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Essays in Development, Governance, and Decisions

A dissertation submitted in partial satisfaction of the
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by

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2011

The dissertation of Michael Callen is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California, San Diego

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DEDICATION

To the memory of my grandfather Joseph Callen, whose habits inspired two chapters and whose unwavering dedication taught me the work ethic to complete the third.

EPIGRAPH

An essential question we must ask is, who makes the rules and for whom and what are their objectives.

—Douglass Cecil North

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ABSTRACT OF THE DISSERTATION

Essays in Development, Governance, and Decisions

by

Michael Callen

Doctor of Philosophy in Economics

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Professor Eli Berman, Chair

In this dissertation I focus on the political economy of corruption in fraudulent elections and on the effects of trauma on economic decision making. In Chapter 1, I provide results from a Randomized Control Trial (RCT) impact evaluation of a novel anti-fraud technology *Photo Quick Count*, designed to reduce fraud involving transactions between corrupt officials and parliamentary candidates. In Chapter 2, I provide results from a novel field experiment which uses methods from lab experimental economics, psychology, and field experimental economics to link trauma to economic decision making. In Chapter 3, I theoretically develop the impact trauma should have on measured time preference and provide a preliminary non-experimental test using data from populations affected by the 2004 Indian Ocean Earthquake.

Chapter 1

Institutional Corruption and Election Fraud: Evidence from a Field Experiment in Afghanistan

Abstract

Elections in developing countries commonly fail to deliver accountability because of manipulation, which often involves corrupt election officials selling votes to candidates. We report the results of an experimental evaluation of Quick Count Photo Capture—a monitoring technology aimed at detecting election fraud that involves collusion between candidates and corrupt officials. We find strong evidence that the intervention reduced election fraud in the 2010 parliamentary elections in Afghanistan. Independent fraud measurements at separate stages of the ballot aggregation process show that the intervention displaced fraud both temporally and spatially. The pattern of displacement depends critically on the strength of candidates' political connections. These results support a theory of corrupt vote transactions in which the capacity of the candidate to protect corrupt officials determines the equilibrium price of illegal votes.

1.1 Introduction

Free and fair elections are central to democracy and provide a vital means of empowering citizens to hold politicians accountable.[1] Election fraud commonly undermines this critical function in many young democracies largely due to weak electoral institutions. In particular, the rents from political office provide strong incentives for candidates to bribe government election commission officials to illegally alter vote totals. Corruption—the illegal selling of votes by a government official with the power to alter candidate vote totals—therefore poses a direct threat to democracy.

The positive analysis of corruption focuses on the determinants of equilibrium patterns of corruption (Shleifer and Vishny, 1993; Cadot, 1987; Rose-Ackerman, 1975). More recent empirical work documents the role of bribe payer endowments in corrupt transactions. Svensson (2003) documents the relevance of firm profitability and outside options for corrupt transactions. More directly related to this study, Fisman (2001) and Khwaja and Mian (2005) substantiate that political connections improve preferential access to capital from government lenders. The idea that political connections influence the quantity and price of bribes holds strong intuitive appeal, especially where institutions are weak. While the relevance of political connections for corruption is well documented, the rationale for this relationship remains poorly understood.

This paper provides experimental evidence consistent with a theory in which the political connections of candidates determine equilibrium vote sales. They do so by influencing the expected punishment faced by their corrupt counterparties in the election commission. During the September 2010 parliamentary election in Afghanistan, we designed, implemented, and experimentally evaluated a novel election monitoring technology aimed at fraud involving collusion between candidates

[1] There is substantial empirical documentation of the benefits of programs aimed at increasing political accountability or empowering citizens through increased enfranchisement and political representation (Besley and Burgess, 2002; Besley et al., 2005; Chattopadhyay and Duflo, 2004; Fujiwara, 2010; Pande, 2003). Recent work indicates that in countries experiencing or emerging from violent contests for state control, such as Afghanistan, fair elections may also undermine popular support for insurgents by promoting an accountable and legitimate government and by providing a forum for reconciliation (Berman et al., Forthcoming; Besley and Persson, 2009; McChrystal, 2009; United States Army, 2006; World Bank, 2011).

and election officials.[2] The experimental sample included 471 polling centers (7.8 percent of polling centers operating on election day) in 19 of the 34 provincial capitals in Afghanistan. The technology works by recording differences between immediate post-election polling center level counts and the corresponding numbers in the certified national aggregate. To obtain immediate post-election counts, pictures are taken at the polling center of Declaration of Results Forms (DR Forms).[3] We call this technology "Photo Quick Count". We find Photo Quick Count is effective and at only a fraction of the cost of more traditional monitoring techniques.[4] Monitoring reduces the incidence of theft or damaging of election materials at polling centers from 18.9 to 11.8 percent (a 7.1 percent decrease) and has a considerable negative effect on the number of votes cast for powerful candidates.

The experimental estimates of the effect of Photo Quick Count on fraud should be consistent and internally valid. However, given that the intervention took place in a complex environment with highly adaptive political agents, we also consider general equilibrium effects. Specifically, we develop a theory in which: (i) a corrupt Official can illegally provide votes to a Candidate using several alternative means and; (ii) a Candidate has an exogenously given Protection Capacity to shield the Official from being fined, which applies to only a subset of illegal transactions. According to this simple model, the Candidate reacts to monitoring by shifting from monitored to unmonitored illegal vote transactions as part of a Recovery Strategy. The Recovery Strategy, in turn, depends on the Protection Capacity of the Candidate.

According to this theory candidates have a set of options for recovering votes

[2]The intervention occurred during an election of particular geopolitical relevance. The international community viewed this election as vital for the Afghan government's attempt to exercise control and achieve stability through the consolidation of democratic institutions. The 2010 election was only the second parliamentary election after the United States and Coalition forces overthrew the Taliban, and was a central benchmark in the US efforts to support democratic gains with the horizon of an eventual drawdown of international troops. This election also presented an important test of the Afghan government's ability to exert control over territory and the implementation of democratic practices.

