," "disabled," "still in school," or "other." Gender was recorded as either male or female.

The targeted model sought to include predictor variables at country- and individual-levels: at the nation level, 1) nation inequality (Gini - disposable income), 2) expenditure-side GDP per capita (matched purchasing power parity), and 3) nation-level self-reported crime victimization (computed from individual measures in the ICVS). A notable preprocessing step is that Gini and GDP values were averaged across 2004-2006 into single indices, to accommodate all of the years where the survey sweeps were conducted. At the respondent level: 1) age (five-year increments), 2) gender (58% female), 3) employment status (49% employed), and 4) number of individual victimization experiences. See Table 3.1 for remaining descriptive statistics, and Appendix Section A.5 for more detailed data cleaning and preparation procedures.

**Income Inequality.** For any efforts to make nation-level comparisons, consistency of statistics across times and countries are a challenge. Comparing income inequality is no different. The most prevalent measure of income inequality, the Gini index, is constructed by surveying the income of sampled households in a target population, and calculating the degree of wealth dispersion (0 being complete equality, 100 being complete inequality). However, measures of household income can vary across countries and times. For instance, since Gini coefficients are calculated from households and not individuals, researchers have made different choices whether and how to adjust incomes for the number of household members (e.g., comparing a household of one

person making $100,000 to a household of two adults and three children making $120,000). In this case, simply copying a Gini index from a second dataset with a different computation risks inaccurate comparisons with a corresponding excessive amount of confidence in the value itself. However, if a researcher only wanted perfectly comparable indices, they would be forced to throw out the majority of available Gini observations. To this end, the SWIID has been constructed in order to maximize the use of available inequality observations, while incorporating any uncomparability as measurement error.

The SWIID is based on the Luxembourg Inequality Study (LIS; AKA. LIS Cross-National Data Center), which consists of micro-level data across countries, harmonized for cross-country comparability. The LIS uses the square-root of household members in order to compare units. These LIS observations serve as baselines to compare against other Gini estimates in the same country-year. These relationships are then used to predict the value of the LIS Gini, when data from the LIS is absent, but other information is available. Together, the SWIID is made up of "15,730 Gini coefficients from 2,984 country-years in 196 countries or territories" (Solt, 2020, p. 5), all extracted from 384 different sources. The SWIID has been tested with 'k-fold cross-validation,' a machine-learning technique to iteratively train and test a model in predicting values in the LIS. These predicted values manifest in the SWIID as 100 separate tables of data, with varying predicted Gini values to reflect the estimate's uncertainty (i.e., larger standard errors) when making comparisons across different countries or time periods.

The SWIID offers a selection of different Gini indices, depending on how income is defined. This study will focus on 'disposable income,' representing incomes after taxes and transfers. Disposable income was selected for this project for two reasons: firstly, it is the most commonly-measured type of income reported in the sourced datasets, resulting in more precise Gini estimates; secondly, disposable income better represents a household's true purchasing power. Other definitions include 'market income' (pre-tax, pre-transfer), " 'absolute redistribution' (the difference between the Ginis for market income and disposable income) and of relative redistribution (this difference divided by the Gini for market income and then multiplied by 100, that is, the percentage by which the market-income Gini is reduced)" (Solt, 2020, pp. 13–14).

**GDP.** Countries' expenditure-side real GDP and populations were retrieved from the PWT. The PWT stands out because it has a long history of using price data from the International Comparisons Program to adjust GDP values to purchasing-

**Table 3.1:** Study 3 Descriptive Statistics, 2004-2006 period

| Variable | Mean | SD |
|---|---|---|
| Nation Gini | 33.1 | 8.5 |
| Nation GDP per-capita | 34,071 | 16,381 |
| Nation-level victimization | 0.6 | 0.2 |
| Age | 44.4 | 17.1 |
| Individual victimizations | 0.6 | 0.9 |
| Security consumed | 1 | 1.1 |

power-matched U.S. dollars (i.e., their 'real' GDP). Expenditure-side GDP reflects the standard of living in an economy, as opposed to production-side GDP, which reflects its industry output. The selected values divided by their nations' population sizes to yield a per-capita GDP value (Feenstra et al., 2015).

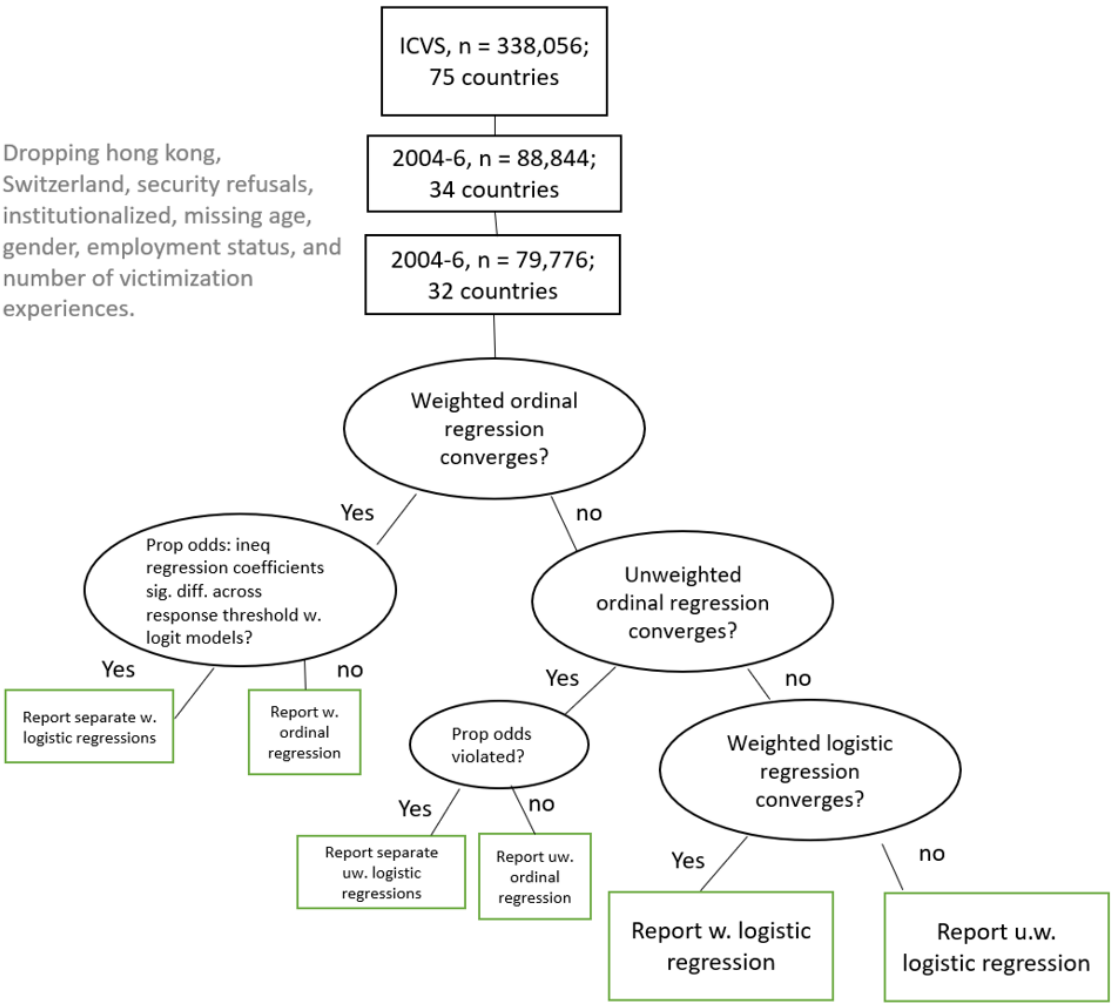
### 3.2.3 Pre-registered plan

The pre-registered analysis plan sought to prioritize treating security consumption as an ordinal variable (0: no security consumption, 1: one unit of security consumption, 2: two units, or 3: three or more units), using a cumulative link mixed model. The distribution of values in security consumption exhibits excessive skewness and asymmetry, and is not suitable to a linear model. Likewise, despite security consumption being a count variable of multiple binary occurrences, it is unlikely to meet the assumptions for a poisson process, which assumes that the maximum value stretches out indefinitely. Because of these limitations, treating the dependent variable as ordinal strikes a balance of model parsimony (minimizing assumptions about the variable), while leveraging information on the data's ranked structure.

Likewise, the ICVS dataset includes frequency weights for all included participants. These weights will be used whenever possible, as country-wide household surveys are unlikely to yield samples that are perfectly representative of their populations. Pilot analyses did indicate possible convergence issues when weights are included. As such, the contingency plan as depicted in Figure 3.3 was proposed for the analysis. 1) Attempt weighted ordinal regression, if the weighted ordinal model fails to converge, 2) conducted the model without frequency weights, elsewise, 3) conduct a weighted mixed logistic regresssion, then 4) attempt an unweighted mixed logistic regression. In the event of logistic regressions being used, security consumption was coded as 0: no consumption, and 1: one or more units reported.

A number of variables had transformations applied to facilitate intelligible in-

**Figure 3.3:** Data and analyses selection flowchart, 2004-06 period (Study 3).




tercepts and control size of eigenvalues. The following variables were centered at their appropriate levels: age, Gini, and nation-level victimization rates. GDP was scaled to reflect the countries' z-score. A supplementary analysis was also proposed as a robustness check, to be conducted on a different sweep period of the ICVS (1999-2003), with a final sample size of n = 70,264 (not repeated from 2004-06), and k = 41 countries (starting with 56 countries and 92,943 participants). In addition to the larger number of countries, this 1999-2003 sweep also has sufficient data on respondent housing to include them in the model (apartment dwelling as reference), which is important because housing is a potential confound in the 2004-06 sweep (the dependent measure is predominantly composed of items on household security). The predictor models for this 1999-03 sweep thus consisted of:

> Nation inequality, GDP per capita, nation-level victimization rates, individual victimizations, gender, age, house (vs. apartment), shanty (vs. apartment), other house (e.g., townhouse vs. apartment), and relationship status (partnered vs. not partnered)

The 1999 sweep covers a number of additional countries outside of the typical WEIRD countries covered in the 2004-06 sweeps:

> Albania, Azerbaijan, Belarus, Botswana, Brazil, Cambodia, Colombia, Croatia, Czech Republic, Georgia, Latvia, Lesotho, Mongolia, Namibia, Panama, Philippines, Korea, Romania, Russia, Slovenia, Swaziland, Uganda, Ukraine, and Zambia

However, it is missing a number of countries that are included in the 2004-06 period

> Argentina, Austria, Estonia, Germany, Greece, Hungary, Iceland, Ireland, Italy, Luxembourg, Mexico, New Zealand, Norway, Peru, and Turkey

Each of the 2004-06, and 1999-3 sweeps intersect in covering the following countries:

> Australia, Belgium, Bulgaria, Canada, Denmark, Finland, France, Japan, Lithuania, Netherlands, Poland, Portugal, South Africa, Spain, Sweden, United Kingdom, and United States

In order to test model robustness with the same variables, the pre-registration included a proposal to analyze the k = 32 data and the k = 41 data with models using a reduced model specification, only including matching variables (excluding partnered, employed, and housing; substituting Gini and GDP values for appropriate sweep periods).

For all analyses, the coefficient estimates and standard errors were extracted through simulations. In practice, any specified regression was conducted on 100 separate dataframes; each of which was a combination of one of the SWIID iterations, and an identical PWT/ICVS combination. The distributions for fixed-effects estimates were then generated by simulation (5000 iterations) from 100 dataframes of regression results, each consisting of coefficients and variance/covariance matrices from their respective model. These fixed-effects distributions were used to calculate point estimates and standard errors for each coefficient.

## 3.3 Results

Attempting to follow the pre-registered contingency plan, as presented in Figure 3.3, all analyses yielded errors or warnings for at least some of the 100 analyzed datasets. Both

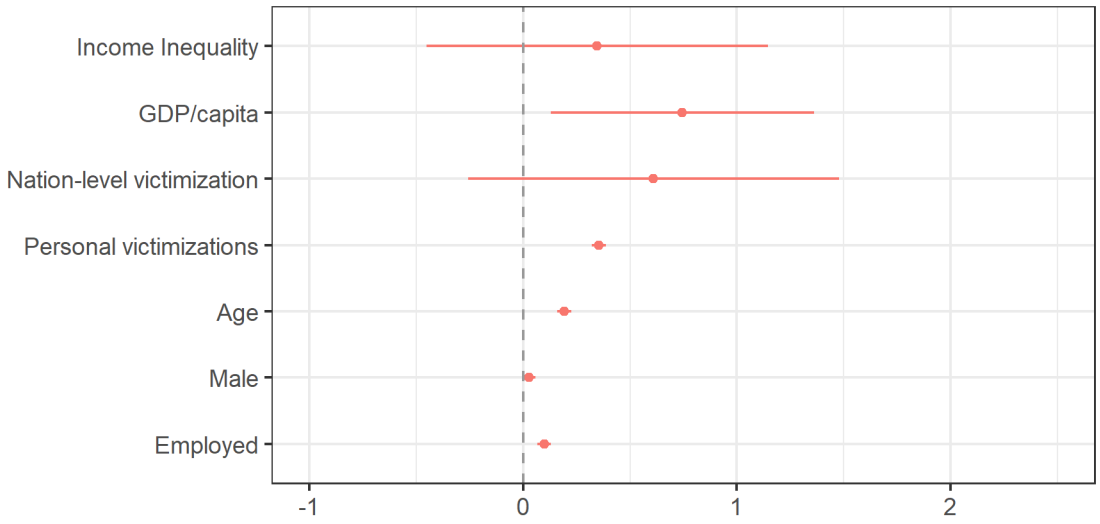
the weighted and unweighted ordinal regressions were unable to compute standard errors for 6/100 analyses. By contrast, each of the weighted and unweighted versions of logistic regressions successfully computed standard errors, but returned warnings for failed convergence for 96/100 of the conducted analyses, along with warnings for excessively large eigenvalues.

