NBER WORKING PAPER SERIES

HITTING ROCK BOTTOM: ECONOMIC HARDSHIP AND CHEATING

Livia Alfonsi
Michal Bauer
Julie Chytilová
Edward Miguel

Working Paper 34695
http://www.nber.org/papers/w34695

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
January 2026

We thank Camilo Gomez, Winnie Njoroge, Eric Ochieng, Alec Brockell, and especially Hao Wang for excellent research assistance, and Johannes Abeler, Piotr Dworczak, Alexandra Schubert, Jonathan Schultz, Joshua Schwartzstein, Jesse Shapiro, Matthias Sutter and seminar participants for helpful comments. We gratefully acknowledge our collaborators (Innovations for Poverty Action), and funding from the Dioraphte Foundation, Givewell, US NIH Awards (R01-TW05612, R01-HD044475, R01-HD090118, and R03-HD064888), US NSF Awards (SES-0418110 and SES-0962614), the Berkeley Population Center, and Economic Research on Identity, Norms, and Narratives (ERINN). Bauer and Chytilov gratefully acknowledge funding from the Czech Ministry of Education, Youth, and Sports (ERC CZ program, project LL2303). The research was approved by the Research Ethics Committee at the Faculty of Social Sciences, Charles University, Maseno University in Kenya, and the UC Berkeley Committee for the Protection of Human Subjects. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Hitting Rock Bottom: Economic Hardship and Cheating
Livia Alfonsi, Michal Bauer, Julie Chytilová, and Edward Miguel
NBER Working Paper No. 34695
January 2026
JEL No. C93, D91, O12

ABSTRACT

This paper investigates whether economic hardship undermines preferences for honesty. We use controlled, high-stake measures of cheating for private benefit in a large sample of 5,664 Kenyans, exploiting three complementary sources of variation: experimentally manipulated monetary incentives to cheat, a randomized increase in the salience of one's own financial situation, and the Covid 19 income shock (exploiting randomized survey timing, with respondents interviewed before vs. during the crisis). We find that cheating behavior is highly responsive to financial incentives in the experiment. Covid-19 economic hardship—marked by a 51% drop in monthly earnings—leads to a sharp increase in the prevalence of cheating, and the effect increases gradually with prolonged hardship. The effects are largest among the most economically impacted and are amplified when the salience of one's own financial situation is experimentally increased. The results demonstrate that while most individuals exhibit a strong preference against cheating under normal conditions (in line with the existing body of work), economic forces can account for a substantial share of variation in dishonesty: the estimated cheating rate rises from 29% under low stakes in normal times to 86% under high stakes during the crisis.

Livia Alfonsi
Harvard University
Harvard Business School
lalfonsi@hbs.edu

Michal Bauer
CERGE-EI
and IZA
michal.bauer@cerge-ei.cz

Julie Chytilová
Charles University
chytilova@fsv.cuni.cz

Edward Miguel
University of California, Berkeley
Department of Economics
and NBER
emiguel@econ.berkeley.edu

A randomized controlled trials registry entry is available at
https://www.socialscienceregistry.org/trials/01191

1. Introduction

When and why do people lie, and do severe hardship and economic struggles make people more prone to break rules and cheat for their own benefit? It is not an exaggeration to say that this question has drawn the attention of philosophers, writers, and social scientists for thousands of years, at least since Aristotle in the 4th-century BCE argued that "*Poverty is the parent of revolution and crime*" (Oxford University Press, 1995). The idea that dire circumstances may force people to make moral compromises and engage in cheating, corruption and crime is a major theme in numerous classic novels, including Victor Hugo's *Les Misérables* and Charles Dickens's *Oliver Twist*. More recent reincarnations of Aristotle's view can be found in Pearl S. Buck's *The Good Earth* ("*Hunger makes a thief of any man*") and in popular culture – for instance, Bob Dylan's song *Like a Rolling Stone* ("*When you ain't got nothing, you got nothing to lose*").[1] Textbook economics largely agrees with this intuition. Reflecting the reasoning of Gary Becker's seminal work on the economics of crime (Becker 1968), it predicts that tighter material constraints increase incentives to engage in dishonest behavior and crime, reflecting diminishing marginal utility of income. Despite this compelling intuition, causal field evidence testing whether economic hardship does indeed undermine individual preferences against cheating and breed dishonest behavior remains limited.[2]

In fact, it is well-established that many people have preferences against cheating and in favor of rule following even when they need to sacrifice a substantial amount of money (Abeler, Nosenzo, and Raymond 2019; Fischbacher and Föllmi-Heusi 2013; Gächter and Schulz 2016). Understanding whether individuals' adverse economic situation influences their cheating decisions is important for thinking about the sources of differences in norm-breaking behavior between the poor and the rich – i.e., whether they are mainly driven by deep personality differences determined by long-term factors (such as genes, culture or the childhood environment), or by

[1] Similarly, Eminem in his song *"Rock bottom"* expresses the idea as follows: "*That's rock bottom, when this life makes you mad enough to kill. That's rock bottom, when you want somethin' bad enough to steal."* To take examples closer to the Kenyan setting we examine, related ideas appear in Ngugi wa Thiong'o's *Weep Not, Child* and in John Kiriamiti's *My Life in Crime*. Yet, not all writers and philosophers agree with this view. In contrast, some philosophers such as Jean-Jacques Rousseau in "*Discourse on the Origin and Basis of Inequality Among Men*" (1755) have painted a different picture about morality of the poor and suggested that poverty embodies innocence and honesty, while material wealth and the temptations of modern civilization foster greed and moral decay.

[2] Many economists have argued that the real-world economic and social consequences of cheating and norm-breaking are substantial, see (Mauro 1995; Bardhan 1997; Olken and Pande 2012), among others.

differences in immediate economic constraints. This distinction has fundamental implications for behavioral responses to economic crises. For instance, if economic hardship fuels dishonest behavior, this may lower people's willingness to engage in informal economic interactions, creating a barrier to any subsequent economic recovery. Yet, to credibly test the causal link between adverse economic conditions and cheating behavior is challenging and requires: (i) an exogenous and large shock to individuals' economic situation, (ii) reliable and high-stakes measures of cheating, and (iii) a research design that allows one to identify whether cheating responses to adverse economic conditions are transient or whether they tend to persist over time.

In this paper, we seek to provide such a causal test through a comprehensive, large-scale field study with a sample of 5,664 middle-aged adults from the Kenya Life Panel Survey (KLPS), broadly representative of the Kenyan population along many demographic and socio-economic dimensions. We combine an incentivized and broadly-validated controlled task that measures lying for private financial gain, with three complementary sources of variation: (i) experimentally varied financial incentives to cheat, (ii) an experimental manipulation of the salience of one's own financial situation, and (iii) a natural experiment created when pre-randomized KLPS wave assignments happened to place interviews with half the respondents before the Covid-19 crisis and half during it, when median income dropped by 51% relative to pre-crisis levels.

We first show that, under normal conditions, most individuals in the sample exhibit strong preferences for truth-telling and are willing to sacrifice substantial income to avoid cheating. This finding is consistent with the previous body of work summarized in Abeler, Nosenzo, and Raymond (2019), who document widespread intrinsic preferences against cheating across 90 studies in 47 countries. Unlike previous work, however, we show that cheating is highly responsive to both financial incentives and to large negative income shocks. Moreover, the effect of the shock is even stronger when we exogenously make individuals' own financial situation salient. Overall, the results taken together suggest that even a simple Beckerian framework goes a long way in predicting when people are more prone to lie and make moral compromises. This contrasts with much of the existing literature, largely based on student samples or online panels in high- and middle-income countries, which has found limited evidence that economic factors systematically shape cheating behavior, as we detail below. By studying large changes in financial incentives and resources in a low-income setting (i.e., among the population for which one would expect the economic factors to be particularly relevant), we show that economic forces can account for a substantial share of variation in dishonesty: the estimated cheating rate rises from 29% under low stakes in normal times to 86% when stakes are high and decisions are made during the crisis.

To identify dishonest behavior, we use a modified version of the Mind Game (Jiang 2013; Lowes, Nunn, Robinson, and Weigel 2017; Abeler, Falk, and Kosse 2025), a well-established experimental task (building on the Fischbacher and Föllmi-Heusi (2013) paradigm) designed to cleanly identify the prevalence of an important form of cheating, lying for one's own private financial benefit. Before seeing an outcome of a simultaneous toss of five coins, participants choose in their minds the winning side of the coin. Importantly, they report their choice only after observing the outcome of the toss. The higher the number of coins displaying the reported winning side, the higher the payout. Although it is impossible to find out if any specific individual has cheated (since we cannot read their mind), we can detect cheating at the group level by comparing the reported share of successful outcomes with the 50% benchmark implied by truthful reporting, given the randomization inherent in a coin flip. This paradigm has become the leading way to measure preferences against cheating in experimental economics (Abeler, Nosenzo, and Raymond 2019). It captures key features of many real-life economic interactions in which people can exploit information asymmetries and misreport private information for personal gain – for instance, borrowers overstating income to qualify for credit, workers underreporting earnings to get subsidies or avoid taxes, or citizens bribing officials to gain advantages. Reassuringly, numerous studies have shown that behavior in this cheating task predicts a range of out-of-lab behaviors.[3]

We start the analysis by documenting several patterns illustrating that participants take economic trade-offs seriously when deciding whether or not to cheat. People's cheating behavior is measured across different levels of financial incentives that were exogenously manipulated in the experiment, ranging from 30 to 151% of the mean daily income (from 82 to 409% of the median). We find that cheating systematically responds to the size of financial incentives. When the outcomes of the coin flips are such that the financial payoff from cheating is 40 Kenyan shillings (KSh), we find that 71.2% of participants report the payoff-maximizing outcome. Increasing the financial payoff from cheating from KSh 40 to KSh 200 raises the share of

[3] Lying in cheating games with a similar structure to the Mind Game was shown to predict transit fare evasion (Dai, Galeotti, and Villeval 2017), student misconduct (Cohn and Maréchal 2018), corrupt behavior by civil servants (Hanna and Wang 2017), in-prison rule violations among inmates (Cohn, Maréchal, and Noll 2010), and the prevalence of rule-violations at a country-level (Gächter and Schulz 2016).

participants reporting the pay-off maximizing outcome to 85.6%, corresponding to an increase in the estimated prevalence of cheating from 42.4% to 71.2%, a 29 percentage-point (or 68%) increase. The effect of incentives is remarkably general within the sample, holding across demographic, economic and cultural subgroups. We also find that cheating rates are inversely related to income in the cross-section: participants from poorer households are significantly more likely to cheat than richer ones, even conditional on other observed covariates.

We next estimate the causal effect of severe economic hardship on dishonest behavior. To do so we take advantage of the randomized order of survey timing, which (by total coincidence) created the following natural experiment: a randomly selected half of KLPS respondents were surveyed shortly before the Covid-19 crisis (as a survey wave concluded in February 2020), while the second half were surveyed during the Covid-19 crisis. Indeed, data collection was paused for several months due to the crisis, and then the second half of the sample (wave 2) was surveyed (from October 2020 to October 2021), enabling a clean comparison of behavior before versus during the crisis. Estimating the effects of the Covid-19 crisis in Kenya is interesting because, as in many low- and middle-income countries (Egger, Miguel, Warren, Shenoy, Collins, Karlan, Parkerson, Mobarak, … Vernot 2021), the crisis caused a substantial and sustained negative income shock in our study population: median per-capita earnings temporarily declined by 51%. In contrast, health outcomes reported in surveys remained stable or even slightly improved (among this population of young to middle-aged adults), and thus do not appear to meaningfully confound the observed behavioral changes.[4]

We find that dishonest behavior in the Mind Game increased substantially during the crisis. Pooled across all levels of incentives, the estimated prevalence of cheating increased by 29 percentage points (or 67%), from 43% to 72%. This effect is large in magnitude, statistically significant, and comparable to the effect of increasing financial incentives fivefold as presented above. The effect is also similar across the whole range of financial incentives to cheat (within the survey experiment), suggesting it is relevant for a variety of real-life situations. In particular, for

[4] The study was pre-registered at the AEA RCT Registry (AEARCTR-0001191). The initial aim was to study a broad set of determinants of cheating behavior (including financial incentives, priming, and human capital interventions). In this paper, in additional to presenting the pre-specified analysis on the impact of financial incentives, a main focus is on the effect of a major negative economic shock as we take advantage of the natural experiment in economic conditions created by the Covid-19 crisis, which we – of course – could not have envisaged at the time of writing the pre-analysis plan.

the lowest financial incentives to cheat, the estimated prevalence of cheating increased from 29% to 55%, for medium incentives from 45% to 75%, and for the highest incentives from 56% to 86%.

To assess whether the observed effects reflect a transient psychological shock or a more persistent response to economic hardship and shifting social norms, we exploit the fact that data collection spanned 18 months before the Covid-19 crisis and 13 months during it. The pattern in the data is clear. There is no evidence of convergence in the cheating rate back to pre-crisis levels. Instead, dishonest behavior rises steadily over time. Specifically, in the "normal" period before the Covid-19 crisis, the estimated prevalence of lying (averaged across all levels of incentives) was around 43% and remained remarkably stable throughout the whole year. This stability of pre-crisis cheating behavior also holds for the sub-sample of farmers, suggesting that predictable income fluctuations due to agricultural seasonality, did not meaningfully affect honesty, in line with previous work (Boonmanunt, Kajackaite, and Meier 2020; Aksoy and Palma 2019). The dynamics during the Covid-19 crisis period are starkly different: the prevalence of misreporting for personal benefit rose gradually, to a striking figure of over 90% cheating by the end of the data collection. Such a profound shift in cheating behavior is consistent with behavioral adaptation and the erosion of honesty norms under prolonged economic hardship. We present a simple conceptual framework below illustrating how a negative economic shock can both increase the marginal utility of cheating and, as beliefs about others' honesty adjust downward, reduce its psychological cost. The process is self-reinforcing: early defections reduce stigma, invite further defections, and honesty may erode gradually rather than in a single jump.

Having established the main effects of the crisis, and its dynamics, we next provide several tests to shed light on whether these effects are primarily driven by the economic mechanism we emphasize or by other factors that may have changed during the pandemic. We begin by examining whether the effects are stronger among individuals more severely affected by the economic shock. Consistent with evidence from other low-income countries (see Egger, Miguel, Warren, Shenoy, Collins, Karlan, Parkerson, Mobarak, Fink, Udry, et al. (2021), Miguel and Mobarak (2022), Mahmud and Riley (2023), Alfonsi, Bassi, Rasul, and Spadini (2025), Pörtner, Alam, and Ahmed (2025), among others), reported income losses were substantially larger for KLPS respondents living in urban areas than in rural ones, with earnings in the city declining by more than twice as much. Mirroring this pattern, the effect of the crisis on cheating behavior is a 38-percentage point increase in urban areas versus 19 points in rural areas.

We then directly test whether considerations of one's own economic situation when facing hardship provide a psychological "license" to cheat. To do so, we exploit a priming experiment embedded in the survey in which a random subset of participants were asked a short module on current savings and borrowing immediately before making choices in the Mind Game, thereby exogenously increasing the salience of their own financial situation. In line with the hypothesized mechanism, the financial prime increases cheating only during the Covid-19 crisis period, with no effect observed before the crisis. Other primes related to individuals' cultural identity and politics do not have meaningful impacts and do not exhibit increased influence during the crisis.

Finally, we take advantage of the richness of the KLPS survey instrument to consider several alternative non-economic channels that might explain the rise in dishonest behavior, namely, changes in health, political norms, religiosity or simply confusion about the Mind Game. We find no evidence of deteriorating health outcomes during the crisis in our sample (which recall consists of middle-age adults), and self-reported health measures are not associated with cheating in the cross-section. Political attitudes shifted in some respects—e.g., the perceived acceptability of bribery increased during the pandemic—but these attitudes are not correlated with dishonest behavior in the game either. Increasing the salience of politics in the priming experiment likewise does not significantly alter cheating behavior. Measures of religiosity and exposure to a religious prime are similarly unrelated to cheating. Lastly, we rule out confusion about the task as an explanation: understanding of the Mind Game was generally high and did not differ before versus during the Covid-19 crisis, and the prevalence of cheating does not vary meaningfully with individual cognitive ability or education (conditional on other covariates). Taken together, these findings support the interpretation that economic hardship is the primary driver of the observed increase in cheating behavior during the crisis period.

The paper unfolds as follows. In Section 2, we review the related literature. In Section 3, we offer a simple conceptual framework that illuminates mechanisms regarding how sustained economic shocks may lead to more cheating over time. In Section 4, we provide background information and describe the experimental design. Next, we describe the results: we first discuss the role of financial incentives and income (Section 5), then establish the effects of the crisis on earnings and prevalence of cheating (Section 6) and, finally, explore mechanisms (Section 7). The final section concludes.

