The Impact of Violence on Individual Risk Preferences: Evidence from a Natural Experiment

Pamela Jakiela and Owen Ozier*

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Abstract

We estimate the impact of Kenya's post-election crisis on individual risk preferences. The crisis interrupted a longitudinal survey of more than five thousand Kenyan youth, creating plausibly exogenous variation in exposure to civil conflict prior to the survey. Our results indicate that the post-election crisis sharply increased individual risk aversion. Immediately after the crisis, the fraction of subjects displaying extreme risk aversion increased by more than 80 percent. Findings remain robust when we use an IV estimation strategy that exploits random assignment of respondents to waves of surveying. The crisis also impacted trust, social capital, and beliefs about the economy.

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*Jakiela: University of Maryland, BREAD, and IZA, pjakiela@umd.edu; Ozier: World Bank Development Research Group, BREAD, and IZA, email: oozier@worldbank.org. We are grateful to the staff at IPA-Kenya for their assistance and support, and to Michael Callen, Pascaline Dupas, Marcel Fafchamps, Jonas Hjort, Diego Garrido Martin, Ted Miguel, Anja Sautmann, Adam Wagstaff, Waly Wane, two anonymous referees, and numerous conference and seminar participants for helpful comments. All errors are our own.

1 Introduction

Armed conflict is a source of untold human suffering. Since 1989, more than one million people have been killed in civil and interstate conflicts (Pettersson and Wallensteen 2015). The majority of these episodes are civil wars in low- and middle-income countries; because the scourge of war falls disproportionately on the poorest nations, armed conflict also perpetuates disparities in human and economic development among the living.

The short-term costs of civil conflict are obvious: in addition to the lives lost, violence destroys physical capital and deters investment. There is also evidence that war and violence limit the accumulation of human capital (Blattman and Annan 2010) and erode trust (Nunn and Wantchekon 2011). This has led some scholars to refer to civil war as "development in reverse" (Collier et al. 2003). Yet, though the short-term human and economic costs of conflict are indisputable, many conflict-affected countries — Rwanda and Uganda, for example — have experienced extremely rapid growth in the wake of civil war, and a number of recent papers have challenged the notion that conflict leads to slower growth and development over the long-term (cf. Miguel and Roland 2011). In fact, several studies have found that exposure to civil conflict increases political engagement (Bellows and Miguel 2009, Blattman 2009), enhances cooperation and pro-sociality (Voors et al. 2012, Bauer, Cassar, Chytilová, and Henrich 2013, Bauer et al. 2016), and makes people more willing to bear profitable risks (Voors et al. 2012, Callen, Isaqzadeh, Long, and Sprenger 2014).

These studies share a common empirical approach. First, they take seriously the idea that exposure to conflict is endogenous, and employ a variety of strategies designed to isolate plausibly exogenous variation in victimization and involvement in violence. Second, given their focus on within-conflict variation in exposure and victimization, these papers empirically frame civilians who lived through civil war but were not victimized (or were less exposed to violence) as a comparison group.[1] This strategy enhances the credibility of the estimated treatment

[1]See the review article by Bauer et al. (2016) for discussion.

effects, but has an obvious drawback: it cannot be used to assess the overall impact of conflict unless one assumes that violence has no impact on the relatively less victimized (who serve as the comparison group). If everyone who lives through a period of conflict — regardless of their victim status — is affected, estimates of the marginal impact of greater conflict exposure may present a biased assessment of the overall social cost of violence. Moreover, since civil conflict typically involves a contest for control of a state government, some increase in political and economic uncertainty almost always arises, and affects everyone; empirical approaches that frame less exposed citizens as the comparison group cannot capture the impacts of these macro-level shocks. Thus, to capture the overall impacts of civil conflict on society, an alternative empirical approach is required.

In this paper, we estimate the impact of a specific episode of civil conflict, Kenya's post-election crisis, on the risk preferences of a broad sample of young adults who lived through it. The post-election crisis was a months-long period of protests, rioting, and ethnic violence that began immediately after a disputed presidential election. The election, in which Raila Odinga challenged incumbent Mwai Kibaki, took place on December 27, 2007. Amidst allegations of electoral fraud by observers, and after three days of uncertainty following the national polls, the incumbent president was both declared the winner and sworn into office on December 30, 2007. Ethnic tensions rose, and rioting ensued. The following two months of civil conflict left more than a thousand people dead and hundreds of thousands more internally displaced. The crisis ended when the two candidates signed a power-sharing agreement on February 28, 2008.[2]

We estimate the impact of Kenya's post-election crisis on individual risk preferences, which we measure using lottery choice questions embedded in a longitudinal survey.[3] The Kenyan Life Panel Survey is a survey of more than 5,000 young adults who were enrolled in rural pri-

[2]This description of Kenya's 2007-2008 post-election crisis draws upon several sources: McCrummen (2007), BBC (2008a), BBC (2008b), Waki (2008), World Bank (2008), World Bank (2010), Van Praag (2010), and Hjort (2014).

[3]The lottery choice questions are described in Online Appendix Table A1.

mary schools in 1998. The second round of the survey (hereinafter KLPS2) was administered between August of 2007 and December of 2009. 1,179 respondents (23.3 percent) were interviewed prior to the crisis, while the remainder were surveyed after experiencing the period of civil conflict. Thus, Kenya's post-election crisis interacted with the timing of the survey to create a natural experiment in exposure to conflict.

We employ two complementary identification strategies to estimate the impact of the crisis on risk aversion. First, we estimate the impact of the crisis in straightforward linear and nonlinear frameworks, using several strategies to control for any time trends or seasonal shocks. Second, we exploit the fact that survey respondents were randomly assigned to one of two waves of interviews; Kenya's election crisis interrupted the first wave of surveys, allowing us to instrument for being surveyed after experiencing the crisis using the randomly-assigned survey waves. Both approaches yield similar results: we find that Kenya's post-election crisis had a large and significant impact on individual risk aversion. Specifically, the crisis led to a 10.9 percentage point increase in the likelihood that a subject always chose the safest, lowest expected value alternative (i.e. lottery) available — this effect constitutes an 81.7 percent increase in the rate of extreme risk aversion. We also observe a 5.6 percentage point (or 26.1 percent) decrease in the fraction of subjects who are classified as either risk neutral or risk loving. Such substantial impacts highlight an important channel through which civil conflict might affect growth and development: increased risk aversion might lead individuals in post-conflict settings to avoid high-risk, high-return activities (e.g. entrepreneurship) that contribute to economic growth.

One important question is whether the primary channel through which conflict affects risk tolerance is economic. The crisis may have lowered real incomes — for example, by driving up prices or reducing employment; if that were the case, we might attribute to the post-election crisis itself the impacts of the resulting economic shock. In fact, while we find that the post-election crisis had a significant negative impact on *beliefs* about the economy,

it did not negatively impact the wages or job prospects of the young adults in our survey — instead, we find a modest positive impact on the likelihood of employment. In addition, though prices increased during the crisis, our results are robust to the inclusion of the inflation rate as a control variable, suggesting that observed effects are not driven by price changes. Thus, our results do not seem to be driven by purely economic phenomena such as changes in wages or prices (though we can't fully rule out the possibility that the crisis made young adults unduly pessimistic about their future incomes). However, we do find that the crisis damaged the fabric of Kenyan society, even though the areas that we study were not the most directly affected: the crisis led to significant declines in participation in community groups and generalized trust, particularly the extent to which respondents trust members of other ethnic groups. Thus, the post-election violence appears to have impacted Kenyan society through several channels, but our results suggest that it primarily (and directly) affected individual values, beliefs, and preferences — observed effects cannot be explained by changes in prices and wages.

The key contribution of our study is that we are able to estimate the impact of civil conflict on the risk preferences of those less exposed to violence, as opposed to the specific (marginal) effect of being more exposed to violence relative to a comparison group that also lived through a conflict. Like all Kenyans, KLPS2 respondents experienced a period of acute anxiety about the political and economic future of their country. Moreover, KLPS2 respondents experienced the violence firsthand, though relatively few were themselves victims. 94 percent indicated that they were "worried" about their own safety and the safety of their family members during the crisis, and 76 percent were unable to procure basic necessities because it was unsafe to travel from their home to the nearest market. 8.9 percent of respondents were victimized in some way — either they were assaulted, a household member was assaulted, they were robbed, or they had property burned during the crisis. Given the short duration of the post-election violence (roughly two months), it is clear that these numbers indicate a relatively

high risk of being victimized. However, no KLPS2 respondent was killed; and the majority of KLPS2 respondents spent the crisis in a rural district that experienced widespread clashes, rioting, and looting, but only recorded 9 conflict-related deaths (Waki 2008). Thus, KLPS2 respondents experienced the conflict, but were not, by and large, among the most impacted Kenyans; they therefore provide an important window into the impacts of civil conflicts on the preferences of non-victims.

Of course, our empirical approach is not without drawbacks. Importantly, we cannot separate the effects of the violence (and the risk of violence) from other aspects of the post-election crisis — for example, political uncertainty and the possibility of future violence. We argue that political uncertainty, almost by definition, is an attendant feature of civil conflict. In many studies of the impact of violence on those more exposed within a conflict, these aggregate impacts are differenced out. Estimating the overall impact of civil conflict — including the effects of the inherent political and economic uncertainty — is therefore an important next step in the literature.

Our results differ from several recent studies. For example, Voors et al. (2012) find that greater exposure to violence leads to an increase in risk-seeking behavior, while Callen, Isaqzadeh, Long, and Sprenger (2014) find that priming subjects with recollections of violent events increases their risk tolerance, particularly in situations where no certain outcomes are available. However, these differing results can be reconciled by the difference in estimands — for example, if those who narrowly escape being victimized become more risk averse, or the political uncertainty inherent in civil conflict makes everyone more risk averse.[4] Our aim in

[4]Importantly, the comparison groups in Voors et al. (2012) and Callen, Isaqzadeh, Long, and Sprenger (2014) are made up of individuals who lived through a civil conflict but were (individually or on average) less exposed to violence than individuals in the treatment groups. For example, Callen, Isaqzadeh, Long, and Sprenger (2014) define treatment at the neighborhood level, based on the polling station closest to a respondent's residence; if an attack occurred within one kilometer of a polling station and that polling station was the one closest to the attack, the neighborhood is defined as treated (in their main analysis, though they provide a series of robustness checks employing alternative definitions of treatment). So, neighborhoods that were closest to successful attacks are treated while those that were further from successful attacks make up the comparison group. However, even the comparison group was exposed to violence, though they were — in expectation — less exposed to violence than the treatment group. Indeed, 89.9 percent of the polling stations in their sample were within three kilometers of a successful attack, so any estimate of the impact of that

this paper is to estimate the overall impact of violence; doing so, we may not find the same effect.[5] Understanding these overall impacts is of critical importance as we seek to characterize the ways that conflict may change a country's overall growth trajectory.

This paper contributes to several strands of literature. First, most obviously, we add to the evidence on the impacts of conflict. As discussed above, this literature has expanded rapidly in recent years as increasingly high-quality micro data from post-conflict settings has become available. See Humphreys and Weinstein (2006), Bellows and Miguel (2009), and Blattman and Annan (2010) for prominent early contributions. Blattman and Miguel (2010) and Bauer et al. (2016) provide detailed discussions of the literature.

Second, we contribute to a growing body of evidence that individual preferences are shaped by life experiences. For example, Malmendier and Nagel (2011) and Fisman, Jakiela, and Kariv (2015) show that preferences are impacted by exposure to economic downturns, while Eckel, El-Gamal, and Wilson (2009), Cameron and Shah (2015), and Hanaoka, Shigeoka, and Watanabe (2015) document the effects of natural disasters on preferences. As discussed above, several papers (cf. Voors et al. 2012) have estimated the marginal impact of greater conflict exposure on the preferences of those most affected. Estimates of the overall impact of civil conflict on the preferences of the population are relatively rare, but our results resonate with recent work by Brown, Montalva, Thomas, and Velásquez (2015), who find that spikes in drug-related violence increase risk aversion among those not directly involved.[6]

Finally, our study contributes to the growing body of evidence documenting the predictive

(lower) level of exposure would be extremely imprecise. Similar identification strategies are used in Bellows and Miguel (2009), Voors et al. (2012), and Bauer, Cassar, Chytilová, and Henrich (2013). What differentiates our identification strategy from these is that our comparison group comprises individuals who had not (yet) experienced the period of violence that we study.