The pre-registered analysis plan did not provide contingencies for such widespread errors, so the relative costs and benefits of choosing analyses must be weighed after the fact. At this next step, it was decided post-hoc to attempt re-scaling age and Gini as standardized variables, in order to address the large eigenvalues and convergence issues.

Both for the models conducted on the 2004-06 and 1999-03 sweeps, scaling predictors for the ordinal analyses either made a negligible improvement to convergence issues (Table A.9, or actually increased the number of models with errors (Table A.8. As such, the logistic variants are reported for the main analyses, but the ordinal versions are available in the appendix (e.g., FiguresA.9).

Initial analyses (weighted for population frequency, two-tailed tests) indicated null effects of income inequality on security consumption, logistic: $b = 0.16$, CI(95%) = [-0.21, 0.53], $p = .39$; however, there was a significant effect of GDP, $b = 0.38$ [0.07, 0.69], $p = .02$. All individual-level predictors, except for gender were likewise significant (victimization experiences, age, employment status, $p < .001$; Figure 3.4; Table 3.2).

**Figure 3.4:** Study 3, dot-whisker plot for mixed weighted logistic regression, predicting security consumption.




*Notes:* Regression results in Table 3.2; coefficients in table will be slightly different, as dotplot terms are standardized by multiplying terms by twice the standard deviation of these variables, for comparable coefficients.

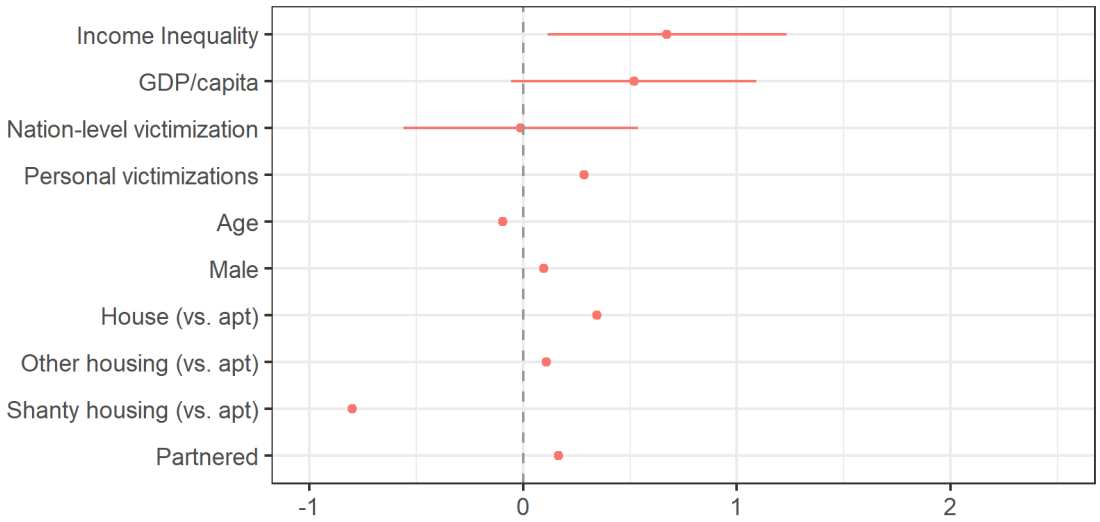
**Table 3.2:** Study 3, weighted mixed logistic regression, predicting security consumption

| term | *b* | 95% CI | *z* | *p* |
|---|---|---|---|---|
| (Intercept) | 0.26 | [0.02, 0.51] | 2.08 | .04 |
| Nation Gini | 0.16 | [-0.21, 0.53] | 0.85 | .39 |
| Nation GDP per-capita | 0.38 | [0.07, 0.69] | 2.38 | .02 |
| Nation-level victimization | 1.23 | [-0.52, 2.97] | 1.38 | .17 |
| Individual victimizations | 0.20 | [0.18, 0.22] | 21.11 | <.001 |
| Age | 0.10 | [0.08, 0.11] | 11.54 | <.001 |
| Employed | 0.10 | [0.07, 0.13] | 6.01 | <.001 |
| Male | 0.03 | [-0.003, 0.06] | 1.74 | .08 |

*Note:* AIC = 96692.73 , BIC = 96776.31, Loglik = -48337.37

When the 1999-03 dataset was analyzed, there was a positive and significant effect of income inequality on security consumption, $b = 0.4$ [0.07, 0.72], $z(36) = 2.36$, $p = .02$. In this model, GDP did not reach significance thresholds, $b = 0.27$ [-0.03, 0.57], $p = .08$. (Figure 3.5; Table 3.3)

**Figure 3.5:** Study 3, dot-whisker plot for logistic regression, 1999-03 period.




*Notes:* Regression results in Table 3.3

**Table 3.3:** Study 3, mixed weighted logistic regression, 1999-03 period.

| term | *b* | 95% CI | *z* | *p* |
|---|---|---|---|---|
| (Intercept) | 0.580 | [0.34, 0.83] | 4.68 | <.001 |
| Nation Gini | 0.400 | [0.07, 0.72] | 2.36 | .02 |
| Nation GDP per-capita | 0.270 | [-0.03, 0.57] | 1.77 | .08 |
| Nation-level victimization | -0.020 | [-1.06, 1.02] | -0.04 | .97 |
| Individual victimizations | 0.170 | [0.17, 0.17] | 1387.05 | <.001 |
| Male | 0.100 | [0.1, 0.1] | 508.48 | <.001 |
| Age | -0.003 | [-0.003, -0.003] | -462.62 | <.001 |
| House | 0.350 | [0.34, 0.35] | 1495.93 | <.001 |
| Other house type | 0.110 | [0.11, 0.11] | 156.48 | <.001 |
| shanty | -0.800 | [-0.8, -0.8] | -1370.63 | <.001 |
| Partnered | 0.170 | [0.17, 0.17] | 823.63 | <.001 |

*Note:*
AIC = 672718569.73 , BIC = 672718679.65, Loglik = -336359272.86

Variability in coefficients between the 2004-06 and 1999-03 periods are difficult to interpret from the above models alone; any differences could be attributable to real differences in the data, or due to differences in model specification. The 1999 period has additional variables such as housing and relationship status, but is missing employment status. As such, robustness check models were conducted with a reduced

variable set: Gini, gdp, victimization (nation- and individual-level), gender, and age.

The coefficient patterns in the two above models generally held with reduced models. In the reduced model analyzing 2004-06 dataset, Gini continued to have no significant effect $b = 0.16$ [-0.21, 0.52], $p = .41$. However, in this reduced model, the effect of gender (reporting as male) did change, reaching statistical significance, $b = 0.04$ [0.01, 0.08], $p = .005$ (Figure A.7 and Table A.10). In the reduced 1999-03 model, the effect of the Gini remained positive and significant $b = 0.43$ [0.09, 0.76], $z(36) = 2.51$, $p = .01$, the effect of GDP varied from the larger model, reaching statistical significance $b = 0.34$ [0.03, 0.64], $p = .03$ (Figure A.8 and Table A.11).

The base model across both sweep periods proved to be a better fit than the reduced models. ($AIC_{(04\text{-}06)base\text{-}reduced}$ = -34.04; $AIC_{(99\text{-}03)base\text{-}reduced} = -6.4659943 \times 10^6$)

($BIC_{(04\text{-}06)base\text{-}reduced}$ = -24.75) ($BIC_{(99\text{-}03)base\text{-}reduced} = -6.4659577 \times 10^6$)

Notably, treating security consumption as an ordinal did not change inequality's pattern of significance (nonsignificant for 04-06 period, significant for 99-03; 2004, Table A.12; 2004 reduced, Table A.13; 1999, Table A.17; 1999 reduced, Table A.18)

**Exploratory analyses:**

The effect of Gini may be better described by something other than a simple linear effect. As exploratory analyses, models were fit for both the 2004 and 1999 datasets with an exponential term for income inequality, and an inequality*GDP interaction. The exponential and interaction terms are reported within separate models, as within the same model, they introduce excessive multicollinearity in the model (max VIF = 6.56 for the Gini*GDP interaction). For the 2004-2006 dataset, analyses of AIC values indicated that compared to the base model, including the interaction term produced a superior fit ($AIC_{base\text{-}int}$ = 4.49). Conversely, the polynomial model is an insufficient improvement to fit beyond the base model ($AIC_{base\text{-}poly}$ = -1.02). Similarly, the polynomial is a worse fit to the interaction model ($AIC_{poly\text{-}int}$ = 5.51).

Another performance metric is the BIC, or Bayesian Information criterion, which has a stronger penalty for additional variables than the AIC. When using the BIC to evaluate the same models as above, the indices recommend using the base model with fewer variables. Smaller BIC values indicate a better fit. Compared to the base model, including the interaction term a worse fit ($BIC_{base\text{-}int}$ = -4.8). Similarly, the polynomial model is an insufficient improvement to fit beyond the base model ($BIC_{base\text{-}poly}$ = -10.31). Again, the polynomial is a worse fit to the interaction model ($BIC_{poly\text{-}int}$ = 5.51.

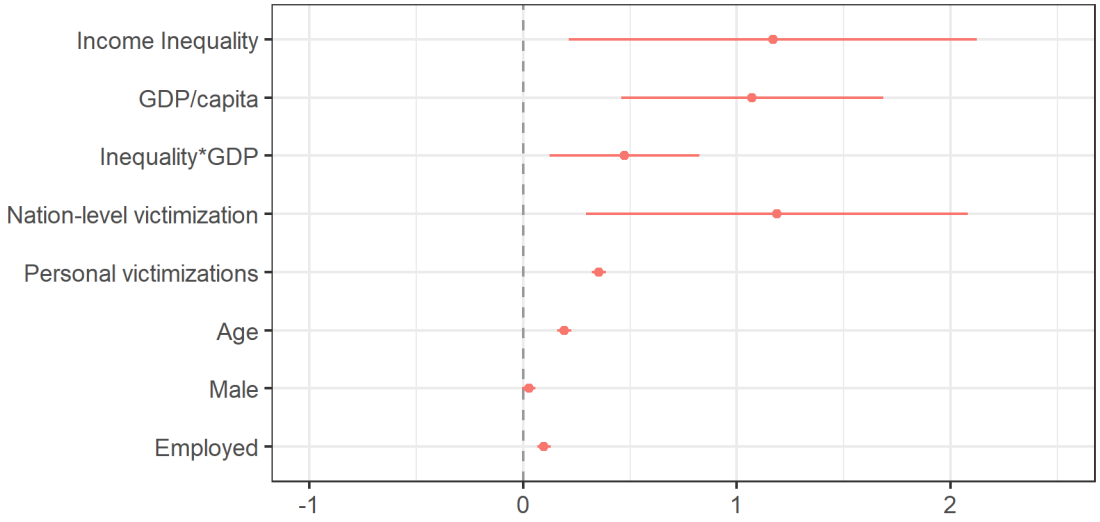
Given the exploratory nature of these analyses, the BIC is likely a better fit index

to guide decisions and inferences, as a means of controlling for Type I errors. In such a case, any coefficient change should be interpreted as a spurious change that has no substantive impact on predicting security consumption.

For the interaction-included model of the 2004 dataset, both the coefficients for income inequality, and the Gini*GDP interaction were significant, $b = 0.54$ [0.1, 0.98], $z(26) = 2.4$, $p = .02$ and $b = 0.47$ [0.12, 0.83], $p = .008$, respectively.

For the 1999 dataset, AIC values indicated that the addition of neither interaction nor exponential terms sufficiently improved model fit beyond the base model (AIC differences = -1.795 and -1.597, respectively. These models are thus only included in the appendix (Figures A.10, A.11 and Tables A.14, A.15). In both of those models, inequality still had a significant and positive association with security consumption.