2. Related Literature

The paper is related to existing work that studies individual preferences against cheating, using controlled non-strategic, experimental tasks (e.g., Fischbacher and Föllmi-Heusi (2013), Gächter and Schulz (2016), Kajackaite and Gneezy (2017), Abeler, Nosenzo, and Raymond (2019), Abeler, Becker, and Falk (2014), Schudy, Grundmann, and Spantig (2024)).[5] This work has established that many people have an intrinsic preference against cheating and are willing to sacrifice substantial financial resources to act honestly (for a comprehensive synthesis of this body of work, see Abeler, Nosenzo, and Raymond (2019)).[6] While a rich literature has explored the role of gender, beliefs about the honesty of others, culture, the social environment during childhood, or historical exposure to formal institutions in shaping this preference (Abeler, Nosenzo, and Raymond 2019; Gächter and Schulz 2016; Lowes, Nunn, Robinson, and Weigel 2017; Abeler, Falk, and Kosse 2025; Houser, List, Piovesan, Samek, and Winter 2016), this study attempts to provide a more comprehensive account of the role of economic factors.

The findings speak to the literature that studies the effects of financial incentives on cheating. The recent meta-study (Abeler, Nosenzo, and Raymond 2019) concludes that most existing experiments (typically implemented among university students in high income countries) have failed to find meaningful effects of incentives on cheating (Fischbacher and Föllmi-Heusi 2013; Balasubramanian, Bennett, and Pierce 2017; Rahwan, Hauser, Kochanowska, and Fasolo 2018; Weisel and Shalvi 2015), although important exceptions to this pattern exist (Kajackaite and Gneezy 2017). To the best of our knowledge, the present study is the largest to date that studies this question among a relatively diverse sample in a low-income setting. We find strong evidence that people's cheating behavior responds to incentives, in line with standard economic theory, and this effect is robust across different socio-demographic sub-groups.

This paper also contributes to work that studies whether economic hardship can undermine preferences against cheating. To move beyond descriptive evidence, two papers exploited regular

[5] Another important strand of the literature on determinants of cheating studies behavior outside of controlled tasks. Prominent examples are List and Momeni (2020) and Flory, Leibbrandt, and List (2016) who study the role of incentive structures on cheating behavior in the workplace.

[6] In line with the evidence, a number of theoretical papers work with the assumption of some preference against cheating (e.g., Kartik, Ottaviani, and Squintani (2007), Matsushima (2008), Ellingsen and Östling (2010), Gneezy, Kajackaite, and Sobel (2018)).

changes in liquidity due to the seasonality of agricultural income.[7] Boonmanunt, Kajackaite, and Meier (2020) find that the prevalence of cheating is similar in the lean season and the harvest season among 568 farmers in Thailand. Aksoy and Palma (2019) find a similar pattern among 109 farmers in Guatemala. In line with this work, we do not find cheating rate to vary across agricultural seasons in the Kenyan sample, suggesting that predictable, short-term drops in income – which people expect and can respond to – are not enough to motivate people to cheat more. At the same time, a main contribution here is evidence from a natural experiment caused by the Covid-19 pandemic showing that a major upheaval in people's economic lives makes them cheat more, in line with the classic Beckerian model. The findings thus relate to work that shows that economic hardship increases engagement in criminal activities, an extreme manifestation of rule-breaking for one's own benefit (Deshpande and Mueller-Smith 2022; Palmer, Phillips, and Sullivan 2019; Khanna, Medina, Nyshadham, Tamayo, and Torres 2023).

To shed light on channels, this paper illustrates the value of combining a natural experiment with rich survey data and a comprehensive priming experiment.[8] This relates our paper to priming studies designed to uncover mechanisms of how environmental factors shape behavior (Callen, Isaqzadeh, Long, and Sprenger 2014; Cohn, Maréchal, and Noll 2010; Mani, Mullainathan, Shafir, and Zhao 2013). In addition, the time span of the KLPS data collection allows us to shed light on the dynamics of cheating behavior during the crisis. Although a stable minority of participants cheated during the one-year period before the crisis, the cheating rate gradually rose throughout the crisis period to more than 90% at the end of the data collection. This pattern is important because a large body of laboratory and field evidence shows that learning that social norm violations are widespread shifts people's perceptions of such norms and, in turn, their behavior (Bursztyn, Egorov, and Fiorin 2020; Bursztyn, González, and Yanagizawa-Drott 2020; Andreoni,

[7] Lab studies with university students that manipulate endowments or earned income to study their influence on cheating show mixed results (Bogliacino and Montealegre 2020; Bogliacino, Charris, Gómez, and Montealegre 2024).

[8] The focus on the Covid-19 crisis relates the paper to lab-in-field experiments that have explored its influence on other behaviorally revealed preferences. While the existing work has focused on time discounting, risk aversion and social preferences (Li, Hong, Huang, and Tam 2022; Terrier, Chen, and Sutter 2021; Bartoš, Bauer, Cahlíková, and Chytilová 2021), this paper studies aversion to cheating. Another difference is the focus on a low-income population that suffered a major economic shock but was largely unaffected in terms of physical health and social isolation. In contrast, the earlier work has mostly focused on the behavior of students or participants of online samples in wealthier countries during lockdowns. The findings support the view that the Covid-19 crisis's negative impacts on social behavior may be particularly profound among economically vulnerable populations, which links to Terrier, Chen, and Sutter (2021), who show that it amplified the prosocial gap between rich and poor households among high-school students in France.

Nikiforakis, and Siegenthaler 2017; Fehr and Gächter 2002; Herrmann, Thoni, and Gächter 2008). Thus, the findings support the view that aggregate economic crises can create conditions in which a mostly norm-obedient society could move into a new equilibrium characterized by a widespread rule-breaking, which may persist even after the original economic shock that triggered the shift in unethical behavior is over.

3. Conceptual Framework

To fix ideas, this section presents a simple framework that describes the mechanism underlying why a negative economic shock is predicted to increase prevalence of cheating behavior, and it illuminates why such effects may gradually increase over time during a crisis.

Setup. Individual i chooses in a Mind Game whether (i) to report honestly ($L_{i,t} = 0$) and give up a payment B, or (ii) to cheat ($L_{i,t} = 1$) in order to receive the payment B, by misreporting the side of the coin that they had previously chosen in their mind as the winning side. Let

- $Y_{i,t}$: income at time t (before, t=0, or during the crisis, t=1);
- B: stake size (low = 40 KSh, medium = 120 KSh, high = 200 KSh);
- $U(\cdot)$: concave utility of money, $U' > 0$, $U'' < 0$;
- $C(\tau_i, \bar{L}_t)$: psychological cost of lying, increasing in the individual's intrinsic preference for truth-telling, τ_i, and decreasing in the overall share of individuals perceived to be cheating in that time period, $\bar{L}_t$.[9]

Decision Rule. Cheating is chosen when

$$U(Y_{i,t} + B) - C(\tau_i, \bar{L}_t) \geq U(Y_{i,t})$$

Define the marginal utility gain

$$\Delta U(Y_{i,t}; B) = U(Y_{i,t} + B) - U(Y_{i,t})$$

[9] We build on insights from previous work regarding the existence of widespread, intrinsic preferences for honesty across many contexts (e.g., Gneezy, Kajackaite and Sobel (2018), Abeler, Nosenzo, and Raymond (2019)), as well as the importance of social learning in shaping individual social behavior, highlighting that many people are more prone to break social norms when they think others do so too or find it acceptable (e.g., Fehr and Gächter (2002), Herrmann, Thoni, and Gächter (2008), Andreoni, Nikiforakis, and Siegenthaler (2017), Bursztyn, Egorov, and Fiorin (2020), Bursztyn, González, and Yanagizawa-Drott (2020)). Variation in individuals' intrinsic preference for truth-telling τ_i can explain why specific people make different choices, but we do not manipulate it experimentally.

and the marginal cost $\Delta C(\tau_i, \bar{L}_t) = C(\tau_i, \bar{L}_t) - 0$. The individual cheats if $\Delta U(Y_{i,t}; B) \geq \Delta C(\tau_i, \bar{L}_t)$, with $\frac{\partial C(\tau_i, \bar{L}_t)}{\partial \bar{L}_t} < 0$.

Utility in a single period is

$$U_{i,t} = U(Y_{i,t} + L_{i,t}B) - L_{i,t}C(\tau_i, \bar{L}_t)$$

We interpret $\bar{L}_t$ as a belief that updates slowly through social observation.

Comparative Statics. The decision rule yields three immediate comparative-static insights.

First, concavity of the utility function implies that the incremental gain from cheating, $\Delta U(Y_{i,t}; B)$, is larger at lower income levels; all else equal, poorer individuals are therefore more likely to cheat.

Second, for any given $Y_{i,t}$ the marginal benefit increases monotonically with the stake size B, so raising the bonus in the experiment from 40 KSh to 120 KSh or 200 KSh mechanically increases the share of respondents for which the marginal benefits of cheating are larger than the marginal costs.

Third, because the psychological cost satisfies $\frac{\partial C(\tau_i, \bar{L}_t)}{\partial \bar{L}_t} < 0$, a higher perceived cheating rate $\bar{L}_t$ lowers the stigma of lying; as agents observe (or infer) greater dishonesty around them, the cost term falls and an ever-larger share of the population finds it optimal to cheat.

2.1 Effects of an Economic Shock on Cheating: Illustrating Mechanisms

The framework implies two potential mechanisms for how a large negative economic shock may increase the prevalence of cheating.

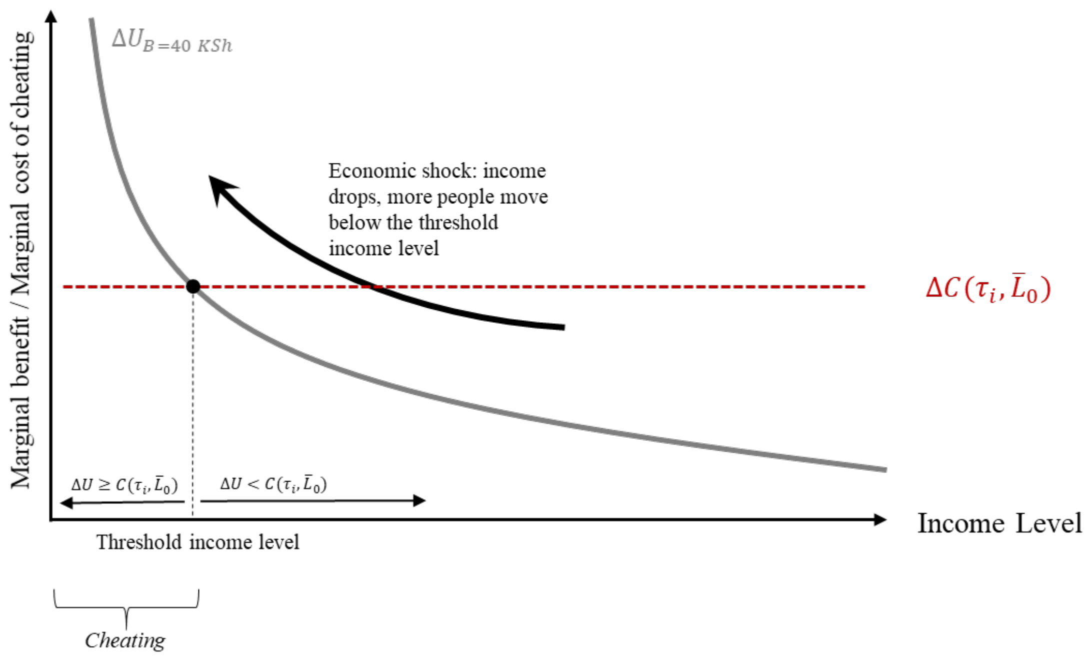
First, in Panel A of Figure 1, we illustrate that as income shrinks, the marginal utility gain from cheating increases, and more people end up below the threshold income level and thus cheat. The figure presents the utility benefits for a fixed stake size B (here at B=40 Ksh). It is straightforward to see that for a higher stake size, this curve would shift up and thus move the threshold income level to the right, implying a wider range of income levels at which individuals will choose to cheat (Panel B).

A second channel may operate through perceptions of social norms. If the actions of others shift towards more dishonesty, and people become aware of this, the individual psychological cost of cheating drops. In addition, if people revise their estimate of the prevailing dishonesty rate gradually, from an initial $\bar{L}_0$ toward successively higher $\bar{L}_t$ over time, this mechanism would imply a steady rise in reported cheating (say, during a crisis period). Because $\frac{\partial C(\tau_i, \bar{L}_t)}{\partial \bar{L}_t} < 0$, each upward revision in $\bar{L}_t$ lowers the psychological cost line, enlarging the range of income levels for which $\Delta U\left(Y_{i,t}; B\right) \geq C(\tau_i, \bar{L}_t)$, as illustrated in Panel C of Figure 1. Note that in our data, we do not measure beliefs directly, but we are able to document the dynamics of cheating behavior during the crisis.

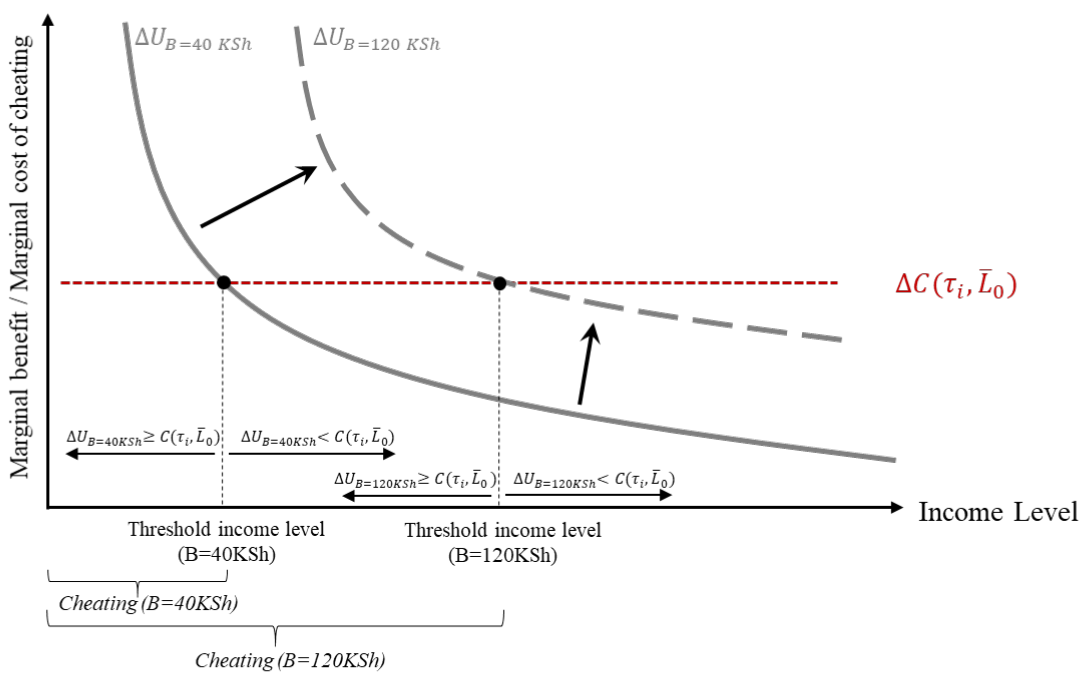
Finally, both mechanisms can operate in parallel: persistent economic strain increases the marginal utility of cheating while gradually shifting beliefs about others' honesty reduce its psychological cost. The process can be self-reinforcing: early defections reduce stigma, invite further defections, and honesty erodes gradually rather than in a single jump. Our data are consistent with this interpretation, though we cannot pin down the exact learning path without direct belief measures. We therefore view the framework as a heuristic that organizes the empirical evidence below, not as a fully specified model of belief formation.

Figure 1: Illustrating Mechanisms for How an Economic Shock May Affect Cheating Behavior

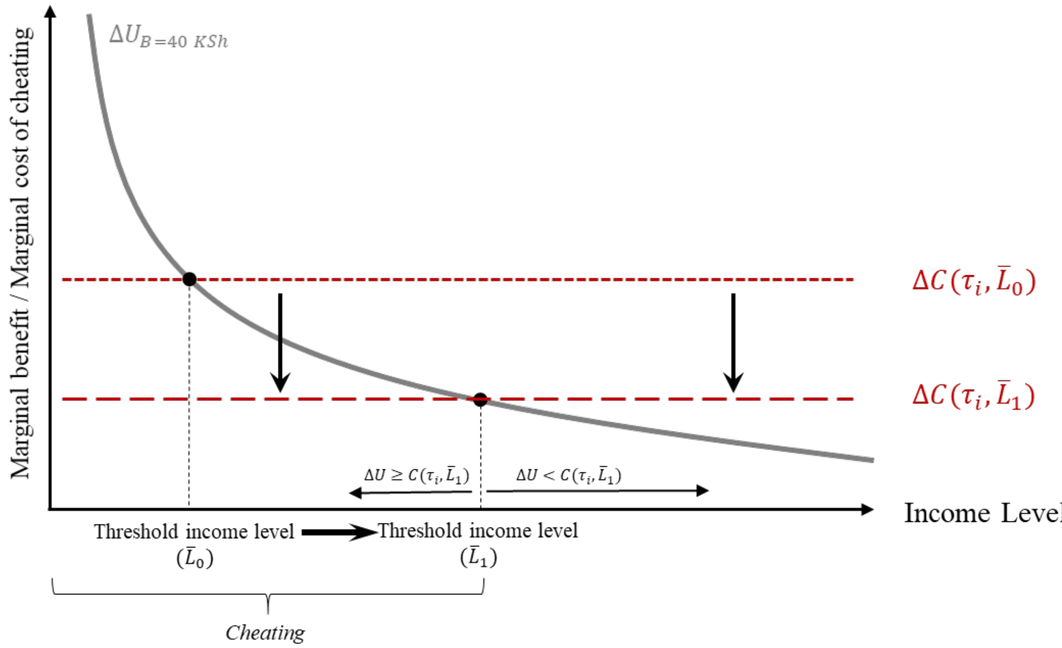
Panel A: Effect of Reduced Income



Panel B: Effect of Increased Stake Size



Panel C: Effect of a Shift in Beliefs about Others' Cheating Behavior



Notes: The figure provides a stylized representation of the cheating decision rule outlined in Section 3. The gray curve plots the marginal utility from cheating as a function of income Y for a given financial stake B; diminishing marginal utility of income implies that the gains from cheating are larger at lower income levels. The red dashed line plots the psychological cost of cheating, which depends on the perceived share of cheaters. The intersection of the gray curve and red dashed line defines a threshold income level separating income ranges in which cheating is and is not optimal. Panel A illustrates how an adverse income shock shifts individuals left on the income axis, increasing the share below the threshold (shown for B=40 KSh). Panel B illustrates that raising the financial stakes (going from B=40 KSh to B=120 KSh) shifts the marginal utility curve upward, moving the threshold income to the right and expanding the set of incomes at which cheating is optimal. Panel C illustrates that if people perceive cheating to be more prevalent, the psychological cost line shifts down, likewise moving the threshold to the right.