[5]It is important to note that the population we study is not representative of Kenya as a whole; it is, however, broadly representative of the population of young people from one particular region — and, as such, provides a window into the impacts of conflict on society. As we discuss further below, our data come from a follow-up survey of a health intervention that was implemented in all the public primary schools in a specific rural district; as such, the population is broadly representative of the sample of individuals enrolled in government schools in that area at the time of the intervention.

[6]Related work by Campos-Vazquez and Cuilty (2014) corroborates this finding by showing that priming Mexican students with information about drug violence increases risk aversion.

power of laboratory-style measures of individual preferences. Numerous studies document the explanatory power of experimental measures of risk, time, and social preferences. For example, Liu (2013) and Liu and Huang (2013) show that experimental measures of risk preferences predict the crop choice and investment decisions of Chinese farmers.[7] To date, the majority of work linking choices in decision experiments to behavior outside the lab has used incentivized measures of individual preferences.[8] Existing evidence suggests the use of incentives shifts individual responses toward greater risk aversion (cf. Camerer and Hogarth 1999, Holt and Laury 2002), leading many to question the broad applicability of hypothetical approaches to risk preference elicitation. We contribute to this literature by demonstrating that hypothetical measures of risk preferences predict real world behaviors in an internally consistent way. In particular, our measure of risk preferences is associated with real-world behaviors that involve risk: migration and entrepreneurship.

The rest of this paper is organized as follows. In Section 2, we describe the KLPS2 data collection effort and Kenya's post-election crisis. In Section 3, we describe our measure of risk preferences and assess the extent to which our hypothetical lottery choice questions predict behaviors likely to depend on risk aversion. In Section 4, we explain our analytic approach and present our main results. Section 5 concludes.

2 Research Design

2.1 The Kenyan Life Panel Survey

We measure the risk preferences of a large and heterogeneous sample of young Kenyans by embedding a series of non-incentivized decision problems in the second round of the Kenyan

[7] In the domain of time preferences, Meier and Sprenger (2012) show that experimental measures of patience predict creditworthiness. Fisman, Jakiela, and Kariv (2017) show that experimental measures of equality-efficiency tradeoffs predict voting behavior, while Fisman, Jakiela, Kariv, and Markovits (2015) show that the same measure predicts the post-graduation career choices of law students.

[8] See Ashraf, Karlan, and Yin (2006) for a notable exception.

Life Panel Survey (KLPS2), which was administered to more than 5,000 individuals who were enrolled in primary schools in Kenya's Western Province in the late 1990s.[9] KLPS2 was administered in person, through one-on-one interviews, between August of 2007 and December of 2009. The survey covers a broad range of topics including educational attainment, labor market and entrepreneurial activities, household composition, migration, and fertility. Our sample includes 5,047 Kenyan youth aged 14 to 31. Summary statistics characterizing our respondents are reported in Online Appendix Table A2.

2.2 Kenya's Post-Election Crisis

The 2008 post-election crisis was a period of violence and political instability following the Kenyan presidential election of December 27, 2007. The two leading candidates in the election were Raila Odinga (the son of Kenya's first vice president), and the incumbent president, Mwai Kibaki (himself a former vice president). For several months preceding the election, opinion polls placed the opposition candidate, Odinga, ahead of Kibaki (Munene and Otieno 2007). As election day neared, the polling gap between Odinga and Kibaki narrowed to what, in some polls, appeared to be a statistical dead heat (Agina 2007, Otieno 2007, World Bank 2008). At the time of the elections, both international observers and members of the opposition made allegations of electoral irregularities; however, after a few days, Kibaki was declared the winner and sworn into office (BBC News 2007, McCrummen 2007, Waki 2008). Opposition supporters organized protests; rioting and violent clashes with police ensued, and the violence soon escalated and took on a strong ethnic dimension (BBC News 2007, Waki 2008, World Bank 2010, Van Praag 2010). By the time Kibaki and Odinga signed a power-sharing agreement at the end of February, 2008, more than one thousand people had been killed, and hundreds of thousands had been internally displaced (BBC 2008a, BBC 2008b).

[9]KLPS2 was a long-term follow-up of the Primary School Deworming Project (PSDP) that was implemented in Busia District, in Kenya's Western Province, between 1998 and 2001. All KLPS2 respondents were enrolled in public primary schools in the project area in 1998. See Miguel and Kremer (2004) for discussion of the PSDP.

All KLPS2 respondents were residing in Busia District, in Kenya's Western Province near the Ugandan border, in 1998; 73 percent were still living in Busia District at the time of the KLPS2 survey, and many others happened to be there during the post-election crisis (because they had returned to their family homes to celebrate the Christmas holiday or to vote). As a result, most were spared the worst of the post-election violence: though protests, riots, and assaults took place throughout Kenya, the most affected areas were Rift Valley Province and the more urban districts across the country. According to the official report of the Commission of Inquiry on Post Election Violence, only 98 of the 1,133 conflict deaths occurred in Western Province (Waki 2008). The majority of conflict deaths in Western Province occurred in districts that bordered Rift Valley Province; only 9 deaths were documented in Busia District (Waki 2008). In addition, most KLPS2 respondents (95.4 percent) are members of the Luhya ethnic group, the majority ethnic group in Western Province; as such, they were somewhat less likely to be singled out as clearly aligned with either the incumbent Mwai Kibaki (a member of a different ethnic group) or the opposition candidate Raila Odinga (a member of a third group).[10,11] Thus, though KLPS2 survey respondents lived through the crisis, both their physical locations and their ethnic identities helped to shield most of them from the very worst of the violence.

This is not to say that KLPS2 respondents were safely removed from the fighting. The second wave of the survey included questions on exposure to violence during the post-election crisis; it documents the fact that relatively few respondents were, themselves, the victims of physical attacks — largely because they were able to hide in their homes during clashes.[12] 71.7

[10]Western Province was an opposition stronghold, and Raila Odinga received 64.6 percent of the votes cast there in the presidential election; thus, individuals perceived as likely supporters of the incumbent were the most at risk.

[11]We thank the World Bank's Kenya Country Office for guidance on wording.

[12]As discussed above, KLPS2 respondents were randomly assigned to one of two waves of surveying. The post-election violence interrupted the first wave of surveying. Questions about exposure to post-election violence were included only in Wave 2, so responses can be seen as representative of the entire respondent population. These additional questions were included near the end of the survey, after the risk preference elicitation questions — so their inclusion could not have impacted measured risk preferences directly by priming respondents. Please see Callen, Isaqzadeh, Long, and Sprenger (2014) for a discussion of the impact of fearful recollections on measured risk preferences.

percent of KLPS2 Wave 2 respondents said that they were "very worried" about their own safety and the safety of their family members during the crisis, and an additional 22.6 percent indicated that they were "somewhat worried." 76.3 percent said that the crisis prevented them from going to local markets to obtain basic necessities. However, only 5.2 percent indicated that someone in their household was physically assaulted during the crisis. An additional 3.7 percent of respondents had property stolen or burnt, but did not have a household member assaulted. Thus, KLPS2 respondents experienced the crisis firsthand, but relatively few were, themselves, victims of violence. Our estimates should therefore be seen as both a lower bound on the impact of civil conflict on risk preferences and a natural complement to studies which estimate the marginal impact of greater exposure to violence.

3 Measuring Risk Preferences

We elicit risk preferences by confronting survey respondents with a series of hypothetical decision problems. Our approach builds on the seminal work of Binswanger (1980) as well as more recent contributions by Barr and Genicot (2008); Harrison, Humphrey, and Verschoor (2010); and Dave, Eckel, Johnson, and Rojas (2010). We focus on simple lottery structures and limited choice sets. Each of our decision problems was a choice between two or three lotteries involving two equally likely potential payoffs. The sequence of decision problems was designed to start with choices that were extremely simple, building slowly toward more complicated choice problems. This sequencing from least to most complex served two purposes. First, the gradual increase in complexity was intended to help address the concern that respondents might not exert sufficient cognitive effort (for example, to calculate expected payoffs) when facing non-incentivized choice problems.[13] Respondents first considered options that could

[13]There is some debate about the extent to which non-incentivized lottery choice questions correctly measure individual risk preferences. See Camerer (1995) for an overview, and Binswanger (1980) and Holt and Laury (2002) for seminal contributions. Existing evidence suggests that financial incentives induce greater risk aversion (Camerer and Hogarth 1999). Embedding non-incentivized decision problems in surveys has nonetheless proven successful in a number of field settings in developing countries, when conducting incen-

be evaluated with minimal cognitive effort, easing into the process of evaluating the expected utility of financial lotteries. Second, we wished to maximize comprehension by subjects with low levels of numeracy (since many of our respondents had relatively little formal education). For this reason, we also limited each decision problem to a maximum of three lottery options, included only three easily understood probabilities (0, 0.5, and 1), and only considered lotteries over financial gains (rather than losses). Our experimental instructions do not assume any familiarity with probabilities, averages, or expected values; lotteries are explained in terms of payoffs and uncertain but equally likely events.[14]

When administering our experiment, survey enumerators began by presenting two practice decision problems which introduced the structure of the lottery choice questions to the respondent. The first practice problem contained only degenerate lotteries — the respondent was asked to choose between 100 and 150 Kenyan shillings.[15] The second practice problem introduced the (non-degenerate) lottery concept in a setting with a clear "correct" answer: one lottery first-order stochastically dominated the other, and both involved risk. Enumerators asked respondents to choose between the lotteries presented in the practice decision problems, and then followed a script which made sure that each respondent fully understood the nature of the choices they were facing.

After completing the practice decision problems, each subject made six choices between lotteries which differed in riskiness. Each choice was presented on a laminated card which depicted either two or three options. Like the practice problems, the first question (described above) provided a test of monotonicity, allowing us to identify subjects who are either not expected utility maximizers (cf. Kőszegi and Rabin 2006) or who simply could not grasp the nature of the choice problems. The second decision problem offered an extremely simple test

tivized experiments at scale is not feasible (cf. Ashraf, Karlan, and Yin 2006, Callen, Isaqzadeh, Long, and Sprenger 2014).

[14]Complete experimental instructions are included in the Online Appendix.

[15]The average US dollar value of 100 Kenyan shillings was 1.36 over the period during which the KLPS2 survey was in the field, from August 2007 to December 2009. During that period, 150–200 shillings was a typical daily wage for informal agricultural labor.

of risk preferences, and one which should have been easily understood by almost all subjects: subjects were asked whether they preferred to receive 100 shillings with certainty, or a lottery that paid zero and 400 shillings with equal probability. The remaining four decision problems presented lottery choices of increasing complexity. Without any functional form assumptions (but assuming subjects are maximizing a well-defined utility function), these decisions can be used to classify subjects into risk preference categories: risk loving, risk neutral, moderately risk averse, and most risk averse (always choosing the lotteries with the lowest expected value and payoff spread). If we assume that consistent preferences can be represented by a utility function of the constant relative risk aversion (CRRA) form, the risk cards were calibrated to distinguish a wide range of coefficients.[16] A subject with a CRRA coefficient of 1.78 or higher would always chose the lowest variance, lowest expected value lottery, while a subject with a CRRA coefficient of 0.19 or less would always choose the lottery with the highest expected value (or, is she were risk-loving, the highest variance). The payouts for the lotteries in all the choice problems included in our experiment are described in Online Appendix Table A1.

3.1 Individual Choices

Histograms of individual choices in all six decision problems are presented in Online Appendix Figure A1. Very few respondents (only 4.14 percent) indicated that they preferred the degenerate lottery which paid 100 Kenyan shillings with certainty to a non-degenerate lottery which paid either 100 or 120 Kenyan shillings, each with probability 0.5. We interpret this as evidence that most subjects understood the nature of the decision problems, at least those that involved relatively simple payoff calculations.[17]

[16]The CRRA utility function takes the form: $u(x) = x^{1-\rho}/(1-\rho)$ where ρ indicates the level of risk aversion. Higher values of ρ indicate greater risk aversion. When ρ equals zero, the agent is risk neutral; the indifference curves represented by the CRRA utility function approach log utility as ρ approaches one. Even if relative risk aversion is not constant, constant relative risk aversion is a reasonable approximation over the relatively small range of payoffs considered in our experiment.