**Figure 3.6:** Study 3, dot-whisker plot for mixed weighted logistic regression, Gini*gdp interaction




*Notes:* Regression results in Table 3.4

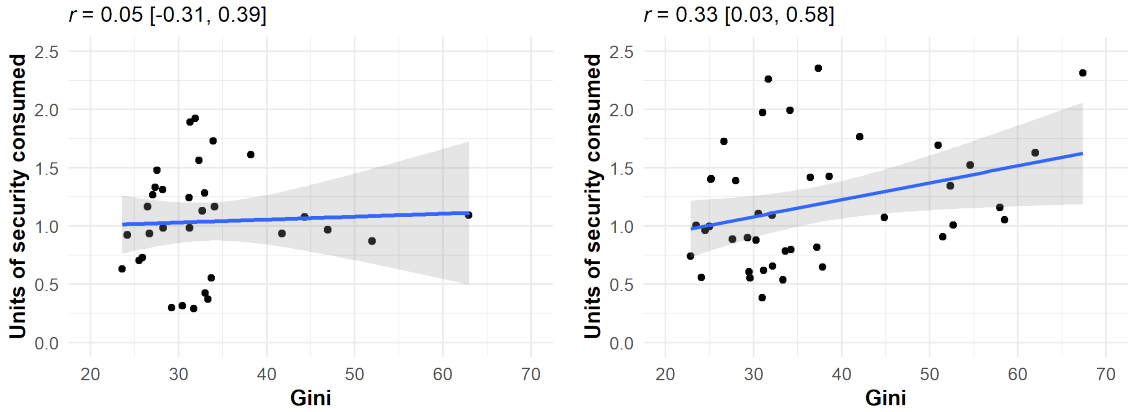
**Table 3.4:** Study 3, weighted mixed logistic regression, Gini*GDP interaction

| term | *b* | 95% CI | *z* | *p* |
|---|---|---|---|---|
| (Intercept) | 0.54 | [0.23, 0.84] | 3.48 | <.001 |
| Nation Gini | 0.54 | [0.1, 0.98] | 2.40 | .02 |
| Nation GDP per-capita | 0.54 | [0.23, 0.85] | 3.43 | <.001 |
| Nation-level victimization | 2.39 | [0.59, 4.19] | 2.61 | .009 |
| Individual victimizations | 0.20 | [0.18, 0.22] | 21.13 | <.001 |
| Age | 0.10 | [0.08, 0.11] | 11.52 | <.001 |
| Employed | 0.10 | [0.07, 0.13] | 5.98 | <.001 |
| Male | 0.03 | [-0.004, 0.06] | 1.74 | .08 |
| Gini*GDP | 0.47 | [0.12, 0.83] | 2.64 | .008 |

*Note:* AIC = 96688.24 , BIC = 96781.11, Loglik = -48334.12

## 3.4 Discussion

**Figure 3.7:** Study 3, country-level relationship between inequality and security consumption, 2004-06 sweep period (L), 1999-03 (R)




*Notes:* Labelled countries are influential observations; shaded regions correspond to 95% confidence interval

The results of Study 3 show mixed support for **H5)**. Although there was a consistently positive effect of inequality on reported security consumption, the effect's statistical significance varied across analyses. The pre-registered focal analysis (2004-06 period) failed to find a significant effect of inequality, until the Gini*GDP interaction was added. The robustness-check dataset from the 1999-03 period found a significant effect of inequality across all model versions. For exploratory analyses of the 2004-06 dataset, the addition of the (positive and significant) interaction term yielded a significant and

positive main effect of inequality. However, adding the Gini*GDP term only improved model fit when assessed by the AIC. When a more conservative index of model fit was used (BIC), adding the Gini*GDP term did not adequately improve model fit.

Even with reduced analyses with identical predictor variables, a significant effect of inequality was found in the 1999-03 period, but not the 2004-06 period. After ruling out differences in model specifications, the change in inequality's significance between sweep periods may be attributable to the slightly larger number of clustering units for this level-2 effect (k = 41 vs. k = 32), resulting in smaller standard errors. Likewise, the effect of inequality trended towards being larger in the 1999-03 period, $b = 0.43$ [0.09, 0.76], compared to the 2004-06 period, $b = 0.16$ [-0.21, 0.52]. As discussed earlier, the two sweep periods differ substantially in the countries covered. For instance, 1999-03 is unique in including countries like Botswana, Panama, Philippines, and Ukraine, but is missing countries such as Austria, Iceland, Italy, and Mexico). The 1999-03 sweep generally has better coverage of countries in Africa, South America, and Asia, and has larger Gini values ($\max_{99} = 67.4$, $\text{mean}_{99} = 36.8$; $\max_{04} = 63$, $\text{mean}_{04} = 33.1$).

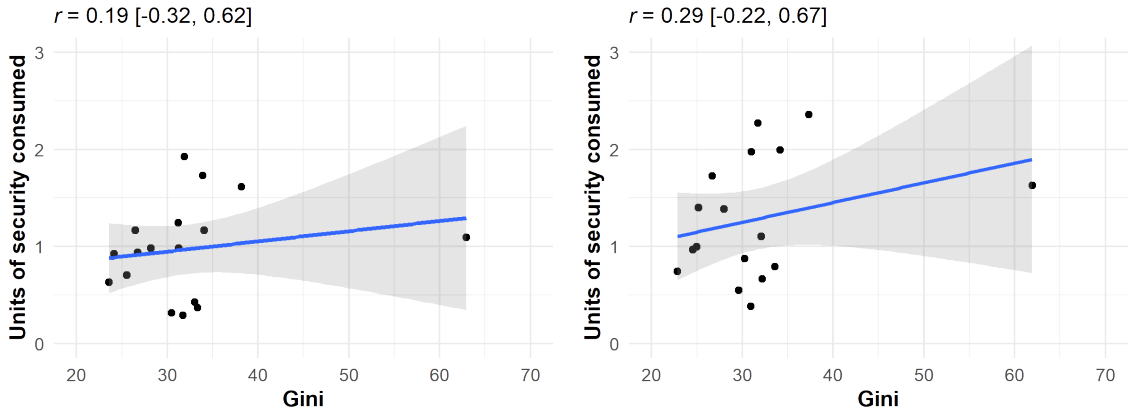
It is unclear why analyses on the 1999-03 sweep trended towards a larger effect of inequality than the 2004-06 sweep. One feasible explanation is that the 1999-03 sweep included more high-inequality, high-security countries. In addition to lower mean levels of inequality, the 2004-2006 period has higher skewness, $\text{skew}_{2004} = 1.9$, $\text{skew}_{1999} = 1$. Such an explanation would suggest that in the 2004-6 sweep, high-inequality countries were simply undersampled, with the few high-inequality countries providing a downward-biased picture of the relationship between inequality ad security consumption, with surveyed countries perhaps having higher country-level trust and institutional integrity to begin with. Another possible explanation is the differences between sweep periods has introduced some meaningful confounding variables. Likely culprits such as GDP or victimization experiences are already accounted for in the regression models. Likewise, a more mature security market seems like an unlikely explanation, as the 1999-03 sweep had reported a higher mean security units consumed than the 2004 period ($M_{99} = 1.2$, $skew_{99} = 0.7$; $M_{04} = 1$, $skew_{04} = 0.1$).

One exploratory consideration could be to analyze both the 2004-06 and 1999-03 sweeps with reduced variables, along with reduced countries, so that the only differences between the 2004-06 and 1999-03 models would be the time period and the individual respondents surveyed. However, this analysis would result in only a small number of level-2 units (k = 17), yielding effect estimates with large standard errors. Figure 3.8 shows that both sweeps have positive trending relationships, and that this relationship is trending towards being larger in the 1999-03 sweep. Since the models

and countries are the same, any difference in the inequality-security effect must be attributable to the time period of the survey, any differences in the survey itself, or the individuals sampled.

A contrast between Figures 3.7 and 3.8 is also suggestive that the inequality-security relationship for the 2004-06 period is strengthened by removing the countries that were exclusively surveyed in this time period. The small number of countries examined is too small for any statistical analysis, but raises the possibility that the countries exclusive to the 2004-06 period may have a particularly low correlation between inequality and security consumption, and that their inclusion reduces the overall correlation coefficient. However, any of these post-hoc, reduced-country analyses are underpowered and should be interpreted with substantial caution.

**Figure 3.8:** Study 3, country-level relationship between inequality and security consumption, only plotting shared countries, 2004-06 sweep period (L), 1999-03 (R)




*Notes:* Labelled countries are influential observations; shaded regions correspond to 95% confidence interval

See though Vrieze (2012) for a comparison of the BIC and AIC ??

Given that the measure of security consumption was constructed largely out of items concerning home security, the 1999-03 sweep period was especially important because it offered the opportunity to control for one's housing. For instance, one would expect the result that individuals report more security measures when they lived in a house instead of apartment, as their residences are more likely to be at the ground level, and they have more autonomy to install security measures after moving in. Likewise, the inclusion of these housing variables helps to control for any confounding or suppressing effects. For instance, high-inequality countries could conceivably have fewer individuals in houses, artificially weakening the relationship

between inequality and security consumption.

There was no single model specification that could be used to consistently describe both sweep periods. For instance, the Gini*GDP interaction improved the model fit for the 2004-06 period, but only when using AIC as the index for model evaluation. Similarly, the interaction did not improve the fit on the 1999-03 data. The Gini*GDP interaction was an exploratory inclusion, so any interpretations are post-hoc. Although the simultaneous inclusion of Gini and GDP as main effect variables means that Gini and GDP are not confounded in this analysis, their interaction might remain practically, if not theoretically, meaningful.

One possible explanation of the Gini*GDP interaction is that people are sensitive to the magnitude of economic differences within a society, recognizing that a large Gini in a wealthy country means that the upper crust of a society controls a much larger absolute dollar value than a poorer country (e.g., one person controlling 50% of $100 versus $1 million). Additionally, the distrust- and fear-inducing effect of inequality may be obscured by the average individuals' reduced purchasing power. High inequality means that any person is less likely to control a sizeable portion of a country's income, so is more likely to be poorer in absolute terms, so they may have less disposable income to spend on security products. Likewise, at least at a cross-sectional basis, the Gini and GDP are negatively correlated (Our World In Data, 2019), generally reflecting that when the pool of a country's income is split more unevenly, the overall standard of living also tends to be lower. The significant Gini*GDP interaction may suggest that if a country is more unequal and more prosperous, individuals do feel more fearful and distrustful, and they may be using their small "slice" of the large economic "pie" to satisfy their felt need for greater security.

The 1999 sweep failed to find an effect of the Gini*GDP interaction, which could be attributable to either the different set of countries that was included, or the 3-7 year difference from the 2004-06 sweep. The security market may have matured significantly between 1999 and 2004-06, where even lower-tech security measures such as window grills may have become more accessible, as the market has matured.

### Limitations

Study 3's use of archival data has some notable limitations. Since many countries in the ICVS were surveyed as part of distinct contracts, they vary to the degree that sampling is representative of an entire country. For instance, some countries are only surveyed from a single city. Similarly, the data reported from the ICVS is fairly old. The overall consumption landscape, and the security industry specifically, has seen

some substantial changes since the late 1990s and early 2000s. As such, the relationship between nation-level inequality and security consumption might change over other years. Likewise, any conclusions drawn from analyzing the ICVS will be limited due to the ICVS' restricted selection of countries. The countries that participated were not randomly sampled, but instead out of convenience and feasibility, possibly introducing a bias into the economic and cultural makeup of the countries selected.

Security consumption can look very different across countries

One market might have walls

one might have locks

burglar alarms

Sampling bias

The cross-sectional analysis of inequality and security may may be a poor specification of how inequality would actualy impact security. For instance, Daly (2016) suggests that for the relationship between homicide and inequality could be better described as time-lagged, where the effects of inequality on behaviour "must be mediated in some sort of cumulative influence of our past experiences" (p.150).

The measurement of security measures is limited to self-report measures of household protection methods. As such, for many individuals, many of their security measures may have been an incidental or pre-existing feature of their residence. For instance, a consumer may choose to purchase a house for its desirable location, but the consumer does not care that the property comes bundled with some security features such as heavy-duty locks or barred windows. Personal protection measures (e.g., carrying a can of mace, avoiding going out at certain times) would likely be more salient and intentional to respondents. With this possibility of consumers being indifferent about some security measures, the self-report nature of security consumption also introduces the possibility of measurement error that respondents simply do not remember or notice some security features on their property.

The use of logistic regressions carries the risk of some misleading conclusions. Dichotomizing security consumption means treating the variable as all-or-nothing. However, there could be a meaningful loss of information by reducing the full range of values. For instance, it could be the case that many people end up with one unit of security consumption spuriously, but it is the higher units of security consumption may have the stronger relationship with inequality.

Additionally, the method of computing security consumption had to make some assumptions of how missing values were generated.

As it stands, individual weights have not been used when computing compositional

variables.

The model specification would be improved by the addition of covariates such as income, ethnicity, neighborhood type

The presence of models that have some convergence issues may reduce the robustness of estimates, whether it is due to the selective use of models without convergence issues, or the inclusion of models that do have convergence issues.

# Conclusion

If someone locks their doors to their neighbors, should they be commended for their prudence or ridiculed for their suspicion? A normative prescription for how much security people should consume is unclear; the "right" amount of security consumption can vary both within and across individuals and circumstances (e.g., a rich vs. poor person purchasing security, or weighing the "ultimate price" of dying).

San-Juan et al. (2012)

According to (Dolan & Peasgood, 2007), fear of crime is, above all, a reason to be concerned about people's self-protective and avoidance measures in response to it, which can have a significant impact

Combined, Studies 1 and 3 provide evidence with both high internal and external validity that inequality is indeed linked to the consumption of security products

Study 2... blah blah

## 3.5 Practical applications

The presence of the envy anticipation effect at such a small scale and artificial scenario raises some interesting questions for how inequality affects trust for different types of groups. For instance, a potential implication of this research is in the workplace. Employees can regularly have the chance to harm co-workers in order to benefit themselves (e.g., gossip, taking credit for work, ostracizing, theft). These results suggest that inequality within the workplace may serve to undermine trust, as individuals seek to insulate themselves from potentially harmful behaviours (e.g., communicating less, not sharing resources), and consequently undermining individual and group-level productivity. Given that nearly all workplaces have some degree of stratification, such as a supervisor being paid more, it is worth considering how and social comparisons may cascade into lower trust. In general, inequality should be most likely to produce lower trust under the same conditions as when inequality will produce experiences of envy and relative deprivation. The effect of inequality will

be stronger when individuals are judged to be similar on some dimension. Likewise, greater reductions in trust will occur when individuals are likely to compete with each other at later points.

