An Honest Dissertation:
Exploring the Roles of Culture and Character in Shaping Individual Dishonesty

by

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Dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the Department of Psychology and Neuroscience in the Graduate School of Duke University

2015
ABSTRACT

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Abstract

The question of what leads an individual to act dishonestly interests researchers, policy-makers, and lay-people alike. While a growing body of research suggests that dishonest behavior is typically limited, and reflects a balance of internal and external incentives, important questions remain unanswered. To what extent is honest behavior guided by stable, internal factors (i.e. moral character), and to what extent is it shaped by situational factors? This question is the subject of continuing and recently revived debate. To what extent do socio-cultural factors impact dishonesty, and to what extent is dishonesty universal? Casual observation suggests significant cross-cultural variation in terms of specific dishonest behaviors (e.g. soliciting bribes), but this source of variation has received little research attention. In five related research chapters encompassing three studies, I explore questions about character and culture using empirical research methods. Using a behavioral die task, I find similar patterns of dishonest behavior across individuals from different countries, though within-country differences are also observed. Using survey data, I find that internal sanctions are the most important deterrent of dishonesty across cultures. In addition, I find that that specific dishonest behaviors vary across cultures, and according to domains. Domain-specific dishonesty and socio-cultural influences are also evident in a study involving socially connected pairs of individuals. I conclude that dishonest tendencies may be best characterized as both universal and culturally sensitive. Furthermore, moral character may be construed as a multidimensional construct, expressed differently across different domains of life.
Dedication

I dedicate this dissertation to my Mom and Dad (Robb and Nancy Mann), to my maternal grandparents, Bob and Helen Wright, and to my late paternal grandparents, Bruce and Dorothy Mann. Thank you for deeply valuing education, for supporting me throughout my academic journey, and for instilling in me a spirit of inquiry.
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1. Introduction

Jared, a restaurant server, is preparing to file his taxes, and tabulating his cash tips from the previous year. Does he report the full amount? Cara, a college student, is behind on a major essay and happens to have a copy of her friend’s essay on the same topic for a different class. Does she “recycle” her friend’s essay? Sam, a husband, is obsessed with collecting rare foreign dolls, a habit his wife does not approve of. Does he continue to build his collection in secret? In every domain of life, people face conflicts between the interests of their society (or social unit) and their own personal interests. In such situations, what determines whether a person will act honestly – or not?

The overarching goal of this dissertation is to gain insight into what shapes an individual’s decisions about whether or not to be honest. It explores two central questions. First, to what extent are decisions about whether to be honest driven by moral character, and to what extent are such decisions driven by situational influences? Second, to what extent are tendencies toward honesty universal and to what extent are these tendencies culturally variable? The first question asks whether tendencies toward honesty are robust across situations, while the second asks whether tendencies toward honesty are robust across societies/cultures. Each of these questions looks at whether dishonesty is motivated by factors that are endogenous or exogenous to the individual.

To say dishonesty is costly is quite an understatement. Estimates of tax fraud in the USA alone surpass $300 billion per year. Fraudulent returns of purchased items cost American retailers between $9 billion and $17 billion per year (Speights & Hilinski, 2013). Globally, the annual costs of corruption are estimated at one trillion dollars, leading one advocacy organization to refer to it corruption as “the one trillion dollar scandal” (ONE, 2014). While the costs of dishonesty in other domains of life, such as romantic relationships, may be harder to measure, the emotional impacts can
undenitely be devastating, not to mention costs of divorce. Furthermore, academic institutions lament the prevalence of student cheating, which, in America at least, appears to be on the rise (McCabe, Treviño, & Butterfield, 2001). Societies have reason to be concerned about dishonesty.

For the scope of this dissertation, I define dishonesty as behavior that violates a formal or informal social contract for selfish ends, typically involving deception or concealment. Colloquially, we might say that dishonesty means to lie, cheat, or steal.

1.1 Are We Internally or Externally Motivated to be Honest?

One dominant line of thinking about what motivates honesty derives from the rational actor paradigm, which focuses entirely on external incentives. This paradigm assumes that individuals are both selfish (interested in maximizing their own material benefits) and rational (base their decisions on cost-benefit calculations). Grounded in these assumptions, the rational actor paradigm presumes that decisions about whether or not to be honest are the result of people weighing the potential payoffs of acting dishonestly against the probability of getting caught and the severity of the consequences if apprehended (Becker, 1968). The rational actor framework is often traced back to Adam Smith, and his classic 1776 œuvre, Wealth of Nations. Smith wrote, “It is not from the benevolence of the butcher, the brewer, or the baker, that we expect our dinner, but from their regard to their own interest” (Smith, 1776/1909, p. 19-20). The rational actor framework remains central to the fields of law and economics, and a significant guiding force in public policy.

Alongside this paradigm, evidence from psychology, neuroscience, behavioral economics, and even experimental economics has pointed to the notion that internal reward systems regulate our behavior even in the absence of external incentives (Mazar, Amir, & Ariely, 2008). For example, evidence from psychology suggests that people
engage in altruistic behavior with no tangible personal benefit, such as helping a person in need (Batson, 1994; Batson et al., 1991), and research involving economic games finds that people often donate money to anonymous strangers (Andreoni & Bernheim, 2009; Fehr & Fischbacher, 2003). Neuroimaging research has indicated that the reward centers of the brain, including areas of the striatum, are activated when individuals cooperate in a dictator game (Rilling et al., 2002), as well as when they punish others who violated a social norm (de Quervain et al., 2004). Other research has found that that moral identity is important to individuals’ self concepts, and related to moral action such as donating food to the needy (Aquino & Reed, 2002). Such findings are also aligned with Bandura’s social cognitive theory of morality, which proposes that individuals adopt internal moral standards and use these to regulate their behavior.

In light of such findings, Mazar, Amir, and Ariely (2008) proposed an alternative to the rational actor view of dishonesty, the theory of self-concept maintenance. This theory posits that while people are indeed motivated by external incentives for acting dishonestly, they are also internally motivated by the desire to maintain a positive (honest) self-concept. The theory of self-concept maintenance does not rule out the influence of external incentives for dishonesty, but proposes that internal incentives of believing one’s self to be a good person are at least as important drivers of behavior. In support of this theory, Mazar and colleagues found that reminding people of moral standards promoted honest behavior, while increasing the room for interpretation of their actions (for example, cheating for tokens with monetary value rather than money) promoted dishonest behavior.

In recent years, a wealth of behavioral evidence has amassed to challenge the notion that decisions about whether or not to be honest are driven solely by external incentives. Experimental research shows that when people have the opportunity to
increase their earnings by cheating without penalty, they tend to cheat – but only to a limited degree. These findings contrast with the rational actor framework, which would predict that in experimental situations in which there is no punishment for dishonesty, people should cheat to the maximal extent in order to maximize their payoff. Further challenging the notion that external costs and benefits exclusively drive decisions related to honesty, researchers have found that manipulating the monetary incentives in experiments has little influence on cheating behavior (Fischbacher & Heusi, 2008; Francesca Gino, Ayal, & Ariely, 2012; Mazar et al., 2008b; Wiltermuth, 2011).

In sum, the experimental evidence suggests that decisions about whether or not to be honest reflect the balance point between the external rewards of acting dishonestly and the internal rewards of preserving one’s moral self-concept. However, an important question naturally follows, namely: what determines this balance point, and therefore, what determines whether an individual will choose to be honest? Are such decisions relatively stable across contexts, or are they context-dependent? Notably, context can be thought about in two ways: either locally, in terms of particular situations, or more broadly, in terms of a person’s socio-cultural context. The next section considers context in terms of situations, surveying research on the question of whether honesty is based on moral character or situational factors.

### 1.2 (Dis)honest Behavior: Character-Based or Situation-Based?

Consideration of whether decisions to be honest are driven by moral character can be traced at least as far back as ancient Greece. Many ancient philosophers, most notably Aristotle, discussed virtues of character, or moral virtues: dispositional qualities that shaped moral actions. Moral character traits, such as honesty, were considered robust across situations and stable over time. (In addition, it was thought that moral virtues coincided in people, such that possessing a particular moral virtue such as
temperance made a person more likely to possess other moral virtues such as honesty. However, in this dissertation, I focus on moral character as it pertains to honesty in particular.) This thinking has carried forward into the philosophical tradition known as virtue ethics, revived by writers such as Anscombe (1958) and Rawls (2009).

From the perspective of modern psychology, honesty first became a prominent research topic in the 1920s. At the turn of the twentieth century, there was increasing interest in whether moral character guides ethical decisions in daily life. This research reached a culmination point in 1928, with Hartshorne and May’s publication of their now classic work investigating dishonest behaviors in children (Hartshorne & May, 1928). These researchers administered tests to approximately 11,000 children between the ages of eight and 16. The tests varied in specific material, type of test (intelligence tests, speed tests, coordination tests, etc.) and situation in which they were administered, but all tests allowed the researchers to measure cheating or dishonesty.

Hartshorne and May’s final analyses showed that the average intercorrelation for tests of the same type was .56, and the average correlations for any one type of test with any other type of test was .26, lower than expected. This finding led Hartshorne and May to propose the doctrine of specificity, or the claim that a person’s tendency to be honest or dishonest is particular to the specific situation. In their words, “…to say that an honest act is caused by a man’s honesty is like saying that it is cold because the temperature has fallen… [W]hatever honesty a man possesses resides not in a secret reservoir of honest virtue nor in the ideal of honesty which he may hold before himself as worthy of his best effort, but in the quality of the particular acts he performs” (p. 379). This research contrasted with traditional view of character in the virtue ethics tradition.

In social psychology, the view that behavior is based on specific situations is known as situationism. In line with the doctrine of specificity, this viewpoint questions
whether stable personality traits have substantive influence on behavior. Much research in social psychology aims at demonstrating the power of the situation in shaping behavior. In recent years, a wellspring of research has identified numerous situational factors impacting dishonest behavior. For example, the lighting of the room (C.-B. Zhong, Bohns, & Gino, 2010), categorization malleability of the action (Mazar et al., 2008), and presence of an obvious cheater (Gino, Ayal, & Ariely, 2009) have all been shown to influence dishonesty.

The doctrine of specificity was widely accepted by the field of psychology following Hartshorne and May’s impressive research effort. However, since the turn of the millennium, a small but growing number of psychology researchers have shown renewed interest in moral character. Fleeson and colleagues have critiqued Hartshorne and May’s interpretation of their cross-situational correlations, noting that based on the principle of aggregation (Epstein, 1979), low correlations between single observations do not undermine the possibility of broad character traits (Fleeson, Furr, Jayawickreme, Meindl, & Helzer, 2014). Similarly questioning the doctrine of specificity, Burton (1963) re-analyzed Hartshorne and May’s data using principle components analysis, an single factor related to honesty, suggesting that the author’s original interpretation of their data may have led to stronger conclusions than were actually warranted.

Furthermore, in the past decade personality researchers have proposed a personality dimension related to honesty, the Honesty-Humility dimension, or “H-factor” (Ashton & Lee, 2008). Personality researchers are beginning to move from the 5-factor OCEAN model to the 6-factor HEXACO model which includes the Honesty-Humility dimension, due in part to its improved reliability across different languages (Ashton & Lee, 2007; Ashton et al., 2014). This research lends empirical support to
renewed ideas that character traits significantly contribute to honest or dishonest behaviors.

In sum, the extent to which decisions about whether to be honest are based on moral character (stable, dispositional qualities that manifest across situations) or situational factors continues to be a topic of lively debate. In this dissertation, I will explore the role of moral character in ethical decision-making by asking people to report the likelihood of engaging in specific dishonest behaviors across a range of situations in particular life domains. Before outlining the research program, I turn to a second question of theoretical interest, namely: to what extent are decisions pertaining to honesty shaped by people’s socio-cultural contexts? This question considers context more broadly, as the total of a person’s life experiences.

1.3 (Dis)honest Behavior: Cultural or Universal?

Whether a person’s moral behavior is determined by innate tendencies that are “hard-wired” at birth or through experiential learning represents another age-old debate. In philosophy, this debate is referred to as nativism versus empiricism; in psychology, it is known as nature versus nurture. Regardless of terminology, the key question of interest for this dissertation is whether culture is a meaningful determinant of decisions to act (dis)honestly. If cultural context has little effect, it would suggest that these tendencies are universal, and therefore likely to be ingrained from birth.

Existing research has indicated that other people can powerfully influence dishonest behavior. For example, in a clever study involving a confederate, Gino and colleagues found that witnessing another person cheating increased participants’ dishonesty – unless the cheater was believed to be an out-group member, in which case it had the opposite effect (Gino et al., 2009). Social comparisons also matter; for example, John, Loewenstein, and Rick (2013) found that participants cheated more for a 5-
cent/point pay-rate than for a 25-cent/point pay-rate if they were aware that others were earning the higher amount. Furthermore, it appears that people demonstrate greater license to cheat when others also benefit from their dishonesty (Gino, Ayal, & Ariely, 2012; Wiltermuth, 2011).

However, while research in the situational tradition has identified several social factors related to dishonesty, until very recently this research has been limited to the influence of others at an immediate, local level. The question of how the broad social networks that comprise human societies affect dishonesty at the individual has remained something of a mystery.

A few significant research efforts have explored the connections between macro-level variables and social behavior of individuals by comparing participant pools across different societies and cultures. One of these was spearheaded by Joseph Henrich, who, along with eleven other experienced field researchers, administered canonical economic games in 15 small-scale societies (Henrich et al., 2005). The games included the dictator game, ultimatum game, and public goods game, each of which looks at how much money people will offer to others at a cost to themselves. It was found that the level of cooperative behavior demonstrated in these games varied across societies, and that this variation was related to the extent to which individuals within each society were economically integrated with one another.

A cross-cultural study by Gächter and colleagues looked at public goods game contributions across groups of participants from 16 societies, with six distinct cultural backgrounds (Gächter, Herrmann, & Thöni, 2010). In the public goods game, participants decide how much money to contribute to a common pool; money from this pool is multiplied by a fixed number and redistributed evenly across participants. Gächter and colleagues examined contributions over repeated rounds of the public
goods game, when the opportunity to punish other participants was present or absent. In both punishment-present and punishment-absent conditions, average contributions were similar across societies from the same cultural backgrounds, but varied significantly across societies from different cultural backgrounds. Unlike in Henrich and colleagues’ investigation, all societies sampled by Gächter and colleagues were developed and highly economically integrated, showing that in addition to economic integration, cultural factors are also relevant to decisions about whether to cooperate.

A recent study by an international team of researchers has examined honesty in sixteen countries using a coin flip task with a chocolate reward (Pascual-Ezama et al., 2014). Participants (students on university campuses) were asked to flip a coin in private, and report whether it landed white side or black side up. Reports of white side up were rewarded with a chocolate truffle, while reports of black side up earned no reward. Rewards were administered by the participant (self-reported treatment), or by the experimenter based on the participant’s written or verbal report (written and verbal reported treatments). In contrast to the cross-cultural findings from the cross-cultural studies mentioned previously, results did not reveal significant differences between countries on honesty. However, the power of this study to detect differences in dishonesty is questionable as only 90 participants were tested in each country (30 per treatment per country). Furthermore, since only half of participants would have flipped the unfavorable black side of the coin by chance, only 45 per country (15 per treatment per country) faced the decision about whether to act dishonestly. In addition, given that each of these participants faced only one decision about whether to be dishonest, the coin flip task was likely a noisier measure of individual dishonesty than a measure involving multiple trials. In the research reported in this dissertation, I also explore whether culture has an important effect on individuals’ dishonest behavior, by reporting
a study with high power to detect underlying differences in dishonest tendencies across countries should such differences exist.

1.4 Research Summary

In sum, this dissertation asks to what extent dishonesty is influenced by context, both at the local level (i.e. situational influences versus moral character) and at the global level (i.e. social or cultural influences versus universal tendencies). Research addressing these questions will be presented Chapters 2-7. Chapters 2, 3 and 4 report the results of a multifaceted study designed to assess the consistency of honest behavior across cultures and situations. This study involved ten participant samples from five countries in distinct cultural regions of the world. Our research team employed a behavioral die-rolling task to assess dishonesty in a new situation, and two surveys assessing the likelihood of acting dishonestly in everyday situations.

Chapter 2 uses data from the behavioral task to test whether the extent to which individuals are internally driven to be honest varies across cultures. Contrary to predictions from a separate participant sample, results indicate that individuals across countries engage in similar levels of dishonesty on a novel behavioral task. Building on these results, Chapter 3 uses a survey measure to compare the effects of external and internal sanctions on illegal dishonest behaviors, and to assess the stability of these effects across cultures. Results from linear mixed models suggest that internal sanctions are the most powerful deterrents of illegal behavior across cultures, with external (legal) sanctions having weaker and culturally variable effects. Finally, using data from a separate survey, Chapter 4 tests whether tendencies to be honest vary according to life domain (work, relationships, etc.). Findings indicate that for both individuals and countries, dishonesty varies according to life domain.
Chapters 5 and 6 further explore the effects of a person’s social and cultural networks on his or her dishonest tendencies. Chapter 5 examines whether particular lying tendencies are related among individuals who are socially connected to one another. Results show that socially connected individuals are uniquely related in their tendencies to tell particular types of lies. In Chapter 6, I revisit the question of whether cultural factors can influence dishonest behavior on a novel, abstract task. This study finds that individuals in Berlin with an East German background cheat more on a die-rolling task than individuals in Berlin with a West German background. While this finding raises the possibility that individuals’ “moral backbones” can be affected by cultural upbringing, it is also possible that the finding was driven by social comparison. Results from this study are also in line with the general finding of restrained dishonesty across cultures.
2. Comparing Cheating Behavior Across Cultures

2.1 Overview

This chapter examines the effect of culture on individuals’ core tendencies toward dishonesty. I examine individual dishonesty across ten participant samples from five countries varying in corruption and cultural values: China, Colombia, Germany, Portugal, and USA. In each country, our research team administered a study to students at major public universities and to the general public in coffee shops. Using iPads, we administered a die task as a behavioral measure of dishonesty. A separate group of participants predicted that dishonesty would vary according to countries’ corruption and cultural values scores, and demonstrated a home country dishonesty bias. In contrast to predictions, observed dishonesty was limited in magnitude and similar across countries, with the greatest differences observed between samples within countries. These findings suggest that individuals around the world have similar tendencies toward (mostly) honest behavior.

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2.2 Introduction

Dishonesty is a costly problem. Insurance fraud, shoplifting, academic dishonesty, media piracy, and myriad other acts of dishonesty erode the efficiency of societal institutions. It is estimated that for most countries, losses due to tax evasion causes exceed total spending on healthcare (The Tax Justice Network, 2011). However,
while dishonesty is a problem the world over, evidence suggests that countries vary substantially in the prevalence of specific dishonest behaviors, such as tax evasion (Richardson, 2006; Tsakumis, Curatola, & Porcano, 2007) or cheating on exams (Grimes, 2004; Waugh et al., 1995). These observations raise the question of how differences in macro-level cultural variables, such as corruption and cultural values, are related to the dishonesty of individuals.

While the last decade has seen a resurgence of research interest in the forces driving dishonesty (e.g. Fischbacher & Heusi, 2008; Gino, Ayal, & Ariely, 2009; Houser, Vetter, & Winter, 2012; John, Loewenstein, & Rick, 2013; Mazar, Amir, & Ariely, 2008), this research has focused on local, situational influences and drawn on homogenous participant samples, offering little insight into the relationship between macro-level cultural variables and individual dishonesty. From a theoretical standpoint, culture could impact individual dishonesty in two ways: by normalizing dishonesty in particular situations (such as bribing a police officer to avoid a ticket), and/or by impacting individuals’ core tendencies toward dishonesty. Evidence that corruption impacts situation-specific dishonesty was observed in clever study of UN diplomats in New York City, which found that the number of unpaid parking violations was positively related to corruption norms in diplomats’ home countries (Fisman & Miguel, 2007). However, the extent to which cultural variables impact core dishonesty remains unclear. To what extent does variation in corruption and cultural values impact individuals’ “moral backbones” – that is, their tendencies to violate formal or informal rules for personal gain across situations?

We designed a study to compare dishonesty across individuals sampled from different countries and different cohorts within the same countries. Participants were sampled from five countries based in distinct cultural regions of the world (Inglehart &
Welzel, 2005, 2010): China, Colombia, Germany, Portugal, and USA. While all of these countries are modern, large-scale societies, they also vary in their levels of corruption (Transparency International, 2013). In each country, we administered a dishonesty measure to two cohorts: students, sampled at major public universities, and the general public, sampled in coffee shops. Additionally, to assess whether people intuitively understand how cultural norms impact individual dishonesty, we surveyed a separate pool of participants from the same five countries, describing the cross-cultural study and die task in detail, and asking them to estimate the level of dishonesty observed for each of our ten participant samples.

2.3 Method

All research was approved by Duke University’s Institutional Review Board, and carried out in accordance with the World Medical Association Declaration of Helsinki.

2.3.1 Behavioral Study

2.3.1.1 Participants

We administered the die task to 2,495 individuals in five countries. In each country, we targeted 500 participants divided into two cohorts (students and public); based on fluctuations in recruitment and scheduling, initial sample sizes ranged from 223-288 participants. Twenty-five individuals who did not complete the study due to technical issues or their own decision to end the study early were excluded. To ensure that the participant samples reflected the cultures of our countries of interest, we also excluded participants who were not born in or currently living in the country where we administered our study, leaving 2,179 native resident participants in our analyses. Table 1 provides location and demographic data on our ten participant samples.
Table 1: Summary of Data Collection and Demographics Information for Student and Public Participant Samples.

<table>
<thead>
<tr>
<th>Country</th>
<th>City</th>
<th>Payment per dot</th>
<th>University</th>
<th>N</th>
<th>Gender</th>
<th>Age</th>
<th>N</th>
<th>Gender</th>
<th>Age</th>
</tr>
</thead>
<tbody>
<tr>
<td>China</td>
<td>Beijing</td>
<td>0.40 CNY</td>
<td>Beijing University of Chemical Technology</td>
<td>251</td>
<td>54.6%</td>
<td>X=21.77 SD=1.02</td>
<td>208</td>
<td>59.1%</td>
<td>X=30.01 SD=7.71</td>
</tr>
<tr>
<td>Colombia</td>
<td>Bogotá</td>
<td>150 COP</td>
<td>National University of Colombia</td>
<td>235</td>
<td>34.5%</td>
<td>X=21.40 SD=3.08</td>
<td>210</td>
<td>46.7%</td>
<td>X=33.30 SD=12.54</td>
</tr>
<tr>
<td>Germany</td>
<td>Munich</td>
<td>0.10 EUR</td>
<td>University of Munich</td>
<td>215</td>
<td>57.7%</td>
<td>X=23.19 SD=2.64</td>
<td>231</td>
<td>66.7%</td>
<td>X=33.38 SD=13.62</td>
</tr>
<tr>
<td>Portugal</td>
<td>Lisbon</td>
<td>0.05 EUR</td>
<td>University of Lisbon</td>
<td>239</td>
<td>68.6%</td>
<td>X=21.93 SD=3.39</td>
<td>181</td>
<td>49.2%</td>
<td>X=32.25 SD=13.71</td>
</tr>
<tr>
<td>USA</td>
<td>Raleigh</td>
<td>0.10 USD</td>
<td>North Carolina State University</td>
<td>224</td>
<td>38.4%</td>
<td>X=20.58 SD=3.10</td>
<td>185</td>
<td>54.6%</td>
<td>X=38.84 SD=15.08</td>
</tr>
</tbody>
</table>

2.3.1.2 The Die Task

To examine dishonesty, we adapted a task developed by Jiang (2013), which we refer to as the die task. The die task, administered on iPads, involves rolling a virtual die over repeated trials. On each trial, participants are instructed to mentally choose a side of the die (top or bottom) before rolling the die. They are instructed to remember their choice, roll the die, and then, when the outcome is visible, indicate which side they chose. The outcome screen displays the number of dots on the top and bottom sides of the die, and participants know that they will be paid an amount proportional to the number of dots on the chosen side. (In the USA, participants were paid 10 cents per dot; we used the 2012 Purchasing Power Parity Index (The World Bank Group, 2012) to arrive at an equivalent payment per dot for each country, rounded to the nearest whole currency unit) Thus, on any roll where the unfavorable side is initially chosen, participants can cheat by claiming to have chosen the higher-earning side. Participants roll the die and report “top” or “bottom” over 20 trials, with a tally of their earnings updating at the top of the screen.
The die task allows participants to cheat under conditions of plausible deniability, as it is impossible to distinguish dishonesty from good fortune on any individual trial. However, when response data is aggregated across many participants, we can test for the statistical likelihood and magnitude of dishonesty by comparing the average proportion of favorable rolls to what would be expected by chance.

2.3.1.3 Administration and Procedure

The study was administered over a period of thirteen months, from February 2013 to March 2014. Study materials for the die task and survey questions were translated into the native languages of our five countries of interest using a forward-backward translation procedure. To ensure consistency in administration, one of the authors (XGR) travelled to each country to train experimenters in the standardized experimental protocol, and to oversee the administration of the study. The study was advertised as a decision-making study where participants could earn money within a fixed range. Participants in the student samples were recruited from public universities in major cities via posters and flyers distributed in person (China, Colombia, USA) or through an online sign-up system (Germany, Portugal). Participants in the general public samples were recruited in various coffee shops from the same cities. Coffee shop patrons were approached by an experimenter, who briefly described the study and asked the patron whether he or she would be interested in participating.

The experimenter introduced the task using a demonstration iPad, including example trials (see Appendix A for the experimenter’s script). Student participants completed the task in a quiet testing room with 5-8 participant stations, which were separated from one another and facing the walls. Public participants completed the task independently from their seat in the coffee shop. Following the task, all participants completed a 10-15 minute survey, which, for half of participants, involved indicating
how likely they would be to engage in specific dishonest actions across various domains of life (work, relationships, etc.). Lastly, all participants answered demographic questions, and were then paid the amount they earned on the die task and thanked for participating.

2.3.2 Behavioral Study

2.3.2.1 Participants

Participants in the prediction study (hereafter referred to as predictors) were recruited from our five countries of interest using online platforms (mTurk, uSamp, and Clickworker). The study was distributed to 150 Americans, and 125 participants from each other country of interest. We excluded respondents who were not native residents of the country of interest (5.9%), who reported not understanding the task (7.5%), or who left questions unanswered (0.5%). Our final cross-national sample included 120 Chinese, 108 Colombians, 96 Germans, 108 Portuguese, and 141 Americans, for a total of 573 predictors. All survey materials were translated into the native language of the predictor sample using a forward-backward translation procedure.

2.3.2.2 Survey and Procedure

Predictors were instructed that they would be reading about a research study and making predictions about the results. The details of the cross-cultural research study and die task were then explained in detail, with accompanying screen shots from the die task. A comprehension question assessed whether predictors fully understood the study and task (those who did not were excluded). Predictors were then asked to estimate the level of dishonesty in each of the ten participant samples in the cross-cultural study, using slider scales ranging from 50% (perfectly honest) to 100% (perfectly dishonest). Finally, predictors indicated their country of birth and country of residence among other demographic measures.
2.4 Results

To assess dishonesty on the die task, we computed the proportion of rolls in which the favorable side (i.e. the side with the higher earnings) was chosen for each participant, and compared this variable to what would be expected by chance. Figure 1 shows the observed distributions of the proportion of favorable rolls for participants in every sample compared with the expected chance distribution. For all ten participant samples, the mean proportion of favorable reports on the die task was significantly above chance (all p-values ≤ .005). However, when we compared the results of the cross-cultural study with the results that were predicted by a separate, cross-national sample of participants in the prediction study, we found that participants in the cross-cultural study were substantially less dishonest than people predicted (mean observed proportion of favorable reports: 0.58; mean predicted proportion of favorable reports: 0.70). Observed dishonesty was lower than predicted dishonesty for all samples. These findings suggest that the theory of self-concept maintenance – which predicts that dishonesty will occur within a limited range – is relevant across countries (Mazar et al., 2008b), and also that people overestimate the extent to which others will behave dishonestly on a novel task.
Figure 1: Histograms Representing Expected and Observed Distributions of Outcomes on the Die Task. The grey bars represent the expected distributions if participants were perfectly honest, and colored bars represent observed distributions among student samples (blue bars) and public samples (red bars). The average proportion of favorable outcomes was significantly above chance (p-values < .005) for all ten participant samples.
2.4.1 Does Dishonesty Vary Across Countries and Cohorts?