[17]We also asked enumerators to indicate whether they believed that respondents fully understood the lottery choice questions; these responses suggest that 99.6 percent of subjects fully comprehended the choices that they were making. There are, of course, several reasons that subjects who understood the decision problem

Using data from the other five decision problems allows us to assign respondents to distinct risk preference categories and to test whether individual decisions are consistent with a CRRA utility representation. There are 162 possible combinations of responses to the last 5 lottery choice problems, only 10 of which are consistent with CRRA utility. 22.5 percent of respondents always choose the lowest variance, lowest expected value lottery. This choice pattern is consistent with a CRRA coefficient of 1.78 or above. 2.4 percent of respondents always choose the highest expected value lottery, while 15.7 percent always choose the highest variance lottery (the highest variance lottery is the highest expected value lottery in four of the five decision problems). A risk-loving individual with a CRRA coefficient below -0.26 would always choose the highest variance lottery, while an individual with a CRRA coefficient between -0.26 and 0.19 would always choose the lottery with the highest expected value. 4.0 percent of respondents made decisions that were consistent with a CRRA coefficient between 0.19 and 1.78, indicating an intermediate degree of risk aversion. Thus, a total of 44.6 percent of respondents made choices which were consistent with the maximization of a CRRA utility function, but almost all of these individuals were at the extremes — consistently choosing either the lowest or the highest variance lotteries offered.

This choice patterns suggests that a CRRA coefficient is not the best way to summarize our respondents' risk preferences. Instead, we construct a simple index of risk tolerance: a count of the number of times (out of five lottery choice questions) that a subject chose the highest variance or highest expected value lottery. This index has two important strengths. First, it obviates the need to impose functional form assumptions on the structure of risk preferences.

might prefer a degenerate lottery to a stochastically dominant one involving risk. One possibility is that some respondents have preferences which are not monotonic. Several non-expected utility models of risk preferences suggest that individuals may prefer to avoid increases in payoff variance, even when the higher variance lottery stochastically dominates the lower variance alternative (cf. Kahneman and Tversky 1979, Kőszegi and Rabin 2006). We interpret the overwhelming tendency to choose the stochastically dominant lottery as evidence against strongly non-monotonic preferences. One plausible interpretation of the observed choice patterns is that respondents have monotonic preferences which they implement with error (Hey and Orme 1994, Loomes 2005, Von Gaudecker, van Soest, and Wengström 2011, Choi, Kariv, Müller, and Silverman 2014). Interestingly, subjects are much less likely to choose the dominated lottery after the post-election crisis; the percentage of respondents choosing the degenerate lottery drops from 11.0 before the crisis to 2.0 afterward.

In the decision problems included in our study, all lotteries involving risk have two equally likely outcomes. In four of the five decision problems, the highest expected value lottery has the highest variance and the lowest minimum payoff (i.e. the lowest payoff in the bad state).[18] Hence, any parametric restriction that permits an ordering of utility functions in terms of risk aversion would predict that relatively more risk averse individuals will be less likely to choose the higher variance lotteries. Second, constructing a measure of risk aversion that does not rely on functional form assumptions allows us to use data from all subjects, including those whose choices were not perfectly consistent with a specific utility representation. As discussed in Loomes (2005) and Choi, Kariv, Müller, and Silverman (2014), most subjects implement their choices with error — and we would expect relatively high levels of error in our sample because of the relatively low level of formal schooling attained by our subjects.[19] Using a simple index of risk tolerance that can be calculated for all subjects allows us to maximize statistical power by using all of our data, and eliminates the possibility that selection into the pool of CRRA-consistent subjects might drive our results.

3.2 Instrument Validity

To address concerns about the validity of hypothetical measures of risk aversion, we test whether individual responses to our hypothetical lottery choice questions explain observed variation in behaviors that we might expect to be driven (at least in part) by risk attitudes. We focus on two such behaviors suggested by the literature: entrepreneurship (Schumpeter 1934, Schumpeter 1939, Skriabikova, Dohmen, and Kriechel 2014) and migration in search

[18]The one exception is the final lottery choice question, which included an alternative designed to distinguish between risk loving and risk neutral types. In this choice problem, we classify *both* the highest expected value lottery and the highest variance lottery as risky when we construct our index of risk tolerance — so both risk loving and risk neutral types have risk tolerance indices equal to five. Though coefficient magnitudes change somewhat, our substantive results are unchanged if we exclude the last decision problem from our index of risk tolerance or code the highest expected value lottery as a less risky choice in the final decision problem.

[19]Respondents are 5.4 percentage points more likely to make choices that are consistent with a CRRA representation after the post-election crisis. If the likelihood of making consistent choices were also associated with risk aversion, omitting the inconsistent subjects could bias our results. However, all of our findings are robust to the exclusion of the inconsistent types from the analysis.

of employment (Jaeger et al. 2010, Bryan, Chowdhury, and Mobarak 2014).[20] Both of these actions can be seen through the lens of risk tolerance: for an unemployed or underemployed young adult residing in a rural area, operating one's own business and migrating are two high-risk but potentially profitable strategies for improving one's long-term income prospects (Filmer and Fox 2014). Both behaviors are also relatively uncommon in our sample: only 12.4 percent of KLPS2 respondents were operating their own business at the time of the survey, and only 3.5 percent had ever moved for a job or in search of work. To increase power, we also consider an aggregate index of risk-taking equal to one if a respondent is either self-employed or has ever migrated for work.

We document the association between these risky but potentially profitable activities and responses to our lottery choice questions in Online Appendix Table A3. The number of times a respondent opted for the riskiest lottery is significantly associated with the likelihood of operating one's own business, migrating for work, and our aggregate index that combines the two behaviors. We show that the same relationships generally hold within the pre-crisis and post-crisis periods in Online Appendix Table A4. Given the many factors underlying occupational choices, we interpret these robust associations as evidence that our lottery choice measure does predict outcomes associated with risk aversion in the cross-section.

4 Analysis

4.1 Identification Strategies

We exploit the fact that Kenya's post-election crisis occurred in the middle of the KLPS2 data collection effort to estimate the impact of the violence on risk preferences. The KLPS2

[20]Though many studies in developed countries find a strong association between gender and risk aversion, the available evidence suggests that this pattern may driven by high levels of risk tolerance among white men (Croson and Gneezy 2009). It is therefore not appropriate to validate our results using gender. In fact, we do not find a strong association between gender and risk aversion: women are less likely to be risk loving, but are also less likely to be extremely risk averse. The absence of a relationship between gender and risk aversion in rural Kenya is consistent with the findings reported in Jakiela and Ozier (2016).

survey was launched in August of 2007, and 1,179 respondents were surveyed in 2007 prior to the elections. The crisis led to a two-month suspension of survey activities, which resumed in March of 2008 and continued through the end of 2009. We employ two complementary identification strategies. First, we estimate the impact of the crisis in a straightforward linear framework. We report specifications that include calendar month fixed effects, enumerator fixed effects, and controls for sociodemographic characteristics such as respondent age, educational attainment, and marital status that we expect to change gradually over time as respondents transition to adulthood.[21,22] Thus, we estimate OLS regressions of the form:

$$Y_{ijm} = \alpha + \beta Post_i + \delta X_i + \eta_j + \lambda_m + \varepsilon_{ijm} \quad (1)$$

where Y_{ijm} denotes the risk aversion index of respondent i surveyed by enumerator j in month m, $Post_i$ is an indicator variable equal to one if respondent i was surveyed after the post-election crisis, η_j is an enumerator fixed effect, λ_m is a calendar month fixed effect, and ε_{ijm} is a conditionally mean-zero error term.[23,24] In the Online Appendix, we also present two alternative specifications as robustness checks: ordered logit specifications that account for the ordered nature of our outcome variable and OLS specifications that replace the month fixed effects with separate linear time trends for the pre-crisis and post-crisis periods.[25]

[21]Because the post-election violence occurred after only five months of surveying, any seasonal variation in risk preferences — for example, variation across the crop cycle — could confound our estimates of the crisis' impact; including calender month fixed effects addresses this concern.

[22]With the exception of age, these characteristics might also be impacted by the crisis. To address concerns about the potential for a bad controls problem, we also report specifications that omit all sociodemographic controls. Results are always similar in magnitude and significance with and without controls.

[23]Since our identification is based on temporal variation in treatment status, we cluster our analysis at the month level. Results are similar in magnitude and significance if we cluster at the primary school (i.e. community) level, or if and include primary school fixed effects.

[24]As discussed above, our main outcome variable is the number of times a respondent chose the highest variance or highest expected value value lottery. In principle, it would be possible to use CRRA coefficients implied by an individual's choices as the outcome variable, but this would require us to exclude from the analysis the 55 percent of subjects whose choices were not consistent with a CRRA utility representation. All our results are similar if we omit those subjects; however, because subjects are 5.4 percentage points more likely to make consistent choices after the post-election crisis, omitting inconsistent subjects could bias our findings.

[25]While it is tempting to view our research design as analogous to a regression discontinuity approach, our natural experiment does not create a valid discontinuity. First, because the post-election crisis halted

We also report complementary instrumental variables estimates of the impact of the post-election crisis on individual risk preferences, exploiting the fact that KLPS2 respondents were randomly assigned to one of two waves of surveying. Wave 1 began in August of 2007 and continued through November 2008, while Wave 2 started in November 2008 and concluded in December of 2009. As discussed above, Kenya's post-election crisis occurred approximately halfway through the first wave of surveys: 1,179 Wave 1 surveys were completed before the crisis, and 1,289 were completed afterward. This enables us to use random assignment to Wave 2 as an instrument for being surveyed after the crisis. Within each wave, participant characteristics may be associated with how quickly an individual was surveyed — for example, subjects who were still residing with their parents in their home villages might have been surveyed earlier because they were easier to locate. However, random assignment to survey wave creates exogenous variation in exposure to civil conflict prior to the survey. Thus, our 2SLS regressions are of the form:

$$Y_{ijm} = \alpha_1 + \beta_1 Post_i + \delta_1 X_i + \eta_{1j} + \lambda_{1m} + \zeta_{1ijm} \tag{2}$$

$$Post_i = \alpha_2 + \gamma_2 Wave2_i + \delta_2 X_i + \eta_{2j} + \lambda_{2m} + \zeta_{2ijm} \tag{3}$$

where all variables are as before, β_1 is the coefficient of interest, $Wave2_i$ is an indicator equal to

surveying for more than two months, sociodemographic characteristics such as age do jump discontinuously at the moment of the crisis. Second, because the crisis lasted months, the limit-based notion of regression discontinuity is not directly applicable to this setting; a discontinuity would involve choosing a single point in time during the crisis, and at any such point, data from this survey would not be immediately adjacent on both sides. Third, there is some evidence that neither the onset nor the conclusion of the crisis were, in fact, instantaneous and completely unexpected. Though opposition candidate Raila Odinga held a convincing lead in opinion polls several months before the election, incumbent Mwai Kibabki caught up with Odinga in the month prior to the polling date, potentially raising fears that one of the candidates might try to manipulate a close vote. The official report Commission of Inquiry also documents several minor clashes in Rift Valley in late November and early December, most of which appeared be connected to attempts by political candidates to rally support by playing off of underlying ethnic tensions and disagreements over land ownership (Waki 2008). There was also a period of uncertainty in the wake of the crisis, between February 28, 2008, when the power-sharing agreement was signed, and April 12 when — after sometimes tense negotiations between the two political parties — Raila Odinga was sworn in as Prime Minister and the new coalition government officially took power.

one for those randomly assigned to the second wave of surveying, and both ζ_{1ijm} and ζ_{2ijm} are conditionally mean-zero error terms. The variable $Wave2_i$ takes the value one for individuals assigned to be surveyed in Wave 2 (which is entirely post-crisis), and zero for those assigned to be surveyed in Wave 1 (which includes both pre- and post-crisis observations).[26]

4.2 Results

We begin with a graphical presentation of our key result: Figure 1 plots the average of our risk index — the number of times a respondent chose the highest variance or highest expected value lottery — as a function of the month and year in which the survey was administered. The figure highlights the marked drop in the tendency to choose risky lotteries after the crisis. Prior to the crisis, the median value of the risk index was three; it dropped to one after the crisis. For a subject whose choices can be represented by a CRRA utility function, a risk index of three indicates a CRRA coefficient between 0.46 and 0.50, while a risk index of one indicates a CRRA coefficient of at least 0.72.[27] Thus, the onset of the crisis is associated with a substantial increase in risk aversion.

Figure 2 presents histograms of our risk index separately for the pre-crisis and post-crisis periods. The figure shows that zero risky choices was the modal outcome after the crisis.