Beyond the workplace, the effects of inequality on security behaviours in a consumer context should generally be strongest at smaller scales. In this respect, the upcoming examination of country-level inequality in Chapter 3 might serve as a conservative test of how inequality affects groups. The trust-eroding effects of inequality might be stronger (and easier to detect) at more "meso" levels of comparison, such as cities or neighborhoods. For instance, Canada may have an intermediate to low level of inequality compared to other countries, but this "macro" picture of income distribution is likely to be an imperfect reflection of whether inequality is a present and noticeable feature in a person's day-to-day life. For instance, as a city Vancouver is likely to be more unequal than St.John's. Likewise, some neighborhoods in these cities will have more or less stratification, such as those areas experiencing gentrification. Depending on one's lifestyle, the relative income of someone in another city or province is unlikely to have a large impact on one's psychology and consumption. By contrast, regularly seeing a large disparity between the "haves" and "have-nots" in one's daily life may come to change one's beliefs and attitudes regarding the social climate.

The next few hundred years may be disproportionately affected by the fortunes of African countries. In addition to their colonial histories, varied ethnic and religious groups, vulnerability to climate change, prevalence of poverty, and rich natural resources, these countries are among the fastest-growing. By 2100, the population in Africa is expected to quadruple from one billion to four billion people. In the meantime, population growth everywhere else has been slowing; in Asia, the most populated region in the world the population will only increase from four billion to five billion Worldometer (2022) . These fast-growing countries are also among the most unequal in the world, meaning that over the next 100 years, most people will be born into high-inequality environments.

In addition to the subsequently high rates of poverty and absolute deprivation, these high-inequality countries may also foster populations with extremely high relative deprivation. The bourgening population will also mean increased investment from foreign markets, whether attempting to secure cheap labour forces, or service growing demand across the region (e.g., China's Belt and Road Initiative; G7's Build Back Better World Partnership). In a fast-moving, high-stakes, and competitive region, individuals in these countries may be acutely aware of how another's gain may be their loss. If the envy anticipation hypothesis holds, then these areas may turn out to

be incredibly lucrative markets for the security industry to target and develop.

Conversely, if inequality is indeed-trust eroding, high-inequality regions may encounter substantial internal tensions and breakdowns in cooperation when faced with collective action problems.

social comparisons - comparison target selected is the higher-up individual

# Appendix A

# The First Appendix

## A.1 Ethics information

All procedures for this project have been approved by the University of Calgary Conjoint Faculties Research Ethics Board (REB 20-1860). For the experimental studies, informed consent will be obtained for all participants, who, unless otherwise noted, will be compensated at a rate of $12.48 CAD/hour. For multilevel analyses, the datasets are archival and publicly-available. As such, ethics approval and informed consent was obtained, and compensation disbursed, by the original researchers.

## A.2 Study 1

### A.2.1 Data exclusion

Participants will be excluded from analyses if any of the following:

- failed attention check,
- failed comprehension check,
- missing data on model-relevant variables
- score of <= 3 on attention boost item

### A.2.2 Materials

**Attention Check**

"To help us understand how people think about different activities, please answer this question correctly. Specifically, we are interested in whether you actually take the time to read the directions; if not, the results of your responses would not be very useful. To show that you have read the instructions carefully, please ignore the items below about activities and instead select the option"other," and type "yes" in field below.

'Based on the information in the preceding paragraph, which of these activities do you engage in most regularly?' Watching Athletics Attending Cultural Events Participating in Athletics Reading Outside of Work or School Watching Movies Travel Religious Activities Needlework Cooking Gardening Computer Games Hiking Board or Card Games Other:"

**Comprehension check**

"What best describes your role in this study?

- Deciding whether to accept an offer
- Placing a bid
- **Deciding whether to purchase a security product** - CORRECT
- Donating funds to a shared project"

**Attention Booster**

- " It is important for the quality of our research that participants focus as they complete this short study. Please close any other tabs or windows that you might have open, turn off any music or distracting media, and give this study your full attention.

Please indicate your likelihood of paying full attention during this study:" [1: Extremely Unlikely... 7: Extremely Likely]

Given the bounded nature of the minimum and maximum values for the variables in the experimental designs, outliers are considered legitimate values and will not be deleted.

The pre-registered mediation plan was mis-specified. As such, the pre-registered mediation plan can be FOUND IN THE APPENDIX ?? , and the following specification is an adjustment:

1a MEDIATION HERE??

### A.2.3 Study 1a

**Study 1a pre-registration**

**Hypotheses**

Hypotheses will be tested in each of 2 studies: 1a and 1b

**Does inequality increase security consumption?** H1) economic inequality increases security consumption, versus

H1null) no significant effect of inequality

H1): Economic inequality increases security consumption, or in terms of the model: H1): In the model ineq_anova_mixed, security spending will be larger when income inequality is present (1) vs. absent (0)

**Does envy anticipation mediate the effect of inequality on security consumption?** H2) The indirect effect (ACME) will be significantly different from 0, versus

H2null) no significant indirect effect through anticipated partner envy from inequality to security consumption.

H2): Economic inequality's effect on security consumption is mediated by the expectation of peers' envy, or in terms of the model, H2): The indirect effect (ACME) will be significantly different from 0. H2.1), mediation step 1: In the model m_mixed_med1, the coefficient for income inequality will have a significant positive effect on participants' belief that their partner is envious. H2.2), mediation step 2: In the model m_mixed_med2, participants' belief that their partner is envious will have a significantly positive effect on security spending. H2.3), mediation step 3: In the model m_mixed_med_full, the indirect effect/ACME (Average Causal Mediation Effect [total effect - direct effect]) will be significantly different from 0. Design Plan

**Study type**

Experiment - A researcher randomly assigns treatments to study subjects, this includes field or lab experiments. This is also known as an intervention experiment and includes randomized controlled trials.

**Blinding**

For studies that involve human subjects, they will not know the treatment group to whic

**Is there any additional blinding in this study?**

No response

**Study design**

Participants will be asked to consider their participation in a game with another participant, where they will each be assigned to different roles (Blue or Yellow), and endowed with money. Participants will always be assigned to the "Yellow" role, in which their partner will choose whether to try stealing from them, beforehand, the participant has the option to spend money on a security product that reduces the likelihood of a theft being successful.

Random assignment and random ordering for between and within conditions, respectively

Example of condition:

"For this scenario, you have been assigned to Yellow.

You have been given $300, but your partner (Blue) has been given $150. Blue knows how much money you each have received.

Shortly, Blue will decide whether they will try to steal $150 from you. If Blue attempts to steal, but fails, you will lose nothing, and they will lose $150. "

Given the mixed design, two sets of responses for game-related self-report items and security consumption decisions will be measured and then analyzed in a mixed model

**Randomization**

Simple randomization will be conducted when assigning participants to both between and within conditions Sampling Plan

**Existing Data**

Registration prior to creation of data

**Explanation of existing data**

No response

**Data collection procedures**

Exclusion: missing data on model-relevant variables, failed atn/comp checks, score of <= 3 on attention boost item

Participants crowdsourced @ Prolific Academic, unless otherwise noted, will be compensated at a rate of $12.48 CAD/hour.

**Sample size**

Study 1a: target n = 978 for analyses, 1,087 participants will be collected, in anticipation of 10% exclusion rate

Study 1b: determined by study 1a results

**Sample size rationale**

Study 1a: Determined by A priori power analysis using R package Superpower @ 95% power, cohen's f = 0.12. Base security spending: M=0.61, SD=0.33

Study 1b: targeted power, and therefore n, determined by observed 1a effect (target 95% power). If Study 1a fails to find a significant effect of inequality, Study 1b will again target an effect size of cohen's f = 0.12 (n=978).

**Stopping rule**

No response Variables

**Manipulated variables**

Across both studies 1a and 1b:

In the equal condition, participants will be told that their partner received the same amount of funds as themselves (e.g., dollar value depending on study/stake size). In the unequal condition, participants will be told that their partner instead received only half their endowment.

Stake size and probability within-subjects manipulations will be implemented when describing the scenario to participants. for example:

"Your partner (Blue) still has to decide whether to try stealing from you.

After you decide how much of a security product to buy, your partner will see their updated chances of successfully stealing.

For this scenario, an attempted theft will succeed 50%[60%] of the time. A successful theft steals $150[$1] from you, and a failed attempt to steal will make your partner lose $150[$1].

Each unit of security costs $3[$0.02], and reduces the chance of successful thefts by 1%.

Use the slider below to indicate how much security you would purchase from a budget of $300[$2]."

**Measured variables**

H1) and H2) participants will be asked to choose how many units of a security product they wish to consume, ranging from 0 to 50.

H2) perceived partner envy (averaged, checked for cronbach's alpha >= .7) p_partner_envy = "My partner probably feels envious of me" p_partner_jealous = "My partner probably feels jealous of me p_partner_bitter ="My partner probably feels bitter"

Demographics: Age Gender Education Income

**Indices**

No response

**Analysis Plan**

Statistical models

H1) Mixed anova conducted using r package afex 1a) ineq_anova_mixed <- aov_ez(id='pnum', dv='security_consumed', data=ineq_data_long, within = c('stake_cond'), between = c('inequality') )

1b) ineq_anova_mixed <- aov_ez(id='pnum', dv='security_spending', data=ineq_data_long, within = c('prob'), between = c('inequality') )

*for study 1b substitute 'stake_cond' for 'prob', and 'security_consumed', for 'security spending* H2) mixed mediation conducted using r packages lme4 and mediate m_mixed_med1 <- lme4::lmer(likely_envy ~ inequality + (1| pnum) + stake_cond, data = ineq_data_long)

m_mixed_med2 <- lmer(security_consumed~ likely_envy + inequality + (1| pnum) + stake_cond, data = ineq_data_long)

m_mixed_total <- lmer(security_consumed~ inequality + (1| pnum) + stake_cond, data = ineq_data_long)

m_mixed_med_full <- mediate::mediate(m_mixed_med1, m_mixed_med2,sims = 5000, treat="inequality",mediator="likely_envy")

summary(m_mixed_med_full)

**Transformations**

Security spending will serve as the final dependent variable where possible. Security spending will be computed by multiplying the number of security units consumed by the good's per-unit cost

**Inference criteria**

$p < .05$ for inferring significant differences, one-tailed

**Data exclusion**

Participants will be excluded from analyses if any of the following: failed attention check, failed comprehension check, missing data on model-relevant variables score of <= 3 on attention boost item

ATTENTION CHECK:

"To help us understand how people think about different activities, please answer this question correctly. Specifically, we are interested in whether you actually take the time to read the directions; if not, the results of your responses would not be very useful. To show that you have read the instructions carefully, please ignore the items below about activities and instead select the option"other," and type "yes" in field below.

'Based on the information in the preceding paragraph, which of these activities

do you engage in most regularly?' Watching Athletics Attending Cultural Events Participating in Athletics Reading Outside of Work or School Watching Movies Travel Religious Activities Needlework Cooking Gardening Computer Games Hiking Board or Card Games Other:"

COMPREHENSION CHECK: "What best describes your role in this study?

Deciding whether to accept an offer Placing a bid **Deciding whether to purchase a security product** - CORRECT Donating funds to a shared project"

ATTENTION CHECK/BOOSTER " It is important for the quality of our research that participants focus as they complete this short study. Please close any other tabs or windows that you might have open, turn off any music or distracting media, and give this study your full attention.

Please indicate your likelihood of paying full attention during this study:" [1: Extremely Unlikely... 7: Extremely Likely]

Given the bounded nature of the minimum and maximum values for the variables in the experimental designs, outliers are considered legitimate values and will not be deleted.

As a robustness check, analyses will be repeated with influential observations being winzorized at the dependent variable to the 5th and 95th percentile values. Influential observations will be flagged as having model residuals' cook's distance values exceeding the threshold 4/n; where n = # of observations.

Missing data

observations with missing data on model-relevant variables will be excluded from analyses

Exploratory analysis

No response Other

Other

No response

### Study 1a Security Game

### Security Game introduction

> "For the following task, imagine that you have been paired with another participant. You will both be given money to play a game, and assigned to different roles: Yellow or Blue.
>
> Blue will have the option to try stealing money from Yellow. If Blue tries and fails to steal, Blue loses money instead, with Yellow losing

nothing. Before Blue makes their choice, Yellow will have the chance to purchase a "security product," which will cost money, but each unit purchased will reduce Blue's chance of successfully stealing. After Yellow makes their purchasing decision, Blue will see the updated chances of success/failure.You will see multiple scenarios that may differ slightly."

**Security Game Inequality Manipulation**

"For this scenario, you have been assigned to Yellow.

You have been given $300[$2], but your partner (Blue) has been given $150[$1] [You and your partner (Blue) have each been given $300[$2]]. Blue knows how much money you each have received.