4. Experimental Design

4.1. Background

While the global effects of the Covid-19 pandemic were unprecedented, their nature varied substantially across countries. In high-income settings, the most acute challenges emerged from pressures on health systems and the disruptions caused by strict lockdowns. The numbers of recorded cases and deaths per capita were generally higher in high-income than in low- and middle-income countries (LMICs), which can be explained – besides via better testing and public health reporting – by demographic differences, specifically by the fact that richer countries have proportionally much larger elderly populations, which were especially vulnerable to Covid-19 mortality (Miguel and Mobarak 2022).

By contrast, in poor countries, the pandemic unfolded primarily as an economic crisis. Multiple surveys confirm the widespread nature of economic distress in LMICs during the pandemic when many households were exposed to dramatic income and consumption shocks. A cross-country study using data from Ethiopia, Malawi, Nigeria, and Uganda by Josephson, Kilic, and Michler (2021) estimated that approximately 77% of people in low-income settings lived in households that suffered income declines during the pandemic, with cascading effects on food security, savings, and household resilience. Egger, Miguel, Warren, Shenoy, Collins, Karlan, Parkerson, Mobarak, Fink, Udry, et al. (2021) further underscore this pattern using representative samples across 16 populations in nine LMICs spanning Africa, Asia, and Latin America. Their survey, covering almost 30,000 respondents and representing combined country populations of nearly 500 million, documents severe declines in living standards: between 8% and 87% of respondents reported income drops during the crisis period, with a median of 70% across samples. Although methodologies varied, the picture of widespread hardship is clear. At the same time, international remittances – a key income stream for many households – fell sharply due to the parallel crisis in rich countries.

The picture was no different in Kenya, the study setting. By June 2020, nearly 40% of Kenyan households reported a job loss or significant drop in earnings, with urban areas hit hardest (Nafula, Kyalo, Munga, and Ngugi 2020). Urban residents, who are often dependent on informal self-employment and lack access to home-produced food, had far fewer buffers than rural households, who could often rely on subsistence agriculture and informal family networks in their home region. The scope of formal relief programs was also very limited. A small cash transfer program reached less than 3% of the Kenyan population and covered only a fraction of lost income: the burden of adjustment largely fell on households themselves via depleted savings, asset sales, or taking on debt to survive. In neighboring Uganda, Alfonsi, Bassi, Rasul, and Spadini (2025) document similar pattern, including employment losses among urban workers, and find that even two years later, earnings remained 34% below trend for skilled workers and 45% below trend for their unskilled peers.

The data from the KLPS sample also clearly document negative impacts of the Covid-19 crisis in the economic domain – including sharp drops in household income of approximately 50% (more details in Sections 6.1 and 7.3), declines in employment, and worsened perceptions of personal economic situation – while showing no systematic negative effects in the health domain (i.e., symptoms of illness, hospital visits, perceptions of own health). In sum, Covid-19 in Kenya was a multidimensional crisis, but for most households the most salient experience was one of economic insecurity, food shortages, and disrupted daily life, with particularly large effects in urban areas.

4.2. Study Sample

The analysis employs data from Round 4 of the Kenya Life Panel Survey (KLPS-4), a large-scale longitudinal study that has tracked the same participants for roughly two decades. The sample consists of individuals who, as children, participated in the Primary School Deworming Program and the Girls' Scholarship Program. KLPS-4 targets the original deworming sample and a subset of scholarship recipients who later enrolled in a vocational education and cash-grant program, using a two-stage tracking method (Miguel and Kremer 2004; Kremer, Miguel, and Thornton 2009; Hamory, Miguel, Walker, Kremer, and Baird 2021). In addition to choices in the Mind Game (our main outcome), the survey includes detailed economic, health and other modules, which we use to explore alternative mechanisms.

The study sample comprises 5,664 respondents who made a Mind Game decision. The average respondent is ~34 years old (range 25–43); 56% are women; 44% attended secondary school; 88% were born in Busia County (the location of the two original randomized controlled trials); 93% identify as ethnically Luhya and 92% as Christian. Residence in the twenty-year follow up in KLPS-4 is roughly evenly split between rural and urban areas, due to extensive migration over time (for schooling, work, and family).

To assess external validity, we benchmark KLPS households against the 2021 Kenya Continuous Household Survey (KCHS), which covers ~68,000 individuals in ~17,000 households nationwide. KCHS household earnings aggregate income from main, secondary, and casual jobs, using a comparable definition to KLPS. Because many KCHS respondents report income in ranges, we construct two measures: one replacing ranges with their midpoints and one using lower bounds. Converting all values to 2017 USD PPP, median household earnings per capita are $17.60 and $12.32 per month, respectively. The two distributions exhibit substantial overlap (Figure A.1), indicating broad common support between KLPS and the national income distribution. Byambaa et al. (2025) similarly show that occupational structures are closely aligned across the two datasets.

4.3. Measuring Dishonesty: The Mind Game

To identify dishonest behavior, we implemented a modified version of the Mind Game (Greene and Paxton 2010; Kajackaite and Gneezy 2017; Lowes, Nunn, Robinson, and Weigel 2017; Abeler, Falk, and Kosse 2025; Jiang 2013), a well-established and broadly validated task, designed to cleanly identify prevalence of dishonest behavior. The respondents were shown five 40-KSh coins on a tablet computer (Figure A.2). They were told that each of the coins displayed either heads or tails, but at the moment they did not know which side the coins displayed – that would be revealed to them later. They were further informed that they could choose which side would be the winning side (either heads or tails) and that after revealing the coins, for each coin displaying the winning side they would receive KSh 40. They were asked to make their choice of the winning side *in their mind* and not to tell their choice to the enumerator. They reported their choice only after the coins were revealed. The choices were incentivized: each participant received an amount between KSh 0 and 200 (USD 2), based on what they reported as the winning side and on the outcome displayed by the five coins. The choices were highly consequential, given that KSh 200 represents approximately 1.5 times average daily household per-capita earnings in the sample.

Since participants reported their choice only after they saw the revealed coins, they could lie about the winning side they had earlier chosen in their mind in order to increase their payoff. Importantly, because they made their choice only in their mind, it is impossible to detect at the individual level whether they reported their choice truthfully or whether they lied about it. At the same time, we can detect cheating at the group level by comparing the reported share of successful outcomes with the 50% benchmark implied by truthful reporting: the higher is the reported winning rate above 50%, the higher is the prevalence of cheating. Furthermore, assuming that all participants reported their true winning side when it implied higher earnings, the prevalence of cheating can be estimated by doubling the difference between reported winning rate and 50%. In the analysis, we will mostly report the winning rate, i.e., the share of respondents who reported that they had chosen the side of the coin which implied a higher payoff in their mind. In some figures, we will display both the winning rate and the estimated prevalence of cheating.

The observed patterns indicate that the level of understanding of the task was very high: the share of reporting the payoff maximizing winning side increases with financial incentives (which we describe in greater detail below) and this pattern holds also for sub-samples of participants with lower cognitive skills and with lower education levels. After receiving the instructions and before they made their choice, when directly asked, 99.8% of respondents confirmed they understood the instructions.[10]

4.4. Manipulating Financial Incentives to Misreport

By designing the Mind Game with six possible coin-toss outcomes, we are able to experimentally manipulate financial incentives to misreport, creating three distinct stake levels. When the coins were revealed, the computer displayed a random draw from six possible options: 0 tails, 1 tail, 2 tails, 3 tails, 4 tails and 5 tails, so that each participant faced one of three levels of financial incentives to cheat. In the *High Stake* condition, all five coins displayed the same side (either heads or tails), and the financial incentives to lie were KSh 200, since the respondent could earn either KSh 200 or KSh 0, depending on their report of the winning side. In the *Medium Stake* condition, four coins displayed the same side, creating an incentive of KSh 120 because participants could earn either KSh 160 or KSh 40. Finally, in the *Low Stake* condition, three coins displayed the same side, yielding a financial incentive of KSh 40, with potential earnings of either KSh 120 or KSh 80.

This random variation allows us to causally estimate how cheating responds to financial incentives. Specifically, we compare the likelihood of reporting the pay-off maximizing size of the coin across the three stake levels: low (KSh 40), medium (KSh 120) and high (KSh 200). Manipulating financial incentives in this way allows us (i) to test whether participants' cheating decisions systematically respond to financial incentives, and (ii) to benchmark the effect of the Covid-19 crisis on misreporting relative to the effects of financial incentives themselves.

4.5. Natural Experiment: Identifying the Effects of Crisis

The KLPS-4 round, like earlier rounds of the Kenya Life Panel Survey, was implemented in two waves of data collection. Assignment of individual respondents between the two waves was randomized and had been determined at baseline nearly two decades before. KLPS is an extensive survey which consists of several modules implemented separately at different times. The Mind

[10] Note that confusion in this task would likely lead to random or arbitrary choices of the winning side, while we see that many people chose the pay-off maximizing side.

Game was a part of the I Module in KLPS-4. By chance, Wave 1 of the I Module was completed just before the onset of the pandemic (in February 2020), while Wave 2 resumed only after lockdowns were lifted (in October 2020). Thus, the random wave assignment provides a credible source of exogenous variation in whether the respondents made choices in the Mind Game just before or during the Covid-19 pandemic. This creates a natural experiment, which allows us to estimate the causal effect of the crisis on decision-making. We label Wave 1 *Before Crisis* and Wave 2 *During Crisis*. See Figure A.3 for more details on the study timeline.

Because the assignment to waves occurred long before the crisis, comparisons across waves are not confounded by selection. In Table A.1, we report comparison of observable characteristics of participants in the *Before Crisis* and *During Crisis* waves. Given that the data were collected one year apart, we focus on variables that do not change over time: gender, age in 2020, parental education, ethnicity, being born in Busia, education level and earnings before the Covid-19 crisis. Reassuringly, we find that most of the variables are balanced, and only parental education is slightly higher for the *During Crisis* wave than for the *Before Crisis* wave, but the magnitude of the difference is small.

4.6. Priming Experiment: Manipulating the Salience of One's Own Financial Situation

We complement the natural experiment with a priming experiment embedded in the survey, in order to exogenously manipulate the salience of respondents' financial situation.[11] This allows us to explore whether the salience of one's own financial situation creates a moral license to cheat, especially during the crisis, outside of "normal" times. Specifically, we randomized which set of questions preceded the Mind Game. In the *Financial Situation Salient* condition, participants first answered a set of questions about their savings and borrowing. These questions may activate thoughts about one's own financial situation – and during a period of hardship, also thoughts of unusually severe financial difficulties – which in turn may provide a psychological license to cheat.

[11] Priming is a widely used method in psychology, and more recently also in economics. It refers to a mental activation of primed concepts and it allows researchers to identify the effects of greater salience of the primed concept. Priming has been employed to study, for example, the effects of a business cycle, recollections of violence, or thoughts about financial difficulties (Mani, Mullainathan, Shafir, and Zhao 2013; Cohn, Engelmann, Fehr, and Maréchal 2015; Callen, Isaqzadeh, Long, and Sprenger 2014; Bartoš, Bauer, Chytilová, and Levely 2021). For a comprehensive review of advantages and limitations of this method, see Cohn and Maréchal (2016).

One important advantage of this approach to increasing salience is that it is relatively subtle and natural. Given that we only vary when subjects are asked standard financial questions, experimenter demand effects are unlikely to confound results. At the same time, this priming technique allows us to identify the marginal, not overall, effect of thinking about one's financial situation, because participants in the *Control* condition might have also thought about their financial situation to some extent.

For comparison, we use a similar approach to increase salience of other issues that may affect decision-making in the Mind Game. In the *Politics Salient* condition, the set of questions right before the Mind Game focused on election participation, knowledge of political leaders, perceptions of democracy and quality of the government, in the *Cultural Identity Salient* condition the questions focused on religious and ethnic identities (e.g., church attendance, religious denomination and practices, relative importance of religion, ethnicity or other group identities), and in the *Control* condition the module did not focus on any specific issue, but included a set of questions aiming to measure economic preferences.[12]

5. Results on the Role of Financial Incentives and Income

If everyone were telling the truth in our experiment, we would expect 50% of participants to report the pay-off maximizing side of the coin, i.e. the "winning side." If, instead, there was no preference for truth-telling, we would expect 100% of participants to report the winning side. Overall, we find that 79% of participants in our sample chose to report the pay-off maximizing outcome. Under the plausible assumption that participants do not misreport in order to lower their earnings, the estimated prevalence of lying in our sample is 58%. This estimate is substantially larger than those reported in the meta-study of Abeler, Nosenzo, and Raymond (2019). Interestingly, this seems largely due to differences in economic conditions and financial incentives. The estimated cheating rate for the sub-sample of participants who made choices in normal circumstances (i.e., *Before*

[12] In addition, we experimentally manipulated the salience of cheating to oneself. Specifically, we implemented two versions of the Mind Game, using a between-subject design. In the *Cheating Salient to Self* condition, participants were asked to make a choice of the winning side in their mind and, in addition, to keep a 40-KSh coin in their fist with the chosen side facing up. After the coins were revealed, they were asked to check in private the winning side by looking at the coin in their fist before reporting their choice. In the *Cheating Not Salient to Self* condition, the standard protocol was followed. The main patterns reported in the paper – effects of financial incentives and of the crisis – hold for both conditions.

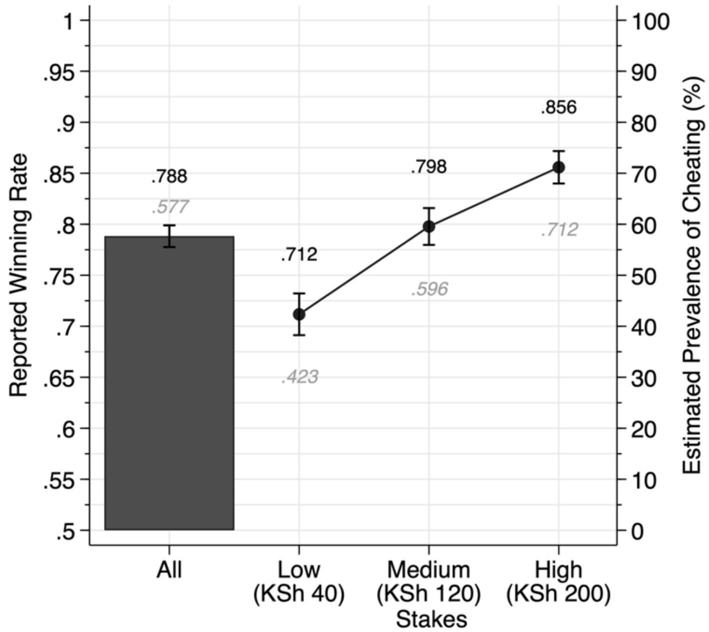
Crisis) and in the *Low Stake* condition (which is most comparable to earlier work, given the relatively high financial stakes used in this paper) is 29% (N = 994), while the average cheating rate estimated in the meta-study is 23%. To motivate our focus on the causal role of economic circumstances, in this section we establish that cheating behavior systematically responds to financial incentives and that poorer participants cheat systematically more than richer participants.

5.1. Financial Incentives

We find that participants respond to financial incentives, and thus provide direct evidence that people consider economic trade-offs when deciding about whether to cheat. To test this, we exploit the exogenous variation in financial incentives to misreport within the survey. In Figure 2, we show that the winning rate is 71.2% when the financial costs of truth-telling are relatively low (KSh 40); that is, when respondents can gain relatively little from misreporting outcomes. The winning rate steeply increases to 79.8% when incentives are KSh 120, and grows further to 85.6% when incentives are KSh 200. These differences are highly statistically significant (p-value<.001) and robust to controlling for variety of background characteristics and design features (Columns 1-3 of Table 1 and Figure A.4).