[3]A standard practice in many countries is for an election official to record vote totals at a particular polling center on a DR form. After votes are counted at the polling center, an official will post the form on the outside of the polling center, indicating vote totals at the polling centers to local residents.

[4]The relative savings come primarily from avoiding the travel and security costs of supporting international observers. Moreover, Photo Quick Count is well-suited to adoption through pre-existing social networks—viral adoption—especially in light of the global increase in cellular connectivity in developing countries.

through alternative illegal means with the support of government officials. They can alter DR Forms at polling centers where they do not predict monitoring (Spatial Recovery). They can also attempt to manipulate the process after becoming aware of monitoring but before the posting of DR Forms (Temporal Recovery). If the expected fine faced by the Official is decreasing in Protection Capacity, then candidates with Strong Protection Capacity have a broader set of Recovery Strategies than candidates with Weak Protection Capacity.[5] To test this implication, we operationalize a measure of Protection Capacity using remarkably rich data on candidates' political networks dating back to the 1979 Soviet Invasion of Afghanistan. We then combine this measure with data on three substitutable types of illegal vote sales and the experimental application of Photo Quick Count. We find that, consistent with the theory, Strong Protection Capacity candidates prefer Spatial Recovery while Weak Protection Capacity candidates prefer Temporal Recovery. Correspondingly, Photo Quick Count appears to have a negative externality for rigging on behalf of Weak Protection Capacity candidates. We also differentiate the short-term and long-term effects of Photo Quick Count. To do so, we use primary data on the stealing and damaging of election materials immediately after the election at the polling center (DR Form Manipulation) and polling center vote totals for the candidate most likely to benefit after the national aggregation process.

While our results are consistent with the model presented in the next section, they have alternative interpretations. For example, candidates with robust political connections may receive stronger support from election officials because they are directly involved in a repeated game. In this case, Officials may be willing to select strategies that provide candidates with more votes, even when they are more likely to be detected. An alternative and related model might be that connected candidates can engage officials in a broader set of unobserved parallel transactions or provide more attractive in-kind transfers than unconnected candidates. Because

[5] McMillan and Zoido (2004) provide the best empirical evidence on corrupt agents' willingness to pay for protection against prosecution for corruption. The authors show that the size of the bribes paid by media houses to Vladimiro Montesinos Torres, the secret-police chief for Peruvian President Alberto Fujimori, were conditional on their political connections to the regime or the opposition. The behavior documented in this paper is highly consistent with our results: the more influence a corrupt counterparty has on the expected downside for engaging in corruption the more leverage they have in defining the terms of the transaction.

of the clandestine nature of corrupt vote transactions, we do not have data which allow us to adjudicate between these models. Our model, however, provides a simple framework for interpreting our results, which we develop using a rich set of primary and administrative data and the experimental application of a powerful monitoring technology.

Our results suggest several considerations for policies aimed at reducing corruption and improving the functioning of democracy. First, our experiment adds to the growing experimental and quasi-experimental body of assessments of democracy and governance strengthening efforts (Aker et al., 2010; Di Tella and Schargrodsky, 2003; Ferraz and Finan, 2008; Fujiwara, 2010; Hyde, 2007; Olken, 2007). Photo Quick Count is highly compatible with Information Communications Technology (ICT). The cost of gathering and centralizing information on diffuse illegal behavior is now nominal. This technology can also be adapted to citizen-based implementation. This should greatly increase the probability of detection for malfeasance is detected and so may improve elections in institutionally weak contexts.[6] Our results indicate promise for future experiments in this direction. Second, in weak institutions with *partial* constraints, corrupt officials may respond to monitoring by providing preferential access only to powerful individuals. This suggests that monitoring may have the perverse effect of further empowering connected individuals by eliminating rivals. Policy-makers, government agencies, and researchers, should remain conscious of recovery strategies and adaptation, particularly where institutions are weak. Last, improving the independence of electoral institutions and constraining the ability of agents to sell votes is critical to the disciplining role of elections in democracy.

We structure the rest of the paper as follows. Section 3.3 develops a theoretical model that relates corrupt vote transactions to Protection Capacity. Section 1.3 describes our experimental setting and relevant features of electoral institutions in Afghanistan. Section 1.4 introduces our experiment, data, and research design. Section 1.5 provides results, and Section 3.8 concludes.

[6]See Becker (1968), Fisman and Miguel (2007), and Levitt (2004) for studies examining the impact of increasing the probability of detection for corruption on the amount of corruption.

1.2 Theoretical Framework

This section presents a basic model of corrupt transactions to help interpret our empirical results. The model characterizes transactions between a Candidate seeking election and an Official who sells illegal votes but has some probability of being caught and fined.[7] Our model departs from existing treatments in two fundamental ways. First, the Official can engage in several different types of corrupt sales of illegal votes, each subject to a different probability of detection. Second, the Candidate has an exogenously given Protection Capacity to shield the Official from being fined, which applies to certain illegal sales but not to others. In an environment with perfect information, the Candidate pays the risk-neutral expected utility maximizing Official an amount equal to the expected fine. Because the Protection Capacity of the Candidate influences this expected cost, it is a key determinant of the price of illegal votes. According to this simple model, the Candidate reacts to monitoring by shifting from monitored to unmonitored illegal vote transactions as part of a Recovery Strategy. The Recovery Strategy also depends on the Protection Capacity of the Candidate as it applies to some sales but not others.