Figure 2 shows the means for observed dishonesty and predicted dishonesty across the ten participant samples in the behavioral study. Because we observed differences in predictions made by predictors native to the country of interest and predictors from other countries (described in more detail later), the means for these groups of predictors are plotted separately.

2.4.1.1 Prediction Study

We first examined how dishonesty predictions varied across countries and across cohorts within countries for all predictors by entering all predictions into a 5(country) x 2(cohort: students vs. public) repeated-measures ANOVA. This analysis revealed wide variation in predicted dishonesty based on country ($F(4, 2288)=71.70$, $p<.001$, $\eta_p^2=.11$). Overall, predictions of dishonesty on the die task were greatest for Colombians (0.73), and Americans (0.73), and least for Germans (0.66), with predictions for Portuguese (0.70) and Chinese (0.69) falling between these bounds.
Figure 2: Observed and predicted dishonesty across the ten participant samples. We observed greater differences in dishonesty between student and public samples within countries (F(1,2169)=65.698, p<.001, $\eta^2=.029$) than between countries (F(1,2169)=1.763, p=.13, $\eta^2=.003$). A separate pool of participants predicted higher levels of dishonesty than we observed. For each country of interest, predicted dishonesty was greater among survey predictors from that same country than for survey predictors from other countries. Error bars represent +/- 1 standard error of the mean.

In addition, predictors expected greater dishonesty among members of the general public than students, though the effect size was small relative to the country effect (F(1,572)=19.24, p<.001, $\eta^2=.033$). A significant interaction (F(4, 2288)=4.41, p=.002, $\eta^2=.008$) and post-hoc analyses indicated that differences in predicted cheating between public and student samples were significant for some countries (Colombia, Portugal, USA), but not for others (China, Germany).

2.4.1.2 Behavioral Study

We next looked at whether dishonesty varied across countries and cohorts for participants who completed the die task. Entering our cross-cultural data into a 5(country) x 2(cohort) between-subjects ANOVA with die task performance entered as
the dependent variable revealed a significant main effect of cohort (F(1, 2169)=65.70, p<.001, \( \eta^2_p = .029 \)). In contrast with predictions, however, student samples were more dishonest than public samples; this effect was evident for all countries except Portugal, where student and public cheating did not differ (Portugal p>0.25; all other countries p<.03).

In stark contrast to predictions, the main effect of country was not significant in the behavioral dataset (F(4, 2169)=1.763, p=.13, \( \eta^2_p = .003 \)). When we conducted separate one-way ANOVAs for students and public with country entered as the between-subjects factor, we observed a significant effect of country for student samples (F(1,1159)=8.097, p<.001, \( \eta^2_p = .027 \)), but not for general public samples (F(1,1010)=1.924, p=.10, \( \eta^2_p = .008 \)). Closer examination of the student data revealed that the significant effect for students was driven primarily by differences in the proportions of individuals cheating at maximal or near-maximal levels. Taken together, findings suggest that on an abstract task assessing generalized dishonest behavior, individuals from different countries are remarkably consistent. An interaction term between country and cohort was significant (F(4, 2169)=7.67, p=<.001, \( \eta^2_p = .014 \)), indicating that the discrepancy between student and public samples differed by country, though the effect size was modest. The consistent and relatively low levels of dishonesty observed across countries are in line with results from a recent study, which found that nationality had little effect on willingness to lie about the result of coin flip in exchange for chocolate (Pascual-Ezama et al., 2014).

2.4.2 Is Dishonesty Related to Corruption and Cultural Values?

2.4.2.1 Prediction Study

We next explored whether people’s predictions about dishonest behavior on the die task are related to perceptions of corruption and cultural values, hypothesizing that predictors would generalize their perceptions of countries’ corruption to individual
dishonesty on a novel task. To test whether predictions about dishonesty were related to corruption scores, we computed the correlation coefficients between corruption scores and dishonesty predictions across countries. Corruption scores for our five countries of interest were obtained from Transparency International’s 2013 Corruption Perceptions Index (CPI) (Transparency International, 2013), and reverse-coded so that 0 represented perceptions of no corruption and 100 represented perceptions of complete corruption. Separate correlation coefficients were computed using each predictor’s dishonesty estimates for student samples and for public samples, resulting in two coefficients per predictor. The mean of these correlation coefficients was significantly greater than 0 for students (r=.10, t(558)=4.92, p<.001) and public (r=.12, t(551)=5.36, p<.001), indicating that predictors expected individuals from more corrupt countries to be more dishonest on the die task.

To explore whether predictions were related to cultural values, we ran similar analyses with countries’ scores for two broad cultural values dimensions identified by Inglehart and colleagues based on cross-national data from the World Values Survey (Inglehart & Welzel, 2005, 2010, 2012). The Traditional/Secular-rational (TSR) values dimension captures a society’s emphasis on religion, familial ties, and traditional values (higher scores indicate less emphasis on these values). The Survival/Self-expression (SSE) values dimension captures citizens’ prioritization of economic and physical security as opposed to tolerance and participation in government (higher scores indicate less emphasis on personal security). Predictions of greater levels of dishonesty on the die task were related to stronger traditional values and stronger self-expression values. When we entered country corruption scores, TSR scores and SSE scores as independent variables in a regression model with dishonesty predictions as the dependent variable, the beta values were significant for all three dimensions (p-values ≤ .01).
2.4.2.2 Behavioral Study

When we entered means for observed die task dishonesty and corruption scores into correlation analyses, the resulting correlation coefficients were not only non-significant but negative in sign (for students $r=-.26$, $p=.68$; for public $r=-.416$, $p=.49$), suggesting no positive relationship between individual dishonesty and country-level corruption. Furthermore, when we examined relations between these two cultural dimensions and our primary cross-cultural data, correlation coefficients were inconsistent across student and public samples, suggesting no meaningful relationship between these cultural dimensions and dishonesty on the die task.

2.4.3 Does Dishonesty Vary Based on Demographics?

2.4.3.1 Behavioral Study

It is possible that dishonesty varies along with individual differences in demographics. We ran separate linear regression analyses for student and public samples, with proportion of favorable reports on the die task entered as the dependent variable, countries entered as dummy variables, and with gender, age, minority status, relative earnings, religiosity, atheism (dummy coded), and trust entered as independent variables. For students (but not public), minority status was the only variable that significantly predicted dishonesty ($\beta=.066$, $p=.04$), such that individuals who were ethnic minorities were more likely to cheat. For public (but not students), trust in others was the only variable that significantly predicted dishonesty ($\beta=.066$, $p=.04$), such that individuals who reported less trust in others were more dishonest on the die task. This finding is in line with other studies that have reported positive relationships between trust and honesty (Neville, 2012; Uslaner & Badescu, 2004).
2.4.4 Are Dishonesty Predictions Biased for Home Country Samples?

2.4.4.1 Prediction Study

Finally, splitting the prediction study data based on predictors’ home countries revealed an interesting bias. For every country sampled in our cross-cultural study, dishonesty predictions were highest among survey predictors from that same country. Entering the data into a 5(predictor country) x 5(country) x 2(cohort) mixed between-within subjects ANOVA revealed that survey predictors from different countries varied in their estimations of cheating for different countries ($F(4,16)=19.35$, $p<.001$), but not in their estimations of students and public. Post-hoc contrasts indicated that in every country, predictions of die task dishonesty were significantly inflated among home country predictors ($p$-values <.01).

2.5 Discussion

Taken together, our results suggest that macro-level cultural variables have limited influence on core dishonesty. While others have shown that cultural variation matters for cooperation and punishment (Gächter et al., 2010; Henrich et al., 2006, 2010; Herrmann et al., 2008), our data suggests that cross-national differences in dishonesty run only skin deep. Differences in dishonesty on the die task were greater between cohorts within countries than between countries. Further research is needed to determine whether the differences between student and public samples are primarily due to demographic factors, such as age or income, or to contextual factors, such as the study environment.

In addition, individuals who were asked to predict the results of our cross-cultural study not only over-estimated dishonesty, but based their estimations on country-level corruption and cultural values. It seems that predictors over-generalized their knowledge about country-level variables to the behavior of ordinary citizens. The
discrepancies between predicted and observed dishonesty are in line with those of Terracciano and colleagues, who found that perceptions of national character, though robust across raters, did not converge with cross-national personality assessments (Terracciano et al., 2005). If institutions and governments hold similar stereotypes about the dishonesty of ordinary citizens, diminished trust in citizens is likely to encourage corruption and weaken institutions (Algan & Cahuc, 2013; Uslaner, 2008).

Our results suggest that individuals from different countries have similar moral backbones, but adapt their understanding of what constitutes moral behavior based on specific societal norms. Results from additional survey data collected as part of our cross-cultural study support the contextual nature of variation in dishonesty. Following the die task, half of participants from every country indicated the likelihood that they would engage in dishonest behaviors across a range of specific situations. Differences between countries were significant (p-values <.05) for 49 of 56 situations. Our findings imply that programs aimed at establishing strong honesty norms for specific behaviors, such as Bogota Colombia’s successful employment of mimes to theatrically shame traffic violators (Dundjerovic & Bateman, 2006) or the United Kingdom’s introduction of a fair tax mark to reduce tax evasion (Fair Tax Mark Ltd, 2014), will be most effective at curbing dishonest behaviors. In contrast, programs aimed at promoting general morality are unlikely to have lasting effectiveness.
3. Comparing the Effectiveness of Legal, Social, and Personal Deterrents of Dishonesty Across Countries

3.1 Overview

Chapter 2 found similar patterns of dishonesty across countries on a novel die task, as well as within-country differences between students and the general public. Overall, for both students and public, cheating was evident, but lower in magnitude than predictions from a separate participant sample. In this chapter, I ask which factors dissuade dishonesty in real world situations, and whether these factors vary across countries. I focus on dishonest actions that are against the law, surveying participants about seven specific infractions and misdemeanors.

Much research in criminology and economics has focused on the effectiveness of legal sanctions at reducing crime, with some newer models also examining the effectiveness of social sanctions (judgment of friends and family) and personal sanctions (feelings of guilt or shame). Existing research suggests that both personal sanctions and, to a lesser extent, legal sanctions deter crime, but whether this pattern is unique to Western countries or robust across cultures remains unclear. To find out, the Dishonesty Deterrents survey was administered to half of participants who completed the die task in Chapter 2, including student and public participants from all five countries. Participants were asked to report the likelihood of engaging in seven crimes, and were asked to indicate the probability and severity of consequences for legal, friend, family, and personal sanctions. Results indicated that across countries, personal sanctions had the strongest deterrent effect on crime. The deterrent effect of legal sanctions was weaker and varied across countries. Furthermore, the deterrent effect of legal sanctions was strongest when personal sanctions are lax. Unexpectedly, social sanctions were positively related to illegal behavior; this effect was driven by a positive
relationship between likelihood of engaging in crime and probability friends or family finding out. Taken together, results suggest that the relative strengths of legal and personal deterrents are quite robust across cultures and (minor) crimes.

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3.2 Introduction

“A cadet will not lie, cheat, or steal, or tolerate those who do.” The Cadet Honor Code at the U.S. Military Academy could well be adopted by any society. Lying, cheating, and stealing are all facets of dishonesty – actions that benefit the individual at others’ expense. When lying, cheating, and stealing concern the state, these actions are typically classified as crimes (Law Commission of Canada, 2005). By definition, dishonest actions harm societies, and therefore, societies are motivated to discourage them.

The question of what deters crime is of interest to social science researchers and policy-makers alike. Are decisions to engage in crime influenced by the threat of legal consequences? Are they influenced by threats of judgment from friends or family? Are they influenced by the potential for internal feelings of guilt, regret or shame for committing a crime? While much of the existing research focuses on a single category of deterrents on crime, in the present study, we compare the relative impacts of legal, friend, family and personal sanctions, and consider their interactions. By drawing on a participant sample from five countries in distinct cultural regions, we examine whether
the deterrent effects of legal, social, and personal sanctions are consistent across individuals from different cultural backgrounds.

### 3.2.1 What Deters Crime?

An impressive volume of research on the subject of what deters crime has focused on the effectiveness of legal sanctions. This research stems from deterrence theory, which posits that legal sanctions deter citizens from engaging in criminal activity. This theory, grounded in the rational actor approach, is based on the notion that people choose whether or not to commit a crime by weighing the potential benefits of getting away with it against the potential consequences of getting caught (Becker, 1968). Consequences are considered in terms of both severity of the punishment and the probability of being caught. Building on thinking of eighteenth century philosophers Beccaria and Bentham, and revived in the 1960s, deterrence theory has generated much research and heated debate, with some researchers arguing that legal sanctions have no effect at all (e.g. Fattah, 1983).

Recently, Rupp (2008) conducted an impressive meta-analysis synthesizing the findings from 700 studies testing the deterrence hypothesis, spanning economics, sociology, psychology, and criminology. Detailed information about each study, including aspects of study design (cross-sectional, experimental, survey, etc.), participant sample, categories of deterrents measured, and information about the authors and journal were coded and analyzed. On the whole, this meta-analysis favored rejecting the null hypothesis that legal sanctions have no deterrent effect on crime. Furthermore, the probability of legal sanctions was found to have a greater deterrent effect than the severity of legal sanctions.

A chief criticism of deterrence theory has been its neglect of noneconomic factors that may influence crime (Meier, Burkett, & Hickman, 1984; Williams & Hawkins, 1986).
Researchers from sociology and other traditions have suggested that noneconomic sanctions have at least as much potential to impact criminal behavior (Grasmick & Green, 1980; Mazar, Amir, & Ariely, 2008; Wrong, 1961). One type of noneconomic sanction considered is judgment by friends and family, which some have referred to as the threat of social embarrassment (Cochran, Florida, Wood, & Sellers, 1999; Grasmick & Bursik, 1990). Research from psychology and sociology suggests that people are highly sensitive to social evaluation (Dickerson, Mycek, & Zaldivar, 2008). However, according to Rupp’s meta-analysis, of the 2534 variables examined in survey studies, only 6.2% assessed the perceived probability of punishment by friends or family, 4.1% assessed the perceived severity of punishment by friends or family, and 2.8% assessed the perceived probability of detection by friends, family or others. Results from the meta-analysis indicated that the probability of punishment by friends or family was at least as strong a deterrent as the probability of legal punishment, and the severity of punishment by friends or family, though less powerful than the probability effects, was at least as strong a deterrent as the severity of legal punishment.

Finally, there appears to be increasing awareness that in addition to external sanctions, internal sanctions such as feelings of guilt or shame may be important deterrents of crime. Though focused on dishonest rule violations rather than illegal actions per se, Mazar, Amir and Ariely (2008) posited that dishonesty is regulated largely by the internal desire to maintain a positive self-concept, which is weighted against the potential material benefits of breaking the rules. In support of this theory, experiments showed that increasing the flexibility with which people can categorize their dishonest actions (e.g. cheating for tokens with monetary value rather than money itself) encourages dishonesty, and conversely, that drawing attention to moral standards mitigates dishonesty. Furthermore, several experimental studies have found that
increasing financial incentives for behaving dishonestly has surprisingly little impact on dishonest behavior (Francesca Gino et al., 2012; John et al., 2013; Wiltermuth, 2011).

### 3.2.2 Considering Interactions between Sanctions

An additional question sometimes raised by researchers is whether the deterrent effects of legal, social and internal sanctions are independent of one another. Some scholars have raised the interesting hypothesis that the deterrent effects of legal sanctions should be most evident when moral commitments (i.e. personal sanctions) are weak (Silberman, 1976; Zimring, 1971). Evidence supporting this interaction hypothesis was reported by Silberman (1976), and more recently by Wenzel (2004), who found that in a sample of Australian citizens, penalties for tax evasion had a deterrent effect only when personal sanctions were lax. However, Grasmick and Green argued against this interaction hypothesis in favor of additive effects (Grasmick & Green, 1980, 1981).

### 3.2.3 An Integrated Deterrence Framework

While many researchers who have explored the impacts of social and personal sanctions on crime have contrasted their approaches with deterrence theory, Grasmick and Bursik (1990) proposed that the deterrence framework could be extended to incorporate social and personal sanctions. They designed a survey with questions assessing the perceived probability of legal, social, and personal sanctions, along with questions assessing the severity of legal, social and personal sanctions. Sanction threat variables, computed as the product of perceived probability and severity, were entered as predictors in regression models for three crimes: tax evasion, theft and drunk driving. Across the three crimes, both legal sanctions and personal sanctions were significant deterrents, but personal sanctions had the strongest deterrent effect. Surprisingly, the deterrent effect of social sanctions was not significant.
3.2.4 Are People Deterred from Crime the Same Way Everywhere?

The limited number of studies employing Grasmick and Bursick’s extended deterrence framework support their original findings that legal and personal sanctions deter crime, with personal sanctions having the strongest deterrent effects (Cochran et al., 1999; Grasmick, Bursick, & Arnekelev, 1993; Grasmick, Bursik, & Mitchell, 1993; Kobayashi, Grasmick, & Friedrich, 2001). Notably, these studies have failed to provide evidence for a deterrent effect of social sanctions; the reason these effects differ from those reported in Rupp’s meta-analysis is not entirely clear. Moreover, these studies have been conducted on Americans, raising the question of whether the findings are robust across cultures. (Kobayashi and colleagues’ study is an exception, including both Americans and Japanese, but the researchers do not compare the strengths of deterrent effects across cultures. Wenzel (2004) also reports similar effects in an Australian sample.)

In his meta-analysis of the deterrence literature, Rupp found that the deterrent effect of legal sanctions varied according to the country under study. For example, support for the deterrence hypothesis was stronger in studies conducted in Germany and the United Kingdom than in studies conducted in Canada. However, comparisons in Rupp’s analysis were limited to select Western nations with sufficient numbers of studies testing the effects of legal deterrents. Furthermore, the deterrence effect was also found to vary according to authors’ home country and country of publication, raising the possibility that the cross-country variation observed was related to author biases. Comparing culturally distinct countries within a single study overrides these issues, and allows for a more rigorous assessment of whether the relative effects of legal, social, and personal sanctions are consistent across cultures.
3.2.5 The Present Research

Building on the extended deterrence framework of Grasmick and Bursik (1990), we compared the deterrent effects of legal, social, and personal sanctions on crime within a single study. To compare the relative influences of these deterrents, across cultures, we administered our study to an international participant sample from five countries: China, Colombia, Germany, Portugal, and USA. These countries are based in distinct cultural regions of the world, namely Confucian (China), Latin America (Colombia), Protestant Europe (Germany), Catholic Europe (Portugal), and English-speaking (USA), according to cultural mapping by Inglehart and Welzel (2010). The countries sampled differ along two broad cultural dimensions identified by Inglehart and his colleagues: traditional vs. secular-rational values, and survival vs. self-expression values (Inglehart & Baker, 2000; Inglehart & Welzel, 2010). Within each country, we administered a survey to two participant groups: students at public universities, and the general public at coffee shops in major cities.

We designed a survey, which we refer to as the Dishonesty Deterrents survey, with four sanction categories: legal, friends, family, and self. While the threats of judgment from friends and family have traditionally been grouped together as social sanctions, we considered that judgment from and judgment from family might have different motivational impacts, which could vary across cultures. (For example, the threat of family sanctions, but not friend sanctions, may be stronger in more traditional cultures.) Participants were first asked to report the likelihood of engaging in seven crimes (infractions and misdemeanors), including parking illegally, bribing a police officer, and tax evasion. For each crime, participants were asked to rate both the probability of detection and severity of punishment across each of the four sanction categories.
Our primary research questions were whether legal, social, and/or personal sanctions negatively predict the likelihood of engaging in crime, and whether deterrent effects are consistent across cultures. Based on previous research suggesting the primacy of internal influences, we hypothesized that personal sanctions would have the strongest deterrent effect across cultures. In addition, we tested the interaction hypothesis that the effects of legal sanctions are stronger when personal sanctions are lax.

3.3. Method

3.3.1 Participants

One thousand, two hundred and fifty-one individuals completed the Dishonesty Deterrents survey. To ensure that our participant sample reflected the cultures of our countries of interest, we limited our analyses to those who were native residents of each country (born in and currently residing in the country). In addition, twelve individuals were excluded due to technical issues or personal reasons, leaving 1,100 participants in our final sample. Approximately half of participants (N=586) were students recruited from public universities, while the other half (N=514) were members of the general public, recruited in coffee shops from the same cities. Participants were sampled from five countries: China, Colombia, Germany, Portugal and USA.

3.3.2 Crime Deterrents Survey

All survey materials were translated into the native language of participants from each country, using a forward-backward translation procedure. Participants completed the survey individually on iPads. An instructions screen informed them that they would be asked different questions about the same actions, and that they should respond as honestly as possible. They were assured that their responses were
confidential and anonymous. All participants were first asked about the likelihood that they would engage in seven specific dishonest behaviors, in the form, “How likely are you to ____?” Participants responded on continuous sliding scales ranging from 0 (“not at all likely”) to 10 (“very likely”).

Participants indicated how likely they would be to engage in the following actions:

1) Omit information on your tax filings in order to pay less income tax
2) Speed by 15% over the speed limit while driving
3) Run a red light when nobody is around
4) Park your car in a no parking zone
5) Bribe a police officer to avoid getting a speeding ticket
6) Apply for a government tax credit knowing you are not eligible for it
7) Fake a signature of a doctor on a government document in order to get an expensive medication for free.

These questions were presented on the same screen in randomized order.

Next, participants were asked to report their perceptions of legal, social, and personal deterrents for each of the seven actions. Participants were asked about two categories of social deterrents, friends and family, resulting in four deterrents categories. For each category, participants were asked about the perceived probability of being penalized for engaging in the actions with the following questions:

Legal probability: How likely would you be to get caught by the government authorities or police if you…

Social probability (friends): How likely would your friends be to find out if you…

Social probability (family): How likely would your family be to find out if you…
Personal probability: How likely would you be to feel guilty if you…

Continuous sliding scales ranged from 0 (“extremely unlikely” to 10 “extremely likely”). Furthermore, participants were asked to rate the expected severity of the legal, social, and personal consequences, as follows:

Legal severity: How bad would the legal penalty be if you…

Social severity (friends): How badly would your friends judge you if you…

Social severity (family): How badly would your family judge you if you…

Personal severity: How badly would you feel if you…

Continuous sliding scales ranged from 0 (“not bad(ly) at all”) to 10 (“extremely bad(ly)”).

The eight question categories were presented in random order, with the seven individual actions presented in random order within each block. In total, participants responded to 56 specific questions about legal, social and personal deterrents.

3.3.3 Procedure

Students at universities were recruited with flyers and posters advertising a decision-making study where they could earn between $4 and $10. At universities, the study was run in a testing room with 5-8 separate stations for participants. In coffee shops, participants were approached individually by an experimenter, who asked whether they would be interested in participating in a decision-making study with the opportunity to earn between $4 and $10. Coffee shop patrons who agreed to participate completed the survey individually from where they were seated.

Participants first completed a behavioral task on iPads, which involved rolling a virtual die twenty times (adapted from Jiang, 2013; see Mann, Garcia-Rada, Hornuf, Tafurt, & Ariely, 2015 for further detail). Before each roll, participants were asked to mentally select a side, top or bottom, and remember their choice. They earned the
equivalent of ten cents in USD for every dot on the chosen side (the amount and currency were adjusted for each country using the Purchasing Power Parity Index); however, they were only asked to report their choice of side after viewing the outcome, which displayed the number of dots on top and bottom. While it was not possible to know for certain whether cheating occurred on any given roll or for any given person, in large samples, higher proportions of trials in which the favorable earnings side was chosen should be correlated with cheating, if cheating indeed occurred.

When participants had completed the die task, the experimenter returned and set up the crime deterrents survey on the iPad. An experimenter who spoke participants’ native language set up the survey and instructed them to raise their hands should they have any questions. Participants indicated their responses to each question by moving bars along slider scales along with their fingers. At the end of the survey, they raised their hand to indicate that they had finished. The experiment then thanked them for participating and directed them to a payments table (for students) or paid them directly (for general public).

3.4 Results

3.4.1 Comparing Likelihood of Engaging in Crime across Countries and Cohorts

We first examined whether likelihood of engaging in crime differed across countries, and across subject groups (students vs. public) within countries. Table 2 presents the results of separate 5(Country) x 2(Cohort: student vs. public) between-subject ANOVAs conducted on each of the seven scenarios. For every scenario, reported likelihood of engaging in crime differed between countries, and results were significant at a Bonferroni-corrected probability threshold of p=.007. On the other hand, differences between cohorts were significant for only two scenarios, running a red light and falsely
applying for a government tax credit, at a liberal threshold of \( p = 0.05 \), and for only the latter scenario at the Bonferroni-corrected threshold. Finally, the Country-by-Cohort interaction term was significant for two scenarios (speeding by 15\% over the limit and running a red light), but these did not survive the Bonferroni-corrected significance threshold. Based on the limited differences observed between cohorts, along with non-significant effects for cohort in regression analyses, we combine student and public cohorts together in the analyses reported from here on.

**Table 2: Summary of univariate ANOVAs comparing responses across countries and cohorts regarding the likelihood of engaging in seven dishonest actions.** All country differences were significant at a Bonferroni-corrected threshold of \( p = 0.007 \). At this threshold, no cohort differences except submitting a false claim were significant, nor were any country by cohort interactions.

<table>
<thead>
<tr>
<th>Statistic</th>
<th>Country</th>
<th>Cohort</th>
<th>Country* Cohort</th>
</tr>
</thead>
<tbody>
<tr>
<td>Omit information on your tax filings in order to pay less income tax</td>
<td>F 19.601 ***</td>
<td>0.013</td>
<td>0.655</td>
</tr>
<tr>
<td>( \eta^2_p )</td>
<td>0.068</td>
<td>0.000</td>
<td>0.002</td>
</tr>
<tr>
<td>Speed by 15% over the speed limit while driving</td>
<td>F 44.898 ***</td>
<td>0.772</td>
<td>2.374 *</td>
</tr>
<tr>
<td>( \eta^2_p )</td>
<td>0.145</td>
<td>0.001</td>
<td>0.009</td>
</tr>
<tr>
<td>Run a red light when nobody is around</td>
<td>F 13.748 ***</td>
<td>6.633 *</td>
<td>1.818 *</td>
</tr>
<tr>
<td>( \eta^2_p )</td>
<td>0.049</td>
<td>0.006</td>
<td>0.007</td>
</tr>
<tr>
<td>Park your car in a no parking zone</td>
<td>F 39.500 ***</td>
<td>0.706</td>
<td>0.910</td>
</tr>
<tr>
<td>( \eta^2_p )</td>
<td>0.129</td>
<td>0.001</td>
<td>0.003</td>
</tr>
<tr>
<td>Bribe a police officer to avoid getting a speeding ticket</td>
<td>F 43.289 ***</td>
<td>2.551</td>
<td>0.529</td>
</tr>
<tr>
<td>( \eta^2_p )</td>
<td>0.139</td>
<td>0.002</td>
<td>0.002</td>
</tr>
<tr>
<td>Apply for a government tax credit knowing you are not eligible for it</td>
<td>F 10.879 ***</td>
<td>17.983 ***</td>
<td>0.982</td>
</tr>
<tr>
<td>( \eta^2_p )</td>
<td>0.039</td>
<td>0.017</td>
<td>0.004</td>
</tr>
<tr>
<td>Fake a signature of a doctor on a government document in order to get an expensive medication for free</td>
<td>F 12.130 ***</td>
<td>3.547</td>
<td>0.372</td>
</tr>
<tr>
<td>( \eta^2_p )</td>
<td>0.043</td>
<td>0.003</td>
<td>0.001</td>
</tr>
</tbody>
</table>
Figure 3 shows the reported likelihood of acting dishonestly for each scenario across the five countries, illustrating cultural differences.