The effects are also relatively large in magnitude. The estimated prevalence of cheating increases by almost 30 percentage points, from 42% to 71% (Figure 2). Put differently, increasing the size of financial costs of truth-telling by KSh 100 increases the prevalence of lying by 18 percentage points (Columns 5-6, Table 1).

Figure 2: Effects of Financial Incentives on Cheating



Notes: The bar represents the reported winning rate aggregated over all stakes of truth-telling, while the line indicates the reported winning rate at respective stakes of truth-telling. Error bars represent 95% confidence intervals of the reported winning rate. Black numbers in the figure indicate the reported winning rate, gray italics numbers indicate the estimated prevalence of cheating. Sample sizes are N = 5,664 overall, and 1,887, 1,904, and 1,873 for the low-, medium-, and high-stake conditions, respectively.

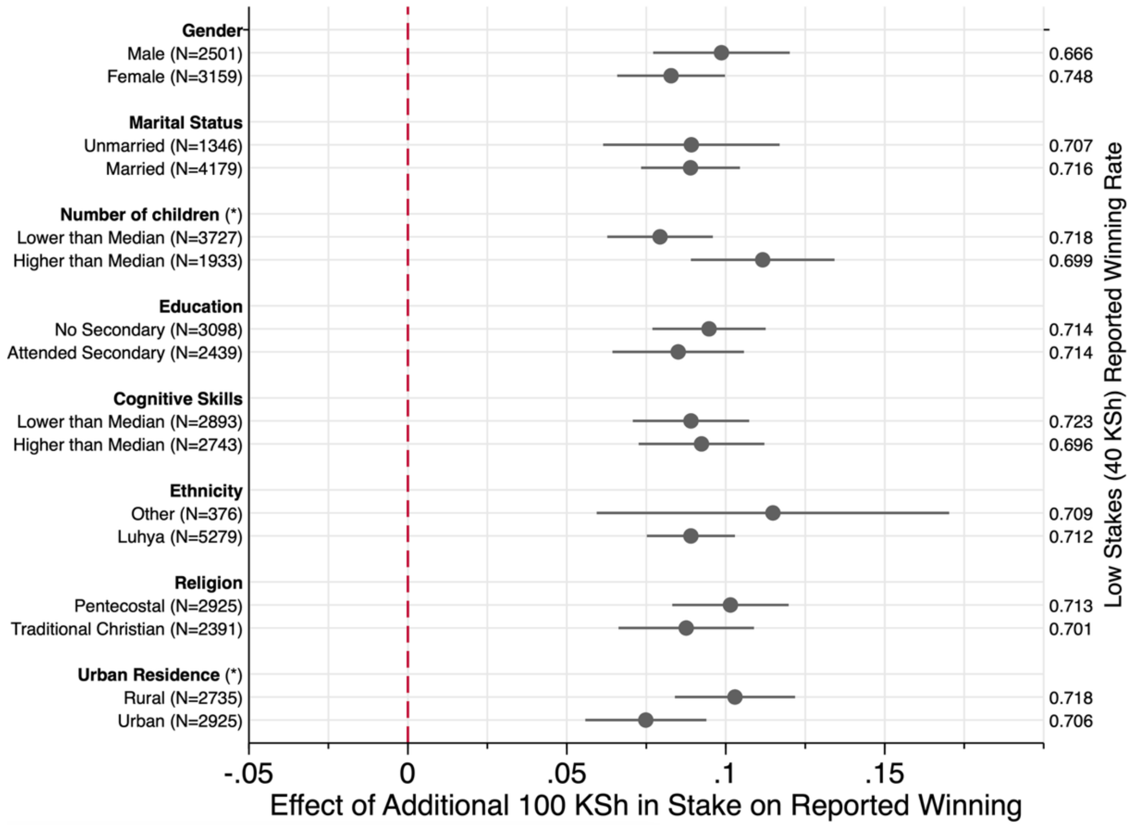
Table 1: Effects of Financial Incentives on Reported Winning Rate

	Effect of Stakes Assignment			Effect of Numeric Amount of Stake		
	(1)	(2)	(3)	(4)	(5)	(6)
High Stakes (Ksh 200)	.144***	.144***	.145***			
	(.013)	(.013)	(.013)			
Medium Stakes (Ksh 120)	.0865***	.0861***	.0894***			
	(.014)	(.014)	(.014)			
Stakes in hundred KSh				.0900***	.0898***	.0904***
				(.008)	(.008)	(.008)
Observations	5660	5660	5660	5660	5660	5660
Low Stakes (KSh 40) Mean	.712	.712	.712	-	-	-
Survey Manipulations	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes

Notes: OLS coefficients. The dependent variable is an indicator equal to 1 if the respondent reports the winning side of the coin. In Columns 1 and 4, there are no controls. In Columns 2 and 5, we control for the following survey manipulations: Financial Situation Salient, Politics Salient, Cultural Identity Salient (Control omitted), and Cheating Salient to Self (Cheating Not Salient to Self omitted). In Columns 3 and 6, we control for the set of survey manipulations (as in Columns 2 and 4) and for the following set of controls: gender, age, education, ethnicity, marital status, number of children, number of siblings, living in urban area, survey wave, survey month (1-12), gender of enumerator, school grade at baseline, indicator for PSDP treatment, indicator for GSP treatment, indicator for SCY treatment, indicator for VOCED treatment. Standard errors in parentheses. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Next, taking advantage of the size and diversity of the sample, we document that the estimated sensitivity of dishonest behavior to financial incentives holds across various social, demographic and cultural sub-groups, rather than being driven by some narrowly defined group. In Figure 3, we divide the sample based on a range of individual characteristics and estimate the effects of financial incentives separately for each group. The estimated sensitivity to the costs of truth-telling is remarkably similar across sub-groups and not meaningfully related to gender, religion, education, ethnicity, or cognitive skills, with some apparent differences related to the number of children and urban residence, although neither is significant at the traditional 95% level.

Figure 3: Effects of Financial Incentives on Cheating: Robustness across Demographics



Notes: Dots represent OLS coefficients of the probability of reporting the winning side of the coin on numeric stake (per 100 KSh), estimated separately by subgroup. The regressor takes values 0.4, 1.2, and 2 (Low, Medium, High Stakes) and is treated as continuous. Confidence intervals are at the 90% level. Stars (if any) following the variable names indicate the significance of the difference in the effects of financial incentives across the two groups in that dimension of heterogeneity. ∗ $p < 0.1$, ∗∗ $p < 0.05$, ∗∗∗ $p < 0.01$.

5.2. Relationship Between Income and Cheating

Next, we test whether misreporting is positively linked with having a lower income. We take advantage of the detailed KLPS survey module on earnings to construct total per-capita household earnings. Use of a detailed module is particularly important in low-income, agricultural settings, in which people often have multiple occupations and income sources, and relying on a simple question on total income could be prone to measurement error. The household per-capita earnings measure is calculated as the sum of earnings from agriculture, job earnings and self-employment profits of the household, divided by the number of household members.[13]

---

[13] Earnings were measured at a different point in time than behavior in the Mind Game. While the Mind Game was included in the I Module, earnings were measured earlier in different parts of KLPS 4 data collection. In Wave 1, they were measured in the E+ Module and in Wave 2 in a phone survey which took place in early to mid-2020 when in-

We find that poorer respondents are more prone to cheat than richer participants. In Table 2, we show that having a lower income is systematically associated with a greater prevalence of reporting the winning side of the coin and this relationship is robust across various specifications with different sets of control variables (Panel A, Columns 1-3), including gender, education, and other socio-demographic characteristics. The relationship is similar when estimated separately for different levels of stakes (Panel B), as well as for sub-samples of individuals who were surveyed *Before Crisis* (Columns 4-6 of Panel A) and *During Crisis* (Columns 7-9 of Panel A).

Table 2: Relationship Between Income and Reported Winning Rate

	Panel A — By Wave								
	All			Before Crisis			During Crisis		
Earnings (in 10k KSh)	-.051***	-.051***	-.036***	-.035**	-.035***	-.028*	-.033*	-.035**	-.051***
	(.011)	(.010)	(.011)	(.014)	(.013)	(.014)	(.017)	(.017)	(.017)
Observations	4994	4994	4994	2318	2318	2318	2676	2676	2676
	Panel B — By Stake								
	High Stake			Medium Stake			Low Stake		
Earnings (in 10k KSh)	-.050***	-.048***	-.022	-.036**	-.036**	-.029	-.073***	-.074***	-.061***
	(.017)	(.016)	(.017)	(.017)	(.016)	(.018)	(.021)	(.021)	(.022)
Observations	1651	1651	1651	1696	1696	1696	1647	1647	1647
Survey Manipulations	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes	No	No	Yes

Notes: OLS coefficients. The dependent variable is an indicator equal to 1 if the respondent reports the winning side of the coin. "Earnings" refers to monthly household per-capita earnings, measured in 10,000 2017 KSh; the top 1% of the earnings distribution is trimmed. Panel A reports results by survey wave; Panel B by stake level. Within each panel, triplets of columns report (i) no additional controls, (ii) survey-manipulation controls only, and (iii) the full set of controls (as specified in Table 1). Standard errors in parentheses. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

6. Results on the Effects of the Crisis

In this section, we take advantage of the random assignment of participants into two waves of data collection. By coincidence, the Covid-19 crisis started shortly after completion of Wave 1 of KLPS-4 data collection (and before Wave 2 had begun), and data collection for Wave 2 took place during the crisis. We will refer to participants in Wave 1 as the *Before Crisis* sample and participants in Wave 2 as the *During Crisis* sample.

person interviews were not possible. Importantly, the comparison of earnings across waves allows us to estimate differences before and during the crisis. Figure A.3 displays the timeline of the data collection.

6.1. Economic Impacts

We find that the Covid-19 crisis caused a severe negative income shock for the households we study (Table 3). Average monthly household per-capita earnings dropped by 38%, from KSh 4,494 before the crisis to KSh 2,773 during the crisis.[14] The median dropped by 51%, from KSh 2,000 to KSh 984. This difference represents a profound income drop, especially considering the length of the covered crisis period, which implies that for a long period of time households were earning only slightly more than half of what they earned before the crisis. In line with this observation, we find drops in the likelihood of having any savings and in individual perceptions of own socio-economic status (Columns 3, 4, 7 and 8 of Table 3)[15]. We do not detect effects on the likelihood of borrowing (Columns 5 and 6). Additional data collected only during the crisis period further illuminates the extent of economic hardship faced by the participants: one half of them reported being worried about food during the week preceding the interview (not shown).

[14] Total per-capita earnings are calculated as the sum of earnings from three sources: wage earnings, profits from self-employment and profits from agricultural activities. Interestingly, we observe substantial drop for the average of all three components of total earnings. In particular, wage earnings dropped by 41%, profits from self-employment by 38% and profits from agricultural earnings by 21%. At the same time, we acknowledge that specific wording of questions in the E+ module (measuring earnings before crisis) is not identical with somewhat simpler wording in the phone survey (measuring earnings during the crisis). This is mainly the case for agricultural earnings. Reassuringly, we find that the earnings drop is mainly driven by the two components for which the wording is essentially comparable (self-employment and wage earnings). The observed pattern is also in line with the observation that the earnings drops were particularly profound for the urban sub-sample, relative to the rural sample.

[15] Perceptions of socio-economic status were measured using the MacArthur Ladder. The respondents were asked to place themselves on a ladder with ten rungs, on top of which are people who are the best off (those who have the most money, most education, and the best jobs) and at the bottom of which are people who are the worst off.

Table 3: Effects of Crisis on Economic Outcomes

	Earnings		Has Savings Activity		Has Borrowing Activity		Current place on MacArthur Ladder	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Effect of Crisis							
During Crisis	-.193***	-.168***	-.054***	-.080***	-.006	-.031**	-.518***	-.527***
	(.017)	(.020)	(.013)	(.015)	(.012)	(.014)	(.060)	(.072)
	Panel B: Effect of Crisis by Urban Residence							
During Crisis	-.117***	-.089***	-.011	-.034*	-.008	-.038*	-.287***	-.343***
	(.018)	(.022)	(.018)	(.020)	(.019)	(.020)	(.095)	(.104)
Urban	.340***	.271***	.070***	.053***	.104***	.079***	.341***	.257***
	(.028)	(.026)	(.018)	(.018)	(.018)	(.018)	(.091)	(.093)
During Crisis × Urban	-.108***	-.150***	-.078***	-.087***	.027	.013	-.422***	-.349***
	(.033)	(.034)	(.025)	(.025)	(.025)	(.025)	(.121)	(.123)
Observations	4994	4994	5659	5659	5658	5658	5655	5655
Before Crisis Mean	.454	.454	.694	.694	.693	.693	4.865	4.865
Before Crisis Mean (Rural)	.270	.270	.655	.655	.634	.634	4.673	4.673
Before Crisis Mean (Urban)	.610	.610	.724	.724	.738	.738	5.014	5.014
Treatment Effect (%)	-42.403	-36.894	-7.776	-11.591	-.801	-4.415	-10.646	-10.835
Treatment Effect (Rural, %)	-43.233	-33.021	-1.634	-5.254	-1.258	-5.939	-6.145	-7.335
Treatment Effect (Urban, %)	-36.847	-39.183	-12.249	-16.777	2.537	-3.291	-14.150	-13.793
Survey Manipulations	No	Yes	No	Yes	No	Yes	No	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes

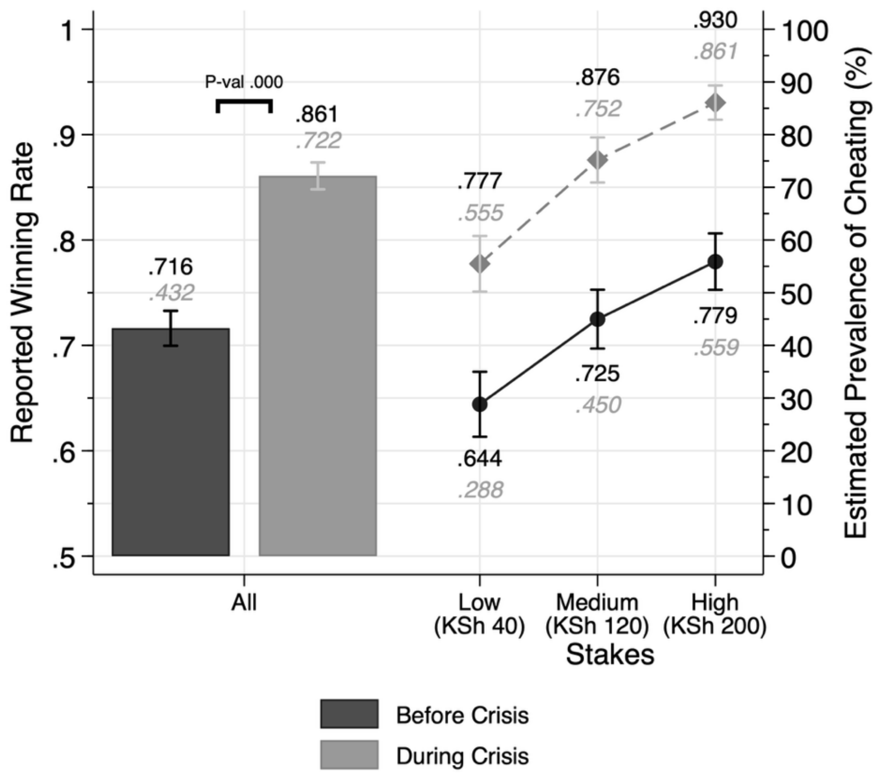
Notes: OLS coefficients. Dependent variables are: Earnings (monthly household per-capita earnings, measured in thousand 2017 KSh; top 1% of the earnings distribution is trimmed), Has Savings Activity (indicator), Has Borrowing Activity (indicator), and Current place on MacArthur Ladder (1–10, higher = higher perceived socio-economic status). Panel A reports average effects of being surveyed during the crisis. Panel B adds an Urban indicator and its interaction with During-crisis; Urban refers to residence at the time of data collection. We report pre-crisis means by rural/urban and percentage treatment effects computed relative to the corresponding pre-crisis mean. Within each outcome, columns alternate between a specification without additional controls and one including the full set of controls (including survey-manipulation controls). Standard errors in parentheses. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

In contrast, in Table A.2, we show that the Covid crisis did not lead to a deterioration in reported physical health. This is not surprising given that our sample are middle-aged individuals, i.e., an age group that was relatively resilient to severe Covid infections (at least compared to the elderly). KLPS data provide rich information on the health status of participants and includes questions about symptoms related to specific diseases, the number of hospital visits, self-reported questions on general health, the number of days that a participant had to skip work due to health issues and whether they experienced a major health issue recently. Across all these measures we do not find that participants interviewed during the crisis had worse health than those interviewed before the crisis (Table A.2). If anything, reported health seems somewhat better during the crisis than before. In Section 7.3 we use other survey data and more comprehensively consider alternative channels through which the crisis could, in principle, affect cheating behavior, in addition to the economic channel that is our focus.