1.2.1 A Model of Corrupt Vote Transactions

Consider a one-period model with two agents: a Candidate, interested in winning an election by obtaining both legal and illegal votes, and an Official, who can provide the candidate with illegal votes. We assume perfect information, that votes are perfect substitutes in providing utility to the Candidate, and that the Official is a risk-neutral expected utility maximizer. The Official controls two polling centers and has three means of providing illegal votes: manipulating the count *before* the DR Form is posted (v_b); changing vote counts on the DR form *after* it is posted at polling center 1 (v_a^1); and also at polling center 2 (v_a^2).

Only polling center 2 can be monitored, which we denote as $m_2 = 1$ in the monitored state and $m_2 = 0$ otherwise. The official subjectively assesses that she will be caught transacting illegal votes with probability ϕ_b, $\phi_a^1(m_2)$, $\phi_a^2(m_2)$ respectively,

[7] Corrupt transactions are therefore a gamble in the spirit of Becker and Stigler (1974) and Cadot (1987).

where we make ϕ_a^1 a function of monitoring at station 2 to allow for the possibility of spillovers. Prior to monitoring, there is no difference in subjective assessments between polling centers ($\phi_a^1(0) = \phi_a^2(0)$). Additionally, we assume that manipulating the count before the posting of the DR form has the lowest chance of detection ($\phi_b < \phi_a^1(0)$). Last, monitoring raises the subjective assessment in both polling centers, but disproportionately in polling center 2 as it is directly monitored ($\phi_a^1(1) < \phi_a^2(1)$).

If the Official is caught transacting illegal votes, she pays a fine F for each vote transacted. The Candidate can leverage political connections to reduce the fine by a share equivalent to their Protection Capacity θ, but only for votes after the DR form is posted. We assume $\theta \in [0, 1]$, where 0 corresponds to connections sufficient to provide complete immunity and 1 corresponds to having no connections. To analyze the pattern of substitution, we find the equilibrium both in the absence of monitoring $m_2 = 0$, and when monitoring is implemented $m_2 = 1$.

The candidate has an amount E to spend on illegal votes and obtains v_0 votes legitimately. Since votes are perfect substitutes each yielding an equivalent amount of utility, the Candidate's utility function is $U = v_0 + v_b + v_a^1 + v_a^2$. In equilibrium, optimal behavior requires that the Candidate offer the Official a unit rate equal to the expected unit cost of transacting: $w_b = \phi_b F$, $w_a^1(m_2) = \phi_a^1(m_2)\theta F$, and $w_a^2(m_2) = \phi_a^2(m_2)\theta F$. Prior to monitoring, $\phi_a^1(0) = \phi_a^2(0)$ and so $w_a^1(0) = w_a^2(0)$, allowing the equilibrium to be completely defined in terms of optimal sales of v_b and v_a^1. The equilibrium transaction will therefore be:

$$v_b^* = \begin{cases} \frac{E}{w_b} & \text{if } w_b \leq w_a^1(0) \\ 0 & \text{if } w_b > w_a^1(0) \end{cases}; \qquad v_a^{1*} = \begin{cases} \frac{E}{w_a^1} & \text{if } w_a^1(0) \leq w_b \\ 0 & \text{if } w_a^1(0) > w_b \end{cases}.$$

The corner solution that obtains depends on the candidate's Protection Capacity θ. To see this, note that indifference between transactions, $w_b = w_a^1$, is equivalent to $\theta = \frac{\phi_b}{\phi_a^1(0)}$. We denote this separating value as θ'. If $\theta' > \frac{\phi_b}{\phi_a^1(0)}$ the Candidate transacts in votes before the DR form is posted, and if $\theta' < \frac{\phi_b}{\phi_a^1(0)}$, the candidate transacts in votes after the posting of the DR Form.

We now solve for the equilibrium if $m_2 = 1$. The key change at this stage is that monitoring increases both ϕ_a^1 and ϕ_a^2, causing a change in corner solutions that depends on Protection Capacity. Because the subjective assessment of monitoring increases more in the directly monitored station, $w_a^2(1) > w_a^1(1)$ for all θ, so that $v_a^{2*}(1) = 0$. The solutions for the remaining vote transactions are therefore:

$$v_b^* = \begin{cases} \frac{E}{w_b} & \text{if } w_b \leq w_a^1(1) \\ 0 & \text{if } w_b > w_a^1(1) \end{cases}; \qquad v_a^{1*} = \begin{cases} \frac{E}{w_a^1} & \text{if } w_a^1(1) \leq w_b \\ 0 & \text{if } w_a^1(1) > w_b \end{cases}.$$

Importantly, the change in parameter values leads to a new separating value for θ, $\theta'' = \frac{\phi_b}{\phi_a^1(1)}$, which separates Candidate types that prefer to transact in v_b from those that prefer to transact in v_a^1.

We now summarize the set of predictions that we take to the data. First, introducing monitoring will weakly reduce transactions for votes in monitored polling centers (i.e. $v_a^{2*}(0) \geq 0$ and $v_a^{2*}(1) = 0$). Second, candidates with strong Protection Capacity (θ low) will react to monitoring by substituting across polling centers. In other words, if $\theta < \theta''$, the Candidate will substitute from v_a^2 to v_a^1. Third, candidates with weak Protection Capacity (θ high) will substitute from transacting *after* votes (v_a^1 or v_a^2) to *before* votes (v_b). Specifically, if $\theta \in [\theta'', \theta']$, the Candidate will completely substitute to v_b out of v_a^2 and v_a^1. Importantly, taken together, the second and third testable implications of our model imply that monitoring should create *positive* spillovers for candidates with strong Protection Capacity and *negative* spillovers for candidates with weak Protection Capacity.