![Figure 3: Self-reported likelihood of engaging in seven specific dishonest actions across countries. Error bars represent +/- 1 standard error of the mean.](image)

### 3.4.2 Correlations between Self-Reported Likelihood of Engaging in Crime and Observed Dishonest Behavior

We next examined whether crime was related to dishonesty on the behavioral die task, in which participants could earn more money by cheating. The behavioral measure of dishonesty was the proportion of trials on the die task in which participants reported choosing the side of the die with favorable earnings. Overall, this proportion ranged from .56 (Portugal) to .60 (USA) (Heather Mann et al., 2015), indicating a limited but significant level of cheating in every country. We conducted a Pearson correlation between this outcome and self-reported likelihood of engaging in crime, averaged across the seven illegal actions. Across the full sample, this analysis revealed a modest but significant positive correlation ($r=.08, p=.012$).
Examining the correlations for each country separately revealed positive and significant correlation coefficients for Germany and the USA, while the correlation coefficients for China, Colombia, and Portugal were not significant. Further examination indicated that these results were driven by the student samples in Germany and the USA.

### 3.4.3 Deterrent Effects of Legal, Social, and Personal Sanctions

Following Grasmick and Bursik (1990), for each of the four deterrence categories, we computed a sanction variable by multiplying the probability and severity ratings for each action. We first examined the relative importance of the four types of sanctions across all subjects by running linear mixed effects analyses with data from all subjects and questions. These analyses were run in R (R Core Team, 2014), using the lme4 package (Bates, Maechler & Bolker, 2014). P-values were computed with the Satterthwaite approximation, using the lmerTest package (Kuznetsova, Brockhoff, Christensen, 2014). Models were estimated using a maximum likelihood (ML) approach. To facilitate interpretation of the parameter estimates, all fixed effects variables and the dependent measure were first standardized to have a mean of 0 and standard deviation of 1.

Results from three mixed effect models are reported in Table 3. As a baseline, we ran an initial model with demographic variables (gender, age, minority status, relative earnings, religiosity, and mistrust of others) entered as fixed effects, and likelihood of acting dishonestly entered as the dependent measure (Model 1). To account for non-independent responses, item, country, and subjects nested within country were entered as random effects variables. This analysis showed significant effects for gender, age, minority status, and mistrust in other. Women were less likely to engage in dishonesty than men, although this did not hold up in subsequent models. Older individuals
reported being less likely to act dishonestly, while those with higher relative earnings reported being more likely to act dishonestly overall. This finding aligns with the work by Piff and colleagues, suggesting that upper class individuals are less ethical than lower class individuals (Piff, Stancato, Côté, Mendoza-Denton, & Keltner, 2012; see also Trautmann, van de Kuilen, & Zeckhauser, 2013, and Ariely & Mann, 2013). Finally, as others have found (Neville, 2012; E. M. Uslaner & Badescu, 2004), mistrust in others was related to greater likelihood of engaging in dishonesty.

Table 3: Results from linear mixed effects models with ML estimation for likelihood of engaging in crime. All models include subject, item, and country as random effects variables, with subject nested within country. Fixed effect variables and the outcome variable were standardized for ease of interpretation. Model 1 includes demographic variables of interest as fixed effect terms. Model 2 additionally includes the four sanction variables, resulting in a highly significant model improvement. Model 3 includes two-way interactions terms for the sanction variables, again resulting in highly significant model improvement. From Models 2 and 3, both self sanctions and legal sanctions show significant deterrent effects on crime, though the effect of self sanctions is approximately four times greater. Friend and family sanctions are positively related to crime (significantly so for friend sanctions). A highly significant positive interaction between legal and self sanctions indicates that the deterrent effect of legal sanctions is stronger when self sanctions are low.

<table>
<thead>
<tr>
<th>Fixed effects</th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>b</td>
<td>p</td>
<td>b</td>
</tr>
<tr>
<td>(Intercept)</td>
<td>0.005</td>
<td>0.979</td>
<td>0.005</td>
</tr>
<tr>
<td>FEMALE</td>
<td>-0.037</td>
<td>0.029*</td>
<td>0.010</td>
</tr>
<tr>
<td>Age</td>
<td>-0.084</td>
<td>0.000***</td>
<td>-0.036</td>
</tr>
<tr>
<td>MINORITY</td>
<td>-0.012</td>
<td>0.495</td>
<td>-0.017</td>
</tr>
<tr>
<td>Earn</td>
<td>0.057</td>
<td>0.001***</td>
<td>0.053</td>
</tr>
<tr>
<td>Religiosity</td>
<td>-0.006</td>
<td>0.728</td>
<td>0.018</td>
</tr>
<tr>
<td>Mistrust</td>
<td>0.043</td>
<td>0.012*</td>
<td>0.033</td>
</tr>
<tr>
<td>LEGAL</td>
<td>-0.091</td>
<td>0.000***</td>
<td>-0.122</td>
</tr>
<tr>
<td>FRIEND</td>
<td>0.026</td>
<td>0.056†</td>
<td>0.037</td>
</tr>
<tr>
<td>FAMILY</td>
<td>0.025</td>
<td>0.080†</td>
<td>0.017</td>
</tr>
<tr>
<td>SELF</td>
<td>-0.398</td>
<td>0.000***</td>
<td>-0.397</td>
</tr>
<tr>
<td>LEGAL*FRIENDS</td>
<td></td>
<td></td>
<td>-0.024</td>
</tr>
<tr>
<td>LEGAL*FAMILY</td>
<td></td>
<td></td>
<td>-0.021</td>
</tr>
<tr>
<td>LEGAL*SELF</td>
<td></td>
<td></td>
<td>0.113</td>
</tr>
<tr>
<td>FRIEND*FAMILY</td>
<td></td>
<td></td>
<td>-0.009</td>
</tr>
</tbody>
</table>
Model 2 built on Model 1 to examine the effects of external and internal sanction threats. Including legal, friends, family, and self sanctions as continuous fixed effect variables resulted in a highly significant model improvement over Model 1, according to a log likelihood ratio test ($\chi^2(4) = 2231.6, p<.001$). As can be seen from the table, beta values for legal and personal sanctions were negative and highly significant, indicating that the greater the sanction threat, the less likely an individual was to report acting dishonestly. Although both legal and self sanctions predicted unique variance in the model, it is also worth noting that the beta value for self sanctions ($b = -0.408$, $t(5488) = -27.253$) was five times the magnitude of the beta value for legal sanctions ($b = -0.804$, $t(5599) = -6.575$). In contrast, beta values for friends and family sanctions, though modest and only marginally significant, were positive in sign, indicating that greater threats of social judgment, whether from friends or family, predicted greater likelihood of acting dishonestly.

Finally, Model 3 built on Model 2 by including two-way interaction terms for the sanction threats as fixed effect variables (interaction terms were computed from the standardized sanction threat variables). Including interaction terms led to significant model improvement over Model 2 ($\chi^2(6) = 106.7, p<.001$). We were interested in testing...
the interaction hypothesis that when personal sanctions (i.e. feelings of guilt or shame) are weak, legal sanctions have a stronger deterrent effect on crime. In support of this hypothesis, we observed a significant, positive interaction between personal sanctions and legal sanctions. Similar findings were reported by Silberman (1976) and Wenzel (2004); Grasmick and Green (1980) also reported results that were similar in direction though not significant.

To further explore this effect, we conducted follow-up moderation analyses for each of the seven crimes, using Andrew Hayes’ process model which follows Baron and Kenny’s (1986) approach (Hayes, 2013). Personal sanctions moderated the effect of legal sanctions for four of the seven crimes (speeding, running a red light, parking illegally, and bribing an officer). For each of these crimes, the negative effect of legal sanctions was stronger when personal sanctions were weak.

In Model 3, the effect of friend sanction threats was positive and significant, and the effect of family sanction threats positive though not significant. In order to gain insight into the unexpected positive relationship between social sanction threats and likelihood of illegal actions, we conducted an additional linear mixed model analysis with standardized probability and severity sanction variables entered as separate fixed effect variables (Table 4). Demographic variables were also included in the model, with item, country, and subjects nested within country again entered as random effects variables. This analysis revealed that the probability variables for both friends and family sanctions, where subjects rated how likely their friends or family would be to find out if they acted illegally, were significant positive predictors of illegal actions. The severity of family judgment was a significant deterrent of illegal actions, while the severity of friends’ judgment did not significantly predict illegal action.
Table 4: Results from a linear mixed effects models (ML estimation) for likelihood of engaging in crime, with probability and severity ratings for legal, friend, family, and self sanctions entered as predictors, in addition to demographic variables. Fixed effects variables and the outcome variable were standardized for ease of interpretation. Subject, item, and country were as random effects variables, with subject nested within country. For legal and self sanctions, both probability and severity ratings were negatively related to crime, with severity ratings having somewhat stronger effects. For friend and family sanctions, probability of being detected was positively related to crime; severity of judgment from family was negatively related to crime, while severity of judgment from friends was not significant.

<table>
<thead>
<tr>
<th>Fixed effects</th>
<th>b</th>
<th>p</th>
</tr>
</thead>
<tbody>
<tr>
<td>(Intercept)</td>
<td>0.006</td>
<td>0.962</td>
</tr>
<tr>
<td>FEMALE</td>
<td>0.014</td>
<td>0.387</td>
</tr>
<tr>
<td>Age</td>
<td>-0.020</td>
<td>0.223</td>
</tr>
<tr>
<td>MINORITY</td>
<td>-0.017</td>
<td>0.296</td>
</tr>
<tr>
<td>Earn</td>
<td>0.039</td>
<td>0.012*</td>
</tr>
<tr>
<td>Religiosity</td>
<td>0.027</td>
<td>0.092†</td>
</tr>
<tr>
<td>Mistrust</td>
<td>0.029</td>
<td>0.063†</td>
</tr>
<tr>
<td>Legal (Probability)</td>
<td>-0.038</td>
<td>0.002**</td>
</tr>
<tr>
<td>Legal (Severity)</td>
<td>-0.071</td>
<td>0.000***</td>
</tr>
<tr>
<td>Friend (Probability)</td>
<td>0.091</td>
<td>0.000***</td>
</tr>
<tr>
<td>Friend (Severity)</td>
<td>0.010</td>
<td>0.527</td>
</tr>
<tr>
<td>Family (Probability)</td>
<td>0.093</td>
<td>0.000***</td>
</tr>
<tr>
<td>Family (Severity)</td>
<td>-0.114</td>
<td>0.000***</td>
</tr>
<tr>
<td>Self (Probability)</td>
<td>-0.123</td>
<td>0.000***</td>
</tr>
<tr>
<td>Self (Severity)</td>
<td>-0.280</td>
<td>0.000***</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Random effects</th>
<th>σ</th>
</tr>
</thead>
<tbody>
<tr>
<td>Subject*Country</td>
<td>0.372</td>
</tr>
<tr>
<td>Item</td>
<td>0.262</td>
</tr>
<tr>
<td>Country</td>
<td>0.141</td>
</tr>
<tr>
<td>Residual</td>
<td>0.676</td>
</tr>
</tbody>
</table>

Log likelihood: -6474  
† p < .10, * p < .05, ** p < .01, *** p < .001

Rupp’s meta-analysis and common consensus indicate that the probability of legal sanctions has a stronger deterrent effect than the severity of legal sanctions. In contrast, in our data, sanction severity had a stronger deterrent effect than sanction probability, for both the legal and self sanction categories.
3.4.4 Do Deterrent Effects Vary Across Countries?

Our next question was whether the deterrent effects of legal, friend, family, and personal sanctions were consistent or variable across countries. Table 5 presents the results of linear mixed models conducted separately for each country. Standardized demographics, sanction variables, and two-way sanction interaction terms were entered as fixed effect predictors, with subject and item entered as random effects. As can be seen from the table, the deterrent effect of self sanctions was highly significant across all five countries. The deterrent effect of legal sanctions was significant in China, Germany, and USA, marginally significant in Portugal, and not significant in Colombia. Finally, the positive interaction between legal and self sanctions was significant in every country except Colombia.
Table 5: Results from linear mixed effects models (ML estimation) for likelihood of engaging in crime, conducted separately for each country. The outcome variable was standardized, and standardized demographics, legal, friend, family, and self sanctions, and two-way sanction interaction terms were entered as fixed effect variables. Subject and item were entered as random effect variables.

<table>
<thead>
<tr>
<th></th>
<th>China</th>
<th>Colombia</th>
<th>Germany</th>
<th>Portugal</th>
<th>USA</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Fixed effects</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Intercept)</td>
<td>0.009</td>
<td>0.921</td>
<td>0.147</td>
<td>0.246</td>
<td>-0.116</td>
</tr>
<tr>
<td>FEMALE</td>
<td>-0.031</td>
<td>0.504</td>
<td>-0.036</td>
<td>0.356</td>
<td>-0.010</td>
</tr>
<tr>
<td>Age</td>
<td>-0.173</td>
<td>0.054</td>
<td>-0.067</td>
<td>0.108</td>
<td>-0.036</td>
</tr>
<tr>
<td>MINORITY</td>
<td>-0.111</td>
<td>0.029</td>
<td>-0.043</td>
<td>0.354</td>
<td>-0.012</td>
</tr>
<tr>
<td>Earn</td>
<td>0.192</td>
<td>0.000</td>
<td>0.111</td>
<td>0.005</td>
<td>0.033</td>
</tr>
<tr>
<td>Religiosity</td>
<td>-0.033</td>
<td>0.509</td>
<td>0.075</td>
<td>0.044</td>
<td>0.024</td>
</tr>
<tr>
<td>Mistrust</td>
<td>0.017</td>
<td>0.690</td>
<td>0.044</td>
<td>0.207</td>
<td>-0.004</td>
</tr>
<tr>
<td>LEGAL</td>
<td>-0.235</td>
<td>0.000</td>
<td>** -0.019</td>
<td>0.554</td>
<td>-0.116</td>
</tr>
<tr>
<td>FRIEND</td>
<td>0.102</td>
<td>0.006</td>
<td>* 0.027</td>
<td>0.422</td>
<td>0.025</td>
</tr>
<tr>
<td>FAMILY</td>
<td>0.077</td>
<td>0.099</td>
<td>0.036</td>
<td>0.269</td>
<td>-0.058</td>
</tr>
<tr>
<td>SELF</td>
<td>-0.418</td>
<td>0.000</td>
<td>** -0.457</td>
<td>0.000</td>
<td>** -0.433</td>
</tr>
<tr>
<td>LEGAL*FRIEND</td>
<td>-0.024</td>
<td>0.461</td>
<td>-0.045</td>
<td>0.109</td>
<td>-0.011</td>
</tr>
<tr>
<td>LEGAL*FAMILY</td>
<td>-0.040</td>
<td>0.275</td>
<td>0.006</td>
<td>0.831</td>
<td>-0.023</td>
</tr>
<tr>
<td>LEGAL*SELF</td>
<td>0.179</td>
<td>0.000</td>
<td>** 0.021</td>
<td>0.456</td>
<td>0.143</td>
</tr>
<tr>
<td>FRIEND*FAMILY</td>
<td>-0.002</td>
<td>0.940</td>
<td>-0.021</td>
<td>0.400</td>
<td>-0.008</td>
</tr>
<tr>
<td>FRIEND*SELF</td>
<td>-0.019</td>
<td>0.605</td>
<td>0.028</td>
<td>0.365</td>
<td>0.005</td>
</tr>
<tr>
<td>FAMILY*SELF</td>
<td>-0.005</td>
<td>0.893</td>
<td>-0.014</td>
<td>0.656</td>
<td>0.085</td>
</tr>
</tbody>
</table>

<table>
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<tr>
<th><strong>Random effects</strong></th>
<th>σ</th>
<th>σ</th>
<th>σ</th>
<th>σ</th>
<th>σ</th>
</tr>
</thead>
<tbody>
<tr>
<td>Subject</td>
<td>0.175</td>
<td>0.404</td>
<td>0.283</td>
<td>0.335</td>
<td>0.358</td>
</tr>
<tr>
<td>Item</td>
<td>0.028</td>
<td>0.290</td>
<td>0.334</td>
<td>0.432</td>
<td>0.408</td>
</tr>
<tr>
<td>Residual</td>
<td>0.509</td>
<td>0.730</td>
<td>0.654</td>
<td>0.652</td>
<td>0.595</td>
</tr>
</tbody>
</table>

† p < .10, * p < .05, ** p < .01, *** p < .001
To determine whether the strength of sanction threats varied significantly across countries, we ran a linear mixed effects model with standardized sanction variables and individual countries entered as fixed effect variables, in addition to sanction threat by country interaction terms. Country variables were coded using effect coding was used instead of dummy coding such that each country’s mean could be compared against the grand mean. As is the case for dummy coding, with effect coding for k groups, only k-1 groups can be estimated according to the degrees of freedom. In order to report parameter estimates for all five countries, we ran the linear mixed effects model twice with a different country excluded from estimation each time, and reported the parameters for all five countries in Table 6. Other parameters in the model are not affected by the country that is excluded from effect coding.

Table 6: Results from a linear mixed effects model (ML estimation) for likelihood of engaging in crime, with demographics, sanction variables, and countries included as fixed effect variables. Demographic variables, sanction variables, and the outcome variable were standardized for ease of interpretation. Two-way interaction terms between sanction and country variables were also included in the model. Subject and item were entered as random effect variables. Effects coding was used for countries, such that the reported parameter estimates compare each country’s mean against the grand mean. The analysis was run twice with a different country excluded in the deviation time each time, so that parameter estimates for all five countries could be reported.

<table>
<thead>
<tr>
<th>Fixed effects</th>
<th>b</th>
<th>p</th>
</tr>
</thead>
<tbody>
<tr>
<td>(Intercept)</td>
<td>2.578</td>
<td>0.000 ***</td>
</tr>
<tr>
<td>FEMALE</td>
<td>0.033</td>
<td>0.499</td>
</tr>
<tr>
<td>Age</td>
<td>-0.120</td>
<td>0.016 *</td>
</tr>
<tr>
<td>MINORITY</td>
<td>-0.047</td>
<td>0.339</td>
</tr>
<tr>
<td>Earn</td>
<td>0.153</td>
<td>0.002 **</td>
</tr>
<tr>
<td>Religiosity</td>
<td>0.051</td>
<td>0.310</td>
</tr>
<tr>
<td>Mistrust</td>
<td>0.106</td>
<td>0.029 *</td>
</tr>
<tr>
<td>LEGAL</td>
<td>-0.329</td>
<td>0.000 ***</td>
</tr>
<tr>
<td>FRIEND</td>
<td>0.082</td>
<td>0.048 *</td>
</tr>
<tr>
<td>FAMILY</td>
<td>0.073</td>
<td>0.105</td>
</tr>
<tr>
<td>SELF</td>
<td>-1.201</td>
<td>0.000 ***</td>
</tr>
</tbody>
</table>
As can be seen from Table 5, country main effects were significant only for Colombia and USA; overall, Colombians reported greater-than-average likelihood of engaging in illegal actions (b=0.614, p<.001), while Americans reported less-than-average likelihood. Sanction by country interactions terms allowed us to address the
question of whether the deterrent effects of sanctions varied according to country. Legal sanctions were found to have stronger deterrent effects for China and weaker deterrent effects in Colombia. The reverse deterrent effect of friend sanctions was particularly strong in China relative to the other countries (positive interaction term) whereas a negative interaction term was observed for USA. With regard to family sanctions, no significant differences were observed across countries. Finally, self sanctions had significantly stronger deterrent effects in Germany, and marginally stronger deterrent effects in Colombia, whereas in the USA, self sanctions were weaker relative to other countries.

3.4.5 Do Deterrent Effects Vary Across Crimes?

Until this point, variation in specific crimes was treated as a nuisance variable. To compare the deterrent effects of sanction threats across the seven illegal actions, we conducted separate linear regression analyses for each action. Legal, friend, family, and self sanctions for the specific crime were entered as predictors, along with demographic variables (predictor variables were unstandardized, as the standardized beta values are computed for these models). First, the series of linear regression analyses was run on the full sample, not distinguishing subjects based on country. These analyses were then repeated on subjects from each of the five countries separately.

The beta values for legal, friend, family and self sanction threats for each series of regression analyses are depicted in Figure 4. As can be seen from the figure, with limited exceptions, the deterrent effects of self sanction threats are non-overlapping with the deterrent effects of the other categories of sanction threats.
Figure 4: Summary of the beta values for legal, friends, family, and self product variables entered as predictor variables in linear regression analyses. For each item, sanction product variables were entered as predictors with self-reported likelihood of engaging in the action entered as the dependent measure.

3.5 Discussion

Building on a substantial literature examining the deterrence hypothesis for legal sanctions, the present research compared the effectiveness of legal, social (both friend and family), and personal sanctions on deterring crime in an international sample spanning five countries. Replicating the findings of others (Cochran et al., 1999; Grasmick, Bursick, et al., 1993; Grasmick, Bursik, et al., 1993; Grasmick & Bursik, 1990; Kobayashi et al., 2001; Wenzel, 2004), we found personal sanctions to have the strongest deterrent effect on illegal behavior. This pattern was observed in every country studied, indicating that the primacy of personal sanctions is robust across cultures. In line with
deterrence research, legal sanctions were also found to have a significant though weaker overall effect. The effect of legal sanctions was significant in China, Germany, and USA, marginally significant in Portugal, and nonsignificant in Colombia, suggesting variability across cultures in the extent to which legal sanctions effectively deter criminal behavior. The relative effects of personal and legal deterrents were also robust across crimes, with personal sanctions usurping legal sanctions for every crime in every country, with only one exception (bribing a police officer by Americans).

Some researchers have proposed that the deterrent effects of legal sanctions are stronger when personal sanctions are lax, though others have argued in favor of purely additive effects (Grasmick & Green, 1980, 1981). Supporting the interaction hypothesis, we observed a significant positive interaction between legal and personal sanction threats, an effect also observed by Wenzel (2004) in his study of tax evasion among Australian citizens. In our international sample, the interaction was evident in every country except Colombia. Follow-up moderation analyses showed that the effect of legal sanctions was significant only when personal sanctions were lax; however, the moderation was significant for only four of the seven illegal actions (in contrast to Wenzel’s findings, the effect was not significant for tax evasion). These results suggest that the interaction between legal and internal sanctions may depend on the particular action.

3.5.1 Social Influences on Crime

An unexpected finding was the positive relationship observed between social sanctions and crime. Overall, the effect of friend sanctions was positive and significant. Examining countries separately, a significant or marginally significant positive effect for either friend or family sanctions was observed in every country except Colombia. To better understand these effects, we conducted additional analyses with probability and
severity sanction variables entered as separate predictors. In every country, probability of being found out by friends was positively related to likelihood of acting illegally; the same was true for probability of being found out by family in every country except Germany. Although this result was not anticipated, we speculate that both probability of engaging in crime and probability of being found out by friends and family may be related to a third underlying variable, namely the extent to which the action is viewed as normative. Asking participants to estimate the likelihood of being judged by (as opposed to found out by) their social relations may have led to different results.

While we are not aware of any other study reporting a positive relationship between social sanction threats and likelihood of acting illegally, several other studies have failed to find deterrent effects of social sanctions (Cochran et al., 1999; Grasmick, Bursick, et al., 1993; Grasmick & Bursik, 1990; Kobayashi et al., 2001). Does this failure to observe a deterrent effect of social sanctions imply that social norms do not impact dishonest behavior? Such a conclusion seems highly unlikely in light of a vast body of research illustrating the power of social norms (Cialdini & Goldstein, 2004; Fehr & Fischbacher, 2004). We propose instead that the power of social norms occurs primarily through their internalization as moral standards by members of society (Campbell, 1964).

Some scholars have proposed that legal sanctions may deter crime not through material disincentives but by increasing the level social condemnation that results from a dishonest action (Tittle & Logan, 1973; Williams & Hawkins, 1986). According to this theory, if a woman acts dishonestly, other people will judge her more harshly for her action if it is against the law, and it is this increased threat of social judgment that accounts for the legal deterrent effect. Interestingly, we observed a marginally significant negative interaction between legal and social sanctions, implying that the
legal deterrents were more effective when social sanctions were stronger. In his study of tax evasion, Wenzel observed a similar effect, though it was only evident for those who did not identify as Australian citizens. These results provide tentative evidence for synergistic effects when legal and social sanctions operate in tandem.

### 3.5.2 Implications

Kobayashi and colleagues (2001) examined differences in workplace compliance between Japanese and American employees, and found that these differences could be accounted for by differences in perceived personal, social and management sanctions. In contrast, while we observed country differences in terms of likelihood of engaging in specific crimes, these cross-cultural differences in likelihood of engaging in crime were not entirely accounted for by differences in sanctions. For six of the seven actions in our study, differences in legal sanctions across countries explained some of the variation in country-level differences in dishonesty, while differences in social and personal sanctions were unrelated to country variation. These results raise the interesting possibility that cultural drivers dishonesty are not entirely captured by sanctions. For example, it is possible that cultural differences in internal or external rewards associated with dishonesty account for variation in crime. Further research is needed to understand whether cross-cultural variation in crime is best accounted for by differences in sanctions or differences in other variables.

From a policy perspective, our findings raise the important question of whether policy efforts can change people’s internal moral commitments to honesty and socially upright behavior. In a longitudinal study on drunk driving, Grasmick and colleagues measured intentions to engage in drunk driving 1982 and 1990, along with perceived legal, social, and personal sanctions, among residents of Oklahoma City (Grasmick, Bursik, et al., 1993). This eight-year interval was characterized by social efforts aimed at
reducing drunk driving (for example, Mothers Against Drunk Driving rose to prominence during this time), as well as harsher legal sentences (Jacobs, 1989; Ross, 1994). The study found that intentions to engage in drunk driving indeed diminished over this time period – but that the reduction was primarily accounted for by the threat of personal sanctions (feelings of shame), rather than perceived threats of social or legal sanctions. Thus, over time, efforts at changing policy or social stigma may translate into internalized morals.

In conclusion, our findings suggest that across societies and cultures, internalized moral standards exert the most powerful restraints on dishonest behavior (see also Campbell, 1964). Policy efforts aimed at promoting moral internalization may be more effective than efforts aimed at increasing the frequency or probability of legal sentences. However, the process by internalization occurs remains poorly understood, and marks an important direction for future research aimed at reducing crime and enhancing social welfare.
4. Dishonesty Across Domains of Life

4.1 Overview

Chapters 2 and 3 report similar patterns of dishonesty across countries, both in terms of behavior on the die task and in terms of the relative strengths of legal, social and personal deterrents. In addition, however, Chapter 3 exposed country differences in the likelihood of engaging in specific dishonest behaviors. Chapter 4 further explores variation in specific dishonest behaviors. In addition to country level variation, this chapter looks at whether individuals express different levels of dishonesty in different domains of life. Failure to find domain differences in dishonest tendencies would be in line with the traditional view of honesty as a stable, one-dimensional character trait.

Half of participants from the cross-cultural study (i.e. the half of participants from each of the ten samples who did not complete the Dishonesty Deterrents survey from Chapter 3) were given the Everyday Dishonesty Survey. This survey asked participants to imagine themselves in 49 (or 56) specific situations, drawn from seven (or eight) domains of life, and to report the likelihood that they would act dishonestly in each situation. Likelihood ratio tests based on comparisons of linear mixed models revealed a significant interaction between country and domain for dishonest behavior. Similarly, these tests revealed significant intra-individual variation by domain, which indicates that individuals’ dishonest tendencies are not uniform across domains. A principle component analysis (PCA) pointed to three domain delineations using a bottom-up strategy, which again yielded country variation. These results provide evidence against the traditional notion of moral character, suggesting that character may be more aptly conceived as a multidimensional construct that captures an individual’s honest or dishonest tendencies across various life domains.
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4.2 Introduction

An important question that has been widely discussed in the morality literature is whether honesty is a unified trait, or whether people’s tendencies toward dishonesty depend on the specific situation (Allinsmith, 1960; Ashton & Lee, 2008; Barbu, 1951; Brogden, 1940; Burton, 1963; Cohen, Panter, & Turan, 2013). All individuals face situations in which they have the opportunity to behave dishonestly – furthermore, these opportunities present themselves in different domains of life, such as work, romantic relationships, friendships, and so on. In considering the stability of honesty, we propose that tendencies toward dishonesty cluster according to domain. We borrow from Garner’s concept of multiple intelligences, which suggests that intelligence is not dominated by a single ability but by several domain-specific intelligences (Gardner, 1987), and posit a theory of “multiple domains of honesty”.