[26]Before proceeding to our 2SLS analysis, we check whether the random assignment of respondents to survey waves generated groups that were comparable in terms of observable characteristics prior to the survey. Because Wave 2 respondents were surveyed approximately one year later than Wave 1 respondents, we would not expect characteristics such as age at the time of the survey to be similar in the two waves. To test whether random assignment created groups that were comparable *ex ante*, it is necessary to focus on fixed traits and outcomes that were recorded in the survey for specific points in time. For example, the survey collects detailed information about school participation in each year between 1998 and the moment of the interview; this allows us to generate a variable indicating the number of years of schooling that a respondent had completed at the start of 2007 (before anyone was surveyed), regardless of the year in which a respondent was actually interviewed. Results are reported in Online Appendix Table A2. We find no statistically significant differences between the survey waves in terms of proportion female, age in 2007, proportion born in Busia District, proportion from the local-majority Luhya ethnic group, years of schooling completed by 2007, and the proportion that were married by 2007. Thus, randomization appears to have succeeded in creating groups of respondents that were similar in terms of observable characteristics prior to the survey and the onset of the post-election violence.

[27]Of course, as discussed above, most respondents in our sample implement their choices with error, so their preferences are not perfectly consistent with a CRRA utility representation.

Respondents were more likely to choose the riskiest lottery zero or one times after the crisis, and they were less likely to choose the riskiest lottery two, three, four, or five times after the crisis. Moreover, all of these differences are statistically significant.[28]

4.2.1 OLS Results

We now proceed to estimate the impact of exposure to the post-election crisis on risk preferences by estimating Equation 1. Our dependent variable is the number of times a respondent chose the highest variance or highest expected value lottery (over the course of five decision problems). Results are reported in Table 1. In Column 1, we include only the indicator for being surveyed after the post-election crisis. In Column 2, we control for gender, age, education level, and marital status at the time of the survey. In Column 3, we control for survey enumerator fixed effects. In Column 4, we control for the month of the year in which the survey took place (to eliminate any seasonal patterns in responses). We include all three sets of controls in Column 5.

In all five OLS specifications reported in Table 1, the post-election coefficient is negative and significant at at least the 99 percent confidence level, suggesting that experiencing the crisis lowered the number of risky choices a respondent made by between 0.559 and 0.798 choices.[29] In the pre-crisis period, the average number of risky choices was 2.564 out of five, so the change observed after the crisis represents a dramatic drop in the willingness to bear profitable risk. Adding controls for gender, education level, age, and marital status has almost no impact on the estimated coefficient.[30]

[28]Respondents were 16.9 percentage points more likely to have a risk index of zero after the crisis (p-value < 0.001), 2.3 percentage points more likely to have a risk index of one (p-value 0.075), 3.7 percentage points less likely to have a risk index of two (p-value 0.037), 3.4 percentage points less likely to have a risk index of three (p-value 0.014), 7.2 percentage points less likely to have a risk index of four (p-value < 0.001), and 4.8 percentage points less likely to have a risk index of five (p-value 0.005).

[29]Across the five specifications reported in Table 1, the p-value on the indicator for being surveyed after the post-election crisis ranges from 1.082×10^{-8} to 4.393×10^{-6}.

[30]Controlling for enumerator fixed effects increases the estimated magnitude slightly, while controlling for calender month fixed effects leads to a small decrease in the estimated coefficient. As discussed above, we report a range of robustness checks in the Online Appendix. In Online Appendix Table A5, we estimate ordered logit specifications that account for the discrete but ordered nature of the outcome variable. Again

We consider alternative formulations of the outcome variable in Table 2. In the first column, the outcome variable is an indicator for always choosing the highest variance or highest expected value lottery option (i.e. behaving in a manner consistent with risk neutral or risk-loving preferences — specifically, a CRRA coefficient no greater than 0.19); in the second column, the outcome is an indicator for always choosing the lowest variance, lowest expected value alternative (i.e. behaving in a manner consistent with a CRRA coefficient of at least 1.78). We find that the post-election crisis led to a 5.6 percentage point (26.1 percent) decrease in the likelihood of making risk neutral or risk loving choices, and an 10.9 percentage point (81.7 percent) increase in the probability of always choosing the lowest variance, lowest expected return lottery. We then consider each decision problem in isolation. The post-election crisis led to a significant decrease in the likelihood of choosing higher variance, higher expected value lottery options in each of the five decision problems.[31] Thus, our main result is robust to a wide range of alternative specifications and formulations of the outcome variable.

4.2.2 IV Results

Next, we employ a complementary identification strategy, exploiting the fact that KLPS2 respondents were randomly assigned to one of two waves of surveying to generate instrumental

we find that the indicator for being surveyed after the post-election crisis is negative and significant in all specifications. In Online Appendix Table A6, we include separate linear time trends for the pre-crisis and post-crisis periods. The indicator for being surveyed in the post-crisis period is negative and significant in all specifications.

[31]The effect is similar in magnitude across the four decision problems that include three lottery alternatives rather than two. The estimated effect is also similar in magnitude in decisions problems that do and do not include a degenerate lottery that pays 100 Kenyan shillings with certainty. To further probe this issue, we examine the likelihood of choosing the *safest* (i.e. lowest variance, lowest return) lottery alternative in each of the five decision problems (results reported in Online Appendix Table A7). We find no evidence that the shift toward lower-variance lotteries is larger in decision problems that included a degenerate lottery that did not involve uncertainty. Thus, our results contrast with those of Callen, Isaqzadeh, Long, and Sprenger (2014). They find a differentially larger impact of violence (specifically, of a psychological prime that induces recollection of fearful episodes) on risk tolerance when no risk-free alternative is available; their treatment appears to increase the preference for certainty in a way that "is at odds with both [expected utility] and cumulative prospect theory, but consistent with models that feature a specific preference for certainty" (Callen, Isaqzadeh, Long, and Sprenger 2014, p. 138). Our results are consistent with expected utility maximization, though they could also be explained by an increase in loss aversion in the model of reference dependence proposed by Kőszegi and Rabin (2006).

variables estimates of the impact of the post-election violence on measured risk aversion. Two-stage least squares (2SLS) regression results are reported in Table 3. In Panel A, the outcome variable is the number of times that a respondent chose the highest variance, highest expected value lottery alternative. In Column 1, we report estimates from a parsimonious specification that includes no covariates. Coefficient estimates indicate that the post-election crisis had a large and statistically significant impact on measured risk preferences, reducing the number of times (out of five) that respondents opted for the riskiest, highest expected value lottery by approximately one full decision problem. Again, relative to a mean number of risky choices of 2.564 in the pre-crisis period, this is an extremely large effect. In Columns 2 through 5, we include controls for individual demographic characteristics (gender, age, education level, and marital status), the identity of the survey enumerator, and the calendar month in which the interview took place. Coefficient estimates are consistently negative and significant. Thus, our 2SLS estimates are consistent with our OLS results: Kenya's post-election crisis appears to have made survey respondents substantially more risk averse.

In Panels B and C of Table 3, we consider two alternative formulations of our dependent variable. In Panel B of Table 3, we estimate the impact of the post-election crisis on the likelihood of making choices consistent with risk neutral or risk loving preferences (by always choosing the highest variance or highest expected value lottery). Coefficient estimates suggest that exposure to the post-election violence decreased the likelihood of being risk neutral or risk loving by between 6.5 and 11.9 percentage points. In Panel C of Table 3, we present 2SLS estimates of the impact of the crisis on the likelihood of always choosing the lowest variance lottery. Coefficient estimates suggest that the crisis increased the likelihood of extreme risk aversion (consistent with a CRRA coefficient in excess of 1.78) by between 21.2 and 34.9 percentage points. In the pre-crisis period, only 13.4 percent of respondents displayed this level of risk aversion. Thus, 2SLS estimates suggest that the post-election violence more than doubled the likelihood of being extremely risk averse.

4.3 Threats to Identification

Thus far, we have shown that two distinct identification strategies generate similar estimates of the impact of Kenya's post-election violence on individual risk preferences. An important caveat is that both identification strategies exploit the timing of the crisis, so any other major shock that coincided with the post-election crisis could be driving our results. As is often the case, Kenya's episode of civil conflict created increased uncertainty about the country's economic and political future (though it did not, in the end, lead to a change in the head of state). As discussed above, such macroeconomic and political uncertainty is typical of civil strife, and a strength of our estimation strategy is that we capture the overall impacts of conflict on society — but this also means that we cannot, for example, separate the impacts of fear of violence from the impacts of political uncertainty. Nonetheless, to the extent possible, it is important to rule out the possibility that shocks coincident with — but not related to — the post-election crisis could explain our results.

One obvious concern is that the crisis might have triggered a period of high inflation, and that our results are driven by price changes rather than the conflict itself. Inflation rates were relatively high during and after the crisis: the CPI increased by more than 2 percent in January, April, and May of 2008.[32] It is likely that these high rates of inflation were at least partly attributable to the crisis itself (though Kenya experienced many other months of high inflation in the years before and after the crisis), and should therefore be viewed as a potential mediating factor rather than a competing explanation of our findings. We test whether inflation alone could explain our empirical results by replicating our main OLS and 2SLS specifications controlling for the inflation rate (results reported in Online Appendix Tables A8 through A11). All of our results are robust to including the inflation rate as a covariate; we find no evidence that the observed impact of conflict is explained by (only) the

[32]Online Appendix Figure A3 plots the monthly inflation rate (the percent change in prices relative to the previous month) from 2005 to 2010, using Consumer Price Index (CPI) data produced by the Kenya National Bureau of Statistics.

price instability created by the crisis.[33]

Another possible concern, already discussed above, is that our results could be driven by the fact that respondents surveyed after the post-election violence were, on average, older when they were surveyed — because the surveys were conducted over a two year period, and we compare those surveyed before the violence to those surveyed afterward. We attempt to address this issue by directly controlling for age and other factors (e.g. marital status and educational level) likely to change gradually over time as respondents grow up. However, if risk preferences change discretely at some point during the transition to adulthood, it is possible that a critical mass of respondents "aged out" of their youthful, risk-tolerant period around the time of the crisis. Fortunately, our data allow for a clear test of this hypothesis, since the sample includes a broad range of birth years. In Online Appendix Figure A4, we plot the average number of risky lottery choices (out of five) among respondents at each age level, separated by whether the survey took place before or after the post-election crisis. The figure shows that there is no discrete drop in risk tolerance at any particular age; instead, risk aversion appears relatively constant across the age range included in our sample. However, at all age levels, we see lower levels of risk tolerance after the crisis. Thus, our results do not appear to be driven by the fact that those surveyed after the post-election violence were, on average, older at the time that they were surveyed.

4.4 Other Impacts of the Post-Election Crisis

We interpret our results as evidence that civil conflict impacts individual risk preferences. An important question is what — if any — mechanism or mechanisms explain these impacts. One

[33]We also find no evidence of other aggregate shocks — for example, shocks to agricultural productivity — that could explain our results. Maize is the main staple crop for subsistence farmers in western Kenya, while sugar cane is the most widely grown cash crop. Data from the FAO statistical database indicate that Kenya's maize yield per hectare was slightly higher in 2007 than in the two previous years, and overall production levels were comparable. Sugar yields and production levels in 2007 were also comparable to the previous five years. Thus, our results do not seem likely to be driven agricultural or other aggregate shocks that were not associated with the post-election crisis.

possibility is that the post-election violence was an economic shock, and that young adults expected it to slow growth and reduce job opportunities; if having a lower expected permanent income makes one less willing to bear risk, such a shock might have impacted risk tolerance through job prospects.