Three features of our data allow us to test these predictions. First, we are able to develop a measure of Protection Capacity, based on detailed data on political networks. Second, a combination of administrative and primary data allows us to observe fraud both *before* and *after* the DR form is posted. Last, we have precise geographic coordinates data provided by the U.S. Military for all of the polling centers in our experimental sample, so we can test for displacement across polling centers in response to the administration of the monitoring technology.

Before proceeding to our research design, we mention two policy-relevant implications of our model. First, in this simple set-up, monitoring raises the price of illegal votes and so reduces the total number of votes that can be purchased with a given endowment E. Accordingly, a corrupt official sells fewer votes in the monitored equilibrium. Second, in this model, the spatial externalities for polling center 1, when polling center 2 is monitored, are *positive* if Protection Capacity is strong and *negative* if Protection Capacity is weak. The negative externality results because monitoring at polling center 2 increases the the subjective assessment of the likelihood of detection at polling center 1 ($\phi_a^1(0) < \phi_a^1(1)$). In the analysis in Section 1.5, we investigate this "caution" effect empirically in addition to the Spatial Recovery strategies of candidates with strong Protection Capacity. Figure 1.2 depicts these predictions graphically and relates them to the corresponding electoral institutions.

1.3 Institutional Background

In this section, we describe the experimental setting and relate it to our model. To demonstrate how officials provide illegal votes after they post DR Forms, we work through a simple example. Specifically, we compare a photograph of a DR Form at a polling center with the copy that was entered into the national count at the end of aggregation. We also describe the fraud monitoring technology that we designed and implemented: Photo Quick Count. Last, we explain how the electoral rules in Afghanistan give rise to a setting where: (i) a large number of candidates compete in parallel elections with close victory margins, creating a viable market for illegal votes; (ii) institutions are weak and election officials face limited accountability for assisting candidates; and (iii) candidates leverage patronage networks which pre-date democratization for corrupt purposes.

1.3.1 Experimental Setting

On Election Day (September 18), voting began at 7am and ended at 4pm. The count started immediately after polling concluded at individual polling centers and and was completed the same evening. In the first period, our intervention announced monitoring to Polling Centers Managers (PCMs) during polling. This intervention leaves two general types of manipulation unmonitored: (i) altering the count by attributing fake votes to the corrupt candidate (Count Manipulation); (ii) and altering DR Forms so that more votes are recorded for a given candidate than were actually cast as depicted in Figure 1.1 (DR Form Manipulation).[8] Count Manipulation happens before the posting of DR Forms and so corresponds to v_b in our model. DR Form Manipulation takes place after posting, corresponding to v_a^1 and v_a^2 in our model.[9] The international community paid considerable attention to this election, given its relevance for global geopolitical stability, and so provided a remarkable amount of administrative data on the electoral process. Section 3.4 describes how we use this data to observe both types of manipulation.

1.3.2 A Simple Example

To see how rigging occurs on DR Forms, Figure 1.1 depicts photos from our dataset. Our research team took the picture on the left immediately after the count (i.e., at the end of the Election Day stage). The IEC produced the picture on the right, as a scanned copy from the IEC aggregation center in Kabul of the same DR form. The DR form on the left should be identical to the picture on the right since it is a carbon copy.[10]

There are three major differences that demonstrate direct evidence of rigging. Someone has converted the Dari script for the polling center and polling station numbers to arabic numerals.[11] Second, the name of the presiding PCM has been changed. Third and most tellingly, while the sheet on the left records votes for most candidates that appear to result from normal polling, the figure on the right records

[8]DR Manipulation can be perpetrated many different ways. These include stealing ballot boxes and sealed Tamper Evident Bags (TEBs) in order to alter their contents.

[9]In some cases, candidates can also influence the post-election fraud investigation and adjudication process. To avoid contamination of our results from this highly politicized and unpredictable process, we scraped the record of the votes from the initial publication of polling station results by the IEC on its website. The IEC posted these after the aggregation of tallies but before the ECC adjudication process and subsequent prosecution of candidates by the Attorney General.

[10]Because it is a carbon copy, it is not possible to have differences that are attributable to recording error.

[11]Polling centers typically have 3-10 stations within them. PCMs are the most senior IEC official at a polling center. They maintain responsibility for opening their center's stations on election day, conducting the vote, closing the polling center, overseeing the count, and posting the final DR Form from each station in a visible location within the polling center.

no votes whatsoever. There are many comparable examples in our data.[12] From this it is clear that election officials assist candidates by manipulating DR Forms. We see here that the official who altered totals did not attempt comparability to the original, consistent with limited accountability.

1.3.3 Photo Quick Count

The fraud we document through this example suggests a powerful monitoring technology. Taking independent photographic records of DR Forms and separating them from the electoral chain of custody allows near certain detection of DR Form Manipulation. This design builds on Parallel Vote Tabulations (PVTs), which have been in use since the 1980s.[13] Two important technological developments allow us to build on the PVT concept. First, it is now common for Election Commissions to release disaggregated results and to post them on the internet. Second, the cheap availability of digital photography allows rapid and perfectly accurate recording of DR Forms.[14]

Photo Quick Count allows us to investigate illegal vote transactions in three ways. First, it narrowly targets fraud through DR Form Manipulation and should only detect differences after PCMs post DR Forms. Second, while we announce our monitoring intervention *during* voting, it is not able to pick up cheating until *after* officials post the DR Form, leaving the probability of detection for Count Manipulation, ϕ_b unchanged. Third, in the absence of our intervention, corrupt agents' subjective assessment that DR Form Manipulation is detected should be uniform across polling centers, consistent with our assumption that $\phi_a^1(0) = \phi_a^2(0)$. This makes the rigging of

[12] While these data provide exceptional and precise documentation of fraud, we show below that our treatment strongly reduced the frequency with which candidates and their agents stole tally sheets. For this reason, attrition in the measure of comparing tallies relates strongly to treatment. We therefore cannot use this as a measure of fraud.