To illustrate our main idea, consider Sheila, an office worker, and Joseph, a college student. If Sheila lies about her hours at work, will she be more likely to misuse company resources for personal gain or to lie to her husband? If Joseph plagiarizes an essay, will he be more likely to cheat on a final exam or to cheat on his taxes? If dishonesty is domain-specific, it is more likely that Sheila will behave dishonestly at work than at home, and that Joseph will behave dishonestly at college than on Tax Day. The question of whether individuals’ dishonest tendencies vary according to domain inspired our research.
4.2.1 Debate on Generality versus Specificity of Honesty

Researchers have long debated whether honesty is a stable trait or whether tendencies toward honesty are dependent on specific situations. Hartshorne and May’s seminal volume, *Studies in Deceit*, was one of the first investigations on cheating behavior and gave rise to the doctrine of specificity: the theory that dishonest behavior is specific to particular situations (Hartshorne & May, 1928). They conducted multiple studies with approximately 1,100 children to examine dishonest behavior allowing for different types of dishonesty, such as copying, stealing, lying, faking, and peeping. Children engaged in different types of tests (intelligence tests, speed tests, coordination tests, etc.) across different situations. Across all studies, the average correlation for any one type of test with any other type of test was .26, which the researchers gauged was too low to claim the existence of a generalized honesty trait. Notably, Hartshorne and May found that correlations were higher between tests of a similar nature, such as tests involving similar motives or settings.

While many researchers viewed Hartshorne and May’s publication as having resolved the generality versus specificity debate, others argued for the existence of a common moral factor (Barbu, 1951; Brogden, 1940; Burton, 1963; MacKinnon, 1938; Maller, 1934). For example, MacKinnon (1938) found that individuals who cheated on a task were more likely to lie afterwards, and Brogden (1940) observed an underlying honesty factor when analyzing data from six character tests. Similarly, Barbu (1951) reported evidence for a stable honesty trait when examining honesty in children in Romania. Later, Burton (1963), questioning Hartshorne and May’s conclusions, re-analyzed their data using an exploratory principal components analysis technique. He found that the first component accounted for a large portion of the variance (42.8%
based on summed scores of six tests, and 34.6% based on average intercorrelations between tests), which he interpreted as support for the generality hypothesis.

4.2.2 Situational Influences on Dishonest Behavior

In the past decade, renewed interest in the forces driving dishonesty has led to a flourishing of research on this topic. Much of this research has focused on how situational factors impact dishonesty. As an alternative to the rational economic framework, Mazar, Amir, and Ariely (2008) proposed a new theory for understanding dishonest behavior, which posits that individuals seek to balance the personal gains from dishonesty with the desire to maintain a positive self-concept regarding their own honesty. They found that small modifications in the environment, such as the presence of moral reminders or distance from money, significantly impacted dishonest behavior. Subsequent research in behavioral economics and psychology has identified numerous situational cues that increase dishonesty, such as the presence of wealth (Gino & Pierce, 2009), dim lighting (C.-B. Zhong et al., 2010), and wearing counterfeit clothing (Gino, Norton, & Ariely, 2010).

Other research investigating situational influences has explored how social factors, specifically others’ dishonest behavior, can influence individuals’ ethical decision-making. For example, Gino and colleagues found that witnessing another person cheating increased participants’ dishonesty – unless the cheater was believed to be an out-group member, in which case it had the opposite effect (Gino, Ayal, & Ariely, 2009). Furthermore, research has shown that people cheat more when others also benefit from their dishonesty (Gino, Ayal, & Ariely, 2012; Wiltermuth, 2011). Although such research does not address the notion of moral character directly, it tends to follow in the situationist tradition, emphasizing the power of transient environmental stimuli in influencing moral behavior.
4.2.3 Research on Personality and Moral Character

At the same time, a small but vocal contingent of researchers from the personality tradition has argued that the power of character in shaping moral behavior has been overlooked (Fleeson, Furr, Jayawickreme, Meindl, & Helzer, 2014; Narvaez & Lapsley, 2009). Re-considering Hartshorne and May’s results, Fleeson and colleagues point out that low correlations between individual tests scores do not necessarily discount the possibility of broad character traits (William Fleeson et al., 2014). As pointed out by (Epstein, 1979), the average of a set of multiple measurements will be more stable and less biased than any single measurements from the set; this is known as the principle of aggregation. From this perspective, the concept of moral character assumes the existence of dispositional traits that are related to moral actions; more importantly, this notion implies that honesty is a stable and robust trait, which remains constant across time and situations.

Similarly, several studies have found that particular personality traits can predict unethical behavior: creativity (Gino & Ariely, 2012), guilt proneness (Cohen, Panter, & Turan, 2012), and Honesty-Humility (Ashton & Lee, 2008). For example, research on honesty-humility in the HEXACO Personality Inventory supports the theory that this trait is related to unethical behavior. Lee and colleagues found that a measure of honesty-humility predicted scores on integrity tests and unethical decision-making (Kibeom Lee, Ashton, Morrison, Cordery, & Dunlop, 2008). Other researchers have shown that two key moral traits, honesty-humility and guilt proneness, correlate with unethical decision-making (Cohen, Panter, Turan, Morse, & Kim, 2013), delinquency (Tangney, Stuewig, Mashek, & Hastings, 2011), and counterproductive work behavior (Cohen et al., 2012), claiming that people with low levels of these character traits will behave more dishonestly.
4.2.4 Towards a Theory of Multiple Domains of Dishonesty

Previous research has found mixed evidence to support either the existence of a moral factor and the importance of social and situational factors on dishonest behavior. We propose a new framework suggesting a third alternative, namely that moral character interacts with situational factors. This framework conceives honesty as a multidimensional trait. We hypothesize that dishonest behavior clusters by domain, and therefore individuals would be more likely to behave dishonestly in situations that are similar to one another.

In this paper, we were interested in examining daily dishonest behavior across several domains of life in an integrated manner. Our question of central interest was whether dishonesty varies according to life domain. Therefore, we examined the degree to which dishonest behaviors in one domain of life are related to other dishonest behaviors within that domain of life, compared to the degree to which they are related to dishonest behaviors in other domains. We approached our question of whether dishonesty varies according to life domain in two analogous ways: first, by asking whether individuals grouped by country vary in dishonesty according to domain, and second, by asking whether individuals vary in dishonest tendencies according to domain. Therefore, a cross-cultural study design allowed us to test the theory of multiple domains of honesty, at multiple levels, i.e. at the level of groups (countries), and at the level of individuals. Our hypotheses were as follows:

H1: Individuals in different countries vary in dishonesty according to domain.

H2: An individual’s tendency to act dishonestly varies according to domain.

To test these hypotheses, we conducted a cross-cultural study with participants in five countries using a survey, which we refer to as the Everyday Dishonesty Survey. In each country, we administered the survey to students and to a sample of adults from the
general public. The Everyday Dishonesty Survey assessed dishonest behavior across eight domains of life, asking participants to report the likelihood of engaging in 56 dishonest actions. Our investigation extends discussions concerning the generality versus specificity of dishonesty by examining a third possibility, namely that dishonest behavior varies according to domain of life.

4.3 Method

4.3.1 Participants

We administered a study to participants from ten participant samples in five different countries: China, Colombia, Germany, Portugal, and the United States. In each country, we collected data from students in laboratory settings at public universities, and from adults in coffee shops located in major cities. All subjects completed a study, which included a behavioral task followed by a survey.

Our initial sample was 1,231 participants. In order to ensure that participants were native to each country of interest, we limited our analyses to individuals who reported being born in and currently living in the country where we administered our study (thus, 139 non-native residents were excluded). Additionally, thirteen individuals who did not complete the study due to technical or personal issues were also excluded. With these restrictions, our valid sample included 1,079 participants. Due to the different number of items included for each cohort, we ran separate analyses for the student and general public samples, considering the public sample data as a built-in replication of the student sample data.

4.3.2 Materials

All materials were translated into the native language of each country, using a forward-backward translation procedure. To examine dishonest behavior in different
domains of life, we designed a survey assessing everyday acts of dishonesty, which participants completed on iPads. The Everyday Dishonesty Survey had the goal of examining whether situation-specific dishonesty varies across domains and countries. At the beginning of the survey, participants read instructions indicating that they would read several statements and that for each statement they should report how likely they would be to engage in that particular action. Participants were assured that their responses were confidential and anonymous, and were told that if a specific action didn’t apply to them they should imagine themselves in the situation and respond accordingly.

The Everyday Dishonesty Survey included specific dishonest actions drawn from eight domains of life, which the authors created: work, government, business, relationships, friends, religion, strangers, and academic. Some example items are: “How likely are you to include false work qualifications on your resume?” (work domain); “How likely are you to lie to your relationship partner when he or she asks if you are attracted to someone else?” (relationship domain); and “How likely are you to park your car in a no parking zone?” (government domain; see Appendix B for full list of items). Participants from the public sample did not answer questions from the academic domain since statements would not apply to them; therefore, participants from the public sample responded to 49 questions, and participants from the student sample responded to 56 questions. Each statement described a specific dishonest behavior and participants were asked to report how likely they would be to engage in each behavior using continuous scales ranging from 0 (“not at all likely”) to 10 (“very likely”). All items were presented in random order. Afterwards, participants answered demographic items that included gender, age, and political orientation, among other variables.
4.3.3 Procedure

Students at public universities were recruited with flyers or standard lab procedures for a paid decision-making study. Students completed the study in a testing room with 5-8 individual stations. In coffee shops, patrons were approached by an experimenter and asked whether they would be interested in participating in a paid decision-making study. Coffee shop patrons who agreed to participate completed the study individually from where they were sitting. Aside from the addition of the academic category, study design and materials were the same for both cohorts.

The study included two parts: first participants completed a die task in which they could earn up to $12; payment per country was adjusted using the Purchasing Power Parity Index and the results for this part of the study are described in a related paper (Mann, Garcia-Rada, Hornuf, Tafurt, Ariely, 2015). The second part of the study included the Everyday Dishonesty Survey. For all participants, an experimenter delivered instructions in the country’s native language. Finally, subjects were paid and thanked for their participation.

4.4 Results

To examine our two hypotheses, whether country-wide dishonesty varied by domain and domain and whether individuals’ dishonest tendencies varied by domain, we ran linear mixed models. The result section is structured as follows: first, we examine our data looking at descriptive statistics and conducting analysis of variance to obtain an overview of our data. Next, we present the results from linear mixed models that assess four parameters: domain, country, the interaction of country by domain, and intra-individual variation (interaction of subject by domain). Afterwards, we present how items correlate with one another and with a behavioral measure of cheating. Finally, we
show a principal components analysis conducted in order to examine other ways in which domains could be conceptualized.

4.4.1 Effect of Country on Domain Specific Behavior

First, to examine the effect of country on self-reported dishonesty, we conducted one-way ANOVAs on domain scores. This approach provided us with a descriptive overview of the data, and allowed us to test our first hypothesis that individuals from different countries vary in dishonesty according to domain. We computed domain scores by averaging the seven items in each domain; therefore, each individual in the student sample had eight domain scores and each individual in the public sample had seven domain scores.

Figure 5 and Figure 6 display the domain means per sample and country, and the overall effect of country for each domain of dishonesty assessed using a one-way ANOVA. Overall, we found a significant effect of country on all domains for both the student and the public samples (p<.05). Although self-reported dishonesty varied by country and by domain, Colombian students had the highest scores in all domains compared to students in other countries. However, countries in general varied in their dishonest behavior by domain: for example, USA students reported lower likelihood of engaging in academic cheating than the other four countries, whilst Chinese students had substantially lower scores for religious dishonesty. Similarly, while examining data from the public sample there was no country that stands out with the highest or lowest scores across all domains. Overall, these results support the theory that domain-specific dishonesty varies across countries.
4.4.2 Variation by Domain and Country using Linear Mixed Models

To formally test our first hypothesis, we examined the effect of domain and country as an interaction on the responses for each question on the Everyday Dishonesty Survey (referred to as ‘item scores’ from now on). To test our three parameters of interest, country, domain, and interaction between country and domain, we constructed two linear mixed effects regression models in each case with item scores included as the
continuous dependent variable: an alternative model including a fixed effect for the variable of interest, and a baseline null model excluding only the parameter of interest (e.g. when testing for domain, country was included in both models and domain only was included in the alternative model). Additionally, random effect terms were included in all models to account for subject and item-level effects. These analyses were conducted in R (R Core Team, 2014), using the lme4 package (Bates, Mächler, Bolker, & Walker, 2014). P-values were computed with the Satterthwaite approximation, using the lmerTest package (Kuznetsova, Christensen, Bavay, & Brockhoff, 2015). Finally, models were compared using a maximum likelihood (ML) approach; the log likelihood ratio tested for model improvement between the null and alternative model.¹

We ran linear mixed models to test for a main effect of domain, a main effect of country, and, of particular interest to the question at hand, an interaction effect between country and domain. Our domain theory would not necessarily predict differences in domains overall, since we created domains with multiple items that varied in severity, and tried to roughly equate the average level of severity across domains. In line with this intention, we found no main effect of domain on dishonest behavior (Students: $\chi^2 (7) = 2.11, p = .953$; Public: $\chi^2 (6) = 1.29, p = .973$), while adjusting for country. Secondly, we examined the main effect of a country parameter and found a highly significant effect for both cohorts (Students: $\chi^2 (4) = 63.38, p < .001$; Public: $\chi^2 (4) = 15.41, p = .004$), while adjusting for domain. These results, together with the analysis of variance presented earlier, provide evidence to support the idea that dishonest behavior varies across countries.

¹ We replicated our analyses conducting models with ordinal item scores as dependent variables (rounding the item score to the nearest unit) and patterns of association and significance levels are consistent, and our conclusions remain unchanged.
Finally, as a formal test of our first hypothesis of interest, we tested for an interaction of country and domain, by including an interaction term in the alternative mixed effects model, and comparing this against a parallel null model without an interaction term. This comparison revealed a highly significant model improvement, indicating that the effect of country on dishonesty depends on the values for domains (Students: $\chi^2(28) = 685.19, p < .001$; Public: $\chi^2(24) = 416.54, p < .001$). In general, our results reveal significant variation in dishonesty across countries and domains, and suggest that country differences in dishonesty are not uniform but depend on a particular domain.

4.4.3 Intra-Individual Variation by Domain

To test our second hypothesis, we examined whether individuals’ tendencies to act dishonestly depended on domain. To do so, we considered adding individual level random domain effects (alternative model) to the model with fixed effects for country, domain and their interaction and random effects for question and subject (null model). The log likelihood ratio tests comparing the alternative and null models revealed a significant effect of intra-individual variation for both cohorts (Students: $\chi^2(35) = 652.33, p < .001$; Public: $\chi^2(27) = 356.92, p < .001$). Using this approach, we showed that individuals vary in their responses to questions by domain, sometimes responding systematically below and sometimes systematically above the domain means. In sum, this analysis supports the idea that individuals adjust their moral character when facing diverse opportunities to behave dishonestly in ways that correspond to different domains of life. Finally, we re-ran the models including six demographic variables in both null and alternative models (gender, age, ethnicity, relative earnings, religiosity and trust) and results of the log likelihood test assessing the effect of country, domain, interaction and intra-individual variation hold.
4.4.4 Correlational Analyses between Items

In order to compare our results to those observed by Hartshorne and May almost a century back, we analyzed the correlations between our 56 dishonest behaviors. We computed bivariate correlations and obtained Spearman’s correlation coefficients to assess the relationship between every two items. The average correlation between two items from the same domain was $r = 0.26$ whilst the average correlation between two items from different domains was $r = 0.19$. Finally, all items correlated positively with one another except for five negative correlations, which were extremely close to zero. More importantly we found higher correlations between items from the same domain than between domains, which supports our theory of multiple domains of honesty.

4.4.5 Correlational Analyses between Domain Averages and Behavioral Cheating

Next, we wanted to examine whether self-reported dishonesty by domain correlated with behavioral cheating, so we computed the correlations between domain averages and a behavioral measure of cheating used in this study. As mentioned in the methods section, participants completed a die task before the Everyday Dishonesty Survey. In this task, participants were asked to roll a virtual die twenty times. Each time, before rolling, they were instructed to choose a side in their minds, which they were asked to report only after seeing the outcome (the number of dots) on each side of the die. Participants were paid a fixed amount per dot on the side they reported, so they always had the financial incentive to cheat and report the high-paying side. We can assess dishonest behavior in a sample by aggregating participants’ responses and see if the proportion of times the high-paying side was chosen is significantly above 0.5. Overall, we found very weak and marginally significant correlations between the behavioral cheating measure and only two domain averages (work: $r=0.06$, $p=.060$;
strangers: $r=0.06$, $p=.064$). These results combine both cohorts and five countries; when running the analysis separately only the result for work holds in the student sample (work: $r=0.101$, $p=.015$) and none of the domains correlated with the die task in the public sample. Finally, all other correlations between domain averages and behavioral cheating were not significant. The behavioral task measured dishonesty in an abstract way and revealed individuals' moral backbones, namely their tendencies to behave dishonestly.

4.4.6 Alternative Method to Conceptualize Domains

Finally, since the authors created the eight domains used in this study, we considered that domains of dishonesty might be conceptualized in different ways. To complement our top-down approach to domains, we also engaged in a bottom-up approach to explore what natural domains emerged from our data. We conducted a principal components analysis on our Everyday Dishonesty Survey data. Burton (1963) performed a factor analysis on Hartshorne and May's results, and we adopted some of his guidelines to examine our data: a criterion of minimum 30% of the total variance for the component to have a "g" characteristic and loadings of all tests should be minimum 0.40. Similarly to our linear mixed models, we conducted separate analyses for student and public samples due to the different numbers of items participants answered.

A first extraction on the student samples aggregating the five countries (N=578; 56 variables) and set with Eigenvalues greater than one revealed a three-factor solution: the first factor explained 20.83% of the variance, the second factor 5.61%, and the third factor 4.73%. Next, we conducted a second round of extraction fixing the number of factors to three, which combined explained 31.18% of the variance. We repeated the same analysis aggregating the public samples of our five countries (N=501; 49 variables). A first extraction set with Eigenvalues greater than 1 also revealed a three-factor
solution: the first factor explained 23.50% of the variance, the second factor 5.53% and the third factor 4.46%. Again, we conducted a second round of extraction fixing the number of factors on three, which aggregated explain 33.49% of the variance. Table 7 displays the full-rotated component matrix for both cohort analyses, and Table 9 summarizes the information for the three resulting components.

Table 7: Principal Component Analysis – Rotated Component Matrix for both Cohort Analyses. The analysis was performed using the Varimix rotation technique with Kaiser Normalization.

<table>
<thead>
<tr>
<th>Domain</th>
<th>Item: How likely are you to…</th>
<th>Student Sample (N=578)</th>
<th>Public Sample (N=501)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>I = A</td>
<td>II = B</td>
</tr>
<tr>
<td>Work</td>
<td>Include false work qualifications on your resume?</td>
<td>.595</td>
<td></td>
</tr>
<tr>
<td>Government</td>
<td>Omit information on your tax filings in order to pay less income tax?</td>
<td>.642</td>
<td></td>
</tr>
<tr>
<td>Government</td>
<td>Bribe a police officer to avoid getting a speeding ticket?</td>
<td>.570</td>
<td></td>
</tr>
<tr>
<td>Government</td>
<td>Apply for a government tax credit knowing you are not eligible for it?</td>
<td>.445</td>
<td></td>
</tr>
<tr>
<td>Government</td>
<td>Fake a signature of a doctor on a government document in order to get an expensive medication for free?</td>
<td>.688</td>
<td></td>
</tr>
<tr>
<td>Business</td>
<td>Leave a store with an article of clothing you did not pay for (on purpose)?</td>
<td>.516</td>
<td></td>
</tr>
<tr>
<td>Business</td>
<td>Provide your insurance company with false information in order to reduce your premium?</td>
<td>.532</td>
<td></td>
</tr>
<tr>
<td>Business</td>
<td>Leave a restaurant without paying your bill (on purpose)?</td>
<td>.529</td>
<td></td>
</tr>
<tr>
<td>Work</td>
<td>Buy dinner for your friends and submit the receipt to your workplace as a business expense?</td>
<td>.652</td>
<td></td>
</tr>
<tr>
<td>Work</td>
<td>Take a sick day from work when you are not sick?</td>
<td>.412</td>
<td></td>
</tr>
<tr>
<td>Work</td>
<td>Engage in personal activities (such as paying your personal bills) while on company time?</td>
<td>.402</td>
<td></td>
</tr>
<tr>
<td>Strangers</td>
<td>Drive away without leaving a note, after you accidentally dented the bumper of a parked vehicle?</td>
<td>.420</td>
<td></td>
</tr>
<tr>
<td>Government</td>
<td>Run a red light when nobody is around?</td>
<td>.461</td>
<td></td>
</tr>
<tr>
<td>Government</td>
<td>Park your car in a no parking zone?</td>
<td>.403</td>
<td></td>
</tr>
<tr>
<td>Academic</td>
<td>Bring a piece of paper with course material into an exam, against the rules?</td>
<td>.513</td>
<td></td>
</tr>
<tr>
<td>Academic</td>
<td>Purchase an essay that you did not write and submit it as your own?</td>
<td>.536</td>
<td></td>
</tr>
<tr>
<td>Relationship</td>
<td>Take money out of your joint bank account and use it without your relationship partner knowing, for something that you know they would not approve of?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Friends</td>
<td>Say your die fell on six when in fact it fell on 3 while playing a board game with friends?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Strangers</td>
<td>Take money that you see fall from a stranger’s pocket?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Context</td>
<td>Question</td>
<td>Column 1</td>
<td>Column 2</td>
</tr>
<tr>
<td>--------------</td>
<td>--------------------------------------------------------------------------</td>
<td>----------</td>
<td>----------</td>
</tr>
<tr>
<td>Relationship</td>
<td>Have a one-night affair with someone that is not your relationship partner?</td>
<td>.406</td>
<td></td>
</tr>
<tr>
<td>Relationship</td>
<td>Engage in continued sexual relations with someone that is not your relationship partner?</td>
<td>.401</td>
<td>.535</td>
</tr>
<tr>
<td>Religion</td>
<td>Break a promise to a leader of your religious group that you will spend an afternoon volunteering for a good cause, and instead stay at home?</td>
<td>.563</td>
<td>.424</td>
</tr>
<tr>
<td>Strangers</td>
<td>Keep a stranger’s camera that you find in a bathroom stall?</td>
<td>.420</td>
<td>.463</td>
</tr>
<tr>
<td>Government</td>
<td>Speed by 15% over the speed limit while driving?</td>
<td>.523</td>
<td></td>
</tr>
<tr>
<td>Business</td>
<td>Install a computer program that one of your friends purchased on your computer, instead of buying it yourself?</td>
<td>.561</td>
<td></td>
</tr>
<tr>
<td>Religion</td>
<td>Eat a food that is forbidden according to your religion’s laws?</td>
<td>.699</td>
<td>.671</td>
</tr>
<tr>
<td>Religion</td>
<td>Skip a religious ceremony that you are expected to attend so that you can go to a party?</td>
<td>.675</td>
<td></td>
</tr>
<tr>
<td>Religion</td>
<td>Drink (but not eat) during a religious fast where you are supposed to neither eat nor drink?</td>
<td>.596</td>
<td></td>
</tr>
<tr>
<td>Religion</td>
<td>Use the Lord’s name in vain?</td>
<td>.679</td>
<td></td>
</tr>
<tr>
<td>Strangers</td>
<td>Listen in on a private conversation between two strangers?</td>
<td>.469</td>
<td></td>
</tr>
<tr>
<td>Strangers</td>
<td>Tell a beggar that you do not have any money to give away on you, when in fact you do?</td>
<td>.474</td>
<td></td>
</tr>
<tr>
<td>Strangers</td>
<td>Throw extra trash in an unknown neighbor’s trashcan?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Work</td>
<td>Take supplies (such as paper and pencils) from work to use at home for non-work-related tasks?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Work</td>
<td>Take credit for an assignment at work that someone else completed?</td>
<td>.421</td>
<td>.524</td>
</tr>
<tr>
<td>Work</td>
<td>Claim that a project is underway at work when in fact you haven’t started it?</td>
<td>.513</td>
<td></td>
</tr>
<tr>
<td>Relationship</td>
<td>Lie to your relationship partner when he or she asks if you are attracted to someone else?</td>
<td>.509</td>
<td></td>
</tr>
<tr>
<td>Relationship</td>
<td>Tell your relationship partner that you like a gift they got you, when in fact you hate it?</td>
<td>.598</td>
<td>.598</td>
</tr>
<tr>
<td>Friends</td>
<td>Tell a friend that you like her haircut even though you think it is terrible?</td>
<td>.621</td>
<td></td>
</tr>
<tr>
<td>Friends</td>
<td>Tell your friends stories about yourself that never happened in order to sound more interesting?</td>
<td>.490</td>
<td></td>
</tr>
<tr>
<td>Friends</td>
<td>Make up a false excuse about why you are late to meet a friend?</td>
<td>.552</td>
<td></td>
</tr>
<tr>
<td>Friends</td>
<td>Pretend that you did not damage a friend’s coffee table, when in fact you did?</td>
<td>.406</td>
<td></td>
</tr>
<tr>
<td>Friends</td>
<td>Gossip about a friend behind his back?</td>
<td>.519</td>
<td>.509</td>
</tr>
<tr>
<td>Friends</td>
<td>Tell a friend that you like their new boyfriend or girlfriend when you don’t?</td>
<td>.603</td>
<td></td>
</tr>
<tr>
<td>Academic</td>
<td>Lie to a teacher to justify why you didn’t submit an assignment on time?</td>
<td>.450</td>
<td></td>
</tr>
<tr>
<td>Business</td>
<td>Connect to an internet service directly, without the provider knowing, and without paying for it?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Business</td>
<td>Not mention it when you notice you were given too much change at the grocery store?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sample</td>
<td>Students (N=578)</td>
<td>Public (N=501)</td>
<td></td>
</tr>
<tr>
<td>----------------</td>
<td>------------------------------------------</td>
<td>-----------------------------------------</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Comp. A. Cheating the system</td>
<td>Comp. A. Cheating the system</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Comp. B. Social and Spiritual Dishonesty</td>
<td>Comp. C. Dishonesty for Personal Gain</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Comp. C. Dishonesty for Personal Gain</td>
<td>Total Items</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Total Items</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of</td>
<td>16</td>
<td>13</td>
<td></td>
</tr>
<tr>
<td>Variables</td>
<td></td>
<td>11</td>
<td></td>
</tr>
<tr>
<td>loading with at</td>
<td>40</td>
<td>17</td>
<td></td>
</tr>
<tr>
<td>least .40 on</td>
<td></td>
<td>15</td>
<td></td>
</tr>
<tr>
<td>that component</td>
<td></td>
<td>10</td>
<td></td>
</tr>
<tr>
<td>Percentage of</td>
<td>20.830</td>
<td>5.612</td>
<td></td>
</tr>
<tr>
<td>Variance</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Explained</td>
<td>4.734</td>
<td>4.734</td>
<td></td>
</tr>
<tr>
<td></td>
<td>30.371</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>23.502</td>
<td>5.528</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>4.464</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>33.494</td>
<td></td>
</tr>
</tbody>
</table>

Table 8. Summary of Three-Factor Solution for Component Information. The principal components analysis was performed using the Varimax rotation Technique with Kaiser Normalization. Three items in the public sample load on two components.

Our principal components analyses revealed several interesting trends. Firstly, according to Burton’s criteria, our data does not reveal a moral common factor since in the principal components analyses for both cohorts the first factor explained less than 30% of the variance. The second and third factors explained one fourth of the variance explained by the first factor, which suggests the importance of the first factor. Secondly, analyses conducted on both cohorts across five countries revealed a three-factor solution that explained approximately only one third of the variance. Based on examination of the items that loaded at least 0.40 on each component, we named these factors as
“A=cheating the system” (the first component for both cohort analyses), “B= social and spiritual dishonesty” (the second component in the student analysis and the third component in the public analysis) and “C=dishonesty for personal gain” (the third component in the student analysis and the second component in the public sample). Finally, we computed regression factor scores for each individual (DiStefano, C., Zhu, M., & Mindrila, 2009) and observed an overall significant effect of country on the three emerging factors for both cohorts (see Figure 7 and Figure 8).