Shortly, Blue will decide whether they will try to steal $150[$1] from you. If Blue attempts to steal, but fails, you will lose nothing, and they will lose $150[$1]."

**Self-report items, Strongly Disagree(1)-Strongly agree(7)**

p_partner_envy = "My partner probably feels envious of me" p_partner_jealous = "My partner probably feels jealous of me p_partner_bitter ="My partner probably feels bitter"

**Security Game Decision**

"Your partner (Blue) still has to decide whether to try stealing from you.

After you decide how much of a security product to buy, your partner will see their updated chances of successfully stealing.

For this scenario, an attempted theft will succeed 60% of the time. A successful theft steals $150[$1] from you, and a failed attempt to steal will make your partner lose $150[$1].

Each unit of security costs $3[$0.02], and reduces the chance of successful thefts by 1%.

Use the slider below to indicate how much security you would purchase from a budget of $300[$2].

Click the blue slider to begin [Prevents non-responding participants proceeding with default 0 value].

[SLIDER HERE, 0-50]

[NEXT SECTION APPEARS AFTER CLICKING SLIDER]

Purchasing [UPDATING NUMBER] units of security, you will have $[UPDATING NUMBER] remaining.

You will reduce your likelihood of being successfully attacked by [UPDATING NUMBER]%.

After your security purchase, [UPDATING NUMBER]% of attempted attacks against you will be successful.

Once you are satisfied with your decision, please click the "next" button to move to the next page."

**Study 1a Power analysis**

**Table A.1:** Study 1a, power analysis, pairwise contrasts, 2b*2w ANOVA

| | Power | Effect size |
|---|---|---|
| ineq_1_histak_1_*VS*_ineq_1_histak_0 | 5.00 | 0.0 |
| ineq_1_histak_1_*VS*_ineq_0_histak_1 | 99.29 | -0.2 |
| ineq_1_histak_1_*VS*_ineq_0_histak_0 | 99.29 | -0.2 |
| ineq_1_histak_0_*VS*_ineq_0_histak_1 | 99.29 | -0.2 |
| ineq_1_histak_0_*VS*_ineq_0_histak_0 | 99.29 | -0.2 |
| ineq_0_histak_1_*VS*_ineq_0_histak_0 | 5.00 | 0.0 |

Why would the effect of inequality on security consumption go from nonsignificant to significant, but opposite sign, when controlling for perceived partner envy?

SUPPRESSION EFFECT

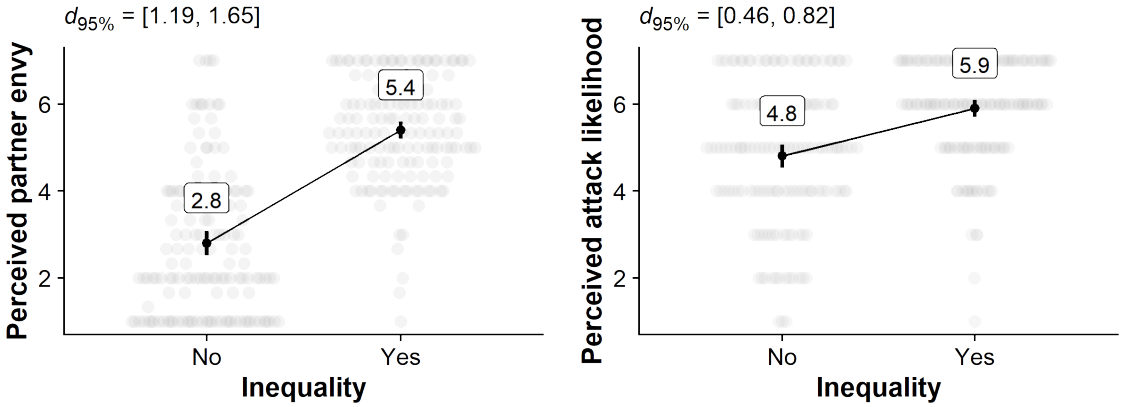
MODERATED MEDIATION NECESSARY?

## A.2.4 Study 1b pilot

**Table A.2:** Study 1b pilot Descriptive Statistics

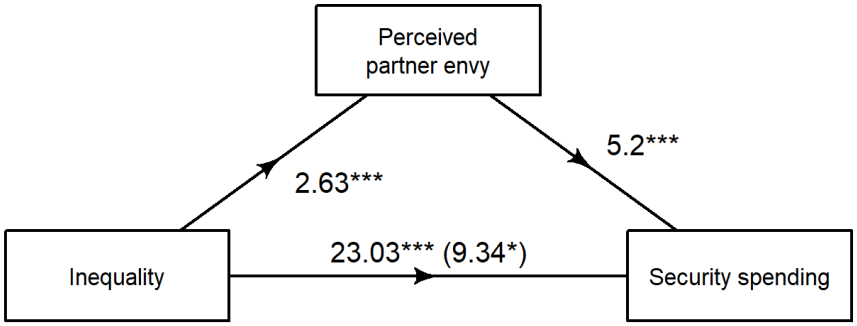
| Variable | Mean | SD | Cron. $\alpha$ 95% CI |
|---|---|---|---|
| Age | 31.8 | 11.9 | |
| Perceived envy | 4.1 | 1.1 | [0.93, 0.97] |
| Perceived attack likelihood | 5.4 | 1.1 | |
| Security consumed | 32.8 | 25.3 | |

**Figure A.1:** Study 1b pilot, within-subjects effects of inequality on perceived partner envy and attack likelihood.




*Notes:* Means (95% CI) and raw data, horizontally jittered according to kernel density.

**Figure A.2:** Study 1b pilot, mediation path diagram.



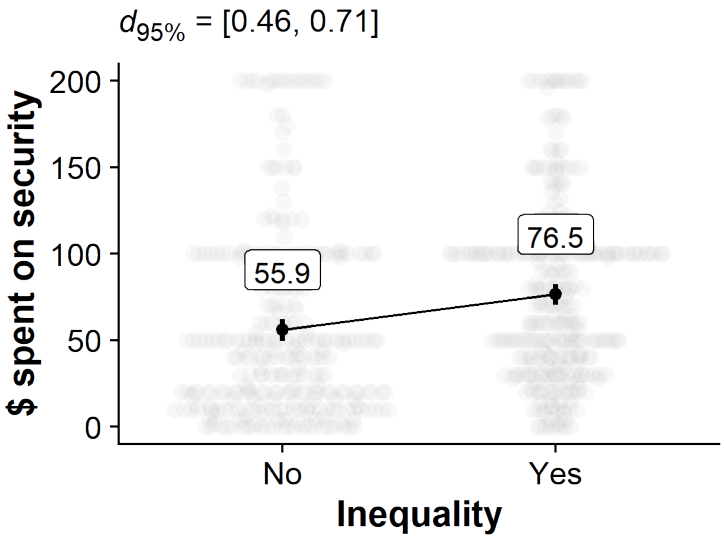

*Notes:* ***$p < .001$; **$p < .01$; *$p < .05$, Table A.3

**Table A.3:** Study 1b, pilot full mediation model

| | $b$ | 95% CI | $p$ |
|---|---|---|---|
| Indirect Effect (ACME) | 13.73 | [6.27, 21.53] | <.001 |
| Direct Effect (ADE) | 9.33 | [0.26, 18.67] | .04 |
| Total Effect | 23.06 | [17.16, 28.97] | <.001 |
| Prop. Mediated | 0.60 | [0.27, 0.99] | <.001 |

### A.2.5 Study 1b

**Figure A.3:** Study 1b, within-subjects effects of inequality on security spending.




*Notes:* Means (95% CI) and raw data, horizontally jittered according to kernel density.

#### Study 1b pre-registration

Hypotheses

**Does inequality increase security consumption?** H1) In a paired t-test, A) security consumption will be higher in the inequality condition than the equality condition, versus Anull) no significant effect of inequality

This positive effect of inequality is also predicted for some manipulation check items: B) perceived inequality C) perceived partner envy D) perceived likelihood of attacking

**Does envy anticipation mediate the effect of inequality on security consumption?** H2) The indirect effect (ACME) will be significantly different from 0, versus

H2null) no significant indirect effect through perceived partner envy from inequality to security consumption. Design Plan

**Study type**

Experiment - A researcher randomly assigns treatments to study subjects, this includes field or lab experiments. This is also known as an intervention experiment and includes randomized controlled trials.

**Blinding**

```
No blinding is involved in this study.
```

**Is there any additional blinding in this study?**

No response

**Study design**

Sequence as follows: 1. Consent 2. Attention boost 3. Demographics 4. security game task intro 5. round 1 a) manipulation, b) self-report items, c) security consumption 6. next scenario screen 7. round 2) 8. Comprehension check

For security game:

Participants will be asked to consider their participation in a game with another participant, where they will each be assigned to different roles (Blue or Yellow), and endowed with money. Participants will always be assigned to the "Yellow" role, in which their partner will choose whether to try stealing from them, beforehand, the participant has the option to spend money on a security product that reduces the likelihood of a theft being successful.

Random assignment of order for within conditions

task intro:

"For the following task, imagine that you have been paired with another participant. You will both be given money to play a game, and assigned to different roles: Yellow or Blue.

Blue will have the option to try stealing money from Yellow. If Blue tries and fails to steal, Blue loses money instead, with Yellow losing nothing.

Before Blue makes their choice, Yellow will have the chance to purchase a "security product," which will cost money, but each unit purchased will reduce Blue's chance of successfully stealing. Blue will see how many units of security that Yellow has purchased.

You will see multiple scenarios that may differ slightly. "

Note: for this study, participants will NOT know:

1) how much is stolen by successful thefts
2) how much the attacker loses on failed thefts
3) the probability of successful thefts
4) the efficacy of security (% success decrease per unit consumed)

**Randomization**

Simple randomization will be implemented Sampling Plan

**Existing Data**

Registration prior to creation of data

**Explanation of existing data**

No response

**Data collection procedures**

Exclusion: missing data on model-relevant variables, failed atn/comp checks, score of <= 3 on attention boost item

Participants crowdsourced @ Prolific Academic, unless otherwise noted, will be compensated at a rate of $12.48 CAD/hour

**Sample size**

n = 271 targeted for analysis, n = 301 collected

**Sample size rationale**

using r package pwr, power analysis for one-tailed paired t-test, d=.2, power = .95

pwr.t.test(d=.2,power=.95,sig.level=.05, type="paired",alternative="greater")

**Stopping rule**

No response Variables

**Manipulated variables**

In the equal condition, participants will be told that their partner received the same amount of funds as themselves (e.g., dollar value depending on study/stake size). In the unequal condition, participants will be told that their partner instead received only half their endowment.

Example:

"For this scenario, you have been assigned to Yellow.

You have been given $300, but your partner (Blue) has been given only $150. Blue knows how much money you each have received.

Shortly, Blue will decide whether they will try to steal money from you."

**Measured variables**

Security consumption

example:

"Your partner (Blue) still has to decide whether to try stealing from you.

A successful theft steals money from you, and a failed attempt to steal will make your partner lose money.

Each unit of security costs $2, and reduces the chance of successful thefts.

Use the slider below to indicate how much security you would purchase from a budget of $300. [0-100] Purchasing 32 units of security, you will have $236 remaining. "

Self-report items : 1-7 In this game, the money has been split unequally
My partner is probably going to try stealing from me
My partner probably feels envious of me
My partner probably feels jealous of me
My partner probably feels bitter

Demographics: Age Gender Education Income

**Indices**

No response

**Analysis Plan**

Statistical models

tidy_ttest(data_long, iv = "inequality", dv = "security_consumed", paired = TRUE) based on stats::t.test

m_mixed_med1 <- lme4::lmer(security_consumed~ inequality + (1| participant_code), data = ineq_data_long)

m_mixed_med2 <- lmer(likely_envy_cent ~ inequality + (1| participant_code), data = ineq_data_long)

m_mixed_total <- lmer(security_consumed~ inequality + likely_envy_cent + (1| participant_code), data = ineq_data_long)

m_mixed_med_full <- mediate::mediate(m_mixed_med2, m_mixed_med_total,sims = 5000, treat="inequality",mediator="likely_envy_cent")

**Transformations**

envy will be mean-centered in mediation analysis

**Inference criteria**

$p < .05$ for inferring significant differences, one-tailed

**Data exclusion**

Participants will be excluded from analyses if any of the following: failed attention check, failed comprehension check, missing data on model-relevant variables score of <= 3 on attention boost item

ATTENTION CHECK:

"To help us understand how people think about different activities, please answer this question correctly. Specifically, we are interested in whether you actually take the time to read the directions; if not, the results of your responses would not be very useful. To show that you have read the instructions carefully, please ignore the items below about activities and instead select the option"other," and type "yes" in field below.

'Based on the information in the preceding paragraph, which of these activities

do you engage in most regularly?' Watching Athletics Attending Cultural Events Participating in Athletics Reading Outside of Work or School Watching Movies Travel Religious Activities Needlework Cooking Gardening Computer Games Hiking Board or Card Games Other:"

COMPREHENSION CHECK: "What best describes your role in this study?

Deciding whether to accept an offer Placing a bid **Deciding whether to purchase a security product** - CORRECT Donating funds to a shared project"

ATTENTION CHECK/BOOSTER " It is important for the quality of our research that participants focus as they complete this short study. Please close any other tabs or windows that you might have open, turn off any music or distracting media, and give this study your full attention.