[13] Through representative sampling and recording of ballots by field staff, PVTs predict national totals within a small margin of error (Cowan et al., 2002). PVTs are an important means of checking votes against results that electoral commission ultimately certify, but cannot identify whether differences occur from Count Manipulation or DR Form Manipulation. Exit polls can also be compared with certified results, under certain assumptions, to provide a check against electoral manipulation (Gibson and Long, 2009; Bjornlund, 2004).

[14] Our team has since implemented Photo Quick Count using a custom application for smartphones during the February, 2011 parliamentary and presidential elections in Uganda with the support of Qualcomm, Inc.

any one tally perfectly substitutable, from the perspective of the Official, with rigging another. Our intervention changes this as we announce monitoring only at specific polling centers.

Illegal votes transacted in equilibrium depend on the probability of detection for both Count Manipulation and DR Form manipulation. However, our technology only changes the probability of DR Form Manipulation detection. The first margin for recovering votes after our treatment is through Count Manipulation (increasing v_b), which we call Temporal Recovery. Commonly, this involves taking votes cast for one candidate and attributing them to another.

An alternative means of recovering votes is through DR Form Manipulation at unmonitored centers (Spatial Recovery). This involves switching votes from v_a^2 to v_a^1. Given monitoring of DR Forms in polling center 2, candidates will try and recover those lost ballots by engaging fraud in polling center 1. Our theory predicts strong Protection Capacity candidates should prefer Spatial Recovery because the expected fine an official faces for DR Form Manipulation is lower in this case.

1.3.4 Electoral Institutions in Afghanistan

In this section, we describe characteristics of Afghanistan's electoral institutions relevant to corrupt electoral practices. We outline the history and characteristics of the rules and institutions that govern elections in Afghanistan. We also discuss how informal networks that link political actors can undermine formal institutions.

After the US invasion and fall of the Taliban in 2001, Coalition forces helped to empanel a Constitutional Loya Jirga that established democratic institutions in Afghanistan after decades of internecine conflict, civil war, and Taliban rule. Hamid Karzai won the first presidential elections in 2004 with 55 percent of the vote. In 2005, Afghans voted in elections for the lower house of parliament, the Wolesi Jirga. Amid claims of rigging and substantial election day violence, Karzai won re-election in 2009. In 2010, the second Wolesi Jirga elections occurred amid a growing insurgency and a US commitment to begin withdrawing troops in July 2011. The international community viewed these elections as a critical benchmark in the consolidation of democratic institutions given doubts about the Karzai government's ability to excercise control in much of the country. Despite lingering memories of violence from the 2009 election, between 4 million and 5.4 million voters cast ballots in the Wolesi Jirga elections.[15]

Afghanistan's 34 provinces serve as multi-member districts that elect members to the Wolesi Jirga. Each province is a single electoral district and the number of seats is proportional to its estimated population. Candidates run "at large" within the province without respect to any smaller constituency boundaries. Voters cast a single non-transferable vote (SNTV) for individual candidates, nearly all of whom run as indepedents.[16] Candidates compete for votes province-wide. The rules declare winning candidates as those who receive the most votes relative to each province's seat share. For example, Kabul province elects the most members to Parliament (33) and Panjsher province the fewest (2). The candidates who rank 1 to 33 in Kabul and 1 to 2 in Panjsher win seats to the Wolesi Jirga.

These rules hold implications for the dynamics of electoral malpractice. First, SNTV with large district magnitudes and a lack of political parties creates a wide dispersion of votes across a large number of candidates. The vote margins separating the lowest winning candidate from the highest losing candidate are often small. This lowers the minimum number of votes required for winning a seat in the parliament and suggests a high expected return for even small manipulation for a large number of candidates. In contrast, electoral systems with dominant parties guarantee victory with large vote margins, and so the likelihood that a non-viable candidate will rig falls. Second, because they compete for votes province-wide, candidates can attempt substitution of legitimate and fake ballots elsewhere. If monitoring leads to a loss of votes in one polling station, candidates will seek to recover lost ballots in other polling

[15]The Independent Electoral Commission projected this number out of what it believes is 11 million legitimate registered voters. This corresponds to an estimated turnout of between 37 and 49 percent. This remarkable turnout resonates with summary statistics from our baseline survey of 2,900 Afghans, which we describe in Section 3.4. 89 percent of respondents view the Wolesi Jirga as important to their lives, 60 percent believe that voting in the Wolesi Jirga elections will increase the quality of services in their area, and 65 percent stated that voting will lead to improvements in the future.

[16]SNTV systems provide voters with one ballot that they cast for one candidate or party when multiple candidates run for multiple seats. If a voter's ballot goes towards a losing candidate, the rules do not re-apportion that vote. Former U.S. Ambassador to Afghanistan Zalmay Khalilzad and President Hamid Karzai promoted SNTV during the first parliamentary elections in 2005 to marginalize warlords and reduce the likelihood they obtained parliamentary seats. As a corrolary, Karzai also decreed that political parties should not be allowed to form.

stations. This directly supports our formulation of the candidate's perfect substitutes utility function in Section 3.3. Third, despite a province-wide race, candidate support usually correlates with geographic proximity. Candidates garner most of their votes in their home districts or towns where they remain popular. Given the areas that powerful candidates exert control over, influential candidates can rig in their home areas but are not likely to do so province-wide. Since provinces are multi-member, even powerful candidates have to compete with and share seats with other powerful candidates.