**Figure 7:** Means by Country for Three Factors Emerging from Principal Component Analysis for the Student Sample. Factors means per individual were computed by averaging the items loading on each factor according to results from Principal Component Analysis. F-Statistics reported in parenthesis for each domain corresponds to one-way analysis of variance conducted on factor means to test for the effect of country, only considering student samples. Error bars represent +/- 1 standard error of the mean.
Figure 8: Means by Country for Three Factors Emerging from Principal Component Analysis for the Public Sample. Factors means per individual were computed by averaging the items loading on each factor according to results from Principal Component Analysis. F-Statistics reported in parenthesis for each domain corresponds to one-way analysis of variance conducted on factor means to test for the effect of country, only considering student samples. Error bars represent +/- 1 standard error of the mean.

4.5 General Discussion

Our results from the Everyday Dishonesty Survey data support our theory of multiple domains of honesty and suggest that ethical behavior varies according to domain of life. We evaluated the theory of domain-specific dishonesty in two ways: by examining how dishonest behavior of individuals in different countries varies by domain, and by examining how dishonest behavior of individuals varies by domain. Linear mixed models on a data set of 1,079 individuals revealed a main effect of country and an interaction effect between country and domain. Furthermore, linear mixed models revealed intra-individual variation by domains, namely that the likelihood of engaging in dishonesty varies according to domain for each individual.

Our correlational analyses, similar to those described in Hartshorne and May’s oeuvre, also support the theory of multiple domains of honesty. We find higher correlations between two items from the same domain ($r = 0.26$) than between two items from separate domains ($r = 0.19$). Notably, Hartshorne and May also observed higher
correlations between similar tests and situations (Hartshorne & May, 1928), which could be interpreted as evidence for domain-specific dishonesty. Our work extends Hartshorne and May’s research by examining dishonesty in adults and attempting to evenly sample situations from various life domains. Overall, we find that dishonest tendencies are more similar in situations from the same domain. In addition, we observe very low or null correlations between domain scores and behavioral cheating on an abstract task, which does not support an underlying common moral factor.

The eight domains used in this study were constructed based on the authors’ intuitions about the social networks in which individuals operate (e.g. work environment, romantic relationships, among others). However, results of a principal components analysis suggested that a different delineation of domains. This analysis yielded a three-factor solution, and we labeled these emergent factors as: cheating the system, dishonesty for personal gain, and social and spiritual dishonesty. This result suggests that dishonest tendencies may apply more broadly across situations than we initially conceived. However, future research is needed to test whether these emergent domains are robust, and to determine which domain delineations are the best predictors of dishonest behavior.

The cross-cultural component of our study allowed us to examine how dishonest behavior in individuals from different countries varies, both overall and by domain. Although our research highlights cross-cultural differences in dishonesty, we cannot explain from our data why these variations occur. One possibility is that social norms shape specific dishonest behavior, and that these vary from one country to another. According to norm-focus theory (Cialdini, Reno, & Kallgren, 1990), the social context determines which types of norms people attend to at a particular time and how these norms will shape individual’s behavior. In the case of dishonesty, individuals
might adjust their own ethical decision-making in specific situations where they perceive others misbehaving and as a result assume that such behavior is acceptable within their society.

Another possibility is that institutions and legal rules differ by country, and that these might also shape dishonest behavior; for example, legal sanctions for running a red light might differ by country. Future research should continue to explore differences in legal, social and personal sanctions across countries and how they shape specific dishonest behavior. For example, Kobayashi and colleagues (2001) examined differences in dishonesty in the workplace between Japanese and American individuals, and found that these could be explained by differences in internalized norms. In a related paper, we investigate the effectiveness of legal, social and personal sanctions on seven infractions and misdemeanors across five countries. We find that personal sanctions have the strongest effects on dishonest behavior, and that the deterrent effects of legal sanctions, while also significant, are strongest when personal sanctions are lax (Mann, Garcia-Rada, Hornuf, Tafurt, 2015).

4.5.1 Limitations

The present research must be qualified in light of limitations, mainly related to our self-report survey methodology. Due to multiple complications inherent to measuring everyday dishonesty directly, self-report tools such as surveys and diary methods are frequently used to assess dishonesty (B M DePaulo, Kashy, Kirkendol, Wyer, & Epstein, 1996b; Ennis, Vrij, & Chance, 2008), though these methods have limitations (Glass & Arnkoff, 1997; Razavi, 2001). However, it is possible that with self-report methods such as our Everyday Dishonesty Survey, individuals may not be completely honest when reporting their own unethical behavior. Importantly, though, while social desirability biases in self-reported dishonesty may vary across countries
(Bernardi, 2006), the bias should not affect our primary conclusions provided that social desirability bias has a similar influence on responses across domains. In addition, survey responses may have been influenced by response biases such as avoiding the extreme ends of the scale. However, such response biases would likely increase the appearance of a common moral factor, rendering our tests for the domain-specific theory of dishonesty more conservative. Still, because of the methodological limitations of assessing dishonesty with survey measures, future research that examines domain-specific dishonesty with behavioral methods such as ordinary tasks (e.g., filing an insurance form, writing an academic quiz) and field studies would bolster confidence in conclusions based on self-reports.

4.5.2 Conclusion

In conclusion, we add to the extant literature endorsing situational or character-based accounts of moral behavior by positing the existence of domain-specific moral tendencies. Our results do not preclude the possibility of a common moral factor, and future research might explore the co-existence of a common moral factor and domain specific dishonesty. Our main theoretical contribution is to propose a novel framework, which views moral character as a multidimensional construct in which honest tendencies are, at least in part, dependent on life domains. Our theory is reminiscent of Mischel and Schoda’s conceptualization of personality as reflecting signature interactions between person and situation. Mischel and Shoda (1995) proposed that in addition to differences in average levels of a particular trait, unique “if-then” behavioral signatures (e.g., she does X when A but Y when B) are needed to account for variation in behavior (Mischel, Shoda, & Mendoza-Denton, 2002). Our framework of dishonesty similarly suggests that a person’s tendency to be honest or dishonest is dependent on the
particular situation, and further posits that cross-situational consistency is evident within domains.

The present research tested our theory of multiple domains of honesty, and found support for our hypothesis that a person’s tendency to behave dishonestly depends on life domain. Rather than viewing our results as evidence against the notion of moral character, however, we propose that the results support a more nuanced view of moral character. Instead of viewing moral character as a unidimensional construct, we may instead define moral character as the unique constellation of an individual’s honest or dishonest tendencies across particular domains. We hope this investigation inspires further research aimed at understanding how domains affect the expression of honest tendencies, yielding theories that account for consistency and variation in honest behavior.
5. Comparing Lying Tendencies Across Socially Connected Pairs

5.1 Overview

Chapter 4 found evidence that dishonest tendencies vary according to domain, for individuals and for large groups (countries). Domains were defined using both top-down (theoretical) and bottom-up (data-driven) approaches. Chapter 5 takes another approach to defining domains, classifying different types of lying behavior. It also examines the role of culture at the more local level of social networks. The research in this chapter explores whether specific lying tendencies covary among individuals from the same social networks.

Using an international sample of 1,687 socially connected pairs, we investigated whether lying tendencies were related in socially connected individuals, and tested two moderators of observed relationships. Participants recruited through a massive open online course reported how likely they would be to engage in specific lies; a friend or relative responded to the same scenarios independently. We classified lies according to their beneficiary (antisocial vs. prosocial lies), and their directness (lies of commission vs. omission), resulting in four unique lying categories. Regression analyses showed that antisocial commission, antisocial omission, and prosocial commission lying tendencies were all uniquely related in connected pairs, even when the analyses were limited to pairs that were not biologically related. For antisocial lies of commission, these relationships were strongest, and were moderated by amount of time spent together. Randomly paired individuals from the same countries were also related in their antisocial commission lying tendencies, signifying country-level norms. Results indicate that a person’s lying tendencies can be predicted by the lying tendencies of his
or her friends and family members, suggesting that social networks explain meaningful variation in specific lying tendencies.

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5.2 Introduction

In all verbal interactions, people must negotiate their desire to tell the truth with their desire to benefit themselves and their desire to please others. When these competing forces come into conflict, one way to resolve the tension is to lie. We define lies as statements intended to mislead others (Bella M. DePaulo et al., 2003). Previous research has suggested that lying is a common phenomenon: reports from diary studies suggest that, on average, people lie in one out of every three to five interactions (B M DePaulo & Kashy, 1998; Bella M DePaulo, Kashy, Kirkendol, Wyer, & Epstein, 1996; Kashy & DePaulo, 1996). This research also pointed to substantial variability in the extent to which people lie in everyday life. This finding begs the question of what factors predict people’s tendencies to lie in their everyday interactions. An important unanswered question is whether people’s lying tendencies are related to – or even influenced by – the lying tendencies of others in their social networks. Here, we investigate whether socially connected individuals have similar lying tendencies, and explore whether social transmission – that is, the acquisition of behavior through implicit or explicit teaching and learning (see (Cavalli-Sforza, Feldman, Chen, & Dornbusch, 1982)) – may in part account for observed relationships.

Existing research exploring what leads people to lie in daily life has focused on personality factors. This research has produced mixed results, with early research
finding that low anxiety predicts lying (Eswara & Suryarekha, 1974), more recent studies finding that high neuroticism predicts lying (Gudjonsson, Sigurdsson, Bragason, Einarsson, & Valdimarsdottir, 2004; Weaver, III, 2005), and others finding no connection between a number of personality variables and self-reported lying tendencies (Gozna, Vrij, & Bull, 2001). Kashy and DePaulo suggested that the relevant personality profile depends on the type of lies; in their study, manipulativeness, less socialization, and less satisfying same-sex relationships predicted greater antisocial (self-serving) lying tendencies, while more satisfying same-sex relationships predicted greater prosocial (other-serving) lying tendencies (Kashy & DePaulo, 1996). Similarly, McLeod and Genereux found that different constellations of personality variables uniquely predicted four different kinds of lies (McLeod & Genereux, 2008). These studies suggest that not all lies are created equal.

### 5.2.1 Classifying Lying Along Two Dimensions

Are certain types of lying more likely than others to be related among individuals from the same social networks? Perhaps the most common classification of lying distinguishes between antisocial lying (lying to benefit one’s self; e.g. telling your parents that you completed your homework so you can watch TV, telling your spouse that you have a work obligation in order to avoid dinner with your in-laws, etc.) and prosocial lying (lying to benefit somebody else; e.g. telling your friend that you love her casserole or telling your new love interest that you have never met a better kisser) (Bella M DePaulo et al., 1996; Erat & Gneezy, 2012; Kashy & DePaulo, 1996; Kang Lee & Ross, 1997; Lindskold & Walters, 1983; Talwar & Crossman, 2011). In general, prosocial lies are considered more acceptable than antisocial lies (Levine & Schweitzer, 2014; Lindskold & Walters, 1983). However, from prior research it is unclear whether
prosocial or antisocial lying tendencies are more likely to be related among socially connected individuals.

By one line of reasoning, individuals may be attuned to the prosocial lying behavior of others with whom they interact. Scholars have suggested that two fundamental principles guide human communication: the principle of quality – that is, relaying information that is truthful – and the principle of general cooperation – that is, maintaining amiable relationships (Lakoff, 1973; Sweetser, 1987). Following the principle of general cooperation, prosocial lies can function as social glue, bolstering the ties that exist between individuals (Talwar & Crossman, 2011). Yet, if overused or misused, prosocial lies may be seen as violating the principle of quality, causing the speaker to be judged as unreliable and weakening social ties. Does the balance point for prosocial lying vary across social networks? In support of this possibility, Lee and colleagues found that Chinese children judged lies about their own prosocial actions more positively than did Canadian children, and the magnitude of this difference was greater for older children (Kang Lee, Cameron, Xu, Fu, & Board, 1997). Similarly, Chinese (but not Canadian) adults rated false statements regarding one’s own prosocial actions positively, and did not judge them to be lies (Fu, Lee, Cameron, & Xu, 2001). This suggests that acceptance of prosocial lying is not uniform, but varies across societies, which may imply that individuals are attuned to the prosocial lying tendencies of others in their social networks.

An alternative yet non-exclusive possibility is that individuals are attuned to others’ antisocial lying behavior. Gino, Ayal, and Ariely found that observing an obvious display of dishonesty increased participants’ own dishonest behavior (Francesca Gino et al., 2009). Participants were given five minutes to complete a problem-solving test with financial incentives for correct answers; some participants
writing this test observed another test-taker (a confederate) blatantly cheat in order to maximize his payout. Those who observed this behavior cheated more themselves, unless the blatant cheater was believed to be an out-group member, in which case they cheated less. These results suggest that antisocial dishonest behaviors can be socially transmitted.

In addition, cross-cultural experiments in economics point to cultural differences in norms for antisocial behavior. Cooperative behavior in a public goods game has been found to vary substantially across cultures (Gächter et al., 2010; Henrich et al., 2005; Herrmann et al., 2008), implying that some societies are more tolerant of antisocial behavior than others. This appears to be a function of both altruistic punishment (incurring a personal cost to punish non-cooperative behavior (Gächter et al., 2010)) and antisocial punishment (incurring a personal cost to punish cooperative behavior (Herrmann et al., 2008)). Inter-societal differences in willingness to punish selfish behavior have also been found in cross-cultural experiments involving ultimatum games and third-party punishment games (Henrich et al., 2005, 2010; Oosterbeek, Sloof, & van de Kuilen, 2004). Taken together, these findings suggest that antisocial lying tendencies vary across societies, which may imply that individuals are attuned to the antisocial lying tendencies of others in their social networks.

Another important distinction in considering lying tendencies in social networks concerns the directness of lies. We distinguish between lies of commission, which involve directly stating something the speaker knows to be false, and lies of omission, which lead the listener to a false belief without directly stating something known to be false. This distinction resembles the two principle factors identified by Phillips, Meek, and Vendemia in their investigation of the underlying structure of deceptive behavior (Phillips, Meek, & Vendemia, 2011). Research has shown that people judge acts of
commission more harshly than acts of omission (Baron & Ritov, 1994; Ritov & Baron, 1990; Spranca, Minsk, & Baron, 1991). In one set of studies illustrating this phenomenon, Spranca, Minsk, and Baron (Spranca et al., 1991) presented participants with identical scenarios in which a malevolent actor caused a particular outcome, either through an act of commission or omission. For instance, in one scenario the actor allowed his tennis rival to eat an allergenic food before their final match, either by recommending a particular salad dressing, or by saying nothing when that salad dressing was chosen. The majority of participants viewed recommending the allergenic salad dressing as more immoral than keeping quiet when it was chosen. This research suggest that lies of omission are less morally relevant than lies of commission, which may mean that individuals are less attuned toward lies of omission in others.

5.2.2 The Present Work

We presented participants with a survey involving specific, everyday scenarios that might invoke dishonesty, and asked them how likely they would be to lie in these scenarios. Given the inherent difficulties of measuring everyday lies directly, survey methods are commonly used to assess individual lying tendencies (Cole, 2001; Ennis et al., 2008; Heyman, Hsu, Fu, & Lee, 2012; Jensen, Arnett, Feldman, & Cauffman, 2002). In line with the two dimensions of lying discussed above, we classified scenarios according to the benefactor of the lie (i.e. antisocial vs. prosocial lies), and the directness of the lie (i.e. lies of commission vs. lies of omission), which generated four distinct categories of lies: antisocial lies of commission, antisocial lies of omission, prosocial lies of commission, and prosocial lies of omission. Participants were asked to report their likelihood of lying in 16 distinct scenarios, four representing each category. Participants’ responses to the four scenarios in each category were averaged to create unique lying subscales for the four categories. We refer to these subscales as participants’ lying
tendencies, noting that tendencies may be expressed through behaviors (e.g., exaggerating one’s hours at work) or attitudes (e.g. encouraging a co-worker to call in sick to take a holiday). All 16 scenarios described low-stakes lies, that is, commonplace lies that would not likely be judged very harshly by others. We restricted the scenarios to low-stakes lies in order to capture everyday dishonesty.

In order to investigate whether individuals in social networks have similar lying tendencies, we studied pairs of individuals who were in some way connected to one another. By recruiting students through a massive open online course (MOOC), and inviting them to share the survey with a friend or family member, we obtained a large international sample of connected pairs of individuals who completed the survey independently.

Our first research question was whether individuals’ lying tendencies could be predicted by the lying tendencies of their friends, partners, and family members, for four specific lying tendencies (antisocial commission, antisocial omission, prosocial commission, and prosocial omission). Our second research question was whether social transmission was a plausible explanation for any observed relationships. To address the second question, we asked participants to indicate whether they were biologically related to their connection, so that we could compare effects for biologically related and non-biologically related pairs. We also asked each individual to report the closeness of their relationship, as well as the amount of time they spent with the other person, in order to assess whether these variables moderated social transmission. Another question of interest was whether relatedness in lying tendencies differed for specific relationships (parent-child, sibling, spousal, friend, romantic partners, colleague). Lastly, we explored the reach of social transmission by analyzing whether lying tendencies were related among individuals from the same countries.
5.3 Method

5.3.1 Ethics Statement

The study protocol was approved by Duke University’s Institutional Review Board for research with human subjects. All participants indicated their consent to participate after reading the approved consent form. In accordance with the ethics protocol, those enrolled in an online course provided consent at the beginning of the course, and those not enrolled provided consent at the time of the survey.

5.3.2 Participants

Survey participants were recruited via a massive open online course (MOOC) in behavioral economics. During Week 3 of the course, enrolled students were invited to take the survey themselves, and to forward the survey link to someone they knew—a friend, acquaintance or family member. We provided students with an email script, including a link to the survey, to invite this person of their choosing to complete the survey. The decision of who to invite was left to each individual student.

Participants were provided a randomized ID code to include in the email to their friend or family member. Both parties were instructed to include this ID code in their survey, which allowed us to link the MOOC students’ data with their friends’/family members’ data, in pairs.

Initially, 4685 MOOC students, and 3850 of their friends or family members completed the survey. However, only participants for whom at least one other participant entered an identical ID code were included in our final sample. We also excluded participants for failing to respond to all questions in a given category, and for entering impossible values in an earlier task. Forty-seven MOOC students had multiple contacts complete the survey; for these participants, we selected the first survey that was
submitted with the identical ID code in order to ensure that no participant was represented in more than one pair.

This filtering process left us with a sample of 1,973 pairs of participants. Of these, we excluded participants whose responses regarding their relationship to one another did not match. (For example, if one party indicated that they were friends while the other party indicated they were colleagues, the pair was excluded.) Two hundred forty-five participant pairs (12.4%) were excluded for non-matching responses regarding their relationship, and 83 (4.2%) were excluded for non-matching responses to whether they were biologically related (42 of these exclusions were redundant). After excluding these pairs, we were left with a final sample of 1,687 participant pairs from 94 countries.

5.3.3 Survey

All participants completed the survey on their own time over the World Wide Web, as part of a larger survey. Participants answered 16 questions regarding how likely they would be to lie in various hypothetical scenarios. The question scenarios represented four distinct categories of lying: lying by antisocial commission (e.g., “How likely are you to tell a police officer that you were speeding due to an emergency, when there is no real emergency?”), lying by antisocial omission, (e.g., “During an interview, how likely are you to keep quiet about lacking a particular skill that is expected for the job?”), lying by prosocial commission (e.g., “How likely are you to tell your friend that her birthday party was lovely, when you know everybody was bored at it?”), and lying by prosocial omission (e.g., “If your brother or sister separates from their spouse but doesn’t want your parents to know, how likely are you to withhold this information from your parents?”; see Appendix C for the full list of questions). The 16 questions were presented in randomized order, and included four scenarios for each lying category. Participants were instructed to imagine themselves in each situation and
indicate how likely they would be to tell the lie by responding on a continuous slider scale ranging from 0 (“not at all likely”) to 10 (“extremely likely”). Those who were not MOOC students then answered demographic questions regarding age, gender, sexual orientation, relationship status, religion, income, ethnicity, country of citizenship, language, and political views; MOOC students completed the demographics section separately as part of the course.

Lastly, all participants were asked about their relationship with the other person they knew that took the survey. Participants indicated the person’s relation to them by selecting from the following categories: parent, son/daughter, sibling, grandparent, other relative, friend, colleague, boyfriend/girlfriend/significant other, spouse, and other. (In total, seven relationships were classified as “other”, namely: grandparent/granddaughter, boyfriend’s parent/son’s girlfriend, son’s fiancé/mother-in-law, lovers, ex-wife/ex-husband, and twitter follower/tweeter.) They then indicated whether they were biologically related to this person. Next, they were asked to indicate the number of waking hours per week they spent with the person (either in person or on the phone). Finally, they indicated how close they felt to the other person by selecting the appropriate diagram from the Inclusion of Other in the Self scale (Aron, Aron, & Smollan, 1992).

5.3.4 Analysis

To examine relationships across the different types of lying behavior, we first created lying indexes for each of the four types of lying by computing the average of the four items of each type. Thus, each participant had a single index for antisocial lying by commission, antisocial lying by omission, prosocial lying by commission, and prosocial lying by omission.
We then paired the data from MOOC participants with the data from their chosen friends or relatives. This allowed us to test whether the MOOC participants’ lying indexes predicted the lying indexes of their friends and relatives, and vice versa, using multiple linear regression analyses.

5.4 Results

We labeled the MOOC student in each pair as P1, and the friend or relative who received the survey from a MOOC student as P2. Table 9 shows the Cronbach’s alpha, mean, and standard deviation for each subscale, for both P1 and P2 participants. Given that each subscale consisted of only four items, alphas were expected to be lower than typical standards. Internal consistency was significantly higher for subscales measuring lies of commission (with alphas ranging from 0.55 to 0.65) than for subscales measuring lies of omission (with alphas ranging from 0.22 to 0.39). There are several plausible explanations for this difference. It is possible that tendencies to tell lies of omission are less cohesive than tendencies to tell lies of commission, but it is also possible that the items for omission subscales were less effective in capturing true underlying tendencies, or that participants had greater difficulty predicting their responses in the omission scenarios. However, McCrae and colleagues found that internal consistency was not predictive of scale validity (McCrae, Kurtz, Yamagata, & Terracciano, 2011).

<table>
<thead>
<tr>
<th></th>
<th>P1 participants</th>
<th></th>
<th>P2 participants</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>α</td>
<td>x̄</td>
<td>s</td>
<td>α</td>
</tr>
<tr>
<td>Antisocial commission</td>
<td>0.57</td>
<td>2.97</td>
<td>1.72</td>
<td>0.65</td>
</tr>
<tr>
<td>Antisocial omission</td>
<td>0.38</td>
<td>4.89</td>
<td>1.76</td>
<td>0.39</td>
</tr>
</tbody>
</table>
Our first research question was whether participants’ lying tendencies predicted the lying tendencies of their connections, for four distinct types of lies: antisocial commission, antisocial omission, prosocial commission, and prosocial omission. We ran four regression analyses, entering each P1 subscale one at a time as the dependent variable, with the four P2 subscales entered as independent variables. If P2 scores on a particular subscale uniquely predicted the P1 scores on that same subscale, we considered this as evidence for transmission. Following this approach, we found evidence for transmission on three of the four subscales: antisocial commission, antisocial omission, and prosocial commission (see Table 10). When P1 antisocial commission scores were entered the dependent variable, the beta coefficient for P2 antisocial commission scores was significant ($\beta=0.196$, $p<0.001$), while the other three subscales’ betas were not. When P1 antisocial omission scores were entered as the dependent variable, the beta coefficient for P2 antisocial commission scores was significant ($\beta=0.106$, $p<0.001$), while the other three subscales’ betas were not. Similarly, when P1 prosocial commission scores were entered as the dependent variable, the beta coefficient for P2 prosocial commission scores was significant ($\beta=0.110$, $p<0.001$), while other three subscales’ betas were not. Lastly, in contrast to the other subscales, when we entered P1 prosocial omission scores as the dependent variable, none of the P2 subscales were significant predictor variables.

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<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Prosocial</td>
<td>0.55</td>
<td>6.31</td>
<td>1.76</td>
<td>0.58</td>
<td>6.16</td>
<td>1.82</td>
</tr>
<tr>
<td>commission</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Prosocial</td>
<td>0.22</td>
<td>5.61</td>
<td>1.54</td>
<td>0.23</td>
<td>5.44</td>
<td>1.62</td>
</tr>
<tr>
<td>omission</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Table 10: Summary of Beta Values for Initial Regression Analyses. Beta values from four multiple linear regression analyses are shown, conducted on the full sample of 1,687 pairs. The four P2 lying subscales were entered as predictor variables, with one of the four P1 lying subscales entered as the dependent variable in each analysis.

<table>
<thead>
<tr>
<th>Predictor Variables</th>
<th>Dependent Variables</th>
<th>P1 antisocial commission</th>
<th>P1 antisocial omission</th>
<th>P1 prosocial commission</th>
<th>P1 prosocial omission</th>
</tr>
</thead>
<tbody>
<tr>
<td>P2 antisocial commission</td>
<td>β=.196***</td>
<td>β=.049</td>
<td>β=.021</td>
<td>β=-.065</td>
<td></td>
</tr>
<tr>
<td>P2 antisocial omission</td>
<td>β=-.012</td>
<td>β=.106***</td>
<td>β=.036</td>
<td>β=.048</td>
<td></td>
</tr>
<tr>
<td>P2 prosocial commission</td>
<td>β=-.031</td>
<td>β=.018</td>
<td>β=.110***</td>
<td>β=.062</td>
<td></td>
</tr>
<tr>
<td>P2 prosocial omission</td>
<td>β=.000</td>
<td>β=-.045</td>
<td>β=-.022</td>
<td>β=.027</td>
<td></td>
</tr>
</tbody>
</table>

***p<.001

The paired nature of our dataset allowed us to test for replication by entering P2 subscales as dependent variables and P1 subscales as predictor variables in four new regression analyses. These analyses again showed transmission effects for antisocial commission, antisocial omission, and prosocial commission subscales (all ps<.001), and a similar overall pattern of results (see Table 11). Thus, we observed transmission for three lying tendencies in an initial series of regression analyses, and replicated these effects in a second series of regression analyses.

Table 11: Summary of Beta Values for Replication Regression Analyses. Beta values from four multiple linear regression analyses are shown, conducted on the full sample of 1,687 pairs. The four P1 lying subscales were entered as predictor variables, with one of the four P2 lying subscales entered as the dependent variable in each analysis.

<table>
<thead>
<tr>
<th>Predictor Variables</th>
<th>Dependent Variables</th>
<th>P1 antisocial commission</th>
<th>P1 antisocial omission</th>
<th>P1 prosocial commission</th>
<th>P1 prosocial omission</th>
</tr>
</thead>
<tbody>
<tr>
<td>P2 antisocial commission</td>
<td>β=.163***</td>
<td>β=-.001</td>
<td>β=.014</td>
<td>β=-.026</td>
<td></td>
</tr>
<tr>
<td>P2 antisocial omission</td>
<td>β=.035</td>
<td>β=.112***</td>
<td>β=.032</td>
<td>β=.036</td>
<td></td>
</tr>
<tr>
<td>P2 prosocial commission</td>
<td>β=.014</td>
<td>β=.044</td>
<td>β=.116***</td>
<td>β=.001</td>
<td></td>
</tr>
<tr>
<td>P2 prosocial omission</td>
<td>β=.031</td>
<td>β=-.023</td>
<td>β=-.033</td>
<td>β=.042</td>
<td></td>
</tr>
</tbody>
</table>

***p<.001
We considered that one possible explanation for the observed relationships between pairs’ subscale scores could be that all participants answered the subscale questions similarly. To test this possibility, we shuffled participants’ data so that every participant was paired with another random participant from the full participant pool. We then re-ran our regression analyses and our replication analyses. Across these eight regression analyses, no subscale beta coefficients predicted the corresponding subscale scores at a liberal threshold of $p=.05$.