6.2. Effects on Cheating

We find that respondents are far more likely to act dishonestly during the Covid-19 crisis. In Figure 4, we show that the prevalence of choosing the winning side of the coin was 71.6% before the crisis, while it increased to 86.1% during the crisis (p-value < 0.001). This difference in behavior is large in magnitude. The estimated prevalence of cheating increased by 30 percentage points, from 43% to 72%. An alternative way to get a sense of the effect size is to benchmark the impacts of making decisions during the crisis with the effects of financial incentives provided experimentally during the survey. The results reveal that the crisis effects are quite comparable to increasing the financial incentives to misreport fivefold (or by an amount that is 121% of the average – and 327% of the median – daily per-capita household earnings in our sample) which led to an increase in the prevalence of cheating from 42% to 71%.

Figure 4: Effects of Crisis on Cheating



Notes: Bars represent the reported winning rate aggregated over all stakes, while lines indicate the reported winning rate at the corresponding stake level. Error bars represent 95% confidence intervals. Black numbers indicate reported winning rates; gray italic numbers indicate estimated prevalence of cheating. We report homoskedastic standard errors for confidence intervals and p-values. Sample sizes are N = 5,664 overall, and 1,887, 1,904, and 1,873 for the low-, medium-, and high-stake conditions, respectively.

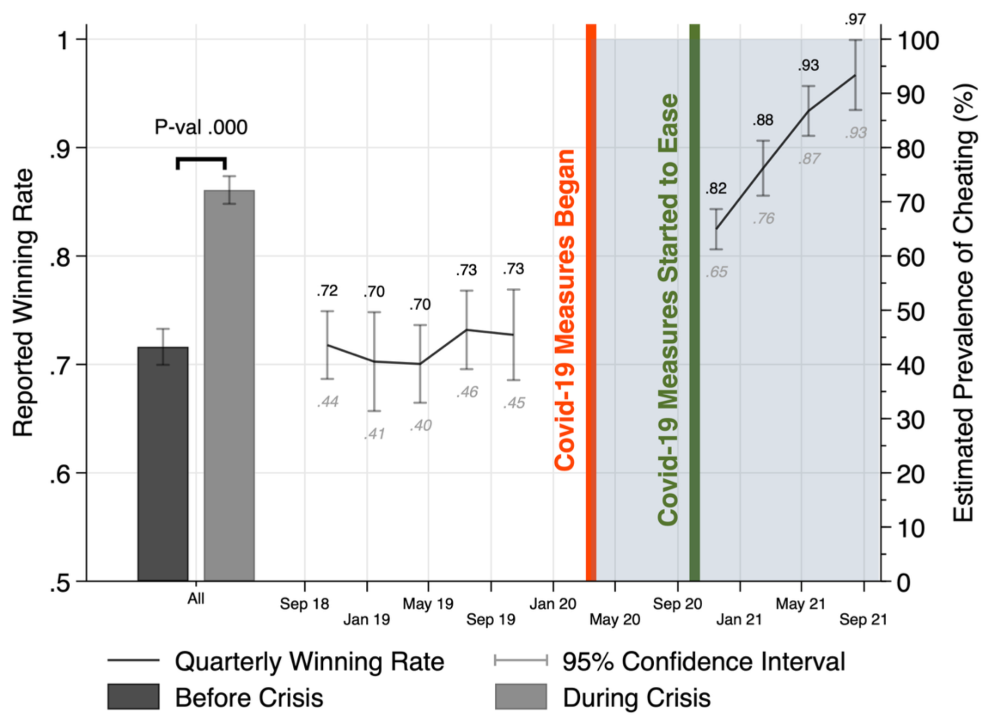
Next, we report several robustness tests. First, the crisis impacts are robust across a range of financial incentives to cheat. The estimated effects are similar and highly statistically significant when the financial costs of misreporting are KSh 40, KSh 120 and KSh 200 (Figure 4 and Table 4). This suggests that hardship reduces honesty in range of situations. Second, the before-during crisis difference in the prevalence of cheating is robust to controlling for a host of characteristics. In Figure A.5, we sequentially control for a range of covariates that are unlikely to be changed by the crisis, including gender, age, cognitive skills, education, pre-Covid earnings and other control variables. Next, we control for the priming manipulations integrated into the survey (described above) and past interventions (i.e., deworming). Third, to address concerns that the behavioral differences may be due to the seasonality of income, festivals or other seasonal events that could affect honesty, we show that the estimated effect is also robust to controlling for month fixed effects. In addition, in Figure 5 (on dynamics), we show that the prevalence of reporting the winning side of the coin was remarkably stable during the whole year of data collection prior the Covid-19 crisis, indicating that the difference in behavior is unlikely to be due to a general secular trend of increasing dishonesty over time.

Table 4: Effects of the Crisis on Reported Winning Rate

	All			Low Stakes (Ksh 40)		Medium Stakes (Ksh 120)		High Stakes (Ksh 200)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
During Crisis	.145***	.141***	.157***	.133***	.150***	.151***	.171***	.151***	.159***
	(.0107)	(.0109)	(.0124)	(.0207)	(.0250)	(.0179)	(.0208)	(.0160)	(.0185)
	[.134, .149]			[.131, .141]		[.135, .157]		[.150, .160]	
Lower Lee Bounds	***			***		***		***	
Upper Lee Bounds	***			***		***		***	
Before Crisis Mean	.716	.716	.716	.644	.644	.725	.726	.779	.779
Survey Manipulations	No	Yes	Yes	No	Yes	No	Yes	No	Yes
Controls	No	No	Yes	No	Yes	No	Yes	No	Yes
Observations	5664	5664	5660	1887	1887	1904	1903	1873	1870

Notes: OLS coefficients. The dependent variable is an indicator equal to 1 if the respondent reports the winning side of the coin. Columns (1)–(3) show results for the full sample; Columns (4)–(9) show results by stake level: Low Stakes (KSh 40), Medium Stakes (KSh 120), and High Stakes (KSh 200). Estimates are weighted for intensive tracking. Lee Bounds (upper and lower) are reported in brackets. Standard errors in parentheses. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Figure 5: Effects of the Crisis on Cheating: Dynamics

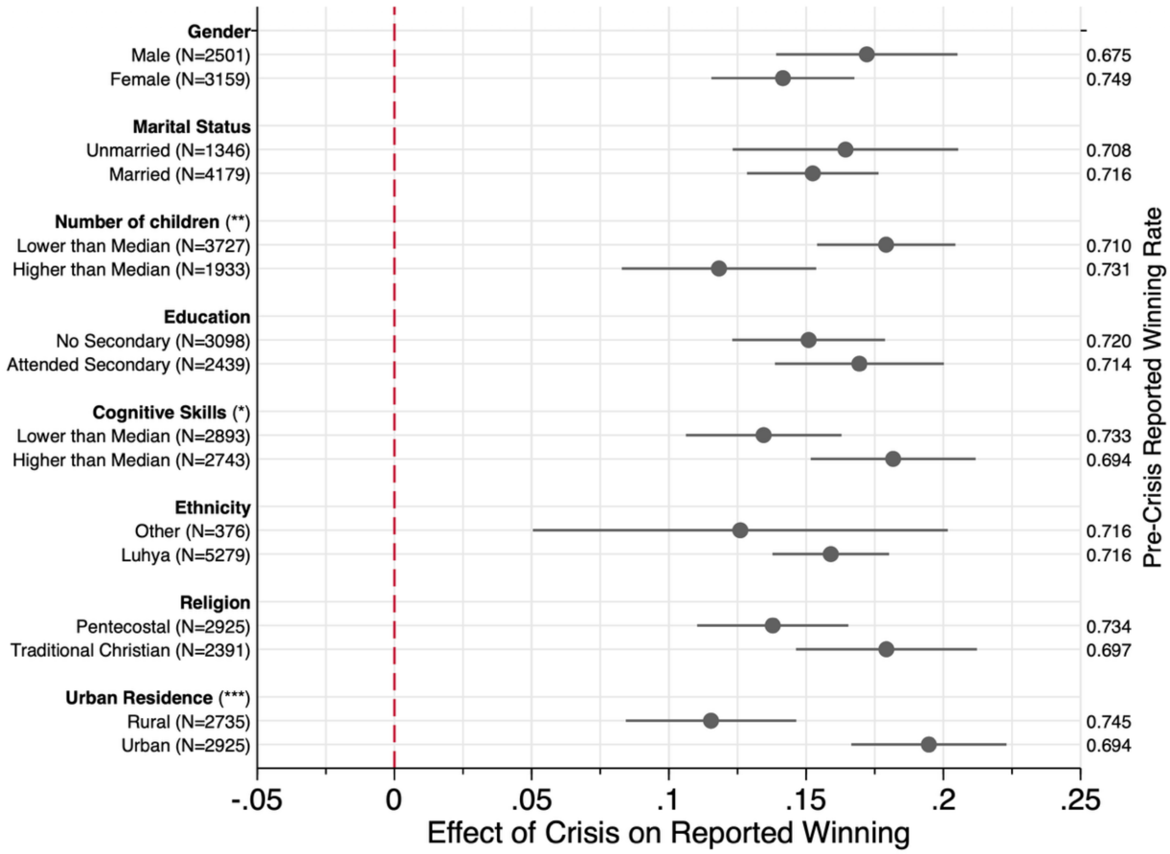


Notes: Winning rates are pooled by three-month intervals. Bars show the reported winning rate aggregated over all stakes; error bars represent 95% confidence intervals. Crisis = 1 for surveys conducted after March 2020. The final six months before Covid-19 (October 2019–March 2020) are merged because only 37 observations were collected in early 2020.

Next, we consider whether the results could be driven by selection of the people who participated in the KLPS survey before versus during the Crisis. Overall, participation rate in the KLPS panel is high, with over an 80% effective tracking rate in all rounds. In Table A.1, we find that differences in survey participation rates across periods is relatively small: the participation rate was 84.8% before the crisis and 80.9% during the crisis. Though such a difference seems unlikely to drive the main findings given the magnitude of the estimated effects, as a sensitivity test we perform Lee bounds, either assuming that all attritors would have reported winning or assuming that all attritors would have reported not winning (see Table 4). Even under these extreme assumptions about selection based on the dependent variable, we still find large effects. The lower bound of the estimated effects is a 13 percentage point increase in reported winning (implying 26 p.p. increase in the cheating rate) and it is highly significant statistically (p-value< 0.001). The upper bound is slightly higher at a 15 percentage point increase during the crisis (implying a 30 p.p. increase in cheating rate). In all, we conclude that slight differences in survey tracking rates are unlikely to meaningfully affect the results.

Finally, we test whether the before versus during crisis shift in dishonest behavior is found across various respondent characteristics. We find that the point estimates of the crisis effects are generally quite similar across socio-demographic subgroups (Figure 6). An exception is that crisis effects are significantly larger for people who lived in urban areas as compared to those in rural areas (significant at 99% confidence). We elaborate on this pattern below in sub-section 7.1 and link it with the observation that economic impacts of the crisis were substantially larger on average for people living in urban areas.

Figure 6: Effects of Crisis on Cheating: Heterogeneity



Notes: The dots represent coefficient for Crisis. We use the same regression specification as above, with Crisis being the independent variable and reported winning being the dependent variable. The confidence intervals are at 90% level. Stars (if any) following the variable names indicate the significance level of the difference in treatment effect across the two groups in that dimension of heterogeneity. ∗ $p < 0.1$, ∗∗ $p < 0.05$, ∗∗∗ $p < 0.01$.

6.3. Cheating Dynamics

In this sub-section, we explore the stability of the cheating rate before the crisis and the dynamics of the crisis effects. If the effects were driven only by an initial shock due to the new economic circumstances, followed by gradual adaptation to greater economic hardship, we might observe a convergence of the cheating rate over time back to its pre-crisis level. If, on the other hand, people respond more strongly to long-lasting economic deprivation, we could observe a gradual increase in the prevalence of dishonest behavior. Such an increase might get re-enforced if people became aware that cheating for one's own gain had become normalized during the crisis, reducing their own psychological reluctance to cheat even after economic circumstances largely return to normal.

To shed light on this question, we exploit the fact that the data collection lasted for 18 months before the crisis started, and for 13 months during the crisis period, allowing us to study its evolution in both periods over a similar timeframe. We did not monitor behavior shortly after Covid-19 hit (in March 2020), because the start of Wave 2 data collection had to be put on hold during the lockdown period. The data collection started almost immediately after the lockdown restrictions were lifted (see Figure A.3 that displays timeline of data collection and government policies in Kenya). This aspect implies that the effects on dishonesty that we estimate are unlikely to be due mainly to any frustration caused by social isolation related to lockdowns.

We find a clear pattern. Before the Covid-19 crises, the estimated prevalence of lying was around 40% and it is remarkably stable throughout the whole year (Figure 5 and Table A.3). This indicates that there was no clear time trend in the evolution of the cheating rate prior to the crisis. Furthermore, the observed stability of the cheating rate, for the sample as a whole (Figure 5) and for a sub-sample of farmers (Figure A.6), also suggests that regular seasonal income fluctuations are unlikely to have systematic influence in this sample, in line with previous work (Boonmanunt, Kajackaite, and Meier 2020; Aksoy and Palma 2019) that found no effects on dishonesty of expected short-term changes in liquidity associated with seasonal agricultural income.

In contrast, the prevalence of misreporting for one's own benefit gradually increased during the Crisis period. From approximately 43% pre-Crisis, it increased to around 65% in October 2020-December 2020 (50% higher relative to pre-crisis), 76% in January-March 2021 (76% higher relative to pre-crisis), 87% in April to June, and the estimated cheating rate reached more than 90% from July 2021 to end of the KLPS-4 data collection round (116% higher than pre-crisis). In the regression framework in Table A.3, we confirm that the prevalence of misreporting increases with number of moths since the start of the crisis, but we do not find any systematic time trend prior to the crisis.

Based on these patterns, we conclude that people's greater tendency to cheat is not a short-term, temporary response, followed by a return to pre-crisis levels. Instead, the patterns are consistent with the interpretation that the initial cheating increase was further amplified with the length of time that people were living in economic hardship.

7. Examining Channels

In this section, we discuss several additional tests that aim to shed light on the question of whether the effects of the Covid-19 crisis are primarily driven by increased economic hardship, or whether it could be caused by alternative channels. In Section 7.1, we test the idea that if the crisis effects are driven by economic factors, we should see stronger impacts among participants living in areas that were more heavily hit by the crisis. Second, if the economic crisis created a psychological license to cheat for one's own benefit, people's decision whether to misreport may become more sensitive to thinking about their own financial situation during the crisis. To test this mechanism directly, in Section 7.2 we explore the effects of the salience of one's own financial situation before making decisions in the Mind Game. Finally, in Section 7.3, we take advantage of the richness of the KLPS survey data and consider alternative channels regarding how the crisis might affect cheating behavior.

7.1. Are the Effects Larger Among Those Who Were More Economically Affected?

The following analysis builds on evidence from several low-income countries showing that urban populations were more severely affected economically by the Covid-19 crisis than rural ones. Although the pandemic depressed incomes in both settings, households dependent on wage or business income – more common in urban areas – suffered deeper losses, while rural households often retained partial buffers through subsistence farm production, and many individuals were able to shift into agricultural work as a fallback (Weber, Palacios-Lopez, and Contreras-Gonzáles 2020; Heeman, Pape, and Vollmer 2022; Pörtner, Alam, and Ahmed 2025).

Consistent with this pattern, earnings in the KLPS sample fell by more than twice as much for the urban sub-sample as compared to the rural sub-sample. This difference in the size of the economic shock is large and highly statistically significant ($p < 0.01$; see Columns 1–2 of Table 5, which report the interaction between an urban residence indicator and being surveyed during the crisis). Similar patterns emerge for other proxies of economic conditions: the likelihood of having savings, perceptions of one's own relative economic situation (Columns 3-4, 7-8), though not for the likelihood of borrowing (Columns 5-6). Echoing the effects on earnings, we find that there is a larger increase in the likelihood of cheating during the crisis period for people who lived in urban areas, as compared to the effects during the crisis among those living in rural areas. Specifically, in Figure A.7 we show that the increase in the estimated increase in cheating rate during the crisis is 19 percentage points for the rural sub-sample, while the effect is twice as large (38 percentage points) for the urban sub-sample. Interestingly, the prevalence of cheating was higher in rural areas than in urban areas prior the crisis – consistent with rural areas being poorer overall – and this pattern flipped during the crisis. In the regression framework, the interaction effect between *During Crisis* and living in an urban area is statistically significant (p-value < .01), and robust to controlling for a host of observable characteristics (Columns 1-3, Table 5).

In Columns 4-6, we address a concern that living in urban areas at the time of the data collection could be partly endogenous because the Covid-19 crisis induced some people to migrate from cities to rural areas, due to the relatively greater hardship experienced in cities. Thus, as a robustness test, we use information about where people had been living before the crisis (instead of at the time of the data collection) to re-estimate the effects, and reassuringly find very similar patterns.