The weak institutions tasked with managing elections in Afghanistan also permit fraud. The Independent Election Commission (IEC) serves as the main electoral body responsible for polling, counting votes, aggregation, and certifying winning candidates. Historically, the IEC has proven susceptible to influence by corrupt agents. A lack of published data on the 2005 Wolesi Jirga elections do not allow for investigations into potentially fraudulent vote returns. But wide-scale rigging occured in the 2009 presidential elections. The IEC initially gave Karzai 53 percent of the vote, above the 50 percent threshold necessary to avoid a run-off. However, the Electoral Complaints Commission (ECC) reduced that margin to 47 percent after investigating numerous allegations of electoral corruption and malfeasance. Evidence from a random sample of ballots along with digit analyses of vote returns provide convincing evidence of widespread manipulation, mostly in favor of Karzai (Callen and Weidmann, 2011). Before the IEC could hold a run-off, the runner-up Dr. Abdullah Abdullah dropped out of the race leaving the presidency to Karzai.[17]

In addition to the challenges of formal institutions, non-formal institutions also play an important role in determining political outcomes in Afghanistan.[18] Despite attempts to grow incipient democratic institutions, pre-existing power structures exert influence over political processes and frequently undermine them. Similar to

[17]Given serious problems with the 2009 presidential election and under pressure from the international community, the IEC attempted some reforms ahead of the 2010 Wolesi Jirga elections. Many observers viewed these as hollow. In our baseline survey, we find that 51 percent of respondents expected problems with counting ballots at polling centers, 50 percent projected problems with the count at the IEC in Kabul, and 53 percent forecasted problems with transporting ballots from polling centers to the IEC aggregation center in Kabul.

[18]Callen and Weidmann (2011) for non-experimental evidence consistent with patronage networks facilitating illegal vote transactions in Afghanistan.

developing countries elsewhere, extra-state networks of patronage that pre-date democratization help to determine lines of political accountability and control between powerful actors. Many new leaders and members of institutions, such as the Wolesi Jirga, candidates running for office, local government councils, and electoral officials make use of existing relationships. For example, Karzai enjoys strong links with government officials in Southern Afghanistan given his family roots in that part of the country. Former warlords fighting in the Northern Alliance against the Taliban exert strong control in Northern Afghanistan. Networks of these powerful actors support corruption within and outside of the state as patrimonial ties link corrupt officials with government entities that can protect them for prosecution. These connections inform our concept of Protection Capacity and influence how we operationalize the measure.

Despite opportunities to illegally provide votes to candidates officials in Afghanistan must weigh these incentives against the expected cost of prosecution. The United Nations backed Electoral Complaints Commission (ECC) exists as a separate and independent body from the IEC. The ECC investigates complaints against polling officials, candidates, or citizens. Any Afghan can lodge such a complaint. Based on the seriousness of a complaint and its likelihood of affecting the election's outcome, the ECC may decide to cancel all of the votes at a given polling station, all of the votes for a particular candidate at a polling station, or the total votes for a candidate across their entire province. The ECC over-turned some 25 percent of the ballots in this process in the 2010 election. Under its purview of fighting corruption, the Attorney General may prosecute specific individuals, including election officials and candidates, it believes to have participated in election fraud and levy fines or prison sentences against them if found guilty. Theoretically the punitive capacity of the Attorney General and the ECC is important as the probability for being punished is non-zero (i.e. $F > 0$). In Section 1.5, we empirically investigate whether these linkages affect Recovery Strategies.

1.4 Research Design

Our experiment estimates the effect of Photo Quick Count on election fraud. The technology narrowly targets DR Manipulation, one of several means of obtaining illegal votes. The theory we present in Section 3.3 predicts that: (i) Photo Quick Count will reduce DF Form Manipulation at monitored stations; (ii) the Recovery Strategy of strong Protection Capacity candidates will be Spatial Recovery; (iii) the Recovery Strategy of Weak Protection Capacity candidates will be Temporal Recovery; (iv) Weak Protection Capacity candidates will reduce DR Form Manipulation at unmonitored polling centers because of a "caution" effect. Our empirical analysis proceeds in two steps. First, we test for a fraud-reducing effect of Photo Quick Count. Second, we test the further implications of our model, using administrative data to operationalize measures of Count Manipulation and Protection Capacity.

1.4.1 Data

Elections in Afghanistan receive considerable international attention because of their importance for the NATO-led occupation. This results in a remarkable range of administrative data documenting details of the electoral process. We use the following administrative data in our empirical analysis: (i) systematic political background investigations of the main candidates from Democracy International (DI); (ii) geographic coordinates and security assessments of polling stations provided by the International Security Assistance Force (ISAF); (iii) complaints about illegal election activities filed at the ECC; (iv) disaggregated vote counts from the IEC; and (v) data on adherence to electoral laws and protocols from the Free and Fair Elections Foundation of Afghanistan (FEFA).[19] Additionally, we fielded a baseline survey in August 2010 of households living in the immediate vicinity of polling centers in our experimental sample. Our experimental sample comprises 471 polling centers (7.8 percent of polling centers operating on election day) in 19 of the 34 provincial capitals in Afghanistan. We designated 450 of the 471 polling centers in our experimental

[19] Democracy International was the leading international mission observing the parliamentary elections and our institutional partner. We obtained the disaggregated data from the IEC website on October 24, 2010.

sample as Primary Sampling Units (PSUs).[20] We obtained an additional measure of DR Form Manipulation by sending field staff to investigate whether election materials were stolen or damaged the day following the election (September 19), which we describe in subsection 1.4.2.

Measuring Protection Capacity

In Section 3.3, candidates' Protection Capacity determines the equilibrium price of DR Form Manipulation by affecting the expected fine corrupt officials face. Protection Capacity does not, however, affect the expected cost of Count Manipulation. Accordingly, candidates' recovery strategy depends on their Protection Capacity in the following way: (i) candidates with strong Protection Capacity should prefer Spatial Recovery, and (ii) candidates with weak Protection Capacity should prefer Temporal Recovery, switching votes from DR Form Manipulation to Count Manipulation.