Please indicate your likelihood of paying full attention during this study:" [1: Extremely Unlikely... 7: Extremely Likely]

Given the bounded nature of the minimum and maximum values for the variables in the experimental designs, outliers are considered legitimate values and will not be deleted.

**Missing data**

observations with missing data on model-relevant variables will be excluded from analyses

**Exploratory analysis**

No response

**Other**

No response

### A.2.6 Study 1c

**Priming intro**

"First Scenario

For this scenario, you have been assigned to Yellow. Your role was randomly assigned.

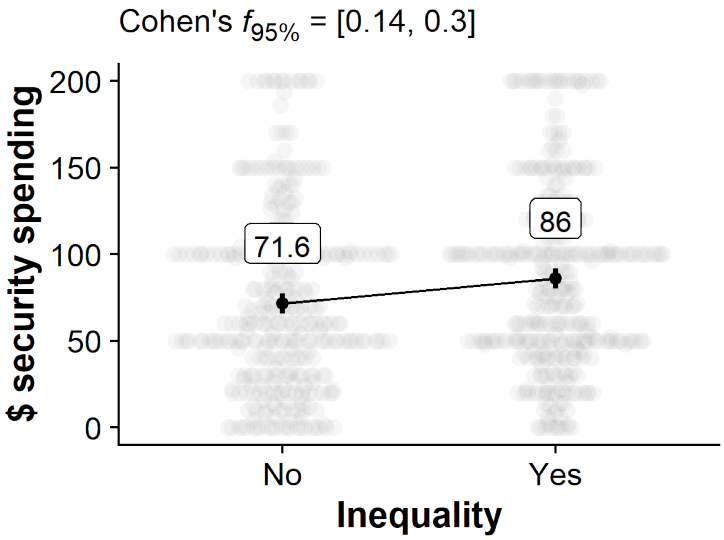
You and your partner (Blue) have each been given $150. Blue knows how much money you each have received.

Shortly, Blue will decide whether they will try to steal money from you."

**Table A.4:** Study 1c Descriptive Statistics

| Variable | Mean | SD | Cron. $\alpha$ 95% CI |
|---|---|---|---|
| Age | 35.2 | 12.9 | |
| Perceived envy | 3.5 | 1.5 | [0.95, 0.97] |
| Perceived attack likelihood | 5.6 | 1.2 | |
| Security consumed | 35.1 | 23.6 | |

**Figure A.4:** Study 1c, between-subjects effects of inequality on security spending.




*Notes:* Means (95% CI) and raw data, horizontally jittered according to kernel density.

**Table A.5:** Study 1c, ANCOVA; inequality on perceived partner envy.

| | df | MSE | $F$ | $p$ | Cohen's $f$ | 95% CI |
|---|---|---|---|---|---|---|
| Inequality | 1, 662 | 2.16 | 510.24 | <.001 | 0.88 | [0.79, 0.97] |
| Intro security spend. | 1, 662 | 2.16 | 1.83 | .18 | 0.05 | [0, 0.13] |

**Table A.6:** Study 1c, ANCOVA; inequality on perceived attack likelihood.

| | df | MSE | $F$ | $p$ | Cohen's $f$ | 95% CI |
|---|---|---|---|---|---|---|
| Inequality | 1, 662 | 2.21 | 57.63 | <.001 | 0.3 | [0.22, 0.37] |
| Intro security spend. | 1, 662 | 2.21 | 6.17 | .01 | 0.1 | [0.02, 0.17] |

# A.3 Study 2

# A.4 Study 2a

## A.4.1 Materials

**Slider task feedback**

"Results

You have correctly moved XX[piped] sliders in xx[piped] seconds. You correctly placed more sliders than your partner.

Performance on this slider task is solely the result of the player's efforts. Studies have found that performance is not related to intelligence or any other personality traits. "

**Meritocracy manipulation**

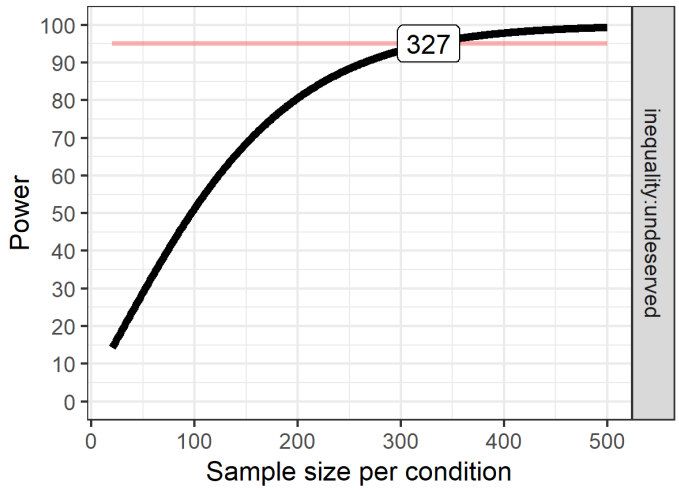
[Merit] "For this scenario, because you correctly moved more sliders than your partner, you have been assigned to Yellow."

[Random] "For this scenario, you have been assigned to Yellow. Your role was randomly chosen."

**Figure A.5:** Values for power analysis, 2b*2b ANOVA (Study 2a & b).


*Notes:* Max possible value is $1.00; bars indicate +/-1 *SD*. Specified sample size is per-between-condition in 2b*2b for interaction effects

**Figure A.6:** Sample size thresholds, 95% power in Studies 2a and b.




*Notes:* Specified sample size is per-between-condition in 2b*2b for interaction effects

# A.5 Study 3

## A.5.1 Study 3 pre-registration

### Research Questions

Does country-level economic inequality lead to the consumption of security products?

**Table A.7:** Power of pairwise contrasts, 2b*2b ANOVA (Studies 2a,b)

| | power | effect_size |
|---|---|---|
| ineq_1_undes_1_*VS*_ineq_1_undes_0 | 99.92 | -0.4 |
| ineq_1_undes_1_*VS*_ineq_0_undes_1 | 99.92 | -0.4 |
| ineq_1_undes_1_*VS*_ineq_0_undes_0 | 99.92 | -0.4 |
| ineq_1_undes_0_*VS*_ineq_0_undes_1 | 5.00 | 0.0 |
| ineq_1_undes_0_*VS*_ineq_0_undes_0 | 5.00 | 0.0 |
| ineq_0_undes_1_*VS*_ineq_0_undes_0 | 5.00 | 0.0 |

**Hypotheses**

H4) In a multilevel regression, Nation-level income inequality will be positively associated with individual-level adoption of security measures (adjusting for covariates) versus H4null) No effect of inequality Data Description

**Datasets used**

Combination of three archival datasets: 1) the ICVS [International Crime Victimization Survey; for security and individual-level measures; Van Kesteren(2010)],

2) Standardized World Income Inequality Database (SWIID, version 9; for nation inequality; Solt, 2020)

3) the Penn World Tables (PWT, version 10.0; for nation GDP; Feenstra, Inklaar, & Timmer, 2015).

2004-2006 time period as main analysis for these databases, due to availability of variables, number of countries, relative recency, and data completeness

**Data availability**

```
The dataset is publicly available
```

**Data access**

No response

**Data identifiers**

ICVS: `http://doi.org/10.17026/DANS-XNJ-RMB2`, SWIID: `http://doi.org/10.1111/ssqu.12795`, PWT: `http://doi.org/10.1257/aer.20130954`

**Access date**

These databases were accessed as follows: ICVS, September 1, 2020; SWIID, October 21, 2020, and PWT, March 17, 2021.

**Data collection procedures**

Together, the SWIID is made up of "15,730 Gini coefficients from 2,984 country-years in 196 countries or territories (Solt, 2020, p. 5)," all extracted from 384 different sources. The SWIID is based on the Luxembourg Inequality Study (AKA. LIS Cross-National Data Center). These LIS observations serve as baselines to compare against other gini estimates in the same country-year. These relationships are then used to predict the value of the LIS gini, when data from the LIS is absent, but other information is available. The SWIID is currently tested with 'k-fold cross-validation,' a machine-learning technique to iteratively train and test the data in predicting values in the LIS.

See attached documents for additional information

**Codebook**

See attached for ICVS codebook, `https://fsolt.org/swiid/` for detailed SWIID information, and `https://www.rug.nl/ggdc/productivity/pwt/?lang=en` for PWT information

`s2_01_icvs_prep_report.html`

**Variables**

**Manipulated** variables

NA

**Measured variables**

Outcome: Security measures (summed following binary items: burglar alarm, special door locks, window grills, high perimeter fence, and presence of a caretaker who provides security) - variable will be winzorized for analysis (robustness check conducted for unwinzorized variant), In event of single logistic regression, variable will be coded as 0: no consumption, and 1: 1 or more

Predictors: Gini index (from SWIID - disposable income)

Covariates Age (5-year bins): 16-70+ Gender (male/female) Employment status (employed (army/working)/unemployed("looking for work," "keeping home," "retired," "disabled," "still in school," or "other.")) Individual victimization experiences (summed binary items: car thefts, bicycle thefts, burglaries, attempted burglaries, robberies, and thefts) Nation-level average of victimization experiences (from above; mean-centered at nation-level) Country GDP per capita (from PWT - real expenditure side; scaled as z-score)

Gini and GDP values will be averaged across study sweep periods and mean-centered at nation-level

**Unit of analysis**

The ICVS (Van Kesteren, 2010), initially had which has surveyed over 338,056 people across 75 different countries since 1989

The ICVS has been collected across eight different sweeps (1: 1989 [13 countries], 2: 1992-1994 [29], 3: 1995-1998 [44], 4: 1999-2003 [46], 5:2004-2006 [18], 6: 2010 [6], 7: 2014 [5], 8: 2015 [1]; with an additional EU-specific sweep in 2005 [16])

United Kingdom data is duplicated with its constituent countries (e.g., scotland, ireland), so observations from lower-level countries are deleted to increase availability of inequality data.

This registration opts to combine the 2004-2006 survey sweep with the 2005 EU-specific sweep, yielding an initial count of 34 countries and 88,844 individuals, but see missing data section for final expected sample size

**Missing data**

Individuals and countries with missing data on model-relevant variables will be dropped (e.g., Switzerland is completely missing security measures)

After screening for country-level missingness on key variables, in addition to "refusals" and "do not know" responses for security measures, and individuals who live in an institution (and were not asked security items), k = 32 countries, and n = 79,864 participants were retained for primary analyses (average 2,496 respondents per country)

**Statistical outliers**

Outliers will be defined using a sample linear model for level-2 variables, and observations flagged as outliers (residuals have a cook's distance value of $> 4/n$) will have those predictor variables winzorized (as they are expected to be legitimate values) and compared with original analysis as a robustness check

**Sampling weights**

The ICVS dataset includes frequency weights for all included participants. These weights will be used whenever possible, as country-wide household surveys are unlikely to yield samples that are perfectly representative of their populations. Pilot analyses did indicate possible convergence issues when weights are included. As such, the following contingency plan will be followed during analysis. 1) Attempt weighted ordinal regression, if the weighted ordinal model fails to converge, 2) the ordinal model will be conducted without frequency weights, elsewise, 3) a weighted mixed logistic regresssion will be conducted, then 4) an unweighted mixed logistic regression will be attempted. Knowledge of Data

**Prior Publication/Dissemination**

NA

**Prior knowledge**

DN: The data has been accessed, cleaned, and explored, with some sample analyses conducted on a similarly-constructed (but theoretically distinct) dependent variable - victimization experiences. DN has also read published research and codebooks on the datasets. Nothing pertaining to security measures

MM: Has seen summary reports of data Analyses

**Statistical models**

Analyses will predict security consumption as an ordinal variable (0: no security consumption, 1: one unit of security consumption, 2: two units, or 3: three or more units), using a cumulative link mixed model.

ordered(security_winz) ~ gini_2004_6_cent + gdppc_2004_6_scale + victim_nation_cent + num_victim_5yr + age_cent + employed + male + (1 | country)

The hypothesis will be tested based on the significance of the coefficient 'gini_2004_6_cent'

**Effect size**

NA

**Statistical power**

NA

**Inference criteria**

$p < .05$, one-tailed b, 95% confidence intervals.

**Assumption Violation/ Model Non-Convergence**

Pilot analyses did indicate possible convergence issues when weights are included. As such, the following contingency plan will be followed during analysis. 1) Attempt weighted ordinal regression, if the weighted ordinal model fails to converge, 2) the model will be conducted without frequency weights, elsewise, 3) a weighted mixed logistic regresssion will be conducted, then 4) an unweighted mixed logistic regression will be attempted.

For the ordinal regression, proportional odds tests do not exist yet for mixed models. As such, separate logistic regressions will be conducted for the analyses for each incremental step in the dependent variable (e.g., 0 vs. 1, 1 vs. 2, 2 vs. 3). The regression coefficients for inequality will then be compared to one another using z-tests. If there is a significant difference amongst the predictors (suggesting a violation of the proportional odds assumption), then the separate logistic regressions will be presented as the primary analysis instead.