Table 5: Effects of Crisis on Reported Winning Rates: Urban and Rural Areas

	(1)	(2)	(3)	(4)	(5)	(6)
During Crisis	.096***	.095***	.118***	.100***	.098***	.111***
	(.016)	(.016)	(.017)	(.016)	(.016)	(.018)
Urban	-.052***	-.050***	-.050***	-.052***	-.050***	-.068***
	(.017)	(.017)	(.017)	(.017)	(.017)	(.022)
During Crisis × Urban	.093***	.090***	.075***	.077***	.073***	.084***
	(.021)	(.021)	(.022)	(.022)	(.021)	(.024)
Observations	5660	5660	5660	5602	5602	5602
Urban Measure	Current	Current	Current	Before Crisis	Before Crisis	Before Crisis
Before Crisis Mean (Rural)	.745	.745	.745	.745	.745	.745
Before Crisis Mean (Urban)	.694	.694	.694	.694	.694	.694
Treatment Effect (Rural, %)	12.908	12.680	15.804	13.392	13.205	14.956
Treatment Effect (Urban, %)	27.253	26.620	27.774	25.421	24.746	28.120
Survey Manipulations	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes

Notes: OLS coefficients. The dependent variable is an indicator equal to 1 if the respondent reports the winning side of the coin. Regressors include an indicator for being surveyed during the crisis, an Urban indicator, and their interaction. In Columns (1)–(3), Urban refers to residence at the time of data collection; in Columns (4)–(6), Urban refers to residence shortly before the crisis. See Table 1 notes for the full list of controls. Standard errors in parentheses. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

7.2. Priming Experiment: Crisis and the Salience of One's Own Financial Situation

In this sub-section, we test the idea that considerations of one's own economic situation when facing a severe hardship provides a psychological license to cheat for at least some people, a mechanism that could help to explain the overall Crisis impact. To test this mechanism directly, we take advantage of the integrated approach which combines (i) a natural field experiment causing variation in terms of whether people make decisions in "normal" times or during unusually severe economic hardship and (ii) a priming experiment embedded in the survey, designed to exogenously increase the extent to which people think about their own financial situation, prior to making a decision about whether or not to cheat in the Mind Game. As described in Section 4.5, a randomly selected group of participants were primed through a series of standard questions about their savings and borrowing. This approach does not guide participants towards thinking about economic hardship per se, and thus differs from studies that directly prime subjects to think about negative income shocks.[16] Instead, we are interested in whether naturally putting one's own financial situation at the top of mind makes it psychologically easier to justify cheating specifically when people are suffering from a large negative income shock in their real life (as compared to their reaction in "normal" times).

In line with the mechanism, we find a statistically significant positive interaction effect between the financial situation salient prime and the during crisis wave (Column 3 of Table 6, p-value = .050). This is because thinking more about one's own financial situation due to the prime during the crisis increases the prevalence of cheating (Column 2) and the effect is marginally significant statistically (p-value = .088), while the prime does not increase the prevalence of cheating before the crisis (Column 1, p-value = .251), and if anything, the point estimate is slightly negative. The estimates are robust to controlling for survey manipulations and observable characteristics (which are controlled for in all columns). The magnitude of the increase in the cheating rate due to this *marginal* increase in thinking about one's own financial situation during the crisis is approximately six percentage points (double the estimated effects on the prevalence of reporting the winning side in Table 6).

Taken at face value, these patterns are consistent with an interpretation that people perceive financial hardship as a justification for cheating, in line with other patterns we document above.

[16] In such priming experiments, researchers ask people to think about hypothetical scenarios describing severe negative income shocks prior making decisions (Mani, Mullainathan, Shafir, and Zhao 2013; Bartoš, Bauer, Chytilová, and Levely 2021), and thus aim to directly activate thinking about situations of financial hardship. Here, we vary thinking about one's own current financial situation and exploit variation in the actual economic crisis in people's life to see whether this *combination* (or interaction) triggers more dishonesty.

At the same time, as we discuss in the next sub-section, the crisis did not increase their sensitivity to thinking about other issues (such as group identity, politics and corruption etc.). That said, we do not want to overstate this result and it should be interpreted cautiously: the reader should bear in mind that we test the influence of several primes in Table 6 and the effect of the financial situation prime is only marginally significant statistically, and actually does not survive a multiple hypothesis testing correction (false discovery rate q-value = 0.15).

Table 6: Crisis, the Salience of One's Financial Situation and Reported Winning Rates: The Role of Priming

	(1) Before Crisis	(2) During Crisis	(3) All
During Crisis			.137*** (.025)
Panel A — Financial Situation Salient			
Financial Situation Salient	-.027 (.024)	.031* (.019)	-.032 (.023)
During Crisis × Financial Situation Salient			.062** (.030)
Panel B — Other Factors Salient			
Cultural Identity Salient	-.026 (.021)	.013 (.018)	-.028 (.022)
During Crisis × Cultural Identity Salient			.044 (.028)
Politics Salient	.019 (.029)	.027 (.018)	.017 (.029)
During Crisis × Politics Salient			.012 (.034)
Panel C — Dishonesty Salient to Self			
Dishonesty Salient to Self	-.018 (.017)	-.043*** (.013)	-.016 (.017)
During Crisis × Dishonesty Salient to Self			-.023 (.021)
Observations	2836	2824	5660
Survey Manipulations	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: OLS coefficients. The dependent variable is an indicator equal to 1 if the respondent reports the winning side of the coin. Panels report main and interaction effects of primes (Financial Situation Salient, Politics Salient, Cultural Identity Salient, and Cheating Salient to Self) with the During-crisis indicator. Columns "Before Crisis", "During Crisis" and "All" are estimated on the indicated subsamples; specifications include survey-manipulation controls and the full set of controls listed in Table 1. For the same specification without controls, see Table A.4. Standard errors in parentheses. $* p < 0.1$, $** p < 0.05$, $*** p < 0.01$.

7.3. Alternative Channels

In the preceding sections, we presented several patterns that are in line with the interpretation that economic considerations are primary drivers of cheating behavior: (i) people systematically respond to the financial benefits of cheating (Section 5.1), (ii) cheating decisions are positively related to lower household income (Section 5.2), (iii) the crisis halved people's earnings for an extended period of time (Section 6.1), (iv) the crisis almost doubled the prevalence of cheating (Section 6.2), and (v) these effects are magnified among people who were more economically hit by the crisis, i.e., urban residents (Section 7.1) and among people who were experimentally nudged to think about their own financial situation (Section 7.2). In this sub-section, we take advantage of the rich KLPS dataset to examine potential alternative non-economic channels through which the crisis could have increased cheating behavior, and assess whether the evidence supports them.

Health. First, we consider changes in health, an obvious consideration in the context of the pandemic. In Table A.2, we find that none of the rich set of health measures in the KLPS data (including subjective assessments of own health, number of hospital visits, and number of symptoms of various diseases such as fever, cough, worms, diarrhea and malaria, etc.) correlates with reporting the winning side of the coin. Furthermore, we do not find that health was negatively affected by the crisis in this population of middle-aged Kenyans. These patterns do not support the interpretation that the crisis increased the cheating rate because participants became less healthy.

Politics and Perceptions of Corruption. Next, we consider shifts in social norms due to the behavior of politicians. The idea is that if people perceived that political leaders had become more corrupt and selfishly motivated during the crisis than before, this in principle could have undermined ethical norms among ordinary citizens (Ajzenman 2021). In Table A.2, we find mixed evidence on how the crisis affected attitudes towards the government and corruption. On the one hand, during the crisis people report being less satisfied with their government and more likely to think that bribes are acceptable than before the crisis, which could be viewed as indirect evidence that respondents believed that honesty norms were eroding in Kenyan society as a whole (supporting the interpretation in the conceptual framework). However, at the same time they were also less likely to think that people cannot get justice. In addition, unfavorable views about the quality of government and the acceptability of bribes are not correlated with the winning rate in the game, suggesting these do not play a major role in predicting individual's cheating behavior. As another test of this channel, we consider whether making politics salient makes participants more dishonest, especially during the crisis. Using the priming experiment – in which we made politics more salient by asking a series of questions related to Kenyan politics – we find that the *Politics Salient* condition somewhat increases the prevalence of misreporting during the crisis (Column 2 of Table 6, although this effect is not significant) but the point estimate of the effects of the *Politics Salient* condition is also positive before the crisis and the interaction effect between the politics prime and the during crisis wave is close to zero and not significant (p-value = .723). Together, there is little evidence in favor of the hypothesized political channel.

Religiosity. Religiosity has often been advanced as a regulator of unethical behavior. Existing evidence suggests that collective survival threat during violent inter-group conflicts can increase the strength of group identity, norm adherence and religiosity (Bauer, Blattman, Chytilová, Henrich, Miguel, and Mitts 2016; Henrich, Bauer, Cassar, Chytilová, and Purzycki 2019; Posch 2021). If such a spike in religiosity also took place during the Covid-19 crisis it would imply that the crisis might *reduce* the prevalence of cheating, in contrast to what we observe. Nevertheless, it is also possible that restrictions to social contact during the crisis could have negatively impacted religious participation (even beyond the period of strict lockdowns), and consequently reduced the role of religion in people's behavior. We find that, as expected, church attendance is lower during the crisis period compared to pre-crisis period, but, importantly, the stated importance of religion to individuals is stable (or, if anything, slightly higher during the crisis). Moreover, neither church attendance nor the stated importance of religion to the respondent are correlated with the reporting cheating rate (Table A.2). These patterns suggest that any crisis effects on the prevalence of cheating behavior are unlikely due to changes in religiosity. We arrive at a similar conclusion when considering the sensitivity of cheating to *Cultural Identity Salient* condition, which included a series of questions about religiosity (Table 6). We find that the point estimate of the effects of this condition on the likelihood of reporting the winning side is not statistically significant either during or before the crisis period, nor is there a significant interaction effect (Column 3).

Respondent Confusion. Another possibility is that participants during the crisis had – for some reason (e.g., perhaps because of having more time to think through responses due to lower opportunity costs) – a better understanding of the Mind Game than participants before crisis, and therefore were better able to understand that they could break the rules and cheat to benefit themselves. Several patterns suggest that this channel does not drive the observed pattern of effects. First, before making decisions in the Mind Game, we asked subjects whether they fully understood the instructions. Only 16 people (out of over 5,600 respondents) reported that they did not fully understand them, and there are no meaningful differences in reported understanding before versus during the crisis. Next, given that respondents with higher cognitive skills and education should be more likely to understand the task, it is noteworthy that the cheating rate correlates neither with higher cognitive skills (measured by the score in Ravens' matrices) nor with higher educational attainment.[17] Finally, if participants were less confused or more attentive during the crisis than before the crisis, we would expect them to be more responsive to the parameters of the choice provided within the survey experiment, in terms of the financial incentives to misreport. Yet this is not what we find: the effect of increasing financial incentives from KSh 40 to KSh 200 on the likelihood of reporting the winning side is nearly identical at roughly 16 percentage points both before and during the crisis (Figure 2).

8. Conclusion

This paper presents rich causal evidence documenting how economic factors shape cheating behavior for one's own financial benefit. The main finding is that people are far more prone to engage in cheating behavior when they face economic hardship and when the financial incentives to cheat are larger. We document the effects of hardship among a large and relatively diverse sample of 5,664 Kenyans surveyed before and during the Covid-19 crisis, exploiting randomized survey timing as a natural experiment. The effects of the crisis are large in magnitude, greater for the most economically impacted, gradually increase with length of hardship experienced, and are amplified when the salience of one's financial situation is experimentally increased.

The findings have several implications. First, they are relevant for economic theory. Experimental economists have established that when people decide whether to break norms and cheat, many reveal a strong preference against cheating and do not strictly maximize their own earnings (Abeler, Nosenzo, and Raymond 2019), contrary to standard neoclassical economics predictions. In this paper, we confirm that finding and show that in "normal" circumstances most

[17] The pairwise correlation between reporting the winning side and education (a secondary school indicator) is .001 (p-value=.95). For cognitive skills (measured by the Raven's matrices score) the correlation is just -.021 (p-value=.12).

people are indeed willing to sacrifice substantial amounts of money to avoid cheating, even in a low-income population like the one we study. At the same time, we show that the traditional neoclassical model is remarkably effective at predicting *when* people are more prone to make moral compromises, namely, when the stakes are larger and they themselves are in a more dire economic situation.

Second, the results speak to a long-standing debate about why the poor sometimes behave differently from the rich, and why the poor may be more prone to break rules and laws. The results suggest that differences in rule-breaking between the poor and the rich are, at least in part, shaped by their economic constraints, rather than by differences in deeper personality traits, preferences or other long-term factors, echoing Becker's (1968) famous early theoretical work. Moreover, given that the economic and social consequences of cheating and norm-breaking are likely substantial (Bardhan 1997; Olken and Pande 2012), the evidence suggests the cheating response to a negative aggregate shock might generate a feedback loop that amplifies dishonest behavior and resulting economic and social problems. In particular, if economic hardship erodes honesty, this may generally lower people's willingness to engage in informal economic transactions that rely on trust, creating barriers to economic recovery and leading to further economic hardship.

Finally, and more speculatively, the findings support the view that aggregate economic crises can create conditions in which a mostly norm-obedient society could move into a new equilibrium characterized by a widespread rule-breaking that may persist well after the crisis is over. In particular, while a stable minority of participants cheated during the one-year period before the crisis, we show that the prevalence of cheating rose gradually throughout the crisis period to more than 90%. In a simple framework, we argue that if people become aware of this behavioral change and start considering cheating as the new normal, this creates conditions in which a temporary crisis may put societies on a new trajectory, by unraveling honesty norms. We believe that improving our understanding of how temporary negative economic shocks – which cause large fractions of the population to "have nothing to lose" after they "hit rock bottom" – may produce lasting impacts on important elements of social capital is a promising avenue for future research.

References

Abeler, Johannes, Anke Becker, and Armin Falk. 2014. "Representative Evidence on Lying Costs." *Journal of Public Economics* 113: 96–104.

Abeler, Johannes, Armin Falk, and Fabian Kosse. 2025. "Malleability of Preferences for Honesty." *The Economic Journal* 135 (667): 982–98.

Abeler, Johannes, Daniele Nosenzo, and Collin Raymond. 2019. "Preferences for Truth-Telling." *Econometrica* 87 (4): 1115–53.

Ajzenman, Nicolás. 2021. "The Power of Example: Corruption Spurs Corruption." *American Economic Journal: Applied Economics* 13 (2): 230–57.

Aksoy, Billur, and Marco A. Palma. 2019. "The Effects of Scarcity on Cheating and In-Group Favoritism." *Journal of Economic Behavior and Organization* 165: 100–117.

Alfonsi, Livia, Vittorio Bassi, Imran Rasul, and Elena Spadini. 2025. "Firm Responses to Uncertainty and Implications for Workers: Experimental Evidence from Uganda During the Pandemic." Working paper.

Andreoni, James, Nikos Nikiforakis, and Simon Siegenthaler. 2017. "Social Change and the Conformity Trap." Working paper.

Aristotle. 1995. *Politics*. Oxford: Oxford University Press.

Balasubramanian, Parasuram, Victor M Bennett, and Lamar Pierce. 2017. "The Wages of Dishonesty: The Supply of Cheating under High-Powered Incentives." *Journal of Economic Behavior & Organization* 137: 428–44.

Bardhan, Pranab. 1997. "Corruption and Development: A Review of Issues." *Journal of Economic Literature* 35 (3): 1320–46.

Bartoš, Vojtěch, Michal Bauer, Jana Cahlíková, and Julie Chytilová. 2021. "Covid-19 Crisis and Hostility against Foreigners." *European Economic Review* 137: 103818.

Bartoš, Vojtěch, Michal Bauer, Julie Chytilová, and Ian Levely. 2021. "Psychological Effects of Poverty on Time Preferences." *The Economic Journal* 131 (638): 2357–82.

Bauer, Michal, Christopher Blattman, Julie Chytilová, Joseph Henrich, Edward Miguel, and Tamar Mitts. 2016. "Can War Foster Cooperation?" *Journal of Economic Perspectives* 30 (3): 249–74.

Becker, Gary S. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy* 76 (2): 169–217.

Bogliacino, Francesco, Rafael Charris, Camilo Gómez, and Felipe Montealegre. 2024. "Negative Economic Shocks and the Compliance to Social Norms." *Judgment and Decision Making* 19: e9.

Bogliacino, Francesco, and Felipe Montealegre. 2020. "Do Negative Economic Shocks Affect Cognitive Function, Adherence to Social Norms and Loss Aversion?" *Journal of the Economic Science Association* 6 (1): 57–67.

Boonmanunt, Suparee, Agne Kajackaite, and Stephan Meier. 2020. "Does Poverty Negate the Impact of Social Norms on Cheating?" *Games and Economic Behavior* 124 (November): 569–78.

Bursztyn, Leonardo, Georgy Egorov, and Stefano Fiorin. 2020. "From Extreme to Mainstream: The Erosion of Social Norms." *American Economic Review* 110 (11): 3522–48.

Bursztyn, Leonardo, Alessandra L. González, and David Yanagizawa-Drott. 2020. “Misperceived Social Norms: Women Working Outside the Home in Saudi Arabia.” *American Economic Review* 110 (10): 2997–3029.

Callen, Michael, Mohammad Isaqzadeh, James D Long, and Charles Sprenger. 2014. “Violence and Risk Preference: Experimental Evidence from Afghanistan.” *American Economic Review* 104 (1): 123–48.

Cohn, Alain, Jan Engelmann, Ernst Fehr, and Michel André Maréchal. 2015. “Evidence for Countercyclical Risk Aversion: An Experiment with Financial Professionals.” *American Economic Review* 105 (2): 860–85.