Investigating these predictions requires a measurement of Protection Capacity. We operationalize this measure by exploiting extensive and systematic background research into candidates performed by DI. The investigations report history of government service, known political affiliates and supporters, as well as demographic, education, and occupation histories for 76 of the leading candidates.

We develop a measure of Protection Capacity in three stages. First, we use the DI political connections data to create a simple index of the political connections for candidate i as:

$$Political\ Connection\ Index_i = Karzai_i + Government_i + DEO_i + PEO_i$$

where $Karzai_i$ equals 1 for an indirect connection to Karzai (e.g. through a rela-

[20] We selected our experimental sample of 471 polling centers by identifying polling centers scheduled to open on election day and deemed secure by ISAF and Afghan National Police (ANP) for the safety of our field staff. The 21 polling centers in the experimental sample not surveyed at baseline are in Kabul. These were subsequently added because of additional funding made available after the baseline. The survey contained 2,900 respondents. To attempt to obtain a representative sample of respondents living near polling centers, enumerators employed a random walk pattern starting at the polling center, with random selection of every fourth house or structure. Respondents within households are randomly selected using Kish grid. The survey had 50 percent male and female respondents and enumerators conducted it in either Dari or Pashto.

tive) and 2 for a direct connection (e.g. having worked directly with the president), $Government_i$ equals 1 for having held a minor government post since 2001 (e.g. teacher) and 2 for having held a major government post (e.g. parliamentarian), DEO_i equals 1 if a candidate has a connection to the District Elections Officer, and PEO_i equals 1 if a candidate has a connection to the Provincial Elections Officer. Second, we take the top 10 vote recipients in our control sample in each province, removing those for which DI did not complete a political connections investigation. From this list, we identify the person with the highest Political Connection Index. We call this candidate the Most Connected Candidate.[21] In the final stage, we identify whether the Most Connected Candidate has a *specific connection* to the Provincial Elections Officer (PEO).[22] This divides our sample into 12 provinces with 244 polling centers where the Most Connected Candidate has connections to the PEO ($Protection\ Capacity = 1$) and 7 provinces with 227 polling centers where the Most Connected Candidate does not have a connection to the PEO ($Protection\ Capacity = 0$). Table 1.1 provides summary statistics for the DI political connections data we use to develop a measure of Protection Capacity.

1.4.2 Experiment

On election day (September 18, 2010), we randomly announced the use of Photo Quick Count by delivering letters to 238 of the 471 polling centers in our experimental sample. We instructed Afghan researchers, that we trained and hired through a local research firm, to deliver letters to Polling Center Managers (PCMs) between 10AM and 4PM, during polling. Researchers visited all 471 polling centers the following day in order to take a picture of the DR form. Of the 471 polling centers, 6 did not operate on election day. Figure 1.3 maps the polling centers in our sample and indicates treatment status across the country. Figure 1.4 depicts the same in Kabul specifically.

The delivery of this letter constitutes the treatment in our experiment. The

[21] We assume that candidates who (i) receive lots of votes in neighborhoods where our intervention takes place and (ii) have robust political connections are the most likely to engage in election fraud.

[22] As the highest ranking provincial election official, the Provincial Elections Officer holds considerable leverage over the punishments meted out to corrupt PCMs.

letter announced to PCMs that researchers would photograph DR forms the following day (September 19). It also explained that Photo Quick Count documents discrepancies between DR forms photographed at the polling center and results certified by the IEC. Appendix Figure 1.5 provides a copy of the letter in English and Appendix Figure 1.6 provides a copy in Dari. We asked Polling Center Managers (PCMs) to acknowledge receipt by signing the letter. PCMs at 17 polling centers (7 percent of centers receiving letters) refused to sign. We designate a polling center as treated if the PCM received a letter (Letter Delivered = 1). Our results remain robust to redefining treatment as both receiving and signing a letter.

To ensure balance, we stratify treatment on province, and, for the 450 PCs for which we had baseline data, on the share of respondents from the baseline survey reporting at least occasional access to electricity and on respondents reporting that the district governor carries the most responsibility for keeping elections fair. All core specifications reflect our assignment strategy, by including strata dummies as suggested by Bruhn and McKenzie (2009).[23] Table 1.2 reports summary statistics and verifies balance.

To obtain a measure of DR Form Manipulation, our researchers investigated whether any of the materials had been stolen or damaged during the night of September 18, after polling.[24] We trained enumerators to investigate by only interviewing local community members and not to engage IEC staff. We focus on reports of theft by candidate agents, who are candidate representatives legally permitted to observe polling and typically present at polling centers in their candidate's constituency. We received reports of candidate agents stealing materials at 60 (12.9 percent) of the 465 operating polling centers. We therefore define our measure of DR Form Manipulation as an indicator equal to 1 if materials were reported stolen by a candidate agent at a

[23]Bruhn and McKenzie (2009) suggest stratified treatment assignment on baseline measurements of the outcome of interest, or variables that are highly correlated with this outcome. Because measures of fraud are unavailable prior to the election, we select our stratifying variables by identifying measures most highly correlated with fraud during the 2009 presidential election. Our strategy finds support in Callen and Weidmann (2011), who demonstrate evidence supporting the involvement of election officials in perpetrating fraud during the 2009 election. However, we do not stratify on 2009 fraud because, at least according to the very coarse measures used in Callen and Weidmann (2011), this did not occur frequently in our sample. We re-randomize to guarantee balance.

[24]We speculate that in many cases the purpose of stealing the materials was to manipulate them and then reintroduce altered DR forms into the aggregation process.

given polling center.