To test this hypothesis, we estimate the impact of the post-election crisis on income-generating activities and wages directly. Results are reported in Online Appendix Table A12. We find no evidence that the post-election crisis reduced employment opportunities or income. The crisis did not have a significant impact on either the likelihood of reporting any income-generating activity (i.e. either employment or self-employment) or wages conditional on employment; it appears to have weakly increased the likelihood of being employed, but weakly decreased the likelihood of self-employment. None of these impacts is significant across the full range of specifications. Though many respondents were unable to work *during* the crisis, any negative impacts on respondents' wages and income seem to have disappeared after the crisis ended.[34]

Interestingly, however, we find strong evidence that respondents *believed* that the post-election violence lowered incomes. The KLPS2 survey asked respondents to report their beliefs about average monthly incomes in both Busia (the rural home area of KLPS2 respondents) and

[34]As a further test of the hypothesis that the post-election crisis impacted risk tolerance because it impacted individual earnings, we interact the indicator for being surveyed after the crisis with two characteristics likely to be associated with lower labor force attachment: educational attainment and gender. Intuitively, if negative wage impacts explain the effect of the crisis on risk tolerance, then we would expect smaller treatment effects in sub-populations that are less likely to be working outside the home. In Online Appendix Table A13, we estimate the differential impact of the crisis on respondents who did not complete primary school (relative to those who did). To address the concern that the crisis might have impacted the likelihood of completing primary school, we use an indicator for completing primary school by the end of 2006. Since most respondents were well above primary school age by 2006, the correlation between the indicators for completing primary school by the end of 2006 and completing primary school prior to the KLPS2 survey is quite high ($\rho = 0.91$). These individuals are likely to engage in subsistence agriculture or work locally in low-skill occupations — making them less vulnerable to changes in urban labor markets where ethnic differences might be more important. We find that the crisis had a significantly larger negative impact on the risk tolerance of the less educated (p-value < 0.001), though the impact is still significant among those who had completed primary school. When we examine treatment effect heterogeneity by gender — since women are less likely to participate in the labor market — coefficient estimates suggest that the crisis had a slightly larger impact on women than on men, though the effect is not statistically significant at conventional levels (p-value 0.103). Thus, we find no evidence that the crisis had a direct impact on wages or labor market opportunities, nor do we find evidence of a larger impact on those more likely to be involved in the formal labor market.

Nairobi. Importantly, these questions ask about current monthly income, so they should not reflect the very short-term impacts of being unable to work during the crisis. Results (reported on Online Appendix Table A12) indicate that the post-election crisis led to a dramatic decline in perceived income levels. Coefficient estimates suggest that the crisis caused more than a 25 percent decline in perceived average incomes; the effect is significant across a range of specifications.[35] Thus, young people believed that the post-election crisis was a negative income shock, but we find no evidence that — for these respondents — it actually was one.[36,37]

The post-election violence also appears to have changed the way KLPS2 respondents evaluate different job opportunities. The survey asks respondents to indicate which of six job attributes are the most important when they search for employment. The results (again reported on Online Appendix Table A12) show that the post-election crisis increased the likelihood of prioritizing job security (specifically, finding "a safe job, with no risk of closing down or unemployment") by at least 13.7 percentage points — as we would expect if the crisis increased risk aversion. The crisis also decreased the likelihood of prioritizing a job that put one close to "friends and relatives." Interestingly, the crisis does not appear to have impacted the likelihood of wanting a job that allowed one to work for or with members of one's own

[35]Incomes in the table are reported in 2007 US dollars. Throughout the paper, incomes are deflated using the Kenya National Bureau of Statistics' Consumer Price Index, and then converted to 2007 US dollars using the average exchange rate from August 2007.

[36]It is important to note that our respondents are not a representative sample Kenyan workers, so it is certainly possible that average incomes did decline after the crisis, but that the impacts were strongly concentrated among older workers or (in Nairobi) those from other ethnic groups — in which case, KLPS2 respondents' beliefs could accurately reflect a change in the average wage that did not impact them.

[37]With only one exogenous variable, it is impossible to fully tease apart any causal relationships between outcomes that were directly or indirectly impacted by the post-election crisis. Nonetheless, the magnitudes of the relevant coefficients suggest that changes in beliefs about income are not a plausible explanation for the entire observed decline in risk tolerance. The 2SLS estimates (which are larger in magnitude than the OLS estimates) suggest that the crisis lowered beliefs about average incomes by between 35.41 and 37.42 US dollars per month. Beliefs about income are positively associated with risk tolerance in the pre-crisis period (p-value 0.004), but the coefficient is small — a 100 dollar increase in one's belief about the average income is associated with a 0.253 unit increase in our index of risk tolerance. If we treat the pre-crisis relationship between beliefs and risk tolerance as causal, then a back-of-the-envelope calculation therefore suggests that the post-election violence lowered perceived incomes enough to lower the risk index by about 0.095 — but observed changes are at least five times larger than that. Thus, beliefs about income are not likely to be the primary mechanism through which the post-election crisis altered risk preferences. Concerns about the "bad controls" problem notwithstanding, our results are robust to the inclusion of beliefs about income as a right-hand-side variable.

ethnic group.

We do, however, find substantial evidence that the post-election crisis changed the social fabric of the country, even in areas like Busia District that were less affected than other parts of Kenya. The crisis destroyed social capital: we find a consistently significant negative impact of the crisis on the likelihood of being involved in at least one community group (Online Appendix Table A12). The post-election crisis also had a dramatic impact on generalized trust, cutting the number of respondents who believe most people can be trusted by more than 50 percent. In addition, the crisis had a consistently significant negative impact on the extent to which respondents trust members of other ethnic groups; point estimates suggest that trust in one's own ethnic group also declined, but the effect is not consistently significant. Though the overwhelming majority of KLPS2 respondents are from the Luhya ethnic group — so neither the president nor the main opposition candidate was a member of their ethnic group — this decline in trust across ethnic lines is consistent with the general pattern of post-election violence.[38,39]

Thus, though the crisis does not appear to have impacted the incomes and job market prospects of KLPS2 respondents, we find suggestive evidence that respondents believed that it did — it is clear that they believed that average incomes were lower after the violence, and they also shifted toward prioritizing job security over other job attributes. Yet those respondents least likely to be engaged in the labor market experienced larger impacts: their preference changes were larger. At the same time, we also find that the crisis eroded trust and destroyed social capital. Of course, violence may impact many aspects of society, and it is impossible to fully distinguish between mechanisms (driving the change in risk preferences) and other outcomes (that were simultaneously affected by the post-election violence). Moreover,

[38]It is worth noting that most of the KLPS2 survey enumerators were members of the local-majority Luhya ethnic group. Our results cannot be explained by the decline in trust of other ethnic groups after the crisis: we see the same robust association between exposure to the crisis and increased risk aversion in the sub-sample of respondents who were interviewed by a member of their own ethnic group.

[39]Trust and social capital are unlikely to explain the observed impact of the crisis on risk tolerance because there is no significant relationship between these variables and our measure of risk tolerance prior to the post-election violence.

as other have noted, the willingness to trust and the willingness to take risks are, from a theoretical perspective, closely related (Karlan 2005, Schechter 2007) — so, in fact, both the observed shift toward prioritizing job security and the observed decline in trust may be partially attributable to the observed increase in risk aversion. Importantly, however, the evidence suggests that the post-election violence did not impact risk preferences primarily through a labor market channel (i.e. through a dramatic drop in income); instead, we find suggestive evidence that the violence had a direct impact on individual preferences, beliefs, and values.

5 Conclusion

We measure the impact of Kenya's post-election violence on individual risk preferences. We find that experiencing the post-election crisis appears to have increased risk aversion significantly. Point estimates suggest that exposure to the crisis decreased the likelihood of making risk neutral or risk loving choices by 5.6 percentage points (26.1 percent), and increase the likelihood of always choosing the lowest variance, lowest expected value lottery by 10.9 percentage points (81.7 percent). Our results are robust to a range of controls and the use of entirely distinct identification strategies. Thus, the evidence suggests that exposure to the post-election crisis led to a statistically and economically significant decrease in the willingness to take profitable risks.

In relation to existing studies of the determinants of individual preferences, our results corroborate a growing body of evidence that preferences are impacted by major life events such as conflict, disasters, and economic downturns. Identification of the impacts of such major events is always challenging since exogenous variation in exposure to historical shocks is rare. Existing studies of the impact of conflict on individual preferences estimate the marginal impact of greater exposure to violence, implicitly treating less-exposed survivors as a comparison group. We are able to expand this literature because our identification strategies enable

us to measure the effect of civil conflict on a broad population, using respondents who had not (yet) lived through the crisis as our comparison group. Relatively more conflict-affected individuals may become more pro-social or less risk averse (Bauer, Cassar, Chytilová, and Henrich 2013, Callen, Isaqzadeh, Long, and Sprenger 2014), but the impact on the population as a whole is a shift toward less willingness to take profitable risks. We estimate the combined effects of violence and any attendant change in perceptions of political uncertainty. The drawback to this approach is that we cannot separate the effect of violence from the effects of concomitant changes in Kenya's political situation. However, this combination of violence and political uncertainty is inherent in almost all civil conflicts: the relevant estimand, is, in fact, their combined effect.

Though both the political crisis and the economic downturn triggered by the post-election crisis were relatively short-lived, we find that the impacts on risk preferences persisted for more than a year. Moreover, our results suggest that the channel of impact is not (primarily) economic since the probability of engaging in any income-generating activity is not impacted by the violence, but generalized trust and social capital are impacted. Our results therefore suggest that conflict may have long-lasting impacts on economic development through channels such as reduced entrepreneurship. That our findings differ from some earlier studies suggests that the impacts of conflict at the scale of affected individuals and villages may not generalize to the scale of a district, or that of a nation.

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Table 1: OLS Regressions of the Impact of the Post-Election Crisis on Risk Preferences

Specification:	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)
Surveyed after post-election crisis	-0.682***	-0.685***	-0.798***	-0.559***	-0.734***
	(0.117)	(0.114)	(0.118)	(0.085)	(0.088)
Demographic controls	No	Yes	No	No	Yes
Interviewer controls	No	No	Yes	No	Yes
Month controls	No	No	No	Yes	Yes
Observations	5047	5047	5047	5047	5047
R^2	0.023	0.028	0.151	0.027	0.157

Robust standard errors clustered at the month level in all specifications. Outcome variable is the number of times a respondent chose the riskiest (i.e. highest variance or highest expected value) lottery (out of five). Columns 2 and 5 include controls for gender, age, education level, and marital status (at the time of the survey).

Table 2: OLS Regressions of the Impact of the Post-Election Crisis on Alternate Measures of Risk Aversion

Dependent Variable:	Risk neutral or loving	Most risk averse	Choice on decision number...				
			2	3	4	5	6
Specification:	OLS	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Surveyed after post-election crisis	-0.056***	0.109***	-0.114***	-0.233***	-0.296***	-0.29***	-0.26***
	(0.017)	(0.023)	(0.027)	(0.045)	(0.061)	(0.031)	(0.053)
Observations	5047	5047	5047	5047	5047	5047	5047
R^2	0.007	0.033	0.015	0.017	0.027	0.023	0.025

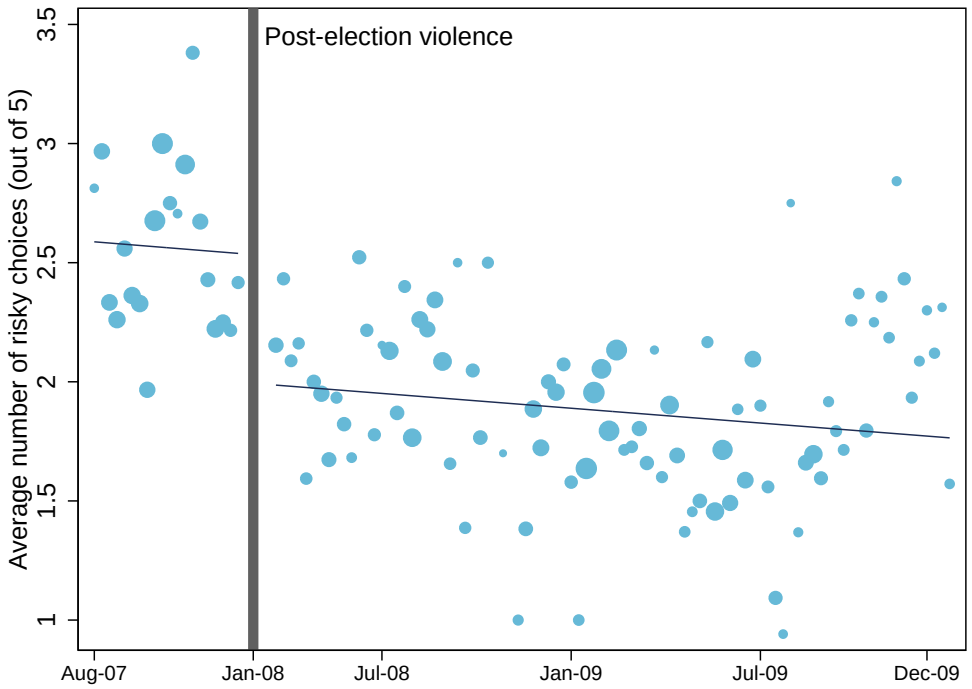
Robust standard errors clustered at the month level in all specifications. All specifications include controls for gender, age, education level, and marital status (at the time of the survey). Thus, the specifications in this table mirror those in Table 1, Column 2. Here, the outcome variables vary by column. The outcome in Column 1 is an indicator for making choices that can be rationalized by a CRRA coefficient of 0.19 or less (i.e. always choosing the highest variance or highest expected value lottery). In Column 2, the outcome variable is an indicator for behaving in a manner consistent with a CRRA coefficient of 1.78 or above (always choosing the lowest variance, lowest expected value lottery), so its sign is opposite those in other columns and tables. Columns 3 through 7 use discrete representations of the separate decision problems as outcomes, ordered by risk aversion; thus, the dependent variable in Column 3 is an indicator for choosing the higher variance lottery in Decision Problem 2. The dependent variables in the remaining columns take on the values 1, 2, and 3, with 1 being least risky (i.e. lowest variance, lowest expected value) lottery and 3 being riskiest (i.e. highest variance, highest expected value) lottery.