**Reliability and Robustness testing**

**Robustness:**

The focal analysis will be compared with the following models, ordinal -> logistic regressions where applicable:

1) For the main model, analyses will be repeated with regular (unwinzorized) security_total (for case of ordinal regression - N/A for logistic)

2) A supplementary analysis will be conducted on a different sweep period of the ICVS (1999-2003). Anticipated n = 70,264 (participants, unique observations from 2004-06), with k = 41 countries (starting with 56 countries and 92,943 rows)

ordered(security_winz) ~ gini_1999_03_cent + gdppc_1999_03_cent + victim_nation_cent + num_victim_5yr + male + age_num + house + other_house + shanty + partnered + (1|country)

partnered: married or living together (vs. single, divorced, widowed)

In addition to the larger number of countries, this 1999-2003 sweep also has sufficient data on respondent housing to include housing variables in the model (apartment dwelling as reference), which is important because housing is a notable covariate/confounder in the 2004-06 sweep (the dependent measure is predominantly about household measures of security).

In order to test robustness with the same variables, the 3) k = 32 data and the 4) k = 41 data will be run with the following reduced model (excluding partnered, employed, and housing)

ordered(security_winz) ~ gini_2004_6_cent + gdppc_2004_6_scale + victim_nation_cent + num_victim_5yr + age_cent + male + (1 | country)

For all of these models, analyses will be repeated with winzorized level-2 variables:

5) regular k = 32 model
6) unwinzorized security_total k = 32 model
7) full k = 41 model
8) small k = 32 model
9) small k = 41 model

**Exploratory analysis**

Model changes such as interactions and exponents may be added to improve model fit

5-year lagged inequality, gini*GDP, polynomials

### A.5.2 Notes on pre-registered plan

An ordinal regression carries an assumption of proportional odds: that the predictors' effects are consistent across the different levels of the dependent variable. Currently, we are not aware of any available methods for testing the proportional odds assumption for a mixed ordinal regression; existing methods only serve as a test on single-level data. As such, we will attempt to test this assumption using multiple logistic regressions, conducted for each rank threshold (e.g., 0|1, 1|2, 2|3). Since inequality is the major predictor of interest, only this coefficient will be compared across logistic models, which will help attenuate the impact of Type I errors. The proportional odds assumption will be deemed as violated if the regression coefficients for inequality are significantly different across models. If there is a significant difference amongst the predictors (suggesting a violation of the proportional odds assumption), then the separate logistic regressions will be presented as the primary analysis instead.

Main analyses will be conducted using the r package ordinal, with the following mixed model specification:

```
ordered(security_winz) ~ gini_2004_6_cent + gdppc_2004_6_scale
+ victim_nation_cent + num_victim_5yr + age_cent + employed
+ male + (1 | country)
```

**Robustness checks**

1) A supplementary analysis will be conducted on a different sweep period of the ICVS (1999-2003). Anticipated n = 70,264 (unique participants), with k = 41 countries

2) For the main model, as determined by contingencies, analyses will be repeated with regular (unwinzorized) security_total

In order to test robustness with the same variables:

the 3) k = 32 data and the 4) k = 41 data will be run with the following reduced model (excluding partnered, employed, and housing)

`ordered(security_winz) ~ gini_2004_6_cent + gdppc_2004_6_scale + victim_nation_cent + num_victim_5yr + age_cent + male + (1 | country)`

A sample linear model revealed nonproblematic levels of multicollinearity, maximum VIF of 2.12

For all of these models, analyses will be repeated with winzorized level-2 variables:

5) regular k = 32 model
6) unwinzorized security_total k = 32 model
7) full k = 41 model
8) small k = 32 model
9) small k = 41 model

For values within reasonable scope of response variables, outliers are considered legitimate values and will not be deleted. Outliers will be defined using a sample linear model for level-2 variables, and observations flagged as outliers if their residuals have a cook's distance value of $> 4/n$ will have those predictor variables winzorized (as they are expected to be legitimate values). As a robustness check, analyses will be repeated with influential observations being winzorized at the dependent variable to the 5th and 95th percentile values. Influential observations will be flagged as having model residuals' cook's distance values exceeding the threshold $4/n$; where n = # of observations. Degrees of freedom were calculated using methods defined in Snijders

**Table A.8:** Convergence issues across 100 analyses for 2004-06 period

| | base | scaled | robust | robust/scaled |
|---|---|---|---|---|
| Unweighted log | 96 | 0 | 100 | 0 |
| Unweighted ord | 6 | 16 | 9 | 9 |
| Weighted log | 96 | 0 | 100 | 0 |
| Weighted ord | 6 | 16 | | |

**Table A.9:** Convergence issues across 100 analyses for 1999-03 period (robustness check dataset)

| | base | scaled | robust | robust/scaled |
|---|---|---|---|---|
| Unweighted log | 91 | 0 | 91 | 0 |
| Unweighted ord | 2 | 0 | 34 | 37 |
| Weighted log | 14 | 18 | 14 | 3 |
| Weighted ord | 80 | 72 | | |

& Bosker (2011)

level 1: $M$-$r$-1

level 2: $N$-$q$-1

$M$: total number of level-one units, $r$: total number of explanatory variables, $N$: number of level-two units, $q$: number of level-2 predictors.

### Study 3 alternate models

**Figure A.7:** Study 3, dot-whisker plot for mixed weighted logistic regression, 2004-6 period robustness variables.

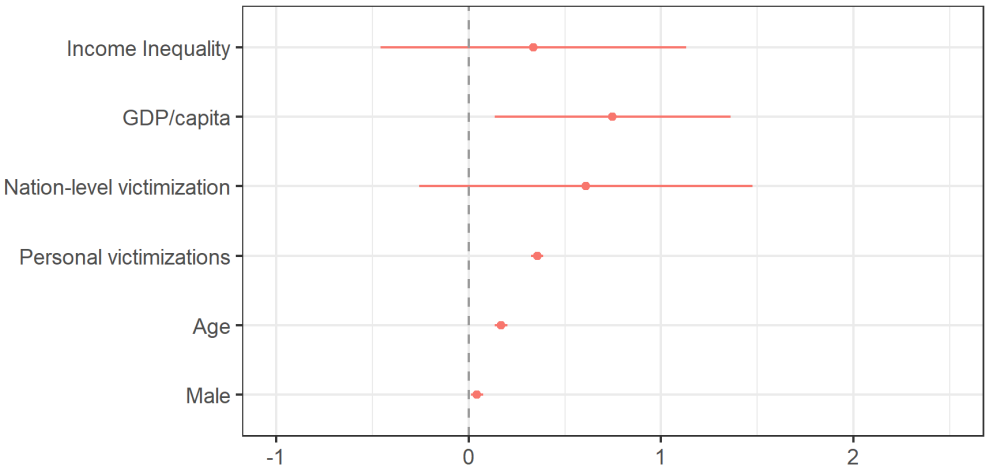



**Table A.10:** Study 3, mixed weighted logistic regression, 2004-6 period, robust.

| term | *b* | 95% CI | *z* | *p* |
|---|---|---|---|---|
| (Intercept) | 0.31 | [0.06, 0.55] | 2.44 | .01 |
| Nation gini | 0.16 | [-0.21, 0.52] | 0.83 | .41 |
| Nation GDP per-capita | 0.38 | [0.07, 0.69] | 2.4 | .02 |
| Nation-level victimization | 1.23 | [-0.51, 2.97] | 1.38 | .17 |
| Individual victimizations | 0.2 | [0.18, 0.22] | 21.25 | <.001 |
| Age | 0.08 | [0.07, 0.1] | 10.42 | <.001 |
| Male | 0.04 | [0.01, 0.08] | 2.82 | .005 |

*Note:* AIC = 96726.77 , BIC = 96801.06, Loglik = -48355.38

**Figure A.8:** Study 3, dot-whisker plot for mixed weighted logistic regression, 1999-03 period, robustness variables.

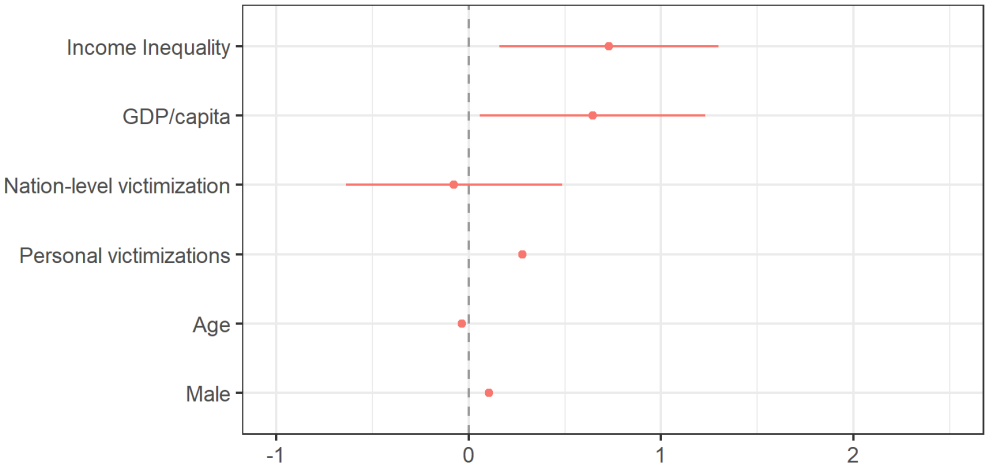



**Table A.11:** Study 3, mixed weighted logistic regression, 1999-03 period, robustness comparison

| term | *b* | 95% CI | *z* | *p* |
|---|---|---|---|---|
| (Intercept) | 0.71 | [0.46, 0.96] | 5.56 | <.001 |
| Nation gini | 0.43 | [0.09, 0.76] | 2.51 | .01 |
| Nation GDP per-capita | 0.34 | [0.03, 0.64] | 2.16 | .03 |
| Nation-level victimization | -0.14 | [-1.21, 0.92] | -0.26 | .79 |
| Individual victimizations | 0.17 | [0.17, 0.17] | 1379.09 | <.001 |
| Age | -0.02 | [-0.02, -0.02] | -181.93 | <.001 |
| Male | 0.11 | [0.11, 0.11] | 568.03 | <.001 |

*Note:* AIC = 679184564.06 , BIC = 679184637.34, Loglik = -339592274.03

2) weighted trimmed ordinal

**Figure A.9:** Study 3, dot-whisker plot for mixed weighted ordinal regression, predicting security consumption.

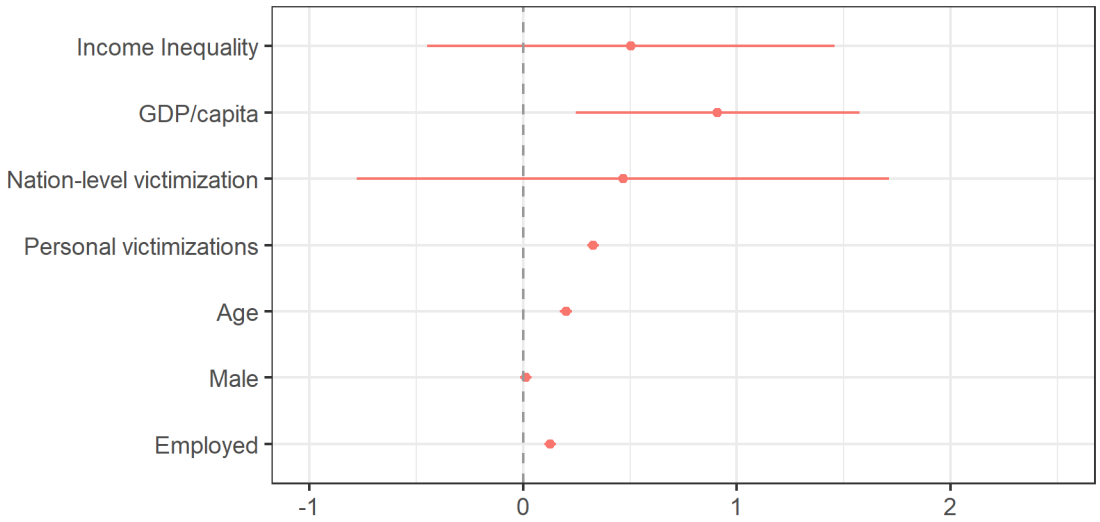



**Table A.12:** Study 3, mixed weighted ordinal regression, 2004-6 period.

| term | *b* | 95% CI | *z* | *p* |
|---|---|---|---|---|
| 0\|1 | -0.3 | [-0.56, -0.03] | -2.21 | .03 |
| 1\|2 | 1.18 | [0.92, 1.45] | 8.75 | <.001 |
| 2\|3 | 2.54 | [2.27, 2.8] | 18.75 | <.001 |
| Nation gini | 0.03 | [-0.02, 0.08] | 1.04 | .30 |
| Nation GDP per-capita | 0.46 | [0.13, 0.8] | 2.69 | .007 |
| Nation-level victimization | 0.94 | [-1.56, 3.45] | 0.74 | .46 |
| Individual victimizations | 0.19 | [0.17, 0.2] | 23.7 | <.001 |
| Age | 0.006 | [0.005, 0.007] | 13.98 | <.001 |
| Employed | 0.13 | [0.1, 0.15] | 8.96 | <.001 |
| Male | 0.01 | [-0.01, 0.04] | 0.95 | .34 |

4) 1999 analyses (following same contingency)

weighted ordinal was taking over 48 hours to run (computer restarted)

5) k = 32 data will be run with the following reduced model (excluding partnered, employed, and housing)