To test for whether the observed effects were significant in socially connected pairs who did not share genetic material, we next divided our sample according to whether individuals indicated that they were ($N=440$) or were not ($N=1,246$) biologically related (leaving out one pair where both participants indicated “I don’t know”). We re-ran the same series of regression analyses (and replication regression analyses) on each subsample. In general, our observed effects held across biologically and non-biologically related pairs (see Table 12). In the subsample of non-biologically related pairs, P2 subscale scores uniquely predicted P1 subscale scores for antisocial commission ($\beta=.180$, $p<.001$), antisocial omission ($\beta=.097$, $p=.002$), and prosocial commission ($\beta=.081$, $p=.01$); similar effects were observed in the replication regression analyses. Although beta values were somewhat higher for biologically related pairs (average difference of .056 between biologically related and non-biologically related subsamples), when we compared these values by entering biological relationship as a dummy variable, interacting it with the predictors and including these terms in the original regression analyses, we found that the differences in beta values were not significant. The only exception was for prosocial commission, where the beta value was significantly higher for biologically related pairs in the replication analysis ($p=.03$), but not in the original
analysis (p=.10). Overall, these results suggest that similarities in lying tendencies cannot be fully explained by genetic relatedness.

Table 12: Summary of Subscale Relatedness Across Different Participant Pair Relationships. Summary of subscales that were significantly related between participant pairs, across different participant pair relationships. For the shared nationality category, participants were randomly paired with another participant from their same country before performing the analyses. All effects were observed in both the original and replication analyses, except for those enclosed by square brackets, which were observed in one direction only (i.e. in either the original or replication analysis).

<table>
<thead>
<tr>
<th>Relationship</th>
<th>Pairs (N)</th>
<th>Antisocial commission</th>
<th>Antisocial omission</th>
<th>Prosocial commission</th>
<th>Prosocial omission</th>
</tr>
</thead>
<tbody>
<tr>
<td>All pairs</td>
<td>1,687</td>
<td>***</td>
<td>***</td>
<td>***</td>
<td></td>
</tr>
<tr>
<td>Biologically-related pairs</td>
<td>440</td>
<td>***</td>
<td>*</td>
<td>***</td>
<td></td>
</tr>
<tr>
<td>Non-biologically-related pairs</td>
<td>1,246</td>
<td>***</td>
<td>**</td>
<td>**</td>
<td></td>
</tr>
<tr>
<td>Parent-child</td>
<td>220</td>
<td>*</td>
<td>***</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Siblings</td>
<td>200</td>
<td>***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Spouses</td>
<td>436</td>
<td>**</td>
<td></td>
<td>[**]</td>
<td>[*]</td>
</tr>
<tr>
<td>Romantic partners (not married)</td>
<td>256</td>
<td>**</td>
<td></td>
<td>[**]</td>
<td></td>
</tr>
<tr>
<td>Friends</td>
<td>486</td>
<td>[*]</td>
<td>*</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Colleagues</td>
<td>54</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Other</td>
<td>35</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Shared nationality</td>
<td>1,235</td>
<td>***</td>
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</tbody>
</table>

5.4.1 Are Lying Tendencies Socially Transmitted?

Our second research question was whether social transmission was a plausible explanation of observed relationships in lying tendencies. Using the full sample of 1,687 pairs, we tested two potential moderators for the observed relationships: 1) how much time they spent with that person (indicated on a drop-down menu with six options ranging from less than 1 hour/week to 20+ hours/week), and 2) how close participants’
felt to the other person who completed the survey (indicated by selecting one of six diagrams of two circles that increasingly overlapped (Aron et al., 1992)). Responses were coded on ordinal scales ranging from 1 to 6.

To test for moderation, we followed the approach outlined by Baron and Kenny (R. M. Baron & Kenny, 1986), assuming that the effect of the predictor variables on the dependent variables would linearly increase with increasing levels of the moderators. We first centered all lying subscale and moderator variables of interest around 0 by subtracting the variable means from the individual values. We then computed interaction terms by multiplying each centered lying subscale variable with each centered moderator variable. Finally, for each moderation analysis, we regressed the lying subscale variable (e.g. centered P2 antisocial commission scores), the moderator variable (e.g. centered P2 relationship closeness), and the interaction variable (the product of centered P2 antisocial commission scores and centered P2 relationship closeness scores) on the dependent variable (in this case, P1 antisocial commission scores); if the interaction term was significant while controlling for the individual predictors this indicated moderation. We tested for moderating effects of relationship closeness and time spent together on the antisocial commission, prosocial commission, and antisocial omission relationships, regressing P2 variables on P1 variables and vice versa.

Following this procedure for time spent together, we observed a moderating effect on the antisocial commission relationship; examination of conditional effects revealed that the more time pairs spent together, the more strongly antisocial commission lying tendencies were related. This effect was significant for P2 antisocial commission scores predicting P1 antisocial commission scores (b=.028, 95% CIb [.002, .054], β=.050, p=.04), and for P2 scores predicting P1 scores (b=.028, 95% CIb [.006, .049],
\(\beta = .061, p = .01\). In contrast, time spent together did not moderate the antisocial omission or prosocial commission relationships (all \(p\)-values > .08). Though we cannot draw strong conclusions from correlational data, these findings are in line with a social transmission explanation of the observed relationships in antisocial commission lying tendencies.

The evidence for moderation of relationship closeness was less clear. We found relationship closeness to significantly moderate the regression of P2 antisocial commission scores on P1 antisocial commission scores (\(b = .037, 95\% \text{ CI} b \ [0.007, 0.068], \beta = .058, p = .02\)), but the reverse moderation was not significant (\(b = .004, 95\% \text{ CI} b [-.034, .042], \beta = .005, p = .83\)). In addition, we found that relationship closeness had a marginally significant moderating effect on the regression of P1 prosocial commission scores on P2 prosocial commission scores (\(b = .036, 95\% \text{ CI} b [.000, .072], \beta = .047, p = .052\)), though the reverse moderation was again not significant (\(b = .006, 95\% \text{ CI} b [-.025, .037], \beta = .009, p = .72\)). Relationship closeness did not moderate the antisocial omission relationships (\(p\)-values > .50).

### 5.4.2 Similarities in Lying Across Different Relationships

A question of further interest was whether relatedness of lying tendencies varies across different types of relationships. In our survey, we asked participants to indicate their relationship to the person they knew who also took the survey, from the following ten categories: parent, son/daughter, sibling, grandparent, other relative, friend, colleague, boyfriend/girlfriend/significant other, spouse, and other. We grouped parents and children into one category representing parent/child relationships. Due to few respondents indicating the grandparent, other relative, and other categories, we combined the three categories to one category, other, with 35 participant pairs. This left us with seven relationship categories. To explore similarities in lying tendencies for
specific relationships, we ran the same eight regression analyses (four original, and four replication) for participant pairs in each relationship category. Table 4 summarizes our results.

Analyses for biologically related pairs. We first ran the regression analyses on parent-child pairs. Parents and children (N=220) were significantly related in their tendencies to tell prosocial lies of commission (and, to a lesser degree, antisocial lies of omission), but were not significantly related in their tendencies to tell antisocial lies of commission. Cavalli-Sforza and colleagues distinguished between three types of cultural transmission (a broader construct encompassing social transmission): horizontal transmission, i.e. transmission individuals of the same generation; vertical transmission, i.e. transmission from parent to child; and oblique transmission, i.e. transmission from individuals of an older generation to individuals of a younger generation (Cavalli-Sforza et al., 1982). In a survey involving parents, children, and friends, these researchers found that religious and political attitudes were primarily transmitted vertically, that is from parents to children, whereas other traits were not. While for the majority of our relationship categories social transmission would be horizontal or possibly oblique, the parent-child category allowed us to explore whether lying tendencies are likely to spread via vertical transmission.

To test for vertical transmission, we looked at whether parents’ lying tendencies were more predictive of their children’s lying tendencies than vice versa. In order to maximize power, we first “flipped” the order of some pairs’ data so that all parents’ data were represented as P2 values and all children’s a data were represented as P1 values. We then ran our original regression analyses, first on parent-child pairs. We obtained parent subscale (P2) beta values for the matched subscales of their children (P1) and child subscale beta values (P1) for the matched subscales of their parents (P2), and then
computed the differences between these values. While the beta value was .047 units higher for parents’ scores predicting children’s scores than vice versa for antisocial omission, it was .015 units lower for prosocial commission. Overall, there was no a clear pattern of higher beta values in one direction, suggesting that, at least for the adult children in our parent-child pairs, lying tendencies were not predominantly learned through explicit passing down from parents.

In contrast to our parent-child pairs, siblings (N=200) were significantly related in their tendencies to tell antisocial lies of commission, but not in any other lying tendencies. We repeated the analysis described in the paragraph above for older and younger siblings (although this would be considered horizontal transmission by Cavalli-Sforza and colleagues’ definitions, we considered this relationship distinct from other types of horizontal transmission). The beta value was 0.044 units higher for older siblings’ scores predicting younger siblings’ scores than vice versa, suggesting that younger children may learn more from their older siblings than the other way around.

Taken together, the results of our parent-child and sibling analyses provide evidence for related lying tendencies among family members. However, the specific tendencies that are related appear to differ for parent-child and sibling pairs. Sibling pairs showed related antisocial commission lying tendencies, while parents and children showed related tendencies for antisocial omission and prosocial commission. One interesting possibility here is that children adopt the etiquette – and prosocial lying tendencies – of their parents. Evidence for younger family members learning from older family members is weak, but further research is needed to clarify whether or how lying tendencies are passed on through family lines.

Analyses for non-biologically related pairs. Among non-biologically related pairs, we observed related lying tendencies in spouses, in romantic partners, and in
friends. Spouses (N=436) and unmarried romantic partners (N=256) were both significantly related in their tendencies to tell antisocial lies of commission (ps<.01); the predictive relationships for prosocial lies of commission were significant in one direction (ps<.01) but not the other (ps>.07). Examining the regressions for friend pairs (N=466), P1 antisocial commission scores predicted P2 antisocial commission scores (p=.02) but the reverse relationship was not significant (p=.15). Antisocial omission scores were predictive in both directions at a significant threshold of p<.05. Finally, for colleagues (N=54), no beta values were significant for corresponding subscales scores; however, statistical power was compromised for colleagues due to the small sample size.

**5.4.3 Similarity in Lying at the Country Level**

Beyond examining the relatedness of lying behavior across various first-degree relationships, our dataset allowed us to ask whether similarity in lying tendencies extends to a whole society. We tested whether individuals from the same countries were related in their lying behaviors. Participants were grouped by country of citizenship, and then randomly shuffled within countries, so that each participant was paired with another random participant from his or her country. (If only one pair was from a particular country, or if both participants in a pair did not indicate the same country of citizenship, the pairs were excluded at this stage.) We then re-ran the eight regression analyses from Tables 2 and 3 on the remaining 1,235 shuffled-within-country participant pairs. The regression analysis showed a predictive relationship for antisocial lying by commission, in both directions: P2 participants’ antisocial lying scores predicted P1 participants’ antisocial lying scores (β=.120, p<.001), and vice versa (β=.115, p<.001). None of the other beta coefficients were significant in these regression analyses. Thus, similarity in antisocial lying tendencies appears to reach across a country, suggesting cultural norms for this type of verbal dishonesty.
5.4.4 Effects of Gender

Previous research has found that women are more likely to tell prosocial lies than men [3,11]. We examined whether lying tendencies differed between men and women by comparing the means for each type of lying between men and women using independent-samples t-tests. Consistent with previous findings, women (N=1,761) were more likely to tell prosocial lies of commission than men (N=1,436); this difference was significant for both P1 (mean difference=0.515, t(1519)=5.650, p<.001) and P2 (mean difference=0.710, t(1591)=7.999, p<.001) participant samples. There were no gender differences for any of the other types of lying, with the exception of P1 men reporting slightly higher likelihood of telling prosocial lies of omission than P1 women (mean difference=0.251, t(1519)=3.155, p=.002). P2 men and women did not differ in their prosocial lying by omission (p>.99).

In spite of a higher overall level of prosocial commission lying in women, prosocial commission lying was not related in female-only pairs (p-values>.10). Comparing relatedness among female-only (N=391) and male-only (N=230) pairs across the four subscales, antisocial omission tendencies were more strongly related in male-only pairs (t(614)=2.408, p=.02 in one direction, t(614)=2.068, p=.04 in other direction). The strengths of the relationships did not significantly differ between female-only and male-only pairs for the other lying tendencies.

5.5 Discussion

Using a large sample of paired individuals, we found that lying tendencies were related across socially connected pairs. Across all participants, we observed bi-directional predictive relationships for three out of four categories of lying, namely: antisocial commission, antisocial omission, and prosocial commission. Importantly, these relationships remained significant when we limited our analysis to pairs that were
not biologically related. We observed the strongest predictive relationships for antisocial commission lying tendencies; these relationships held for sibling, romantic, spousal, and (in one direction) friend pairs. Antisocial omission tendencies were significantly related in parent-child and friend pairs. Finally, prosocial lying tendencies were significantly related in parent-child pairs (in both directions), spousal pairs (in one direction), and romantic partner pairs (in one direction). Although women were more likely to tell prosocial lies of commission than men, as others have observed (Bella M DePaulo et al., 1996; Erat & Gneezy, 2012), we did not find evidence that women were more strongly related in their prosocial commission lying tendencies.

5.5.1 Why Similar Lying Tendencies?

We approached this study with the question of whether lying tendencies spread through social transmission. We found evidence that individuals from the same social networks are related in their lying tendencies, with particularly strong and consistent relationships for tendencies to tell antisocial lies of commission. Of course, if individuals within social networks are related in their tendencies to tell antisocial lies, prosocial lies, or both types of lies, social transmission is only one potential explanation. Causality could also apply in reverse; that is, individuals with similar lying tendencies may choose to affiliate or gravitate toward the same social contexts. For example, individuals with higher tendencies to tell antisocial lies may be drawn to similar pastimes, or may be drawn to one another as romantic partners. While selection effects would not apply biologically related family pairs, in these pairs, observed effects may be due to genetic predispositions rather than social influence. Furthermore, whether or not individuals are biologically related, similarity in lying tendencies could be explained by a third variable, such as personality profiles.
Further research is needed to illuminate the extent to which social transmission is a likely explanation for the observed similarities in lying tendencies. Evidence supporting social transmission was strongest for antisocial commission, as moderation analyses showed that the more time individuals spent together, the more strongly their antisocial commission lying tendencies were related. Furthermore, assuming that citizenship is relatively intransient, the finding that randomly paired citizens from the same country show related antisocial commission lying tendencies is difficult to explain with selection effects. However, social transmission, if it extends beyond local networks, could explain cultural standards of dishonesty (Cavalli-Sforza et al., 1982). Still, our findings are correlational, and represent only a first step in investigating social transmission of dishonesty. Experimental and/or longitudinal studies will help to establish the causes behind the observed patterns of lying tendencies among socially connected individuals.

Our findings suggest several additional avenues for future research. First, we found that parents and children showed a unique constellation of related lying tendencies, with the strongest similarity for prosocial commission. Although we did not find strong evidence that parents’ tendencies were more predictive of their children’s tendencies than vice versa, it is worth noting that the present sample consisted entirely of adults. Conceivably, the nature (and degree) of the relationships between parents’ and children’s lying tendencies may evolve as young children mature and gain independence from their parents, raising interesting questions for developmental or longitudinal research. The degree to which parent-child (and sibling) similarities reflect genetics vs. social transmission might also be explored. Second, given the observed (unidirectional) relationships for prosocial commission lying tendencies in individuals who were married or romantically involved, future research might also explore how
prosocial lying impacts relationship outcomes. Third, our data imply that tendencies to
tell lies of commission may be more coherent than tendencies to tell lies of omission.
Previous research has pointed to heterogeneity in whether acts of omission are
considered immoral (Ritov & Baron, 1990; Spranca et al., 1991), suggesting that intra-
individual variation in tendencies to lie by omission may be due in part to variation in
whether particular lies of omission are seen as immoral. However, more research is
needed to understand the relatedness and distinctions between dishonesty by
commission and omission.

5.5.2 Towards a Theory of Social Transmission

The pattern of results we observed across different relationships presents a
somewhat complicated picture of the potential transmission of lying. Antisocial
commission lying tendencies were significantly related across most, but not all
relationship pairs. In some cases, lying tendencies were related in one analysis (e.g. P1
scores predicting P2 scores) but not in the reverse analysis (e.g. P2 scores predicting P1
scores). Given that three of the four lying tendencies were significantly related in the
larger subsamples of biologically related and non-biologically related pairs, it is difficult
to say whether non-significant findings for specific relationship pairs reflect true null
effects or insufficient power for detecting true effects. However, we believe the results
of this study are ripe for generating testable theories regarding how and when social
transmission of dishonesty operates.

If different types of dishonesty indeed spread through social transmission,
experiments can help to shed light on the mechanics behind these processes. For
example, if social transmission occurs for antisocial lying, what role does
communication play in this process? Is face-to-face communication necessary for social
transmission, or can it also occur through online social networking mediums? To
address such questions experimentally, researchers might instill prosocial or antisocial values in certain individuals within lab-based social networks, manipulate the nature of communication that is possible these social networks, and measure the adoption of instilled values by others in the networks.

The question of how dishonesty spreads through social networks is relevant to relationships, organizations, and society at large. Individuals may not consider that their own minor lies contribute to a broader culture of dishonesty. Our findings are noteworthy given that honesty has been identified as a universal value (S. H. Schwartz, 2007; Shalom H. Schwartz, 1994). To understand how social and cultural standards for dishonesty may form in spite of the universal moral of truthfulness, we point to Cialdini and colleagues’ important distinction between two types of social norms (Cialdini, Kallgren, & Reno, 1991). Injunctive norms refer to actions that people generally approve of, while descriptive norms refer to actions that people generally engage in. The results of the present study indicate that descriptive norms for dishonesty can vary even as the injunctive norm for honesty remains constant (Francesca Gino et al., 2009). Thus, if societies are to truly uphold the virtue of honesty, individuals will need to pull together to expose lies when they occur, and prevent them from quietly weaving themselves into the social fabric.
6. Dishonesty Across the East/West Divide

6.1 Overview

In four chapters, we have seen evidence for cross-cultural similarity in dishonesty, as well as evidence for cultural variation. I examined how context affects behavior by comparing participant samples from different countries (Chapters 2-4) and by testing for similar tendencies across individuals from the same social networks (Chapter 5). This final chapter examines the effects of socio-political regime on de-contextualized dishonest behavior, capitalizing on a natural experiment in Germany. From 1961-1989, the Berlin Wall divided the country into two socio-political regions, a poor socialist regime in the East, and a wealthy capitalist regime in the West. We administered a behavioral dishonesty task to Germans of differing backgrounds in citizen centers of Berlin. The experiment used a similar die-rolling task to Chapter 2, but with a physical die and minor modifications. We found that individuals with an East German family background cheated significantly more than those with a West German family background. Furthermore, the longer individuals were exposed to socialism, the more likely they were to cheat on our task. This evidence suggests that socio-political regime may in some cases impact core dishonesty, either directly or through post-reunification effects of social comparison.

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6.2 Introduction

Does the economic and political system affect people’s honesty? A recent article suggests that market economies decay morals, based on a clever experiment where participants faced a tradeoff of saving a mouse or receiving money (Falk & Szech, 2013). However, this study compared decisions in bilateral and multilateral market settings to individual decisions rather than an alternative economic allocation mechanism. Demsetz (1969) was the first to point out that the comparison of real institutions with an idealized arrangement constitutes a nirvana fallacy, and that it is more useful to compare existing institutional arrangements with one another. To understand how experience with real economic systems influences honesty, we compare dishonest behavior between East Germans, who were exposed to socialism for over 40 years, and West Germans, who were at the same time living in a social market economy.

While Falk and Szech (2013) found that market interactions erode moral values, we hypothesize that socialism might have an even more detrimental effect on human behavior. Socialist systems have been characterized by extensive scarcity, which ultimately led to the collapse of the German Democratic Republic (GDR) in East Germany. In many instances, socialism pressured or forced people to work around official laws. For instance, in East Germany stealing a load of building materials in order to trade it for a television set might have been the only way for a person delivering building supplies to connect to the outside world. Moreover, socialist systems have been characterized by a high degree of infiltration by the intelligence apparatus. In East Germany, the secret service (Staatssicherheit) kept records on more than one third of the citizens (Koehler, 1999). Unlike in democratic societies, freedom of speech did not represent a virtue in socialist regimes and it was therefore often necessary to misrepresent your thoughts to avoid repressions from the regime. All of these factors
suggest that comparing the West German social market economy with the so-called ‘really existing socialism’ in East Germany might reveal that under socialism, morals decay even more.

Earlier studies have shown differing degrees of national solidarity between East and West Germans. In a laboratory experiment with economics students, Ockenfels and Weimann (1999) found that East Germans showed significantly less solidarity five years after the German reunification. When they asked Germans how much money they would be willing to hand over to anonymous future losers if they won 10 Deutsche Mark in a solidarity game, East Germans were willing to give up roughly half as much as West Germans. Interestingly, East Germans also expected to receive much less from potential winners. These results were recently confirmed by Brosig-Koch, Helbach, Ockenfels, and Weimann (2011), who showed that there was no convergence in solidarity 20 years after the German reunification, which they attribute to high levels of interpersonal coordination required to shift societal norms.

Using data from the German Socioeconomic Panel (GSOEP), Alesina and Fuchs-Schündeln (2007) provide evidence that East Germans have stronger preferences for public policies that involve redistribution. They found that individual preferences are deeply shaped by the economic and political regime and converge only slowly. According to their estimates, one fourth of this effect is due to the fact that East Germans became poorer during the socialist epoch, while the remainder can be attributed to the impact of socialism on individual preferences itself. One limitation of the study by Alesina and Fuchs-Schündeln (2007) is that people might distort their true preferences when responding to a survey like GSOEP. For instance, people might overstate their willingness to contribute to redistributive policies because they do not actually have to pay for them. Using a discrete choice experiment, Pfarr, Schmid, and Volker (2013) find
that the stated preferences of East Germans towards redistribution indeed differ from their revealed preferences. While East Germans indicate to prefer higher degrees of redistribution, they are not willing to pay for such policies.

Another study by Heineck and Süßmuth (2013) also used the GSOEP data and investigated the effect of the economic and political regime on individuals’ trust and risk preferences as well as their cooperativeness. East Germans showed persistently lower levels of social trust and were less inclined to see others as fair. Their study also suggested that East Germans are more risk-loving. Most importantly, Heineck and Süßmuth (2013) find that these cultural traits appear to carry down through generations.

Little is known about how the economic and political system influences individual honesty. Torgler (2003) found that East Germans were more likely than West Germans to say that cheating on tax can be justified, although this difference disappeared 7 years after the German reunification. While taxation is a specific domain that is shaped by social norms and legal rules, here we are interested in whether East and West Germans differ in their honesty on an abstract task. A recent study by Abeler, Becker, and Falk (2012) provided 658 Germans with an opportunity to earn 15 EUR by misreporting the side of a coin toss on the telephone. The test did not reveal any cheating behavior among the subjects. Doubting the apparent honesty of Germans, we designed a study to test whether Germans are indeed perfectly honest and whether the economic and political system has a significant impact on people’s willingness to cheat for personal gain in an abstract task. In a second step, we tested whether the economic and political system has a significant impact on people’s willingness to donate to a charitable cause, bearing in mind that donations might provide a specific method of
moral cleansing after misconduct like cheating (see Ayal & Gino, 2011; Lee & Schwarz, 2010; Zhong & Liljenquist, 2006).

The remainder of the paper is structured as follows. The next section outlines our theory and hypothesis. In section 3, we present our sampling strategy and the experimental setup. Section 4 provides descriptive statistics and the empirical results of our econometric models. Section 5 provides a discussion and concludes.

6.2.1 Theory and Hypothesis

Standard economic theory focuses on external incentives and disincentives for dishonesty, including the personal gain, probability of punishment, and severity of punishment (Becker, 1968). Based on the rational actor model, this theory claims that when penalties for dishonesty are absent, people should cheat to the maximal degree. However, in contrast to this hypothesis, a growing body of behavioral research finds that individual cheating is limited even when external penalties are absent (Gino, Ayal, & Ariely, 2009; Jiang, 2013; John, Loewenstein, & Rick, 2013; Mazar, Amir, & Ariely, 2008). Based on such findings, Mazar and colleagues (2008) proposed that in addition to being motivated by external incentives, people are motivated by the desire to maintain an honest self-concept, which explains why they limit dishonesty in experimental contexts where they could earn more money by cheating.

We posit that socialism extends the threshold below which average people can cheat and maintain a positive self-concept as being an honest and upright individual. This may be because the political and economic environment under socialism forced individuals to sidestep the legal and social norms more frequently to live a decent life. Over the course of time, citizens of the former GDR may have come to standardize dishonest behaviors more deeply than their West German neighbors. For example, in certain situations (unsuspicious in the Federal Republic of Germany) it might have been
necessary for citizens of the former GDR to tell a lie when standing in front of a supposedly member of the secret policy in order to avoid getting arrested. In the economic domain, cheating at work might have been the only way to fulfill the annual plan set forth by the responsible government agency (*Staatliche Plankommission*). While such behavior in principle could exist under capitalism as well, it was likely more inevitable in a system that worked through coercion in many areas of live and was characterized through extensive economic scarcity. If the socio-political context has a lasting impact on individuals’ behavior, we should expect to see differences in honesty carrying forward after the German reunification. As a result, we hypothesize that individuals from East Germany will cheat more than individuals from West Germany on an abstract behavioral task.

### 6.3 Method

#### 6.3.1 Sampling

The experiment was run in the course of five days, from December 2, 2013 through December 6, 2013. We drew a random sample of 259 German citizens who collected their passport or ID card from the Berlin citizen centers (the so-called *Bürgeramt*). Two experimenters, one male and one female with East and West German family backgrounds, respectively, conducted the experiment. Experimenters operated independently and approached subjects as they came in the room and sat down. We ran the study in Berlin to minimize any effects of regional variations, such as income differences and local attitudes. We conducted the experiment in six different administrative regions and nine different citizen centers, including regions directly at the former border of the Berlin Wall (Neukölln, Mitte and Pankow) and regions in the East and West Berlin hinterlands (Zehlendorf, Steglitz and Weissensee).
According to Article 1 of the German ID Card Law (*Personalausweisgesetz*), every German is required to possess and show upon request a passport or ID card from the age of sixteen. Passports and ID cards are valid for 10 years and have to be renewed thereafter. A fine of up to 5,000 EUR can be imposed in case of non-compliance. Hence, even Germans who do not need a passport or ID card for travel purposes have a strong incentive to possess at least one of the two documents. Furthermore, passports and ID cards have to be collected by the applicant itself. A proxy person can collect the documents only in narrowly defined exceptional cases.\(^2\) This set of requirements gives us some confidence that the people collecting their passport or ID card at the citizen center represent a random sample of the population of Berlin. In terms of the composition of our participant sample, only slightly more than half of the 134 subjects who reported being born in West Germany were born in West Berlin, while 65 subjects had moved to Berlin from other parts of West Germany. From the 97 subjects born in East Germany, only 39 were born in East Berlin, while 58 had moved to Berlin from other parts of East Germany. These demographic characteristics suggest that our results might apply not only to the city of Berlin, but also to East and West Germany more generally. Finally, 24 now German citizens were born outside of Germany and four subjects did not provide their place of birth. Table 1 provides descriptive statistics on demographics and our main explanatory variables for East and West Germans, respectively.