Cohn, Alain, and Michel André Maréchal. 2016. “Priming in Economics.” *Current Opinion in Psychology* 12: 17–21.

———. 2018. “Laboratory Measure of Cheating Predicts School Misconduct.” *The Economic Journal* 128 (615): 2743–54.

Cohn, Alain, Michel André Maréchal, and Thomas Noll. 2010. “Bad Boys: How Criminal Identity Salience Affects Rule Violation.” *Review of Economic Studies* 82 (4): 1289–1308.

Dai, Zhixin, Fabio Galeotti, and Marie Claire Villeval. 2017. “Cheating in the Lab Predicts Fraud in the Field: An Experiment in Public Transportation.” *Management Science* 64 (3): 1081–1100.

Deshpande, Manasi, and Michael Mueller-Smith. 2022. “Does Welfare Prevent Crime? The Criminal Justice Outcomes of Youth Removed from SSI.” *The Quarterly Journal of Economics* 137 (4): 2263–2307.

Egger, Dennis, Edward Miguel, Shana S. Warren, Ashish Shenoy, Elliott Collins, Dean Karlan, Doug Parkerson, A. Mushfiq Mobarak, Günther Fink, Christopher Udry, et al. 2021. “Falling Living Standards during the COVID-19 Crisis: Quantitative Evidence from Nine Developing Countries.” *Science Advances* 7 (6).

Ellingsen, Tore, and Robert Östling. 2010. “When Does Communication Improve Coordination?” *American Economic Review* 100 (4): 1695–1724.

Fehr, Ernst, and Simon Gächter. 2002. “Altruistic Punishment in Humans.” *Nature* 415 (6868): 137–40.

Fischbacher, Urs, and Franziska Föllmi-Heusi. 2013. “Lies in Disguise—an Experimental Study on Cheating.” *Journal of the European Economic Association* 11 (3): 525–47.

Flory, Jeffrey A, Andreas Leibbrandt, and John A List. 2016. “The Effects of Wage Contracts on Workplace Misbehaviors: Evidence from a Call Center Natural Field Experiment.” NBER Working Paper 22342.

Gächter, Simon, and Jonathan F. Schulz. 2016. “Intrinsic Honesty and the Prevalence of Rule Violations across Societies.” *Nature* 531 (7595): 496–99.

Gneezy, Uri, Agne Kajackaite, and Joel Sobel. 2018. “Lying Aversion and the Size of the Lie.” *American Economic Review* 108 (2): 419–53.

Greene, Joshua D, and Joseph M Paxton. 2010. “Patterns of Neural Activity Associated with Honest and Dishonest Moral Decisions.” *Proceedings of the National Academy of Sciences* 107 (30): 12506–11.

Hamory, Joan, Edward Miguel, Michael Walker, Michael Kremer, and Sarah Baird. 2021. “Twenty-Year Economic Impacts of Deworming.” *Proceedings of the National Academy of Sciences*, Mimeo, 118 (14): e2023185118.

Hanna, Rema, and Shing Yi Wang. 2017. “Dishonesty and Selection into Public Service: Evidence from India.” *American Economic Journal: Economic Policy* 9 (3): 262–90.

Heeman, Markus, Utz Johann Pape, and Sebastian Vollmer. 2022. “The Labor Market Implications of Restricted Mobility during the COVID-19 Pandemic in Kenya: Evidence from Nationally Representative Phone Surveys.” World Bank Group Policy Research Working Paper 9963. Policy Research Working Paper.

Henrich, Joseph, Michal Bauer, Alessandra Cassar, Julie Chytilová, and Benjamin Grant Purzycki. 2019. “War Increases Religiosity.” *Nature Human Behaviour* 3 (2): 129–35.

Herrmann, Benedikt, Christian Thoni, and Simon Gächter. 2008. “Antisocial Punishment across Societies.” *Science* 319 (5868): 1362–67.

Houser, Daniel, John A. List, Marco Piovesan, Anya Samek, and Joachim Winter. 2016. “Dishonesty: From Parents to Children.” *European Economic Review* 82 (February): 242–54.

Jiang, Ting. 2013. “Cheating in Mind Games: The Subtlety of Rules Matters.” *Journal of Economic Behavior and Organization* 93: 328–36.

Josephson, Anna, Talip Kilic, and Jeffrey D. Michler. 2021. “Socioeconomic Impacts of COVID-19 in Low-Income Countries.” *Nature Human Behaviour 2021 5:5* 5 (5): 557–65.

Kajackaite, Agne, and Uri Gneezy. 2017. “Incentives and Cheating.” *Games and Economic Behavior* 102: 433–44.

Kartik, Navin, Marco Ottaviani, and Francesco Squintani. 2007. “Credulity, Lies, and Costly Talk.” *Journal of Economic Theory* 134 (1): 93–116.

Khanna, Gaurav, Carlos Medina, Anant Nyshadham, Jorge Tamayo, and Nicolas Torres. 2023. “Formal Employment and Organised Crime: Regression Discontinuity Evidence from Colombia.” *The Economic Journal* 133 (654): 2427–48.

Kremer, Michael, Edward Miguel, and Rebecca Thornton. 2009. “Incentives to Learn.” *Review of Economics and Statistics* 91 (3): 437–56.

Li, King King, Ying-yi Hong, Bo Huang, and Tony Tam. 2022. “Social Preferences before and after the Onset of the COVID-19 Pandemic in China.” *Judgement and Decision Making* 17 (6): 1313–33.

List, John A., and Fatemeh Momeni. 2020. “Leveraging Upfront Payments to Curb Employee Misbehavior: Evidence from a Natural Field Experiment.” *European Economic Review* 130 (November): 103601.

Lowes, Sara, Nathan Nunn, James A. Robinson, and Jonathan L. Weigel. 2017. “The Evolution of Culture and Institutions: Evidence From the Kuba Kingdom.” *Econometrica* 85 (4): 1065–91.

Mahmud, Mahreen, and Emma Riley. 2023. "Adapting to an Aggregate Shock: The Impact of the Covid-19 Crisis on Rural Households." *Review of Economics of the Household* 21 (1): 19–36.

Mani, Anandi, Sendhil Mullainathan, Eldar Shafir, and Jiaying Zhao. 2013. "Poverty Impedes Cognitive Function." *Science (New York, N.Y.)* 341 (6149): 976–80.

Matsushima, Hitoshi. 2008. "Role of Honesty in Full Implementation." *Journal of Economic Theory* 139 (1): 353–59.

Mauro, Paolo. 1995. "Corruption and Growth." *The Quarterly Journal of Economics* 110 (3): 681–712.

Miguel, Edward, and Michael Kremer. 2004. "Worms : Identifying Impacts on Education and Health in the Presence of Treatment Externalities." *Econometrica* 72 (1): 159–217.

Miguel, Edward, and Ahmed Mushfiq Mobarak. 2022. "The Economics of the COVID-19 Pandemic in Poor Countries." *Annual Review of Economics* 14 (Volume 14, 2022): 253–85.

Nafula, Nancy, Dennis Kyalo, Boaz Munga, and Rose Ngugi. 2020. "Poverty and Distributional Effects of COVID-19 on Households in Kenya." African Economic Research Consortium Working paper. Nairobi.

Olken, Benjamin A., and Rohini Pande. 2012. "Corruption in Developing Countries." *Annual Review of Economics* 4 (Volume 4, 2012): 479–509.

Palmer, Caroline, David C. Phillips, and James X. Sullivan. 2019. "Does Emergency Financial Assistance Reduce Crime?" *Journal of Public Economics* 169 (January): 34–51.

Pörtner, Claus C., Shamma A. Alam, and Ishraq Ahmed. 2025. "Impact of Twin Lockdowns on Hunger, Labor-Market Outcomes, and Household Coping Mechanisms: Evidence from Uganda." *The World Bank Economic Review* 39 (3): 731–62.

Posch, Max. 2021. "Do Disasters Affect the Tightness of Social Norms?" Working Paper.

Rahwan, Zoe, Oliver P Hauser, Ewa Kochanowska, and Barbara Fasolo. 2018. "High Stakes: A Little More Cheating, a Lot Less Charity." *Journal of Economic Behavior & Organization* 152: 276–95.

Schudy, Simeon, Susanna Grundmann, and Lisa Spantig. 2024. "Individual Preferences for Truth-Telling." CESifo Working Paper 11521.

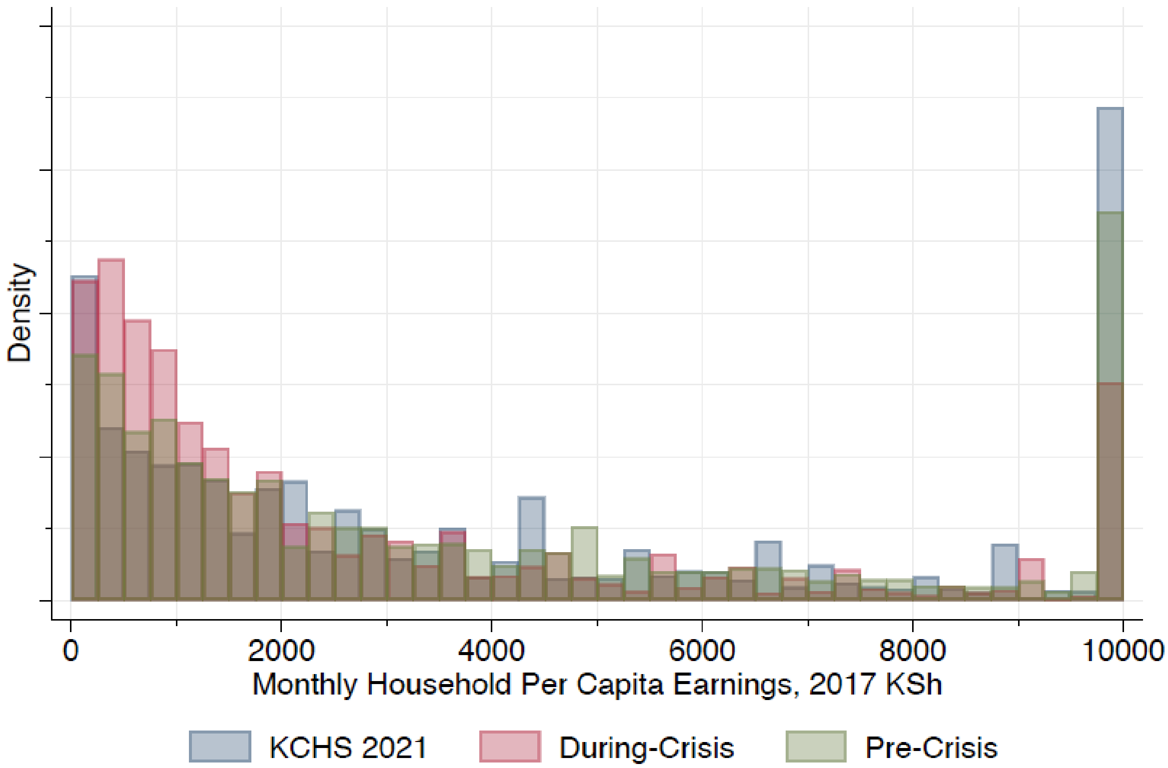
Terrier, Camille, Daniel L. Chen, and Matthias Sutter. 2021. "COVID-19 within Families Amplifies the Prosociality Gap between Adolescents of High and Low Socioeconomic Status." *Proceedings of the National Academy of Sciences of the United States of America* 118 (46): e2110891118.

Weber, Michael, Amparo Palacios-Lopez, and Ivette Maria Contreras-Gonzáles. 2020. "Labor Market Impacts of COVID-19 in Four African Countries." *World Bank Data Blog*, 2020.

Weisel, Ori, and Shaul Shalvi. 2015. "The Collaborative Roots of Corruption." *Proceedings of the National Academy of Sciences* 112 (34): 10651–56.

APPENDIX FIGURES AND TABLES

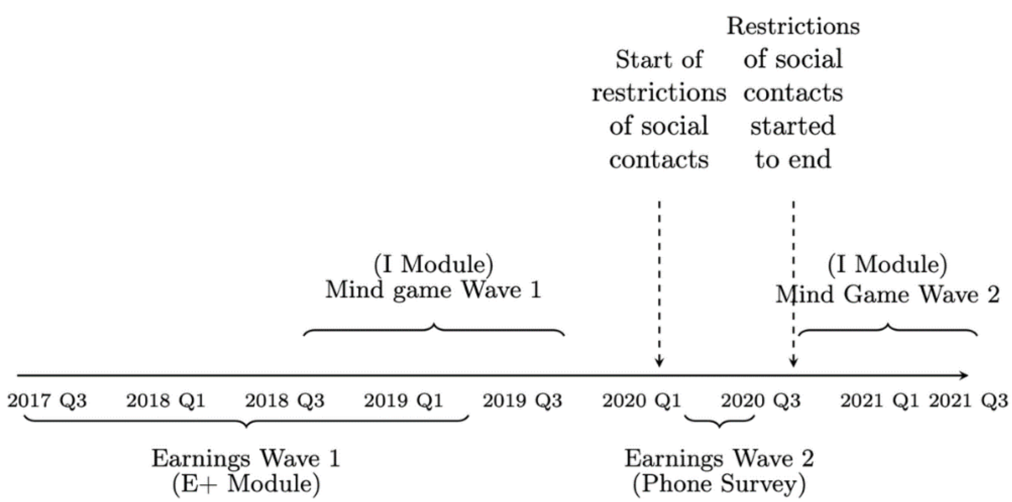
Figure A.1: Comparison of Earnings: KLPS and Kenya Continuous Household Survey (KCHS)



Notes: The figure plots histograms of monthly per-capita household earnings in 2017 KSh. The blue bars are the earnings of a national representative sample from the Kenya Continuous Household Survey (KCHS) 2021. The red bars are the during-crisis earnings of the study sample, using data from Phone module. The green bars are the pre-crisis earnings of the study sample, using data from E+ module. For all three samples, earnings are trimmed at 0 KSh and top-coded at 10,000 KSh.

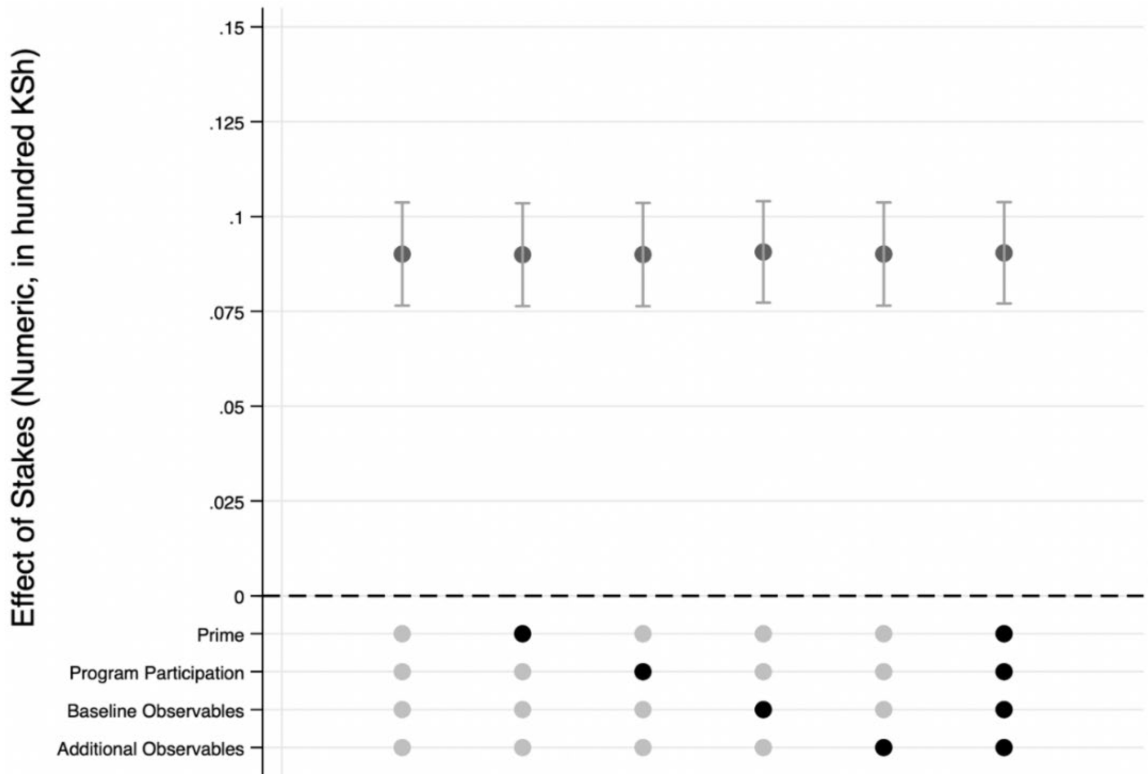
Figure A.2: Mind Game: Visual Aid

Figure A.3: Timeline of the Data Collection



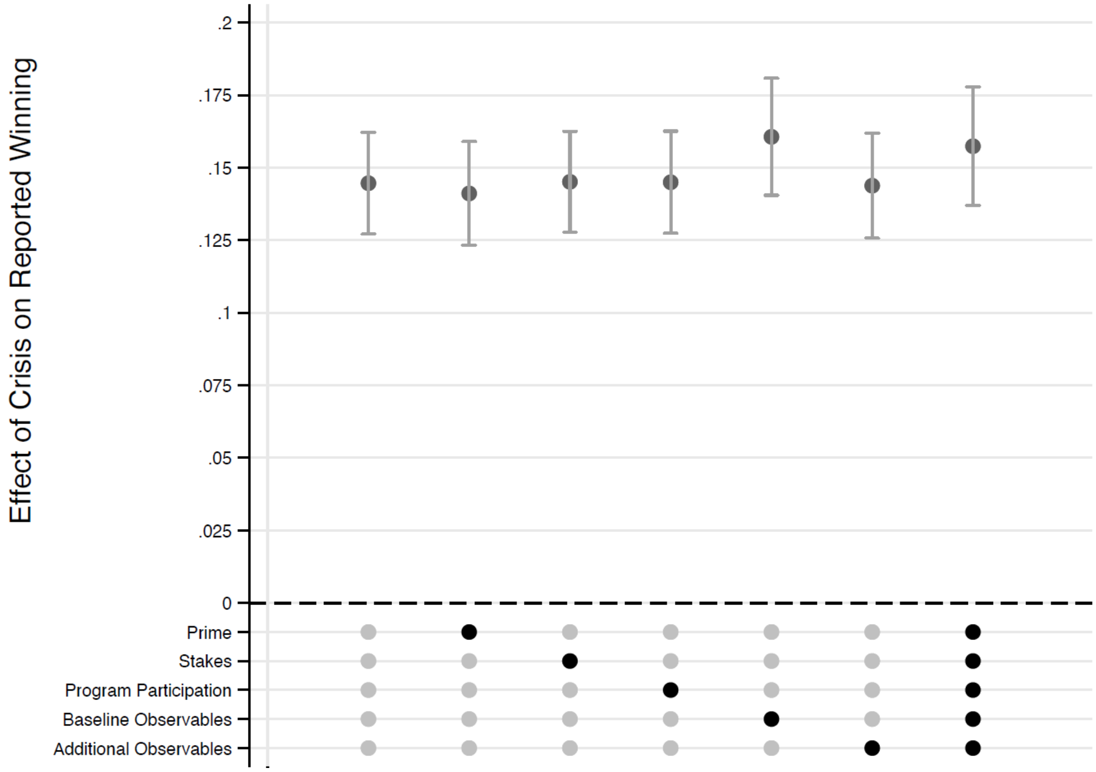
Notes: I module includes the Mind Game and measures of religiosity, attitudes to politics, health, and savings activities. E+ module and phone survey include questions on earnings.