6) k = 41 reduced model

**Table A.13:** Study 3, mixed unweighted ordinal regression, robust set, scaled

| term | *b* | 95% CI | *z* | *p* |
|---|---|---|---|---|
| 0\|1 | -0.35 | [-0.62, -0.09] | -2.63 | .009 |
| 1\|2 | 1.12 | [0.86, 1.39] | 8.33 | <.001 |
| 2\|3 | 2.48 | [2.21, 2.74] | 18.33 | <.001 |
| Nation gini | 0.22 | [-0.22, 0.67] | 0.99 | .32 |
| Nation GDP per-capita | 0.46 | [0.13, 0.8] | 2.71 | .007 |
| Nation-level victimization | 0.93 | [-1.7, 3.57] | 0.69 | .49 |
| Individual victimizations | 0.19 | [0.17, 0.2] | 23.88 | <.001 |
| Age | 0.09 | [0.07, 0.1] | 12.25 | <.001 |
| Male | 0.03 | [0.007, 0.06] | 2.51 | .01 |

S1) ordinal analyses will be repeated with regular (unwinzorized) security_total

S2) regular k = 32 model S3) unwinzorized security_total k = 32 model S4) full k = 41 model S5) small k = 32 model S6) small k = 41 model

**Figure A.10:** Study 3, dot-whisker plot for mixed weighted logistic regression, 1999, gini*GDP interaction

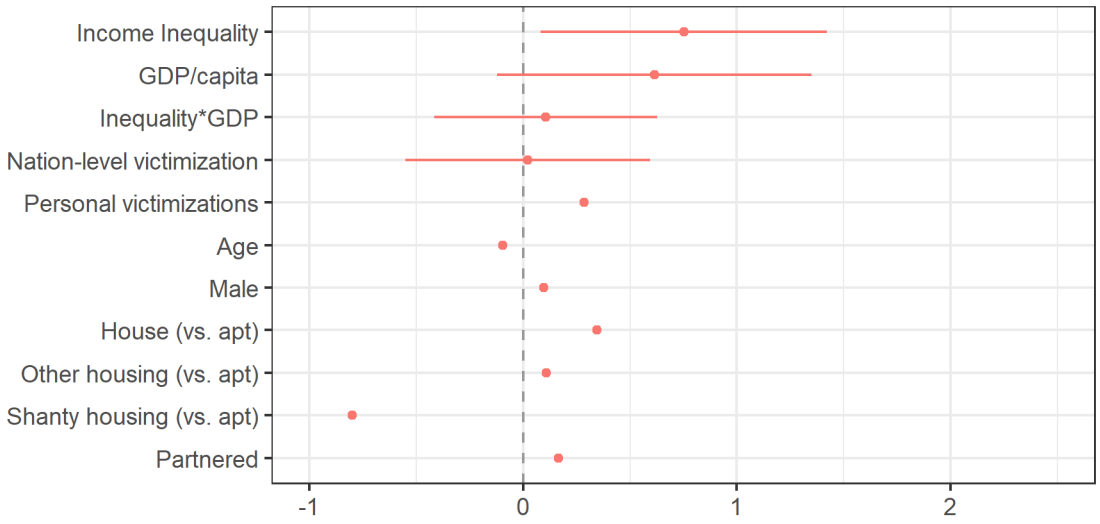


**Table A.14:** Study 3, weighted mixed logistic regression, 1999, gini*GDP interaction

| term | *b* | 95% CI | *z* | *p* |
|---|---|---|---|---|
| (Intercept) | 0.64 | [0.28, 0.99] | 3.52 | <.001 |
| Nation gini | 0.44 | [0.05, 0.84] | 2.2 | .03 |
| Nation GDP per-capita | 0.32 | [-0.06, 0.7] | 1.64 | .10 |
| Nation-level victimization | 0.04 | [-1.04, 1.13] | 0.08 | .94 |
| Individual victimizations | 0.17 | [0.17, 0.17] | 1388.18 | <.001 |
| Male | 0.1 | [0.1, 0.1] | 508.78 | <.001 |
| Age | -0.003 | [-0.003, -0.003] | -462.8 | <.001 |
| House | 0.35 | [0.34, 0.35] | 1494.5 | <.001 |
| Other house type | 0.11 | [0.11, 0.11] | 156.46 | <.001 |
| shanty | -0.8 | [-0.8, -0.8] | -1371.96 | <.001 |
| Partnered | 0.17 | [0.17, 0.17] | 824.88 | <.001 |
| Gini*GDP | 0.11 | [-0.41, 0.63] | 0.4 | .69 |

*Note:* AIC = 672718571.520821 , BIC = 672718690.600829, Loglik = -336359272.76041

**Figure A.11:** Study 3, dot-whisker plot for mixed weighted logistic regression, 1999, squared gini

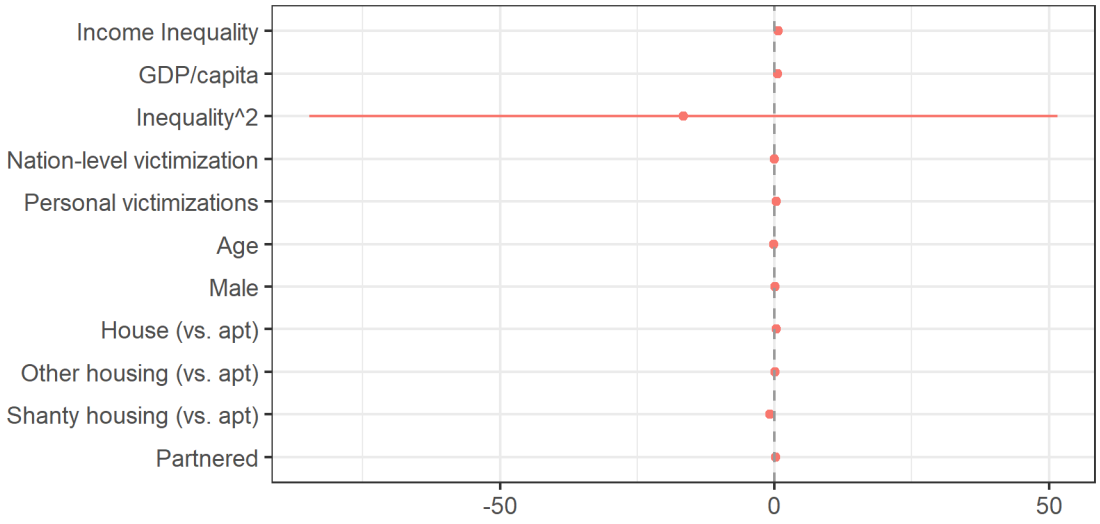


**Table A.15:** Study 3, weighted mixed logistic regression, 1999, squared gini

| term | *b* | 95% CI | *z* | *p* |
|---|---|---|---|---|
| (Intercept) | 0.58 | [0.34, 0.83] | 4.71 | <.001 |
| Nation gini | 0.41 | [0.08, 0.74] | 2.43 | .01 |
| Gini$^2$ | -16.58 | [-84.76, 51.6] | -0.48 | .63 |
| Nation GDP per-capita | 0.29 | [-0.02, 0.6] | 1.85 | .06 |
| Nation-level victimization | 0.03 | [-1.03, 1.08] | 0.05 | .96 |
| Individual victimizations | 0.17 | [0.17, 0.17] | 1386.73 | <.001 |
| Male | 0.1 | [0.1, 0.1] | 508.19 | <.001 |
| Age | -0.003 | [-0.003, -0.003] | -462.98 | <.001 |
| House | 0.35 | [0.34, 0.35] | 1498.21 | <.001 |
| Other house type | 0.11 | [0.11, 0.11] | 156.17 | <.001 |
| shanty | -0.8 | [-0.8, -0.8] | -1373.02 | <.001 |
| Partnered | 0.17 | [0.17, 0.17] | 823.87 | <.001 |

*Note:* AIC = 96693.75 , BIC = 96786.62, Loglik = -48336.88

**Figure A.12:** Study 3, dot-whisker plot for mixed weighted logistic regression, including polynomial

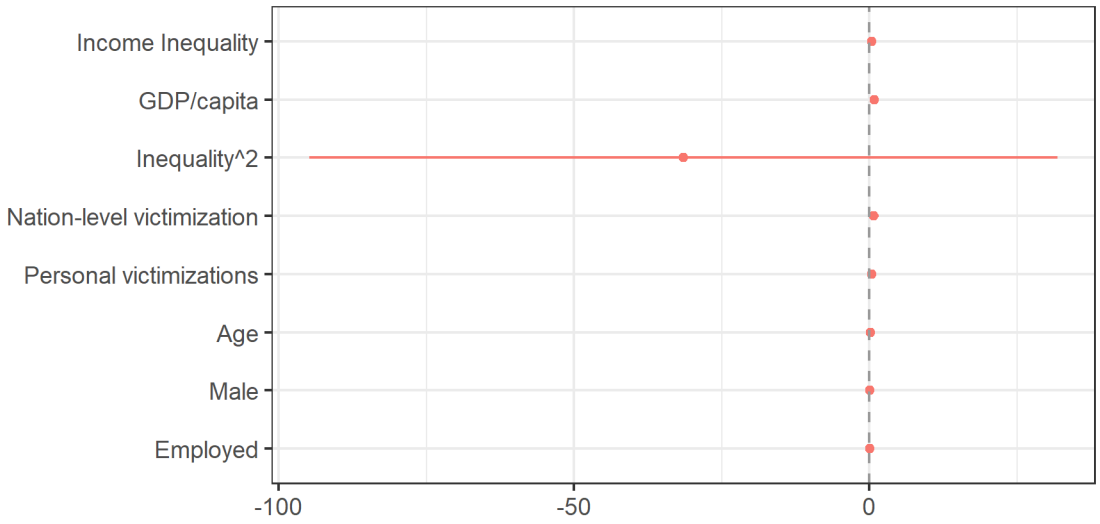


**Table A.16:** Study 3, weighted mixed logistic regression, Gini squared

| term | *b* | 95% CI | *z* | *p* |
|---|---|---|---|---|
| (Intercept) | 0.27 | [0.02, 0.51] | 2.15 | .03 |
| Nation gini | 0.17 | [-0.2, 0.53] | 0.9 | .37 |
| Gini$^2$ | -31.5 | [-94.85, 31.85] | -0.97 | .33 |
| Nation GDP per-capita | 0.41 | [0.1, 0.72] | 2.55 | .01 |
| Nation-level victimization | 1.45 | [-0.33, 3.22] | 1.6 | .11 |
| Individual victimizations | 0.2 | [0.18, 0.22] | 21.08 | <.001 |
| Age | 0.1 | [0.08, 0.11] | 11.52 | <.001 |
| Employed | 0.1 | [0.07, 0.13] | 6 | <.001 |
| Male | 0.03 | [-0.004, 0.06] | 1.74 | .08 |

*Note:* AIC = 96693.75 , BIC = 96786.62, Loglik = -48336.88

**Table A.17:** Study 3, ordinal regression, 1999 period

| term | *b* | 95% CI | *z* | *p* |
|---|---|---|---|---|
| 0\|1 | -0.47 | [-0.69, -0.25] | -4.11 | <.001 |
| 1\|2 | 1.34 | [1.11, 1.56] | 11.67 | <.001 |
| 2\|3 | 2.59 | [2.37, 2.82] | 22.54 | <.001 |
| Nation gini | 0.04 | [0.01, 0.06] | 2.83 | .005 |
| Nation GDP per-capita | 0.42 | [0.16, 0.69] | 3.12 | .002 |
| Nation-level victimization | 0.2 | [-0.73, 1.13] | 0.42 | .67 |
| Individual victimizations | 0.17 | [0.15, 0.19] | 18.63 | <.001 |
| Male | 0.11 | [0.09, 0.14] | 7.91 | <.001 |
| Age | -0.003 | [-0.004, -0.002] | -6.68 | <.001 |
| House | 0.45 | [0.42, 0.49] | 24.96 | <.001 |
| Other house type | 0.21 | [0.11, 0.31] | 4.02 | <.001 |
| shanty | -0.72 | [-0.82, -0.63] | -14.86 | <.001 |
| Partnered | 0.26 | [0.23, 0.29] | 17.5 | <.001 |

*Note:* AIC = 96693.75 , BIC = 96786.62, Loglik = -48336.88

**Table A.18:** Study 3, weighted ordinal regression, reduced variable set, 1999 period

| term | *b* | 95% CI | *z* | *p* |
|---|---|---|---|---|
| 0\|1 | -0.69 | [-1.67, 0.29] | -1.38 | .17 |
| 1\|2 | 1.09 | [0.11, 2.07] | 2.18 | .03 |
| 2\|3 | 2.33 | [1.35, 3.31] | 4.64 | <.001 |
| Nation gini | 0.04 | [0.006, 0.07] | 2.31 | .02 |
| Nation GDP per-capita | 0.5 | [0.17, 0.83] | 2.94 | .003 |
| Nation-level victimization | 0.05 | [-2.38, 2.48] | 0.04 | .97 |
| Individual victimizations | 0.16 | [0.14, 0.18] | 17.4 | <.001 |
| Age | -0.001 | [-0.002, 0] | -2.8 | .005 |
| Male | 0.13 | [0.1, 0.16] | 8.98 | <.001 |

*Note:* AIC = 96693.75 , BIC = 96786.62, Loglik = -48336.88

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