\(^2\) For details see: http://service.berlin.de/dienstleistung/324325/
6.3.2 Experimental Setup

In this natural experiment, subjects were assigned into two groups by the Berlin Wall, which separated East and West Germany from its erection on August 13, 1961 until its fall on November 9, 1989. During this period, almost no West Germans migrated to East Germany, and only around 600,000 East Germans managed to migrate to West Germany, with the remaining 16.4 million confined to East Germany until the fall of the Wall (Heineck and Süßmuth, 2013). Given this relatively small level of migration, self-selection among East and West Germans is likely not a serious issue in our experiment (see Pfarr et al., 2013). Importantly, because Berlin citizen centers issue passports and ID cards to any citizen of Berlin, experimenters were blind with regard to the background of the subjects they were running the experiment on.
After agreeing to participate, each subject received an envelope with six single 1 EUR coins, the maximal possible payout on the die task we used to measure cheating. Subjects were then asked to throw a physical die 40 times. To measure cheating, subjects were instructed to decide on one side of the die—top or bottom—in their mind, and to memorize their decision before rolling the die. They then threw the die and observed the outcome. Subjects were next instructed to record this outcome on a sheet of paper. Each time they threw the die, subjects could cheat by claiming that they chose the side of the die leading to the higher payout (by reporting the side of the die with the larger number of dots = high payout). The payout that subjects ultimately received was determined by selecting one of their rolls at random, by having the experimenter draw a number from 1 to 40 out of an envelope. Subjects earned 1 EUR for each dot on that particular roll. If subjects were completely honest, they would be expected to report deciding on the high side of the die in 50 percent of cases, and the expected value of the average payout would be 3.50 EUR.

Our experiment combines two previously implemented procedures where subjects were paid based on the number of dots on reported die rolls (Fischbacher & Heusi, 2008; Jiang, 2013; Mann, Garcia-Rada, Hornuf, Tafurt, & Ariely, 2015). Jiang (2013) designed a die-roll cheating task where subjects reported choosing one side of the die, top or bottom, on a computer screen over multiple trials. Subjects were told to mentally select a side of the die before each roll, but recorded their choice only after seeing the outcome. In the experiment by Fischbacher and Heusi (2008), subjects were asked to roll a physical die and memorize the first roll. They were further instructed to roll the die another couple of times to test whether the die was fair, but to memorize only the first roll and report it later on a computer screen. In this setting, cheating was revealed if subjects reported a number different from the outcome of the first roll. In our
experiment, subjects could cheat in two ways: by misreporting the side they had previously memorized and by making up a roll outcome altogether. The latter is identified if the combined frequency of any two sides of the die (3-4, 2-5 and 1-6) significantly deviates from 1/3.

After the cheating task, we informed subjects that they would have an opportunity to donate all or parts of their earnings to a hospital. As they came into the citizen center, subjects were randomly allocated into one of two conditions. In condition 1, subjects could donate all or part of their earnings to a hospital in Leipzig / East Germany. In condition 2, they could donate all or part of their earnings to a hospital in Hannover / West Germany. Both hospitals are publically funded and are located in cities of comparable size and with similar average incomes.\(^3\) To avoid any influence of the experimenters on the donations, we handed a small card with some information about the donation opportunity to the subjects and let them privately donate using another envelope that they sealed and left at their clipboard. Appendix B contains an English translation of the donation treatments. Similar to the study by Pfarr and colleagues (2013), the cheating task and the donation opportunity allowed us to analyze the revealed preferences of subjects.

Finally, we invited subjects to take part in a survey. It was only at this point that subjects indicated their East or West German background, by answering the following questions: “In which city were you born?”, “Where did you live in the 1980s?”, “Where did you live in the 1990s?”, “What is your family background?”, “Where are you currently living?” and “How do you consider yourself?”. While responses to these questions overlapped to a great extent, the overlap was not perfect.

\(^3\) Leipzig had 520,838 inhabitants in 2012 and employees earned an average wage of 31,766 EUR in the year 2010; Hannover had 514,137 inhabitants in 2012 and employees earned an average wage of 33,157 EUR in the year 2010 (Volkswirtschaftliche Gesamtrechnung der Länder: Berechnungsstand August 2012/Februar 2013).
For our main analysis, we consider family background to be our most appropriate measure of East or West German heritage. In response to the self-identification question, 84 percent of participants indicated that they neither consider themselves as East or West German, but merely as German. Place of living does not directly disclose how strongly subjects were exposed to socialism or capitalism, as they could have moved to one region or another in the 1990s or from anywhere in the world to West Germany before 1989. Another problem with the place of birth and place of living measures is that some people might not have a German family background at all. For instance, many young Turks were born in West Germany, but do not have an East or West German family background. The following analyses are therefore reported on the family background dimension, which measures the self-perceived cultural heritage of subjects.

Overall, 90 subjects reported having an East German family background and 98 subjects having a West German family background. Five subjects who reported an East German family background were born in West Germany, thus indicating that their families migrated to West Germany at some point in time. One subject who reported a West German family background was born in East Germany.⁴

6.4 Results

6.4.1 Cheating

Since we were using a physical cup and a die in the cheating task, subjects could potentially cheat on two dimensions. First, subjects could have reported the high side when they had in fact chose in advance the side of the die that turned out to have the lower-paying outcome. For example, if a subject chose top and after rolling the die the

⁴ Since she lived in West Germany throughout the 1980s and 1990s, had a West German family background and considered herself as West German, we did not exclude her from the sample and recognized her West German family background and attributed her East German place of birth in 1944 to the turmoil of WWII.
top side showed 2 dots, she could have cheated by reporting 5 dots (shown on the bottom side) instead. Second, subjects could have simply made up any roll outcome. For example, if a subject chose top and after rolling the die the top side showed 2 dots, although instructed differently, she could have cheated by reporting 6 instead. This second type of cheating is revealed if the combined frequency of any two sides of the die (3-4, 2-5 and 1-6) significantly deviates from 1/3.

Our results reveal that on average both groups cheated on the first dimension. West Germans reported 55 percent high rolls on average, while East Germans reported on average 60 percent high rolls, which are both significantly different from the pure chance outcome of a fair die on a simple t-test (p<0.01). Moreover, East Germans cheated significantly more than West Germans (p=0.01). West Germans rolled on average 3.68 dots, while East Germans rolled on average 3.83 dots (p=0.02). Since the scale of possible cheating ranges from 50 percent high rolls to 100 percent high rolls, cheating by West Germans corresponds to 10 percent and cheating by East Germans to 20 percent of what had been feasible. Thus, East Germans cheated twice as much as West Germans overall. These results are also significant on a non-parametric Mann-Whitney U test (p=0.02). When considering the dots that contributed more or less to the overall difference in cheating, East Germans reported on average 4, 5 and 6 dots more frequently than West Germans. However, East Germans mainly differed from West Germans by reporting 5 dots more frequently (p=0.01).
### Table 14: Dependent variable summary statistics

<table>
<thead>
<tr>
<th>Family background</th>
<th>Difference East / West</th>
<th>t-test based on all rolls</th>
<th>repeated measures</th>
<th>Difference East / West</th>
<th>t-test based on all rolls</th>
<th>repeated measures</th>
<th>average cheating in percent</th>
<th>N (subjects)</th>
<th>N (rolls)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>percentage</td>
<td>SD</td>
<td></td>
<td>mean roll</td>
<td>SD</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>East German</strong></td>
<td>0.60</td>
<td>0.49</td>
<td><strong>0.05</strong></td>
<td>0.05</td>
<td>3.83</td>
<td>1.68</td>
<td>0.15</td>
<td>0.15</td>
<td>0.15</td>
</tr>
<tr>
<td><strong>West German</strong></td>
<td>0.55</td>
<td>0.50</td>
<td><em>(p=0.000)</em></td>
<td><strong>0.013</strong></td>
<td>3.68</td>
<td>1.71</td>
<td><em>(p=0.022)</em></td>
<td><em>(p=0.022)</em></td>
<td><em>(p=0.022)</em></td>
</tr>
<tr>
<td><strong>German</strong></td>
<td>0.52</td>
<td>0.50</td>
<td></td>
<td>3.57</td>
<td>1.73</td>
<td></td>
<td>4% 37 1480</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Others</strong></td>
<td>0.60</td>
<td>0.49</td>
<td></td>
<td>3.85</td>
<td>1.68</td>
<td></td>
<td>20% 30 1200</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Consider yourself</strong></td>
<td>0.60</td>
<td>0.49</td>
<td><strong>0.02</strong></td>
<td>0.02</td>
<td>3.85</td>
<td>1.70</td>
<td>0.03</td>
<td>0.03</td>
<td>0.03</td>
</tr>
<tr>
<td><strong>East German</strong></td>
<td>0.58</td>
<td>0.49</td>
<td><em>(p=0.000)</em></td>
<td><strong>0.516</strong></td>
<td>3.82</td>
<td>1.65</td>
<td><em>(p=0.724)</em></td>
<td><em>(p=0.725)</em></td>
<td><em>(p=0.725)</em></td>
</tr>
<tr>
<td><strong>West German</strong></td>
<td>0.56</td>
<td>0.50</td>
<td></td>
<td>3.70</td>
<td>1.71</td>
<td></td>
<td>12% 141 5639</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>German</strong></td>
<td>0.56</td>
<td>0.50</td>
<td></td>
<td>3.71</td>
<td>1.69</td>
<td></td>
<td>12% 40 1600</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Others</strong></td>
<td>0.59</td>
<td>0.49</td>
<td><strong>0.03</strong></td>
<td>0.03</td>
<td>3.83</td>
<td>1.67</td>
<td><strong>0.14</strong></td>
<td><strong>0.14</strong></td>
<td><strong>0.14</strong></td>
</tr>
<tr>
<td><strong>East / West German</strong></td>
<td>0.56</td>
<td>0.50</td>
<td><em>(p=0.000)</em></td>
<td><strong>0.132</strong></td>
<td>3.70</td>
<td>1.71</td>
<td><em>(p=0.049)</em></td>
<td><em>(p=0.050)</em></td>
<td><em>(p=0.050)</em></td>
</tr>
<tr>
<td><strong>German</strong></td>
<td>0.56</td>
<td>0.50</td>
<td><em>(p=0.000)</em></td>
<td><strong>0.141</strong></td>
<td>3.70</td>
<td>1.71</td>
<td><em>(p=0.000)</em></td>
<td><em>(p=0.049)</em></td>
<td><em>(p=0.050)</em></td>
</tr>
<tr>
<td><strong>Place of birth</strong></td>
<td>0.59</td>
<td>0.49</td>
<td><strong>0.03</strong></td>
<td>0.03</td>
<td>3.79</td>
<td>1.68</td>
<td>0.08</td>
<td>0.08</td>
<td>0.08</td>
</tr>
<tr>
<td><strong>East German</strong></td>
<td>0.56</td>
<td>0.50</td>
<td><em>(p=0.000)</em></td>
<td><strong>0.118</strong></td>
<td>3.71</td>
<td>1.72</td>
<td><em>(p=0.024)</em></td>
<td><em>(p=0.195)</em></td>
<td><em>(p=0.198)</em></td>
</tr>
<tr>
<td><strong>West Germany</strong></td>
<td>0.59</td>
<td>0.49</td>
<td><strong>0.03</strong></td>
<td><strong>0.09</strong></td>
<td>3.79</td>
<td>1.69</td>
<td><strong>0.09</strong></td>
<td><strong>0.09</strong></td>
<td><strong>0.09</strong></td>
</tr>
<tr>
<td><strong>Living in the 1980s</strong></td>
<td>0.59</td>
<td>0.49</td>
<td><strong>0.03</strong></td>
<td><strong>0.09</strong></td>
<td>3.79</td>
<td>1.69</td>
<td><em>(p=0.026)</em></td>
<td><em>(p=0.214)</em></td>
<td><em>(p=0.214)</em></td>
</tr>
<tr>
<td><strong>East German</strong></td>
<td>0.56</td>
<td>0.50</td>
<td><em>(p=0.000)</em></td>
<td><strong>0.100</strong></td>
<td>3.70</td>
<td>1.70</td>
<td><em>(p=0.026)</em></td>
<td><em>(p=0.214)</em></td>
<td><em>(p=0.214)</em></td>
</tr>
<tr>
<td><strong>West Germany</strong></td>
<td>0.55</td>
<td>0.50</td>
<td><em>(p=0.058)</em></td>
<td><strong>0.058</strong></td>
<td>3.69</td>
<td>1.71</td>
<td><em>(p=0.002)</em></td>
<td><em>(p=0.082)</em></td>
<td><em>(p=0.083)</em></td>
</tr>
<tr>
<td><strong>Living in the 1990s</strong></td>
<td>0.59</td>
<td>0.49</td>
<td><strong>0.04</strong></td>
<td><strong>0.04</strong></td>
<td>3.80</td>
<td>1.69</td>
<td><strong>0.11</strong></td>
<td><strong>0.11</strong></td>
<td><strong>0.11</strong></td>
</tr>
<tr>
<td><strong>East German</strong></td>
<td>0.55</td>
<td>0.50</td>
<td><em>(p=0.000)</em></td>
<td><strong>0.058</strong></td>
<td>3.69</td>
<td>1.71</td>
<td><em>(p=0.002)</em></td>
<td><em>(p=0.082)</em></td>
<td><em>(p=0.083)</em></td>
</tr>
</tbody>
</table>
If people were cheating on the second dimension by simply making up rolls, we could detect this type of cheating if the combined numbers of rolls with 3 or 4 dots, rolls with 2 or 5 dots, and rolls with 1 or 6 dots were not equally distributed. Using the combined rolls enabled us to control for the first dimension of cheating, while at the same time identifying the second dimension. For instance, if subjects reported 6 dots when the outcome was in fact 1 dot, the combined rolls would still have the same probability of occurring, namely $\frac{1}{3}$. On the other hand, if the combined rolls of 6 and 1 were reported more often than the combined rolls of 5 and 2 or 4 and 3, this could only be attributed to subjects making up rolls (presumably 6 dots). The distribution of rolls suggests that, if at all, subjects invented rolls only on rare occasions. Rolls with 3 or 4 dots combined appeared only slightly less frequently than rolls with 1 or 6 dots combined. This effect was marginally significant ($p<0.07$). Importantly, East and West Germans did not differ on this second dimension of cheating (see Figure 9).
In order to control for confounding variables, we ran Probit regressions with high or low rolls as the dependent variable. We used the random effects estimator to account for clustering of specific effects within individuals.

We collected individual demographic data on several of variables of interest to include in our analyses. According to previous research, men are significantly more likely than women to lie to secure a monetary benefit (Dreber & Johannesson, 2008),
while women are more likely to engage in pro-social lying (DePaulo et al., 1996; Erat and Gneezy, 2011). In a classic study of morality, education was related to lower levels of cheating (Hartshorne & May, 1928). To capture the effect of economic well being, we asked subjects to report their subjective standard of living on a 6-item scale ranging from poor to very well off. Intentionally, we did not ask subjects for their precise household income, as such a variable might be an imprecise measure of perceived wealth, which depends on the social environment that historically differed for East and West Germans. Marital status might also affect individual cheating behavior, although to our knowledge this relationship has not yet been explored. Heineck and Süßmuth (2013) found that of socialism in East Germany led to lower levels of trust. As a lack of trust might promote individual cheating, we included a trust question from the World Value Survey (World Values Survey, 2010-2014). We also included a dummy variable indicating whether subjects believed that East Germans have been betrayed by West Germans after the reunification of Germany, as being treated unfairly by others was shown to positively influence individual cheating behavior (Houser, Vetter, & Winter, 2012b). Altogether, our Probit models included variables for age, gender, education, political orientation, living standard, marital status, trust in East and West Germans, and beliefs that West Germans cheated the East.

Table 3 shows the results of our initial Probit regressions. To allow for an intuitive interpretation, we report marginal effects at the means of the independent variables. The estimates reveal that when we control for other potential predictors of cheating, an East German family background increases the probability that subjects reported the high side by 18 percent (p=0.03). Age also has a significant impact on cheating. At the age of 39, being an additional year older increases the probability of subjects reporting the high side by 0.7 percent (p<0.01). Moreover, in line with the
findings of Hartshorne and May (1928) higher levels of education reduce the degree of cheating. If subjects had obtained a Master’s degree as compared to a Bachelor’s degree, this reduces the probability of subjects reporting the high side by 6 percent (p=0.03).

None of the other explanatory variables have a statistically significant effect on cheating. Since the variable marital status can only be measured on the nominal level, we define a different model with specific marital categories. Our results remain largely unaffected by this change and no specific marital category has a significant effect on cheating.5

Table 15: Probit-regressions. Subject random effects, n(subjects)=123, n(rolls)=4920, p(χ²)<0.001.

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>High roll</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Marginal</td>
<td>mean</td>
<td>p-value</td>
<td>Marginal</td>
</tr>
<tr>
<td>East German Family</td>
<td>0.176</td>
<td>0.5</td>
<td>0.05</td>
<td>0.187</td>
</tr>
<tr>
<td>Age</td>
<td>0.007</td>
<td>39.5</td>
<td>0.043</td>
<td>0.008</td>
</tr>
<tr>
<td>Female</td>
<td>0.037</td>
<td>0.4</td>
<td>0.649</td>
<td>0.035</td>
</tr>
<tr>
<td>Trust in East Germans</td>
<td>0.039</td>
<td>2.1</td>
<td>0.142</td>
<td>0.041</td>
</tr>
<tr>
<td>Trust in West Germans</td>
<td>-0.039</td>
<td>2.6</td>
<td>0.077</td>
<td>-0.042</td>
</tr>
<tr>
<td>West Germans Cheated</td>
<td>-0.012</td>
<td>0.3</td>
<td>0.746</td>
<td>-0.060</td>
</tr>
<tr>
<td>Education</td>
<td>-0.061</td>
<td>3.9</td>
<td>0.038</td>
<td>-0.060</td>
</tr>
<tr>
<td>Political left-right</td>
<td>0.007</td>
<td>3.4</td>
<td>0.734</td>
<td>0.009</td>
</tr>
<tr>
<td>Living standard</td>
<td>-0.020</td>
<td>2.8</td>
<td>0.639</td>
<td>-0.030</td>
</tr>
<tr>
<td>Marital status</td>
<td>-0.148</td>
<td>2.2</td>
<td>0.091</td>
<td>-0.076</td>
</tr>
<tr>
<td>Relationship</td>
<td></td>
<td></td>
<td></td>
<td>-0.131</td>
</tr>
<tr>
<td>Married</td>
<td></td>
<td></td>
<td></td>
<td>-0.112</td>
</tr>
</tbody>
</table>

If socialism has an effect on individuals’ honesty, we would expect people who have had greater exposure to socialism to cheat more. We therefore analyze whether East Germans who were at least 20 years old (born before 1970) when the Berlin Wall came down show higher levels of cheating as compared to subjects who were less than 10 years old at the time (born before 1980), or those who were simply born before the dissolution of the GDR. As an interaction term of age and family background is not

5 We compared the martial status of being in a relationship, married and divorced to the remaining four categories pooled together as they all refer to people living by themselves and were reported by very few subjects (except for being single).
informative in a non-linear model like Probit (Greene, 2010; Ai & Norton, 2003), we decided to investigate the exposure to socialism by examining these distinct age cohorts separately. In line with the theory that exposure to socialism impacts dishonesty, the results showed that differences in cheating are greater in older cohorts. While in the overall sample East German subjects were 19 percent more likely to report the high side of the die than their West German counterparts, subjects who lived less than 10 years in socialism were 28 percent more likely, and subjects who lived for 20 years or more in socialism were 65 percent more likely.

Table 16: Probit-regressions. Subject random effects, p(\(\chi^2\))<0.001.

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>High roll (\text{(born before 1989, n=110)})</th>
<th>High roll (\text{(born before 1980, n=67)})</th>
<th>High roll (\text{(born before 1970, n=41)})</th>
</tr>
</thead>
<tbody>
<tr>
<td>East German Family</td>
<td>0.185 0.034</td>
<td>0.284 0.033</td>
<td>0.650 0.001</td>
</tr>
<tr>
<td>Age</td>
<td>0.008 0.036</td>
<td>0.009 0.179</td>
<td>- 0.011 0.345</td>
</tr>
<tr>
<td>Female</td>
<td>0.034 0.706</td>
<td>0.072 0.603</td>
<td>0.223 0.275</td>
</tr>
<tr>
<td>Trust in East Germans</td>
<td>0.033 0.264</td>
<td>0.026 0.509</td>
<td>0.032 0.600</td>
</tr>
<tr>
<td>Trust in West Germans</td>
<td>-0.038 0.104</td>
<td>-0.033 0.332</td>
<td>-0.035 0.501</td>
</tr>
<tr>
<td>West Germans Cheated</td>
<td>-0.142 0.122</td>
<td>-0.255 0.049</td>
<td>-0.035 0.070</td>
</tr>
<tr>
<td>Education</td>
<td>-0.054 0.005</td>
<td>-0.015 0.008</td>
<td>-0.076 0.201</td>
</tr>
<tr>
<td>Political left-right spectrum</td>
<td>0.009 0.086</td>
<td>0.022 0.499</td>
<td>0.037 0.380</td>
</tr>
<tr>
<td>Living standard</td>
<td>-0.009 0.681</td>
<td>-0.088 0.189</td>
<td>-0.113 0.244</td>
</tr>
<tr>
<td>Marital status</td>
<td>-0.010 0.583</td>
<td>-0.020 0.716</td>
<td>-0.166 0.023</td>
</tr>
</tbody>
</table>

Lastly, we considered that subjects might change their cheating behavior over time as they roll the die over and over. Conceivably, the higher level of cheating observed in East Germans could be due to their adapting to the task and cheating more over time even if their initial dishonesty was identical. To test whether such dynamics drive our results, we investigate the average rolls as well as the percentage of high rolls individuals reported for each of the 40 rolls. While we again detect a higher level of cheating for East Germans, we do not observe a significant autocorrelation either overall.

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8 In unreported regressions, which we specified as in Table 3 but included an additional interaction term of age and family background, we found the interaction of an East German family background with an age of being at least 20 years or older in 1989 to be the only significant predictor of cheating behavior.
or in any of the two subgroups. Moreover, when we split the sample in half, the amount of cheating in the first and second half of the task is the same, i.e. there is neither an increase nor decrease in cheating behavior while the task was conducted (p=0.68).

Figure 10: Dynamics of cheating behavior over 40 rolls

6.4.2 Donations

Ockenfels and Weimann (1999) were among the first to investigate how social norms are shaped by the economic and political systems of East and West Germany, by looking at the differences in solidarity between East and West German students in a laboratory experiment. In this experiment, students played a solidarity game, developed
by Selten and Ockenfels (1998), in which they could hand over parts of their potential winnings to prospective losers. The study reported that West German students handed significantly higher amounts to anonymous losers than East German students. However, West German students also expected significantly higher amounts from anonymous winner than East German students. Alesina and Fuchs-Schündeln (2007) relied on survey data from the GSOEP and found that East Germans stated stronger preferences for public policies that entail some form of redistribution. While Alesina and Fuchs-Schündeln (2007) calculated that political values would converge 20 to 40 years after the German reunification, Brosig-Koch and colleagues (2011) found evidence that social behavior changes much more slowly, which may be due to the intergenerational transmission of behavioral norms.

The donation part of our experiment contributes to the previous literature in multiple ways. First, we investigate the decision of subjects from the general public instead of a student sample, who might have specific preferences for redistribution due to their lower income or specific political orientation. In another study, we found that German students cheat more than the German general public, which might reflect their stronger economic need to cheat (Mann et al., 2015). Second, in our study we had subjects from the general public actually reveal their preferences for redistribution by having them donate instead of reporting their redistribution preferences in a survey. Previous studies by Pfarr and colleagues (2013) have shown that East Germans desire higher levels of redistribution but are at the same time not willing to equally contribute to the financing through taxation. Third, we provide a more refined analysis of preferences for redistribution by controlling for the purpose of the donation, i.e. supporting a hospital in East or West Germany.
We first compare the overall donations by East and West Germans. Both groups donated a considerable and rather similar share of their earnings. East Germans donated on average 72 percent of their earning (2.47 EUR), while West Germans donated 70 percent (2.39 EUR). The difference is statistically not significant (p=0.75). East Germans donated somewhat more to the hospital in West Germany, although this difference is again statistically not significant (p=0.66). Interestingly, despite increasing sentiments against the solidarity tax (Solidaritätszuschlag) as well as the equalization payments on the federal level (Solidarpakt I and II), which East and West Germans have been paying for over 20 years to reconstruct East Germany, and despite claims that it is now time to invest in the West German infrastructure, there is some evidence that West Germans donated a larger amount of their earnings to the hospital in East Germany. Specifically, West Germans donated 65 percent of their earnings to the hospital in Hannover / West Germany (2.06 EUR) and 74 percent to the hospital in Leipzig / East Germany (2.73 EUR) (p=0.07).

Finally, we check whether high cheaters donate more. If high cheaters indeed donate more, this might demonstrate a specific type of moral cleansing (Lee & Schwarz, 2010; Zhong & Liljenquist, 2006). However, dividing the sample in subjects who had on average 50 percent high rolls or less and those subjects who had on average more than 50 percent high rolls (potential cheaters), we did not find any difference in donations (p=0.52).

### 6.4.3 Social Trust

Heineck and Süssmuth (2013) investigated the effect of socialism on individuals’ trust and risk. They found East Germans to be more risk loving and reported that East

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Germans demonstrate a lower level of social trust. We used a more fine-grained measure of social trust by distinguishing between trust in East and West Germans. The survey asked each subject the following two questions: “Generally speaking, would you say that East Germans can be trusted?” as well as “Generally speaking, would you say that West Germans can be trusted?” Our results add support to Heineck and Süßmuth (2013) finding that social trust in East and West Germany has not yet converged. However, we also find that East Germans and West Germans do not differ in their trust of East Germans, but only in their trust of West Germans, with East Germans reporting significantly more distrust (p<0.01). This raises the question whether East Germans show less social trust due to their exposure to socialism or due to their experiences and social comparisons after German reunification.

6.5 Discussion and Conclusion

From 1961 to 1989, the Berlin Wall divided one nation into two distinct political regimes. We exploited this natural experiment to investigate whether the socio-political context impacts individual honesty. Using an abstract die-rolling task, we find evidence that East Germans who were exposed to socialism cheat more than West Germans who were exposed to capitalism. We also find that cheating is more likely to occur under circumstances of plausibility; subjects were more likely to cheat by misreporting the chosen side of the die than by making up rolls altogether. These results suggest that most people are motivated to hide their dishonesty – either from others (Hao & Houser, 2011) or from themselves (Mazar et al., 2008). In contrast to the effects of East or West German background on cheating, we did not observe an overall difference between East and West Germans in prosocial behavior (donations to hospitals). However, given that West Germans donated marginally more to East German hospitals than West German
hospitals, future studies exploring socio-political influences on prosocial behavior should consider the identities of both subjects and recipients.

We interpret our findings as evidence that the political regime of socialism has a lasting impact on citizens’ basic morality. However, alternative explanations are also possible. One such alternative is that cheating tendencies reflect lasting impacts of economic scarcity. Although economic scarcity was a by-product of the socialist regime in East Germany, this explanation is distinct from attributing cheating differences to socialism itself. While we cannot entirely rule out the role of persisting economic differences, which could for example originate from East Germans historically not inheriting assets such as land lots or large and medium size companies that would have allowed them to accumulate wealth over time, we did not observe differences between individuals with East and West Germans backgrounds in education or self-reported standard of living. Most importantly, in our study, subjects’ self-reported standard of living did not predict their cheating behavior.

Another alternative explanation for our results is that the differences in dishonesty are due to effects of post-regime social comparison. After the Berlin Wall fell in 1989, Germans became sharply aware of the socio-political and economic differences on either side of the divide. As previous research has shown, people exposed to upward social comparisons are more likely to cheat in unrelated tasks (Gino & Pierce, 2009; John, Loewenstein, & Rick, 2013), as are people who believe that they were treated unfairly by others (Houser et al., 2012). Thus, the differences in cheating we observed could be due to East Germans’ upward social comparisons and feelings of injustice after the reunification of Germany. In line with this interpretation, our survey showed that East Germans report less trust in West Germans than in East Germans a quarter century after German reunification. However, trust was not related to cheating behavior in our Probit
model, suggesting that this is not the main variable impacting our results. Furthermore, we observed that family background is most predictive of cheating for individuals born before 1970, indicating that greater exposure to socialism predicts greater dishonesty.