Table 3: IV Regressions of the Impact of the Post-Election Crisis on Risk Preferences

Specification:	2SLS (1)	2SLS (2)	2SLS (3)	2SLS (4)	2SLS (5)
Panel A. Dependent variable: number of risky (highest variance) choices (out of 5)					
Surveyed after post-election crisis	-0.986***	-1.050***	-1.670***	-1.010***	-1.994***
	(0.202)	(0.224)	(0.443)	(0.229)	(0.59)
Panel B. Dependent variable: choices consistent with being risk neutral or risk loving					
Surveyed after post-election crisis	-0.065**	-0.083**	-0.067*	-0.08***	-0.119**
	(0.029)	(0.034)	(0.035)	(0.031)	(0.053)
Panel C. Dependent variable: choices consistent with being most risk averse					
Surveyed after post-election crisis	0.212***	0.212***	0.322***	0.223***	0.349***
	(0.063)	(0.065)	(0.117)	(0.071)	(0.119)
First stage F-stat	9.66	9.23	8.54	14.38	11.54
Demographic controls	No	Yes	No	No	Yes
Interviewer controls	No	No	Yes	No	Yes
Month controls	No	No	No	Yes	Yes
Observations	5047	5047	5047	5047	5047

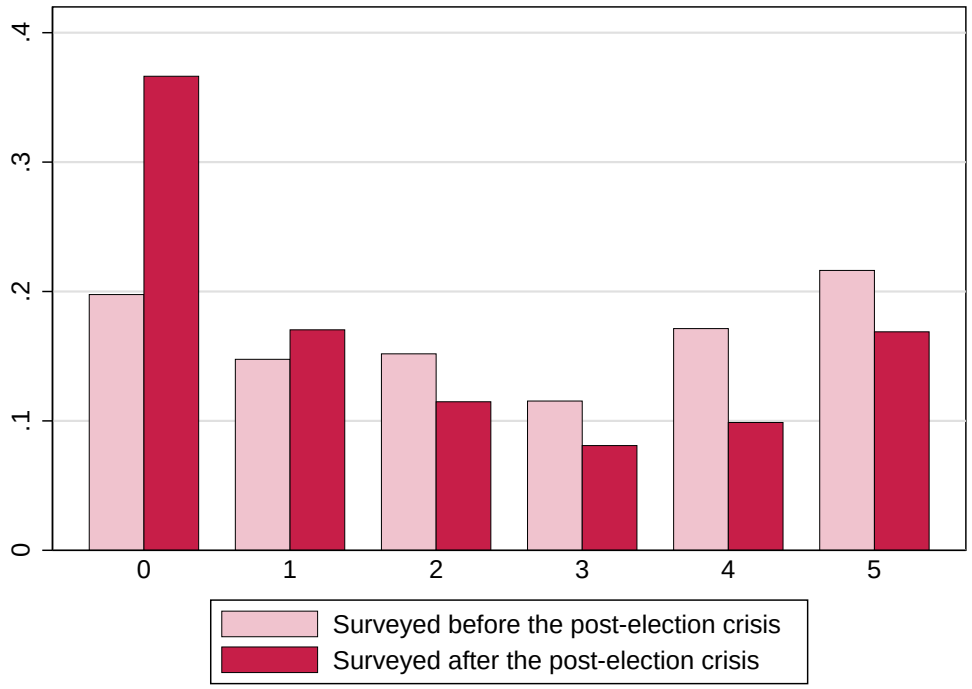
Robust standard errors clustered by survey month in all specifications. All specifications estimated via two-stage least squares. All specifications include controls for gender, age, education level, and marital status (at the time of the survey). In all specifications, we instrument for being surveyed after the post-election crisis with the indicator for being randomly assigned to the second wave of surveying. The outcome variable in Panel A is the number of times a respondent chose the riskiest (i.e. always choosing the highest variance or highest expected value lottery) lottery (out of five). The outcome variable in Panel B is an indicator for making risk neutral or risk loving choices (always choosing the highest variance, highest expected value lottery). The outcome variable in Panel C is an indicator for always choosing the lowest variance, lowest expected value lottery.

Figure 1: Risk Preferences Before and After Kenya's Post-Election Crisis



Notes: Gray circles indicate the average number of risky (i.e. highest variance or highest expected value) choices (out of five) by respondents surveyed in a given week, weighted by the number of surveys conducted during the week. Lines are linear regressions of the number of risk choices on the survey week, counting from the first week in which surveys were conducted.

Figure 2: Histogram of Primary Risk Index Before and After the Post-Election Crisis



Notes: The primary risk index is the number of times a respondent chose the highest variance or highest expected value lottery alternative over the course of five lottery choice questions. The probability of having a risk index of zero or one is significantly higher after the post-election crisis (p-values < 0.001 and 0.075, respectively, after adjusting for clustering by survey month). The probability of having a risk index of two, three, four, or five is significantly lower after the post-election crisis (p-values 0.037, 0.014, < 0.001, and 0.005, respectively, after adjusting for clustering).

A Online Appendix: not for print publication

A.1 Experimental Instructions

A.1.1 English

Now I am going to show you some cards, each with a game. The games are about money, so although we are not playing the games with real money, I would like you to take them seriously, and decide which choices you would make if we were playing for real money.

First I will show you an example.

Show example card 1.

The games will be like this one. Each game has two or three sections; this one has two sections. In each section, there are two amounts of money. In each section, imagine that we will flip a coin.

Show coin.

The coin has two sides: heads and tails.

If the coin lands on heads, you would receive the first amount of money. In the first section here, that is 100 shillings. If the coin lands on tails, you would receive the second amount of money. In the first section here, that is also 100 shillings. So in the first section, there is an equal chance that you will get the 100 shillings on the left, or the 100 shillings on the right.

In the second section, if the coin lands on heads, you would receive 150 shillings; and if the coin lands on tails, you would also receive 150 shillings. So in the second section, there is an equal chance that you will get the 150 shillings on the left, or the 150 shillings on the right.

Although we are not playing for real money, if this game did use real money, which section would you prefer, section A or section B?

Whichever the respondent chooses, ask her to explain. If the respondent chooses section A, probe and see if she understands that section B would definitely mean more money.

Here is another example game.

Show example card 2.

In the first section, if the coin lands on heads, you would receive 100 shillings; but if it lands on tails, you would receive 150 shillings. In the second section, if the coin lands on heads, you would receive 200 shillings, but if it lands on tails, you would receive 250 shillings. In this game, which section would you choose?

Whichever the respondent chooses, ask her to explain. If the respondent chooses section A, probe and see if she understands that section B would definitely mean more money.

For each game, ask the question in a similar way. Prompt the respondent to read aloud the values of the money, to keep them involved and to make sure they understand. For each of the cards, please indicate which choice the respondent made: A, B, or C.

A.1.2 Swahili

Sasa, nitakuonyesha kadi, kila kadi na mchezo. Michezo hizi ni kuhusu pesa, ingawaje hatuchezi mchezo na pesa zenyewe, ningependelea uichukulie kimanani, na uamue ni ipi utachagua ikiwa unachezea pesa zenyewe.

Kwanza, nitakuonyesha mfano.

Show example card 1.

Michezo yenyewe itakuwa kama hii. Kila mchezo ina sehem mbili au tatu; hii ina sehemu mbili. Katika kila sehemu, kuna kiasi mara mbili ya pesa. Katika kila sehemu, ufikirie kuwa tutarusha shilingi.

Show coin.

Hii shilingi ina upande mbili: kichwa na simba.

Ikiwa shilinga itaangukia upande wa kichwa, utapata kiasi cha kwanza cha pesa. Katika sehemu ya kwanza hapa, kuna shillingi mia moja (100). Ikiwa shilingi itaangukia upande wa simba, utapata kiasi cha pili cha pesa. Katika sehemu ya kwanza hapa, kuna shilingi mia moja (100) pia. Kwa hivyo, katika sehemu ya kwanza, kuna wezekano sawa kuwa utapata shilingi mia moja ya kushoto, au shilingi mia moja ya kulia.

Katika sehemu ya pili, ikiwa shilingi itaangukia upande wa kichwa, utapata shilingi mia moja hamsini (150); na ikiwa shilingi itaangukia upande wa simba, pia utapata shilingi mia moja hamsini (150).

Ingawaje hatuchezi mchezo na pesa zenyewe, ikiwa mchezo huu ulikuwa unatumia pesa zenyewe, ungependelea sehemu ipi, sehemu ya A au sehemu ya B?

Whichever the respondent chooses, ask her to explain. If the respondent chooses section A, probe and see if she understands that section B would definitely mean more money.

Hapa kuna mfano wa mchezo mwingine.

Show example card 2.

Katika sehemu ya kwanza, ikiwa shilinga itaangukia upande wa kichwa, utapata shillingi mia moja (100). Lakini, ikiwa shilingi itaangukia upande wa simba, utapata shilingi mia moja hamsini (150). Katika sehemu ya pili, ikiwa shilinga itaangukia upande wa kichwa, utapata shillingi mia mbili (200). Lakini, ikiwa shilingi itaangukia upande wa simba, utapata shilingi mia mbili hamsini (250). Ungependelea sehemu ipi, sehemu ya A au sehemu ya B?

Whichever the respondent chooses, ask her to explain. If the respondent chooses section A, probe and see if she understands that section B would definitely mean more money.

For each game, ask the question in a similar way. Prompt the respondent to read aloud the values of the money, to keep them involved and to make sure they understand. For each of the cards, please indicate which choice the respondent made: A, B, or C.

MCHEZO 2

Chaguo la A

KSh 100

Chaguo la B

KSh 0

KSh 400

A.2 Additional Tables and Figures

Table A1: Decision Problems Embedded in Kenyan Life Panel Survey 2

	Option A		**Option B**		**Option C**	
	Heads	**Tails**	**Heads**	**Tails**	**Heads**	**Tails**
Practice Decision Problem 1	100	100	150	150	-	-
Practice Decision Problem 2	100	150	200	250	-	-
Decision Problem 1	100	100	100	120	-	-
Decision Problem 2	100	100	0	400	-	-
Decision Problem 3	30	340	100	100	0	400
Decision Problem 4	100	100	55	240	30	340
Decision Problem 5	30	230	60	170	90	110
Decision Problem 6	10	200	70	160	90	110

All payouts in Kenyan shillings.

Table A2: Balance Check — KLPS2 Wave 1 vs. Wave 2 Comparison

Variable	Wave 1	Wave 2	Difference
Female	0.489	0.490	0.001
	(0.010)	(0.010)	(0.014)
Age in 2007	21.126	21.212	0.085
	(0.052)	(0.051)	(0.073)
Born in Busia District	0.878	0.877	-0.002
	(0.007)	(0.006)	(0.009)
Luhya ethnic group	0.955	0.952	-0.003
	(0.004)	(0.004)	(0.006)
Highest grade completed by 2006	8.560	8.480	-0.079
	(0.043)	(0.042)	(0.060)
Married by 2006	0.309	0.301	-0.009
	(0.009)	(0.009)	(0.013)
Number of observations	2468	2579	

Standard errors in parentheses. Respondents were randomly assigned to one of the two waves of surveying.