Figure A.4: Effects of Financial Incentives on Reported Winning Rate: Specification Chart



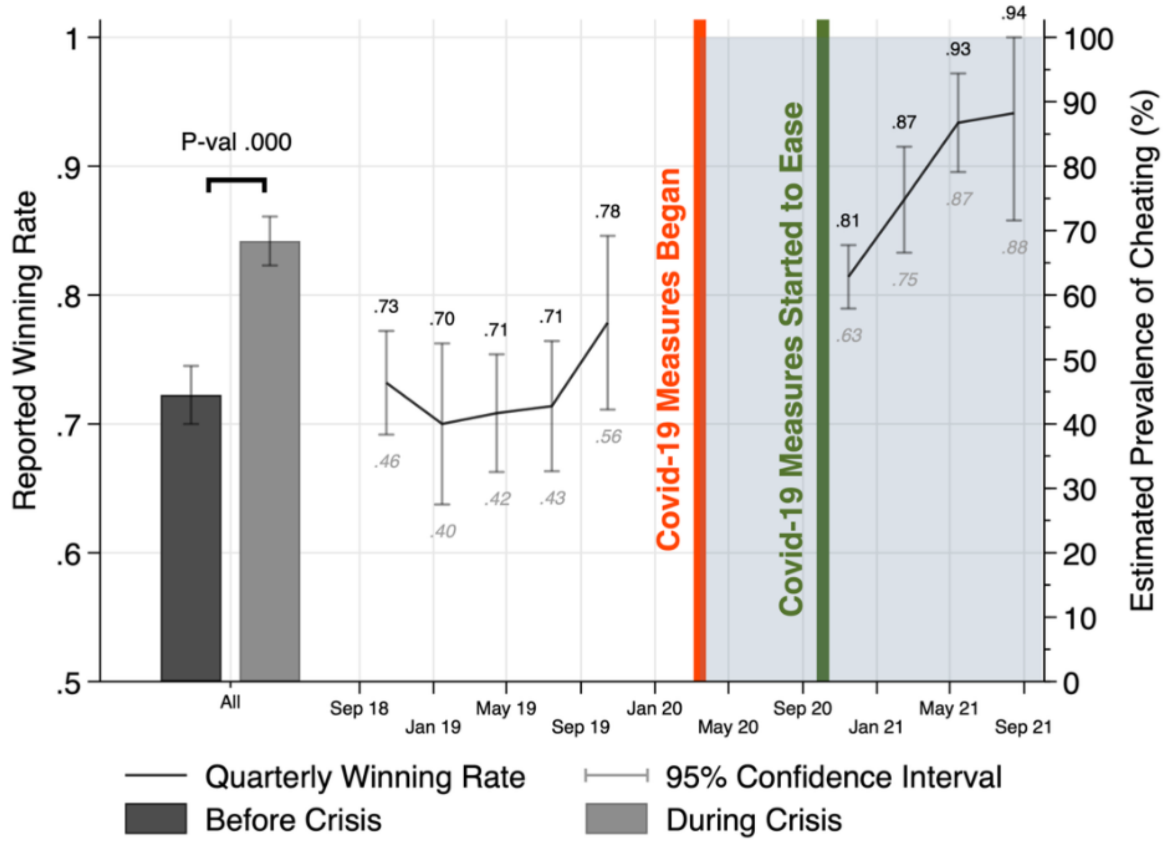
Notes: Effect of numeric stakes (40, 120, 200 KSh) in hundred KSh on reporting the winning side of the coin. Controls: Row 1 (Primes) indicates controlling for four indicators for making own financial situation, religion, and politics salient and making cheating more salient to self in a priming experiment embedded in the survey. Row 2 (Program participation) indicates controlling for a set of indicators for participation in PSDP, GSP, SCY, and VOCED treatment. Row 3 (Baseline Observables) indicates controlling for a set of Baseline and survey observables consisting of gender, grade (1-6), gender of enumerator, and indicators for survey calendar month. Row 4 (Additional Observables) indicates controlling for additional personal observables consisting of year of birth, number of living siblings, indicator for having attended secondary school, indicator for Luhya ethnicity, marriage status, number of children, urban residency.

Figure A.5: Effects of Crisis on Reported Winning Rate: Specification Chart



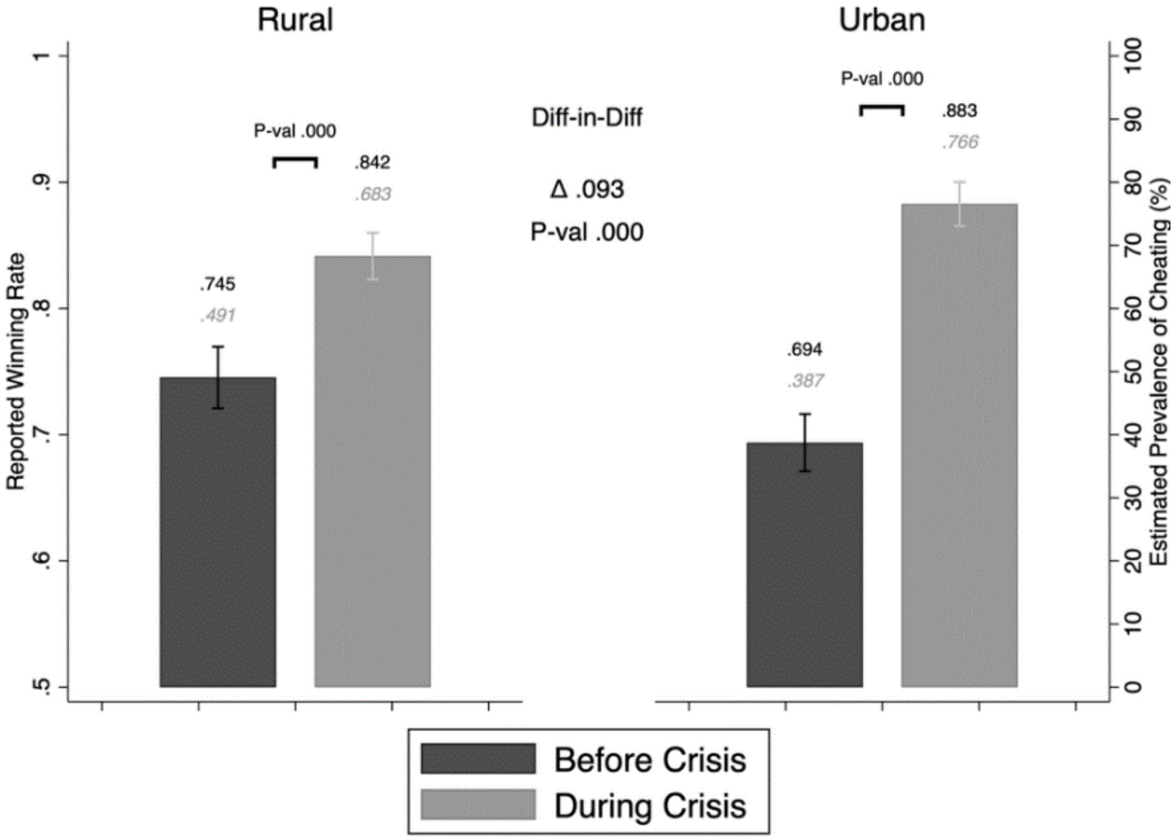
Notes: Effect of Crisis on reporting the winning side of the coin. Controls: Row 1 (Primes) indicates controlling for four indicators for making own financial situation, religion, and politics salient and making cheating more salient to self in a priming experiment embedded in the survey. Row 2 (Program participation) indicates controlling for a set of indicators for participation in PSDP, GSP, SCY, and VOCED treatment. Row 3 (Baseline Observables) indicates controlling for a set of Baseline and survey observables consisting of gender, grade (1-6), gender of enumerator, and indicators for survey calendar month. Row 4 (Additional Observables) indicates controlling for additional personal observables consisting of year of birth, number of living siblings, indicator for having attended secondary school, indicator for Luhya ethnicity, marriage status, number of children, urban residency.

Figure A.6: Effects of Crisis on Cheating: Dynamics, Agricultural Households Only



Notes: Winning rates are pooled by three-month intervals. Bars show the reported winning rate aggregated over all stakes; error bars represent 95% confidence intervals. The final six months before Covid (October 2019–March 2020) are merged because only 37 observations were collected in early 2020. The sample are households who report to preform agricultural or pastoralist activities.

Figure A.7: Effects of Crisis on Cheating: Urban and Rural Areas



Notes: The bars represent the reported winning rate aggregated over all stakes. Error bars represent confidence intervals of reported winning rate at 95% level. Black numbers in the figure indicate reported winning rate, gray italics numbers indicate estimated prevalence of cheating. We report homoskedastic errors for p-values.

Table A.1: Balance Test

Variable	(1) Before Crisis N	(1) Before Crisis Mean/(SE)	(2) During Crisis N	(2) During Crisis Mean/(SE)	(1)-(2) Pairwise t-test N	(1)-(2) Pairwise t-test Mean difference
Panel A - Respondent Observables						
Female	2840	.556	2824	.561	5664	-0.005
		(.009)		(.009)		
Age in 2020	2840	33.871	2824	33.995	5664	-0.124*
		(.051)		(.049)		
Mother Education	2822	5.681	2804	5.909	5626	-0.228**
		(.071)		(.071)		
Father Education	2801	8.282	2781	8.466	5582	-0.184*
		(.075)		(.077)		
Luhya Ethnicity	2840	.931	2819	.935	5659	-0.004
		(.005)		(.005)		
Born in Busia County	2582	.883	2603	.884	5185	-0.001
		(.006)		(.006)		
Attended Secondary Education	2717	.450	2823	.431	5540	.019
		(.010)		(.009)		
Panel B - Before Crisis Economics Well-Being						
Earnings	2320	4538.895	2454	4452.011	4774	86.885
		(148.990)		(131.932)		
Panel C - Survey Takeup (Weighed for Intensive Tracking)						
Surveyed	3290	.848	3312	.809	6602	.039***
		(.008)		(.010)		

Notes: "Earnings" in Panel B refers to household per-capita monthly earnings measured in 2017 KSh. The Earnings data is Pre-Crisis, from the E+ Module. Standard errors in parentheses. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$

Table A.2: Testing Alternative Channels

Variable	Impact of Crisis: Before	During	Diff (Before - During)	p-value	Correlation with Reported Winning: ρ	p-value
Health						
Num Symptoms Last 4W	2.02	1.09	0.930***	0.000	-0.011	0.425
Num Hospital Visits Last 4W	0.39	0.34	0.050**	0.037	0.004	0.780
General Health (1 Very good, 0 Very bad)	0.83	0.89	-0.060***	0.000	0.044***	0.001
Num Days Missed Work Due to Health 4W	0.90	0.84	0.058	0.426	0.020	0.145
Experienced Major Health Issue Since Last Health Survey	0.19	0.17	0.016	0.111	-0.002	0.871
Politics						
People Cannot get Justice (Agreement, 0-1)	0.44	0.42	0.022**	0.043	0.007	0.585
Okay to Pay Bribe (Agreement, 1-4)	3.73	3.70	0.034	0.101	-0.001	0.963
Qaulity of Government. Compared to 2 Years Ago (1 Better, 0 Worse)	0.41	0.21	0.196***	0.000	-0.050***	0.000
Religion						
Important of Religion (0-2)	1.94	1.95	-0.015**	0.031	0.022	0.104
Regular Church Attendance (dummy)	0.73	0.67	0.066***	0.000	-0.001	0.965
Attended Church Last Week (dummy)	0.65	0.59	0.062***	0.000	-0.008	0.573

Notes: The first four columns report mean values of health, political, and religious observables before and during the crisis, along with mean differences and homoskedastic p-values. The last two columns report the Pearson correlation between each observable and the probability of reporting the winning side of the coin. Standard errors in parentheses. $*$ p < 0.1, $**$ p < 0.05, $***$ p < 0.01.

Table A.3: Effects of Crisis on Reported Winning Rate: Dynamics

	All			Before Crisis			During Crisis		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
During Crisis	-.0682**	-.0682	-.0726						
	(.0335)	(.0540)	(.0547)						
Num Months Since Start of Crisis	.00280	.00226	.00178	.00280	.00280	.00150	.0181***	.0181***	.0174***
	(.00188)	(.00219)	(.00224)	(.00188)	(.00188)	(.00202)	(.00211)	(.00211)	(.00223)
During Crisis × Num Months Since Start of Crisis	.0153***	.0160***	.0175***						
	(.00283)	(.00349)	(.00356)						
Observations	5660	5660	5401	2836	2836	2583	2824	2824	2818
Survey Manipulations	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls (Excl. Survey Months.)	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Control for Additional Observables	No	No	Yes	No	No	Yes	No	No	Yes

Notes: OLS coefficients. The dependent variable is an indicator equal to 1 if the respondent reports the winning side of the coin. Regressors include a During-crisis indicator, the number of months since the start of the crisis (set to 0 in March 2020), and their interaction. Columns 1–3 report specifications without additional controls; Columns 4–6 add survey-manipulation controls; Columns 7–9 add the full set of controls. Standard errors in parentheses. $*$ p < 0.1, $**$ p < 0.05, $***$ p < 0.01.

Table A.4: Crisis, Salience of Financial Situation and Reported Winning Rate: The Role of Priming, No Regression Controls.

	(1) Before Crisis	(2) During Crisis	(3) All
During Crisis			.122***
			(.024)
Panel A — Financial Situation Salient			
Financial Situation Salient	-.027	.032*	-.027
	(.024)	(.019)	(.024)
During Crisis × Financial Situation Salient			.059**
			(.030)
Panel B — Other Factors Salient			
Cultural Identity Salient	-.024	.015	-.024
	(.022)	(.019)	(.022)
During Crisis × Cultural Identity Salient			.038
			(.029)
Politics Salient	.014	.031*	.014
	(.029)	(.019)	(.029)
During Crisis × Politics Salient			.018
			(.034)
Panel C — Dishonesty Salient to Self			
Dishonesty Salient to Self	-.019	-.041***	-.019
	(.017)	(.013)	(.017)
During Crisis × Dishonesty Salient to Self			-.022
			(.021)
Observations	2836	2824	5660
Survey Manipulations	No	No	No
Controls	No	No	No

Notes: OLS coefficients. Same specification as Table 6, but estimated without survey-manipulations or covariate controls. The dependent variable is an indicator equal to 1 if the respondent reports the winning side of the coin. Panels report main and interaction effects of primes (Financial Situation Salient, Politics Salient, Cultural Identity Salient, and Cheating Salient to Self) with the During-crisis indicator. Columns “Before”, “During” and “All” are estimated on the indicated subsamples. Standard errors in parentheses. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.