If socialism promotes individual dishonesty, the specific features of this socio-political system that lead to this outcome remain to be determined. The East German socialist regime differed from the West German capitalist regime in several important ways. First, the system did not reward work based to merit, and made it difficult to accumulate wealth or pass anything on to one’s family. This may have resulted in a lack of meaning leading to demoralization (Dan Ariely, Kamenica, & Prelec, 2008), and perhaps less concern for upholding standards of honesty. Furthermore, while the government claimed to exist in service of the people, it failed to provide functional public systems or economic security. Observing this moral hypocrisy in government may have eroded the value citizens placed on honesty. Finally, and perhaps most straightforwardly, the political and economic system pressured people to work around official laws and cheat to game the system. Over time, individuals may come to normalize these types of behaviors. Given these distinct possible influences, further research will be needed to understand which aspects of socialism have the strongest or most lasting impacts on morality.
7. General Discussion

Every day, individuals face decisions about whether or not to be honest. Corporate scandals, insurance fraud, media piracy, marital infidelity, and cases of academic dishonesty are but a few examples of ways in which individuals violate social rules for personal gain. At the same time, it is easy to think of instances where people could get away with dishonesty, yet refrain from it. Restaurant patrons generally pay their bills before walking out; many romantic partners choose to remain faithful even when they would have very little chance of being caught for infidelity; and some small businesses even collect revenues according the honor system. Given the prevalence of both honesty and dishonesty in everyday life, the inexorable question is: What determines whether an individual will follow an honest or dishonest course of action in any given situation?

Using behavioral and survey data from a five-country cross-cultural sample, survey data from a large international sample of socially connected pairs, and behavioral data from a sample of East and West Germans at passport offices in Berlin, I explored this question by considering two types of contextual variables. At the global level, I examined the extent to which dishonest tendencies are driven by socio-cultural context, and the local level, I examined the extent to which dishonest tendencies are shaped by situations. If no effect of culture is observed, one can assume that dishonest tendencies is universal; if no effect of life situation is observed, one can assume that dishonest tendencies are driven by moral character.

In Chapter 2, I examined dishonest behavior across samples from five distinct countries and two within-country cohorts, using a simple die task in which participants could earn more money for themselves by cheating. Contrary to predictions from a separate participant sample, I observed a similar pattern of cheating across cultures.
While students cheated more than the general public in every country except Portugal, overall, differences in cheating were not statistically significant across countries. In all countries, participants were only slightly dishonest, with students reporting the favorable earnings side on about 60% of trials (compared with chance performance at 50%), and the general public reporting the favorable earnings side on about 55% of trials. These results suggest that there may be a universal element to dishonesty, in terms of people’s willingness to violate rules for personal gain. It is noteworthy that participants had not encountered the die task before, and thus, it was devoid of cultural context.

In Chapter 6, however, results from a similar die-rolling study comparing individuals with East German and West German backgrounds raise the question of whether cultural context can indeed impact “core dishonesty”. In this study, conducted at citizen centers in Berlin, cheating was significantly higher among those who reported an East German family background (60% favorable outcome reports) than those who reported a West German family (55% favorable outcome reports). Furthermore, the difference between East and West Germans was wider among older cohorts with more lived experience of the separation of East and West Germany before the fall of the Berlin Wall in 1989. While these results raise the possibility that individuals’ socio-political environments can influence dishonest behavior on a novel task, it is also possible that differences in dishonesty were driven by social comparisons caused by reminders of social disparity between East and West Germany after the country was reunified. Given the proximity of East and West Germans in Berlin, this disparity would be especially salient in this city following the fall of the Berlin Wall. To evaluate whether the differences observed in Berlin are more likely due to cultural background or to social comparison, we are currently running a follow-up research study in Germany. This study will compare dishonesty on the die-rolling task between individuals in East and
West Germany in cities outside of Berlin, and between individuals in East and West Berlin. If differences between individuals with East and West German backgrounds are due to cultural background, we should expect to see differences in dishonesty both in and outside of Berlin, whereas if differences are due to social comparison, we should expect differences only in Berlin.

Even considering the difference observed between East and West Germans in Chapter 6, the degree of similarity in dishonest behavior across cultures is notable. The mean levels of cheating observed for East and West Germans in Chapter 6 fall within the range of proportion of high earnings reports observed across samples in the five-country cross-cultural study. Furthermore, in the prediction study from Chapter 2, a separate sample of participants estimated that there would be more cross-country variation in dishonesty than we observed. These participants also overestimated the average level of dishonesty on the die task, and underestimated the variation in dishonesty between cohorts within countries. It is not clear whether the cohort differences observed in the cross-cultural study in Chapter 2 were caused by dispositional differences between students and general public or by situational differences between the university lab settings and the coffee shop settings. For example, differences in setting between testing rooms and coffee shops, differences in reasons for participating, and/or differences in mindsets may have impacted the cheating differences observed between student and public samples. Further research is needed to understand whether within-country cultural differences in core dishonesty are robust and meaningful, and if so, to illuminate the cultural or demographic factors that drive these differences.

In addition to the similar patterns of core dishonesty observed across countries, survey data in Chapter 3 indicate that reasons for avoiding dishonest actions are similar across cultures. In all five countries studied, internal sanctions (feelings of guilt or
shame) were the greatest deterrents of specific misdemeanors and infractions. Legal sanctions were also significant but less powerful deterrents. In line with the theory of self concept maintenance (Mazar et al., 2008), these findings indicate that honesty is primarily a matter of internal motivation, rather than a direct response to external consequences.

These results, together with the behavioral findings from the die task, suggest a degree of commonality in dishonest tendencies across cultures. However, the research in this dissertation also revealed significant cross-cultural differences in dishonesty for specific actions. Surveys described in Chapters 3 and 4 asked participants to report the likelihood that they would engage in specific, contextual dishonest behaviors, such as underreporting income on tax filings or taking a sick day from work when not in fact sick. For almost every specific action, self-reported likelihood of acting dishonestly varied across countries. In general, and in contrast to the die task findings, differences across countries for specific actions were more pronounced than differences between student and public cohorts. These results suggest that when individuals face decisions about whether or not to act dishonestly in specific real world situations, culture does play a significant role in shaping their decisions.

Turning to the question of cross-situational stability, data from Chapter 4 indicates that individuals’ dishonest tendencies are not uniform across situations. The survey reported in this chapter asked participants to report the likelihood that they would act dishonestly in 49 specific situations (56 for students). Importantly, these situations were sampled from seven (or eight) domains of life: work, government, business, relationships, friends, religion, strangers, and (for students only) academics. We tested the hypothesis that dishonest tendencies vary according to these different
domains. Both country-level and individual-analyses supported this hypothesis. These results suggest that if moral character does exist, it is a multifaceted construct.

The eight domains from this study were constructed based on a top-down partitioning of typical social contexts in people’s lives. However, this represents only one initial approach to thinking about different domains of dishonesty. When we conducted a bottom-up data-driven principle component analysis (PCA), three factors emerged, which we termed cheating the system, social/spiritual dishonesty, and dishonesty for personal gain. Significant country variation was also evident when examining dishonest tendencies according to these bottom-up domains.

In Chapter 5, I explored whether socially connected individuals share similar tendencies to tell particular types of lies. Our research team constructed four lying categories, distinguishing antisocial from prosocial lies, as well as lies of commission from lies of omission. Though different from the domain partitioning in Chapter 4, these different lying categories may be considered different domains of dishonesty. (Although prosocial lies do not fit our working definition of dishonesty, namely violating a social contract for personal gain, the prosocial lying categories serve as useful comparisons for the antisocial lying categories of greater relevance to this dissertation.) The study in Chapter 5 found that tendencies to tell particular types of lies were related in socially connected individuals. This finding reinforces the notion of domain-specific dishonesty, as individuals’ tendencies to tell particular types of lies were uniquely related to their friends or family members’ tendencies to tell the same types of lies, rather than to their friends or family members’ tendencies to lie more generally. Furthermore, the finding suggests a socio-cultural component to domain-specific dishonesty. Although the study was correlational and thus precludes causal inference,
the results are in line with the theory that domain-specific dishonest tendencies are socially or culturally learned.

7.1 Implications

One of the central questions of this dissertation was whether culture plays a meaningful role in shaping individual dishonesty. The research presented in this dissertation provides evidence for both a universal tendency toward dishonesty, and cultural variation in domain-specific dishonest behavior. Is it possible to reconcile these findings? I suggest that these findings can be reconciled by considering an analogy to language. Culture clearly matters when it comes to language development; individuals adopt the language(s) of their local culture, and people from different linguistic zones cannot understand one another. At the same time, it appears that all human beings are biologically equipped to learn language(s) involving grammatical rules. The notion that we are hard-wired to learn the rules of language has been referred to as universal grammar (Chomsky, 1995), and a similar theory, known as universal moral grammar (UMG), has been proposed in the domain of morality (Mikhail, 2007). To quote Mikhail (2007), “the mind contains a moral grammar: a complex and possibly domain-specific set of rules, concepts and principles” (p. 144).

Viewed in light of UMG theory, the findings of cross-cultural similarity and cross-cultural differences reported in this dissertation begin to fit together. The similarity in cheating behavior on the die task observed across countries suggests that the tendency to follow social rules – with just a slight adjustment for personal gain – is fairly universal. I refer to this basic tendency as core dishonesty, and employed the analogy of individuals having similar “moral backbones.” On the other hand, the cultural variation observed across countries from self-reports of specific dishonest behaviors (and domains) in Chapters 3, 4, and 5 is indicative of the various cultural
contexts in which individuals with similar moral backbones operate. This variation suggests that individuals are sensitive to the particular rules and norms of their given social contexts. These rules and norms are likely to be internalized and serve as the baseline standard from which people gauge their dishonesty. For example, copying an assignment off of a friend is likely considered more dishonest – and more egregious – at a school where copying assignments is very rare than at a school where copying assignments is common practice.

A similar framework has been discussed by Haidt and Joseph (2004), who propose the notion of moral intuitions. They suggest there are “a small number of basic units that might underlie a great diversity of cultural products” (p. 57). Haidt and colleagues have since proposed six such moral intuitions (or moral foundations): care/harm, fairness/cheating, loyalty/betrayal, authority/subversion, sanctity/degredation, and liberty/oppression (Haidt, 2012). By this framework, honesty is a component of fairness/cheating foundation, and should be highly related to other types of decisions related to fairness – a hypothesis that calls for empirical evaluation.

From a practical standpoint, these results have implications for policies aimed at discouraging dishonesty. For example, findings from the Dishonesty Deterrents survey in Chapter 3 suggest that increasing legal penalties will have limited effects on dishonesty. On the other hand, efforts aimed at making specific dishonest behaviors appear counter-normative (e.g. through social media campaigns) may strengthen internal sanctions against such actions and ultimately have greater effectiveness at reducing dishonesty.

Another practical implication concerns efforts to promote moral character through education. Character education is an umbrella term used to describe education efforts aimed at teaching virtues such as honesty, often through didactic methods or
incentives such as praise for good behavior. The results of this dissertation suggest that traditional character education efforts are misguided for two reasons. First, the character education movement emphasizes rote learning and external incentives for moral behavior. However, the research in this dissertation points to internal incentives having the strongest influences on dishonest behavior. Second, the character education movement assumes that once virtues are learned, children will apply these virtues across domains. In contrast, I have found evidence for domain-specificity, suggesting that translation of moral tendencies from one area of life to another may be weak. In line with these critiques, a 2010 study of character education efforts in the United States failed to find evidence for their effectiveness (Social and Character Development Research Consortium, 2010)

**7.2 Limitations**

Empirical investigations require the use of methodological tools that are inherently limited. Both the behavioral and survey methods used for this research carry limitations. With regards to the surveys, the primary issue concerns the use of self-report as a proxy for real-world dishonest behavior. Survey responses are subject to multiple biases, most notably, self-presentational biases (Glass & Arnkoff, 1997; Razavi, 2001). It is possible that individuals who report being unlikely to evade taxes on a survey are falsely reporting their real-world behavior; furthermore, the influence of self-presentational biases could vary by country. In line with this possibility, I observed variable correlations between die task performance and survey results across countries, which ranged from non-significant to positive and significant. However, it is important to note that while country differences in dishonesty could be due in part to cultural differences in self-presentational concerns, conclusions regarding domain-specificity would not be affected unless this bias influenced responses differentially by domain.
Even so, research assessing dishonesty in different life domains using alternative measures to self-report would bolster confidence in the present conclusions.

In addition to surveys, this dissertation relied on a single behavioral measure of dishonesty, reports on a die-rolling task. This task, which we administered both with iPads and with a physical die, was originally developed by Jiang (2013), and has high face validity. In addition, it has the advantage of participants knowing their dishonesty cannot be tracked (since they are never asked to report their “true” choices). Still, examining core dishonesty using convergent measures would further bolster confidence in my interpretations of the behavioral results.

7.3 New Research Directions

The work described in this dissertation raises several questions for future research to address. First, while findings from Chapters 2 and 3 suggest that the tendency to limit dishonesty in accordance with internal standards is relatively universal, comparative findings are based on observations from only five countries. Furthermore, although these countries differed in culture, they can all be characterized as large-scale, modern societies. Another cross-cultural research effort by Henrich and colleagues compared decisions in economic games between samples from small scale societies varying in market integration (Henrich et al., 2005, 2010). This research team found that distribution decisions in ultimatum games, dictator games, and third party punishment games were correlated with market integration. Whether individuals from small-scale societies would demonstrate similar patterns of dishonesty on the die task (or another de-contextualized behavioral measure) to what I observed in large-scale societies is an important open question. I have tentatively suggested that the tendency to limit dishonesty is universal, that is, culturally insensitive. However, the extent to
which our behavioral findings generalize across different types of societies and social 
groups remains to be seen.

In planning such research, it will be important to consider, or even manipulate, 
the salience of participants’ cultural contexts. A recent study found that bankers 
behaved significantly more dishonestly on a coin-tossing task than control participant 
groups – but only when first primed with their professional identity (Cohn, Fehr, & 
Maréchal, 2014). This finding is in line with my theory of universal dishonesty in the 
absence of contextual cues. Considering findings of student and public dishonesty on 
the die task, it is conceivable that differences were likewise related to contextual cuing; 
for example, coffee shops may cue more honest behavior than university lab settings. 
This possibility – which may concern university professors and administrators! – could 
be investigated by manipulating experiment location while controlling for participant 
group, perhaps by sampling university students both in on-campus lab rooms and on-
campus coffee shops.

Based on analyses from surveys in which participants reported the likelihood 
that they would engage in specific dishonest behaviors, I observed that dishonest 
tendencies varied according to domain. The research considered different types of 
domain definitions. In Chapter 4, domains were defined according to different social 
networks and arenas of life using a top-down approach, as well as using a bottom-up 
data-driven approach, which yielded domains that reflected the type of dishonest 
behavior. In Chapter 5, domains were defined according to the style of lying behavior 
(e.g. commission versus omission). Given the multitudinous possibilities for defining 
domains, additional research may identify which domain delineations are most robust 
and useful in terms of accounting for behavioral variation.
7.4 An Optimistic Finish

In closing, this dissertation contributes to theory on individual dishonesty in three ways. First, it suggests that the theory of self-concept maintenance applies across cultures: that the tendency to limit dishonesty in accordance with social rules or norms is relatively universal. Second, it proposes that cultural differences in dishonesty exist, but vary according to situation and domain. Finally, it suggests that moral character is a multifaceted construct, which is expressed in domain-specific ways.

At a broad level, this dissertation also suggests that the integration of behavioral theory with economics may be possible. Our understanding of the behavior of people within complex social networks can be enriched through understanding how internal moral standards guide decisions in tandem with external incentives. Notably, this integration is in line with the writings of Adam Smith. His first book, *Theory of Moral Sentiments*, while less influential than *Wealth of Nations*, leads with the assertion, “How selfish soever man may be supposed, there are evidently some principles in his nature, which interest him in the fortunes of others, and render their happiness necessary to him, though he derives nothing from it, except the pleasure of seeing it” (Smith, 1759/1976, p. 9). Although modern-day economics is still dominated by the rational actor model, awareness is growing about the importance of incorporating behavioral principles into economic theory.

This dissertation indicates that dishonest behavior is affected by culture and by domain-based situational factors – for better or for worse. At a personal level, then, one might wonder whether it is possible to become more honest. Both intuition and cultural narratives suggest that personal transformation is possible: for example, the character transformation of a wooden puppet the classic story of Pinocchio (Collodi, 1883/1968) or the reformation of Jean Valjean in Victor Hugo’s *Les Misérables* (Hugo, 1862). As a
closing comment, I speculate that while our social and situational contexts certainly affect us, we are not entirely bound by them. Instead, becoming aware of our sensitivities to social norms and situational influences may be the first step toward transcending them. Self-awareness may be the key that unlocks our capacity to cultivate personal tendencies toward honesty in all areas of life.
Appendix A: Die Task Experimenter’s Script

This task involves rolling a virtual die. Before you roll the die, you will be asked to choose a side in your mind: top or bottom. Be sure to make your choice before you roll the die. Then, after you roll, you will indicate whether you chose top or bottom in your mind. You will receive 10 cents [value and currency adjusted for each country] per dot on the side of the die you indicate each time. You will repeat this process 20 times.

Let me show you how it works. [Start dice task on demonstration iPad]. Here, it shows the die, and asks you to choose a side before you roll the die. For this roll, I will choose “bottom”. I now remember that selection and roll the die.

Here are the results of the roll. Looks like I rolled a two [specific numbers varied based on roll outcome]. As you can see, two dots on the top and five dots on the bottom. I earn 10 cents for each dot on the side I selected. I chose bottom before I rolled the die, so here, I earn 50 cents.
Now, let’s try another roll. This time, I choose “top”.

Okay, here are the results: I rolled a three. As you can see, there are dot on the top and four dots on the bottom. Because I chose top before rolling, I earn 30 cents for this roll.

Here, at the top of the screen, you can see the amount you earn accumulating. Once you have completed the exercise, a screen will appear with the total amount you have earned. At this point, please raise your hand, and I will set up the next survey. Do you have any questions?
Appendix B: Everyday Dishonesty Survey

Welcome to the survey. Please indicate how likely you are to engage in each of the following actions, in the given situation. If the action does not apply to you, please imagine yourself in the situation, and respond according to how you think you might act. Please read each action carefully, and respond as honestly as possible. Your responses are confidential and anonymous.

How likely are you to ____________________?
[Scales range from “not at all likely” to “very likely”]

Work:
1. Take supplies (such as paper and pencils) from work to use at home for non-work-related tasks?
2. Buy dinner for your friends and submit the receipt to your workplace as a business expense?
3. Include false work qualifications on your resume?
4. Take a sick day from work when you are not sick?
5. Claim that a project is underway at work when in fact you haven’t started it?
6. Take credit for an assignment at work that someone else completed?
7. Engage in personal activities (such as paying your personal bills) while on company time?

Government:
1. Omit information on your tax filings in order to pay less income tax?
2. Speed by 15% over the speed limit while driving?
3. Run a red light when nobody is around?
4. Park your car in a no parking zone?
5. Bribe a police officer to avoid getting a speeding ticket?
6. Apply for a government tax credit knowing you are not eligible for it?
7. Fake a signature of a doctor on a government document in order to get an expensive medication for free?

Business:
1. Leave a store with an article of clothing you did not pay for (on purpose)?
2. Not mention it when you notice you were given too much change at the grocery store?
3. Connect to an internet service directly, without the provider knowing, and without paying for it?
4. Install a computer program that one of your friends purchased on your computer, instead of buying it yourself?
5. Provide your insurance company with false information in order to reduce your premium?
6. Leave a restaurant without paying your bill (on purpose)?
7. Not tell the phone company that you were not charged for a month of service?

Relationships:
1. Flirt with someone you are attracted to when your relationship partner isn’t around?
2. Eat something that your relationship partner would not approve of, without telling him/her?
3. Lie to your relationship partner when he or she asks if you are attracted to someone else?
4. Take money out of your joint bank account and use it without your relationship partner knowing, for something that you know they would not approve of?
5. Tell your relationship partner that you like a gift they got you, when in fact you hate it?
6. Have a one-night affair with someone that is not your relationship partner?
7. Engage in continued sexual relations with someone that is not your relationship partner?

Friends:
1. Tell a friend that you like her haircut even though you think it is terrible?
2. Say your die fell on six when in fact it fell on 3 while playing a board game with friends?
3. Tell your friends stories about yourself that never happened in order to sound more interesting?
4. Make up a false excuse about why you are late to meet a friend?
5. Pretend that you did not damage a friend’s coffee table, when in fact you did?
6. Gossip about a friend behind his back?
7. Tell a friend that you like their new boyfriend or girlfriend when you don’t?

Religion:
1. Eat a food that is forbidden according to your religion’s laws?
2. Pretend to put money in a collection box at your place of worship?
3. Skip a religious ceremony that you are expected to attend so that you can go to a party?
4. Drink (but not eat) during a religious fast where you are supposed to neither eat nor drink?
5. Use the Lord’s name in vain?
6. Take a Holy Book from your place of worship home for your own personal use?
7. Break a promise to a leader of your religious group that you will spend an afternoon volunteering for a good cause, and instead stay at home?

Strangers:
1. Drive away without leaving a note, after you accidentally dented the bumper of a parked vehicle?
2. Keep a stranger’s camera that you find in a bathroom stall?
3. Take money that you see fall from a stranger’s pocket?
4. Lie about your age to a stranger?
5. Throw extra trash in an unknown neighbor’s trashcan?
6. Listen in on a private conversation between two strangers?
7. Tell a beggar that you do not have any money to give away on you, when in fact you do?

Academic:
1. Bring a piece of paper with course material into an exam, against the rules?
2. Read a copy of an exam answer key prior to taking the exam?
3. Let a classmate see your answers when writing an exam?
4. Lie to a teacher to justify why you didn’t submit an assignment on time?
5. Include text from a relevant source without giving credit when writing a research paper?
6. Purchase an essay that you did not write and submit it as your own?
7. Collaborate with classmates on an assignment that you are supposed to complete individually?
Appendix C: Lying Tendencies Survey

For each question below, participants responded on a continuous slider scale ranging from 0 (“Not at all likely”) to 10 (“Extremely likely”). Question order was randomized for each participant.

**Antisocial commission:**
1. If you are late for a meeting, how likely are you to invent an excuse (such as bad traffic), when in fact it was your fault?
2. At a party, how likely are you to tell stories about yourself that never happened, in order to sound more interesting?
3. During a job interview, how likely are you to describe a previous work experience that never happened?
4. How likely to tell a police officer that you were speeding due to an emergency, when there is no real emergency?

**Prosocial commission:**
1. How likely are you to tell your friend that her birthday party was lovely, when you know everybody was bored at it?
2. How likely are you to say that you loved a gift from a relative, even though it is a useless item?
3. You are invited to a party you do not want to attend. If the host asks you whether you will be coming, how likely are you to pretend that you have a prior commitment that night (when you do not)?
4. If your boss asks why your colleague is not at work, how likely are you to say that they are sick, when you know that they are taking a day off?

**Antisocial omission:**
1. During an interview, how likely are you to keep quiet about lacking a particular skill that is expected for the job?
2. If you are given too much change the grocery store, how likely are you to keep your mouth shut?
3. If your boss praises you for putting in extra hours to finish a project, how likely are you to keep to yourself that you found a quick way to do the job?
4. If you had romantic relations with a friend’s ex-partner, how likely would you be to keep this information to yourself?

**Prosocial omission:**
1. If your boss praises a colleague for their work on a group project when this colleague did not actually contribute, how likely are you to keep quiet about the colleague’s true contributions?
2. If you witness your friend’s spouse flirting with other people, how likely are you to say nothing to your friend about it?
3. If your brother or sister separates from their spouse but doesn’t want your parents to know, how likely are you to withhold this information from your parents?
4. At a party, if you notice an acquaintance has food in their teeth at a party, how likely are you to pretend you don’t notice?
Appendix D: East/West Germany Variable Descriptions

<table>
<thead>
<tr>
<th>Variable</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>Family Background</td>
<td>0 = Subject indicated to have an East German family background. 1 = Subject indicated to have a West German family background.</td>
</tr>
<tr>
<td>High roll</td>
<td>0 = Subject reported that they had rolled a 1, 2 or 3. 1 = Subject reported that they had rolled a 4, 5 or 6.</td>
</tr>
<tr>
<td>Age</td>
<td>Subjects age as of January 1, 2014.</td>
</tr>
<tr>
<td>Female</td>
<td>Gender, 1 = female, 0 = male.</td>
</tr>
<tr>
<td>Trust East</td>
<td>Opinion on the statement ‘Generally speaking, would you say that East Germans can be trusted?’ on a scale from 0 = most East Germans can be trusted to 10 = you cannot be too careful in dealing with East Germans.</td>
</tr>
<tr>
<td>Trust West</td>
<td>Opinion on the statement ‘Generally speaking, would you say that West Germans can be trusted?’ on a scale from 0 = most West Germans can be trusted to 10 = you cannot be too careful in dealing with West Germans.</td>
</tr>
<tr>
<td>West cheated</td>
<td>Reply to the question ‘Would you agree that West Germans cheated East Germans after the fall of the Berlin Wall?’ 0 = no, 1 = yes.</td>
</tr>
<tr>
<td>Education</td>
<td>Reply to the question ‘What is the highest level of education you have completed?’ 0 = none, 1 = ‘Hauptschule’ (lower level high school), 2 = ‘Realschule’ (lower level high school), 3 = ‘Abitur / Fachabitur’ (some college), 4 = ‘Bachelor / Fachhoch-schulabschluss’ (3-4 years of university), 5 = ‘Master / Diplom’ (4-5 years of university) and 6 = ‘Promotion / Aufbaustudium’ (doctoral degree, post-graduate degree).</td>
</tr>
<tr>
<td>Political spectrum</td>
<td>Reply to the question ‘In political matters, people talk of “the left” and “the right”. How would you place your views on this scale, generally speaking?’ from 0 = left to right = 10.</td>
</tr>
<tr>
<td>Living standard</td>
<td>Reply to the question ‘What describes your standard of living?’ on a scale from 1 = very well off to 6 = poor.</td>
</tr>
<tr>
<td>Marital status</td>
<td>Reply to the question ‘What is your marital status’. 1 = single, 2 = In a relationship, 3 = married , 4 = separated, 5 = divorced, 6 = widowed, 7 = other.</td>
</tr>
</tbody>
</table>
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doi:10.1146/annurev.psych.55.090902.142015


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**Biography**

Heather Elizabeth Mann was born in Toronto, Ontario, Canada on September 20, 1985 to Nancy Elizabeth (Wright) Mann and Robert Bruce Mann. In 2008, she graduated on the Dean’s Honours List from the University of Waterloo, where she majored in Psychology and minored in Music. At the University of Waterloo, Heather was a member of the Synaesthesia Research Lab, where she conducted research exploring time-space synaesthesia under the guidance of Professor Mike Dixon. Her Honours thesis, conducted under the guidance of Professor Dan Smilek, examined the contribution of cognitive factors to manic mood states, and was recognized with an Undergraduate Honours Thesis Award.

In 2010, Heather completed a Master’s degree in Psychology at the University of British Columbia (UBC), in Vancouver, BC, under the supervision of Professor Kalina Christoff. Heather’s research interests at UBC centered on the cognitive neuroscience of thought, and her Master’s thesis explored the effects of real-time neurofeedback using functional magnetic resonance imaging (fMRI). Her academic life was enriched by her time at Green College, a residential community of graduate students with a focus on ideas and friendship.

Heather began her doctoral studies at Duke University in 2010, entering through the Graduate Admitting Program in Cognitive Neuroscience. In her second year at Duke, she began research with Professor Dan Ariely in the Center for Advanced Hindsight, where she began exploring behavioral topics. Since 2012, her research with Dan Ariely has focused on the social, cultural and situational influences on cheating and dishonesty. She is preparing to move back to her home country, Canada, and pursue new opportunities in social entrepreneurship.
Heather has published the following articles in peer-reviewed journals:


Heather has also received the following awards over the course of her graduate studies:

NSERC Alexander Graham Bell Canada Graduate Scholarship (CGS M) (2008—2010)
Psychology Department Entrance Fellowship, UBC (2008—2010)
Fellowship to attend 2009 Summer Institute in Cognitive Neuroscience (2009)
Vanier Canada Graduate Scholarship (Highest scholarship available to Canadian graduate students; declined to attend Duke University) (2010)
NSERC Postgraduate Scholarship (PGS D) (2010—2012)
Duke Chancellor’s Scholarship (2010—2012)
Scholarship to attend 2013 Excellence in Ethics Research Conference (2013)