Table A3: Probit Regressions of Self-Employment and Migration on Risk Index

Dependent Variable:	Self-Employed		Migrated Seeking Work		Either Risky Behavior	
Specification:	Probit (1)	Probit (2)	Probit (3)	Probit (4)	Probit (3)	Probit (4)
Times respondent chose riskiest lottery	0.007***	0.005*	0.003**	0.002*	0.008***	0.005**
	(0.002)	(0.002)	(0.001)	(0.001)	(0.003)	(0.003)
Demographic controls	No	Yes	No	Yes	No	Yes
Interviewer controls	No	Yes	No	Yes	No	Yes
Month controls	No	Yes	No	Yes	No	Yes
Observations	5047	5047	5045	5005	5047	5047
Pseudo R^2	0.002	0.112	0.004	0.196	0.002	0.119

Robust standard errors in parentheses. Probit marginal effects reported. Independent variable is the number of times a respondent chose the riskiest (i.e. highest variance or highest expected value) lottery (out of five). The dependent variable in Columns 5 and 6 is an indicator equal to one if a respondent is self-employed or has migrated in search of work. Even-numbered columns include controls for gender, age, education level, marital status, and survey month and interviewer fixed effects.

Table A4: Probit Regressions of Self-Employment and Migration on Risk Index — Pre-Crisis vs. Post-Crisis

Dependent Variable:	Self-Employed		Migrated Seeking Work		Either Risky Behavior	
Pre-Crisis or Post-Crisis?	Pre-Crisis	Post-Crisis	Pre-Crisis	Post-Crisis	Pre-Crisis	Post-Crisis
Specification:	Probit	Probit	Probit	Probit	Probit	Probit
	(1)	(2)	(3)	(4)	(3)	(4)
Times respondent chose riskiest lottery	0.014***	0.005	0.004	0.003*	0.015***	0.006*
	(0.005)	(0.003)	(0.003)	(0.002)	(0.006)	(0.003)
Observations	1179	3868	1178	3867	1179	3868
Pseudo R^2	0.008	0.001	0.005	0.003	0.007	0.001

Robust standard errors in parentheses. Probit marginal effects reported. Independent variable is the number of times a respondent chose the riskiest (i.e. highest variance or highest expected value) lottery (out of five). The dependent variable in Columns 5 and 6 is an indicator equal to one if a respondent is self-employed or has migrated in search of work. Even-numbered columns include controls for gender, age, education level, marital status, and survey month and interviewer fixed effects.

Table A5: Ordered Logit Regressions of the Impact of the Crisis on Risk Preferences

Specification:	Ordered Logit (1)	Ordered Logit (2)	Ordered Logit (3)	Ordered Logit (4)	Ordered Logit (5)
Surveyed after post-election crisis	-0.64***	-0.64***	-0.787***	-0.538***	-0.742***
	(0.105)	(0.103)	(0.12)	(0.088)	(0.102)
Demographic controls	No	Yes	No	No	Yes
Interviewer controls	No	No	Yes	No	Yes
Month controls	No	No	No	Yes	Yes
Observations	5047	5047	5047	5047	5047
Pseudo R^2	0.007	0.009	0.045	0.008	0.048

Robust standard errors clustered at the month level in all specifications. Outcome variable is the number of times a respondent chose the riskiest (i.e. highest variance or highest expected value) lottery (out of five). Columns 2 and 5 include controls for gender, age, education level, and marital status (at the time of the survey).

Table A6: The Impact of the Crisis on Risk Preferences — Controlling for Time Trends

Specification:	OLS (1)	OLS (2)	Ordered Logit (3)	Ordered Logit (4)
Surveyed after post-election crisis	-0.606***	-0.79***	-0.531***	-0.315*
	(0.184)	(0.116)	(0.174)	(0.169)
Demographic controls	No	Yes	No	Yes
Interviewer controls	No	Yes	No	Yes
Linear time trends	Yes	Yes	Yes	Yes
Observations	5047	5047	5047	5047
R^2	0.024	0.155	.	.
Pseudo R^2	.	.	0.007	0.052

Robust standard errors clustered at the month level in all specifications. Outcome variable is the number of times a respondent chose the riskiest (i.e. highest variance or highest expected value) lottery (out of five). Columns 2 and 4 include controls for gender, age, education level, and marital status (at the time of the survey).

Table A7: The Impact of the Crisis on the Likelihood of Choosing the Safest Alternative

Dependent Variable:	Safest Choice on decision number...				
	2	3	4	5	6
Specification:	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)
Surveyed after post-election crisis	0.114***	0.114***	0.109***	0.203***	0.18***
	(0.027)	(0.027)	(0.029)	(0.016)	(0.028)
Observations	5047	5047	5047	5047	5047
R^2	0.015	0.017	0.019	0.037	0.033

Robust standard errors clustered at the month level in all specifications. All specifications include controls for gender, age, education level, and marital status (at the time of the survey). Thus, the specifications in this table mirror those in Column 2 of Table 1. The outcome in each case is an indicator variable for taking the safest (lowest variance, lowest expected value) lottery in a given decision.

Table A8: OLS Regressions of the Impact of the Crisis — Controlling for Inflation

Specification:	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)
Surveyed after post-election crisis	-0.682***	-0.679***	-0.798***	-0.559***	-0.739***
	(0.117)	(0.112)	(0.118)	(0.085)	(0.101)
Demographic controls	No	Yes	No	No	Yes
Interviewer controls	No	No	Yes	No	Yes
Month controls	No	No	No	Yes	Yes
Observations	5047	5047	5047	5047	5047
R^2	0.023	0.029	0.151	0.027	0.158

Robust standard errors clustered at the month level in all specifications. Outcome variable is the number of times a respondent chose the riskiest (i.e. highest variance or highest expected value) lottery (out of five). Columns 2 and 5 include controls for gender, age, education level, and marital status (at the time of the survey).

Table A9: Ordered Logit Regressions of the Impact of the Crisis — Controlling for Inflation

Specification:	Ordered Logit (1)	Ordered Logit (2)	Ordered Logit (3)	Ordered Logit (4)	Ordered Logit (5)
Surveyed after post-election crisis	-0.64***	-0.634***	-0.787***	-0.538***	-0.749***
	(0.105)	(0.1)	(0.12)	(0.088)	(0.117)
Demographic controls	No	Yes	No	No	Yes
Interviewer controls	No	No	Yes	No	Yes
Month controls	No	No	No	Yes	Yes
Observations	5047	5047	5047	5047	5047
Pseudo R^2	0.007	0.009	0.045	0.008	0.048

Robust standard errors clustered at the month level in all specifications. Outcome variable is the number of times a respondent chose the riskiest (i.e. highest variance, or highest expected value) lottery (out of five). Columns 2 and 5 include controls for gender, age, education level, and marital status (at the time of the survey).

Table A10: The Impact of the Post-Election Crisis — Inflation and Time Trends

Specification:	OLS (1)	OLS (2)	Ordered Logit (3)	Ordered Logit (4)
Surveyed after post-election crisis	-0.606***	-0.792***	-0.531***	-0.273**
	(0.184)	(0.121)	(0.174)	(0.122)
Demographic controls	No	Yes	No	Yes
Interviewer controls	No	Yes	No	Yes
Linear time trends	Yes	Yes	Yes	Yes
Observations	5047	5047	5047	5047
R^2	0.024	0.155	.	.
Pseudo R^2	.	.	0.007	0.053

Robust standard errors clustered at the month level in all specifications. Outcome variable is the number of times a respondent chose the riskiest (i.e. highest variance or highest expected value) lottery (out of five). Columns 2 and 4 include controls for gender, age, education level, and marital status (at the time of the survey).

Table A11: IV Regressions of the Impact of the Crisis — Controlling for Inflation

Specification:	2SLS (1)	2SLS (2)	2SLS (3)	2SLS (4)	2SLS (5)
Panel A. Dependent variable: number of risky (highest variance) choices (out of 5)					
Surveyed after post-election crisis	-0.986***	-1.054***	-1.670***	-1.010***	-1.784***
	(0.202)	(0.222)	(0.443)	(0.229)	(0.48)
Panel B. Dependent variable: risk neutral or risk loving					
Surveyed after post-election crisis	-0.065**	-0.083**	-0.067*	-0.08***	-0.129***
	(0.029)	(0.034)	(0.035)	(0.031)	(0.048)
Panel C. Dependent variable: most risk averse					
Surveyed after post-election crisis	0.212***	0.215***	0.322***	0.223***	0.297***
	(0.063)	(0.065)	(0.117)	(0.071)	(0.09)
First stage F-stat	9.66	9.27	8.54	14.38	15.53
Demographic controls	No	Yes	No	No	Yes
Interviewer controls	No	No	Yes	No	Yes
Month controls	No	No	No	Yes	Yes
Observations	5047	5047	5047	5047	5047

Robust standard errors clustered by survey month in all specifications. All specifications estimated via two-stage least squares. All specifications include controls for gender, age, education level, and marital status (at the time of the survey). The outcome variable in Panel A is the number of times a respondent chose the riskiest lottery (out of five). The outcome variable in Panel B is an indicator for making risk neutral or risk loving choices (always choosing the highest variance or highest expected value lottery). The outcome variable in Panel C is an indicator for always choosing the lowest variance, lowest expected value lottery.

Table A12: Impacts on Labor Market Outcomes, Beliefs, and Social Capital

Specification:		OLS (1)	OLS (2)	2SLS (3)	2SLS (4)
Income-generating activities and beliefs about the labor market:					
Any off-farm income-generating activity (IGA)	0.202	0.086**	0.027	0.070	-0.044
		(0.033)	(0.029)	(0.057)	(0.058)
Self-employed	0.126	-0.003	-0.035**	-0.016	-0.071**
		(0.019)	(0.016)	(0.029)	(0.033)
Paid work for someone else	0.081	0.087***	0.059***	0.089*	0.028
		(0.021)	(0.018)	(0.052)	(0.052)
Total off-farm labor income (conditional on any IGAs)	35.320	4.909*	0.448	2.233	-5.140
		(2.718)	(2.814)	(10.625)	(10.494)
Belief about average income from paid work	90.270	-25.364***	-24.943***	-35.411***	-37.423***
		(2.301)	(2.563)	(6.002)	(7.016)
Job attributes chosen as most important:					
Earning a high income	0.484	-0.017	-0.026	0.077	0.073
		(0.033)	(0.033)	(0.065)	(0.069)
Job security	0.488	0.146***	0.139***	0.207***	0.206***
		(0.019)	(0.024)	(0.052)	(0.058)
Proximity to friends, relatives	0.189	-0.050***	-0.049***	-0.066**	-0.069**
		(0.014)	(0.013)	(0.034)	(0.033)
Working with people one likes	0.199	-0.020	-0.010	-0.024	-0.011
		(0.018)	(0.021)	(0.032)	(0.034)
Doing an important job	0.555	-0.038	-0.039	-0.175**	-0.189**
		(0.027)	(0.025)	(0.086)	(0.089)
Working for one's own ethnic group	0.085	-0.022	-0.015	-0.018	-0.008
		(0.019)	(0.017)	(0.026)	(0.027)
Trust and social capital:					
Member of at least one community group	0.730	-0.277***	-0.245***	-0.480***	-0.460***
		(0.027)	(0.030)	(0.109)	(0.108)
Most people can be trusted	0.118	-0.061***	-0.057***	-0.095***	-0.095***
		(0.010)	(0.011)	(0.022)	(0.024)
Feels members of own tribe can be trusted	0.539	-0.047**	-0.021	-0.106*	-0.079
		(0.022)	(0.026)	(0.057)	(0.063)
Feels members of other tribes can be trusted	0.254	-0.065***	-0.058**	-0.129**	-0.134**
		(0.021)	(0.023)	(0.051)	(0.057)

Robust standard errors clustered by month in all specifications. Columns 1 and 2 (resp. 3 and 4) replicate the specifications presented in Columns 1 and 2 of Table 1 (resp. Table 3). In Columns 3 and 4, we instrument for being surveyed after the crisis with a respondent's randomly-assigned survey wave. Columns 2 and 4 include controls for gender, age, education level, and marital status (at the time of the survey). Outcome variables are listed in the leftmost column. Any off-farm income-generating activity is an indicator for reporting either self-employment or paid work for others. Total off-farm labor income is income from self-employment and paid work over the month prior to the survey, conditional on reporting any off-farm IGA. Belief about average income from paid work is the average of a respondent's beliefs about the average income of those with either a primary school or secondary school education living in either Nairobi or Busia, Kenya. Most people can be trusted denotes the response to the question "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?"

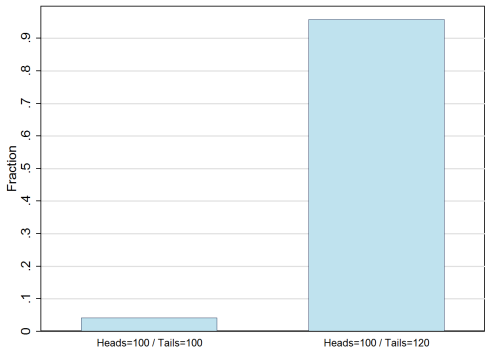
Table A13: Impact of the Crisis on Risk Preferences — Treatment Effect Heterogeneity

Specification:	OLS (1)	OLS (2)
Surveyed after post-election crisis	-0.48***	-0.532***
	(0.12)	(0.088)
Surveyed after post-election crisis × incomplete primary	-0.633***	.
	(0.088)	
Surveyed after post-election crisis × female	.	-0.333
		(0.197)
Observations	5047	5047
R^2	0.033	0.03

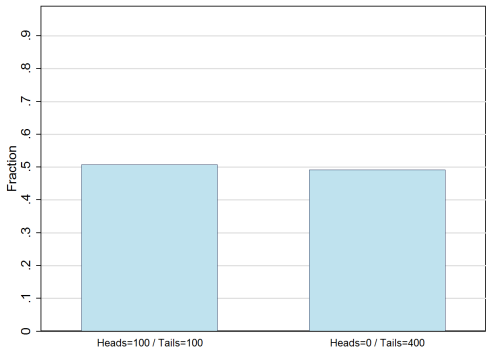
Robust standard errors clustered at the month level in all specifications. Outcome variable is the number of times a respondent chose the riskiest (i.e. highest variance or highest expected value) lottery (out of five). The specification in each column includes a different indicator variable as well as its interaction with the indicator for being surveyed after the crisis. Only the coefficient on the interaction is shown.

Figure A1: Histograms of Individual Choices in Each Decision Problem

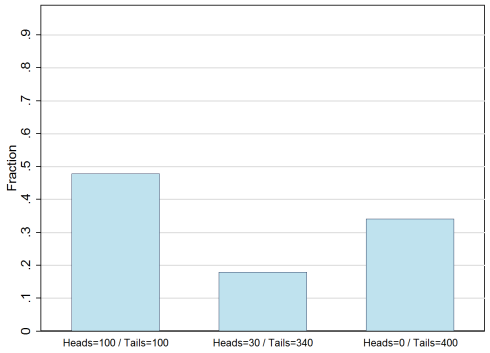
Decision Problem 1



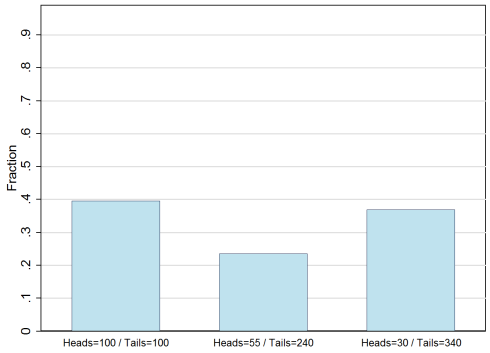
Decision Problem 2



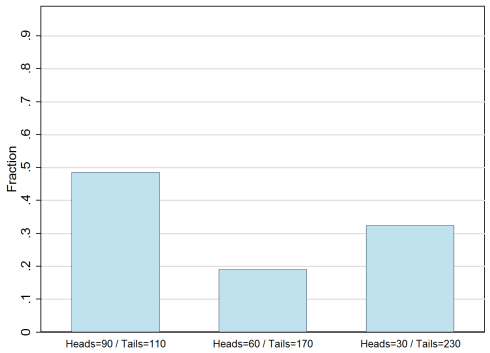
Decision Problem 3



Decision Problem 4



Decision Problem 5



Decision Problem 6

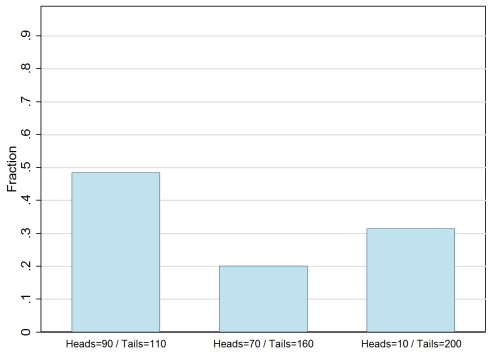
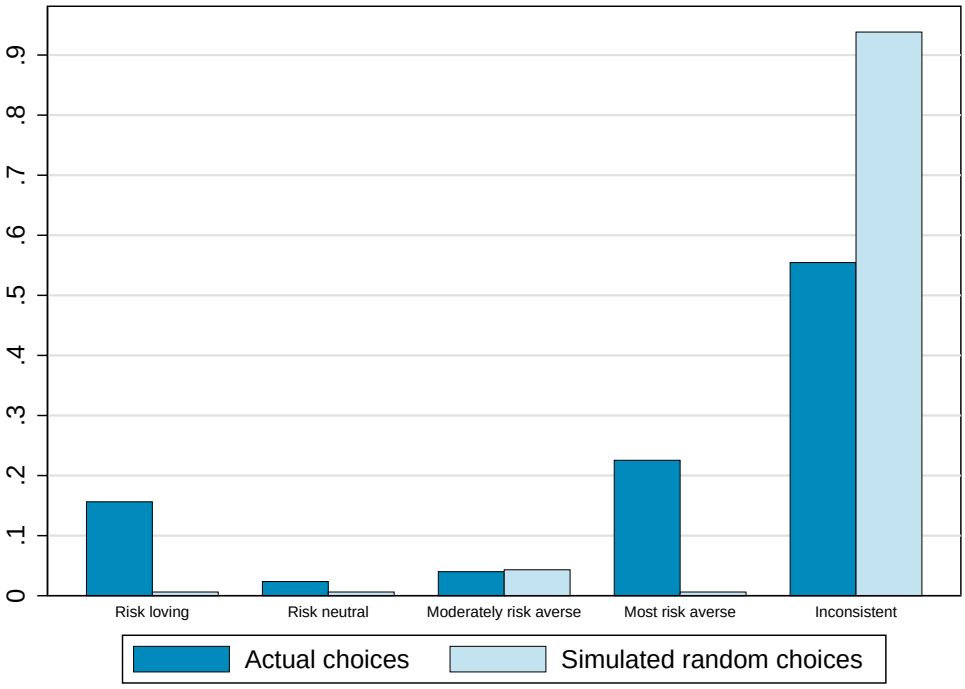
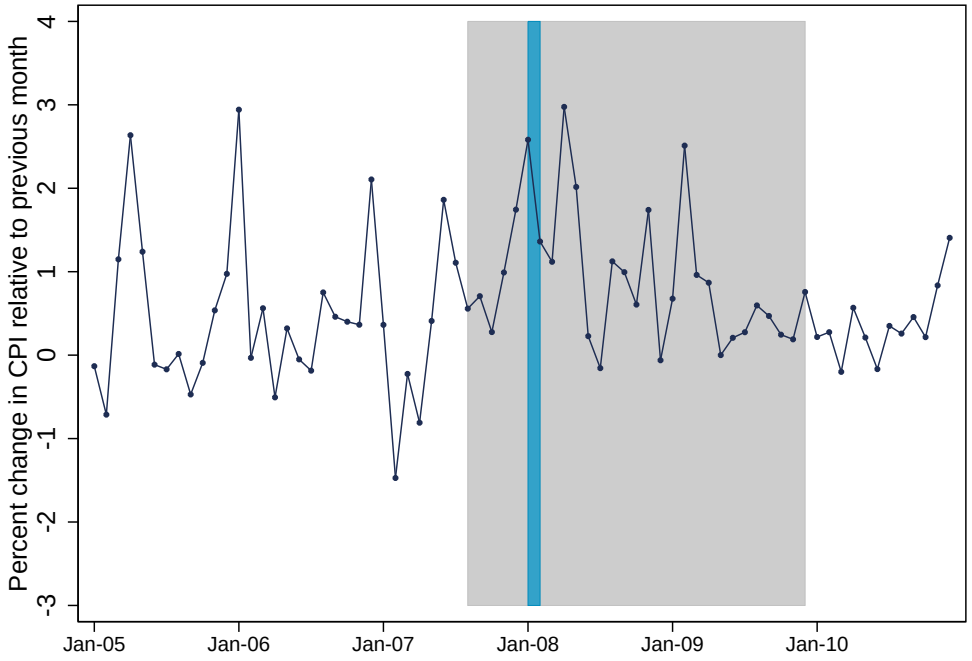


Figure A2: Histogram of Patterns of Responses to Lottery Choice Questions



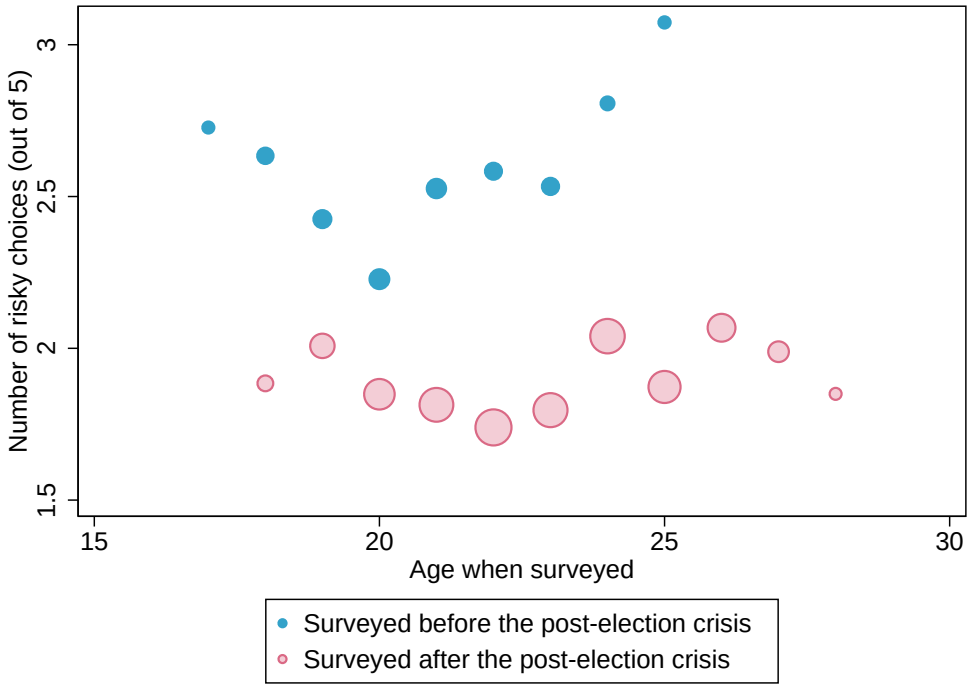
Notes: The Figure presents the fraction of respondents falling into each of five different categories: (1) those who always choose the highest variance lotteries ($\rho < -0.26$), (2) those who always choose the highest expected value lotteries ($-0.26 \leq \rho < 0.19$), (3) those who always choose the lowest variance lotteries ($\rho \geq 1.78$), (4) other subjects whose choices were consistent with a CRRA utility representation ($0.26 \leq \rho < 1.78$), and (5) those whose choices were not consistent with a CRRA utility representation. The figure compares the observed distribution of choice patterns to what we would observe if respondents chose lotteries at random (and each alternative had an equal probability of being chosen). Patterns of choices that are consistent with the maximization of a CRRA utility function occur substantially more frequently than we would expect if respondents were selecting lotteries at random: the probability of making CRRA-consistent choices by chance is only 0.06. However, the figure shows that only the three extreme types of consistent behavior (always choosing the lowest variance lottery, always choosing the highest expected value lottery, and always choosing the highest variance lottery) occur more frequently than random choice would suggest

Figure A3: Percent Changes in the Consumer Price Index — 2005–2010



The figure plots the percent change in the Consumer Price Index relative to the previous month. Data were downloaded from the website of the Kenya National Bureau of Statistics (`http://www.knbs.or.ke/index.php?option=com_phocadownload&view=category&download=14:quarterly-cpi&id=8:consumer-price-indices-cpi&Itemid=562`). The gray area indicates the period of the Kenyan Life Panel Survey; the darker blue area indicates the period of the post-election crisis.

Figure A4: Risk Preferences by Age when Surveyed — Before and After the Crisis



Notes: Blue circles indicate the average number of risky (i.e. highest variance or highest expected value) choices (out of five) among respondents of each age (in years) who were surveyed prior to the post-election crisis, weighted by the number of surveys in that cell. Red circles indicate the average number of risky choices among respondents of each age (in years) who were surveyed after to the post-election crisis, weighted by the number of surveys in that cell.