To obtain estimates of the effect of Photo Quick Count, we regress our measure of DR Form Manipulation at polling center c on the treatment status of the polling center, covariates from our baseline survey, and a set of stratum fixed effects:

$$DR\ Form\ Manipulation_c = \gamma_1 + \gamma_2 Letter\ Delivered_c + \boldsymbol{\gamma}_3'\mathbf{X}_c + \varepsilon_c \tag{1.1}$$

where $\mathbf{X}_c$ is a vector of polling center attributes including stratum fixed effects. Equation 1.1 permits a test of the first prediction of the theory presented in Section 3.3, and provides a consistent estimate of the effect of Photo Quick Count on DR Form Manipulation.

In Section 1.5, we estimate a variant of Equation 1.1, replacing $DR\ Form\ Ma_c$ with the number of votes cast for the candidate with the highest political connections index at polling station s ($Most\ Connected\ Candidate\ Votes_{cs}$) to provide a cardinal measure of the number of fraudulent votes eliminated through Photo Quick Capture.

Our research design mirrors that of many randomized control trials. It relies entirely on primary data and experimental assignment to obtain internally valid estimates of the effect of an intervention. Our theory, however, predicts that candidates should implement a Recovery Strategy in response to the intervention. In order to understand the ultimate effect, we therefore investigate the broader general equilibrium response of candidates. To investigate Recovery Strategies, we now turn to an analysis of administrative records of Count Manipulation.

1.4.3 Recovery Strategies and Protection Capacity

Analysis of Temporal Recovery

Our field staff delivered letters announcing monitoring from 10PM to 4PM on September 18, when voting concluded. PCMs then counted ballots at the polling station and filled out a DR form, completing the process around 8PM. Importantly, because of this timeline, Photo Quick Count cannot detect Count Manipulation, while it is virtually guaranteed to detect any DR Form Manipulation. PCMs, aware that our researchers would take photographs of DR Forms on the morning of September

19, could in response recover votes for candidates by engaging in Count Manipulation in place of DR Form Manipulation.

We obtain data on Count Manipulation from the ECC. These include complaints about the electoral process made by candidates, observers, and candidate agents. Count Manipulation occurred widely in our sample. For example, a complaint made by a candidate about the Charahi Taymani neighborhood in Kabul reports "in Ismailya Polling station, 10 of my family members voted for me, but the Declaration of Results Form displayed only 7." Similarly, at the Sayedullah Khan Bazaar High School in Terin Kot in Urozgan province, a candidate reports "382 votes were cast, but then the voting papers were inexplicably lost. Later that evening, I observed the brother of Sema Joyenda replacing the vote papers into the boxes." The ECC received 5,869 total complaints regarding the September 2010 parliamentary elections, of which 4,138 were made by candidates and 944 were made about IEC polling officials violating protocols. 650 of the 944 complaints about polling staff were made by candidates. In our sample of 2,004 polling stations in 471 polling centers, 1,858 complaints were filed with the ECC. 1,227 of these complaints were filed by candidates and 900 were filed regarding polling center staff. We measure Count Manipulation using these two variables: the number of complaints filed by candidates about a given polling station and the number of complaints filed against IEC staff about a given polling station.

The second prediction of the model we present in Section 3.3 is that Photo Quick Count should increase Count Manipulation. We investigate this using the specification:

$$Count\ Manipulation_{cs} = \beta_1 + \beta_2 Letter\ Delivered_c + \boldsymbol{\beta}_3' \mathbf{X}_{cs} + \nu_{cs} \tag{1.2}$$

where, $\mathbf{X}_{cs}$ is a vector of polling station attributes which includes stratum fixed effects. Our estimates for β_1 will be consistent as they we estimate them using random assignment to treatment. The prediction of Temporal Recovery corresponds to $\beta_2 > 0$.

Additionally, to test if candidates with weak Protection Capacity prefer to substitute temporally, we repeat Specification 1.2, interacting *Letter Delivered* with

Protection Capacity:

$$Count\ Manipulation_{cs} = \phi_1 + \phi_2 Letter\ Delivered_c + \phi_3 Protection\ Capacity_c + \\ \phi_4 Letter\ Delivered_c \cdot Protection\ Capacity_c + \boldsymbol{\phi}_5' \mathbf{X}_{cs} + \eta_{cs}. \tag{1.3}$$

Analysis of Spatial Recovery

Our model additionally predicts that Strong Protection Capacity candidates will prefer Spatial Recovery. Precise geographic coordinates of the polling centers provided by the U.S. Military allow us to test for spatial externalities consistent with Spatial Recovery:

$$Most\ Connected\ Cand.\ Votes_{cs} = \varphi_1 + \varphi_2 Letter\ Received_{cs} + \sum_{i=1}^{3} \psi_i \bar{T}_c^i + \boldsymbol{\varphi}_6' \mathbf{X}_{cs} + \zeta_{cs} \tag{1.4}$$

where $\bar{T}_c^1$ indicates the sum of treatment statuses for the 5 nearest polling centers, $\bar{T}_c^2$ indicates the sum of treatment statuses of the next 5 nearest polling centers and $\bar{T}_c^3$ is the sum of treatment statuses of the next nearest 5 polling centers after that. Thus, each of the sums form a mutually exclusive group.

This specification, run only on the weak Protection Capacity sample, allows us to test for the Caution Effect. The Caution Effect in our model predicts that $\psi_i < 0$, $\forall i \in \{1, 2, 3\}$ in the weak Protection Capacity sample, and Spatial Recovery predicts that $\psi_i > 0$, $\forall i \in \{1, 2, 3\}$ in the strong Protection Capacity sample. We investigate both predictions in subsection 1.5.2 below.

1.5 Results

This section provides evidence that Photo Quick Count reduced DR Form Manipulation, which we measure as the illegal stealing of election materials. Second, we find a corresponding reduction in votes for the Most Connected Candidates by about 25 percent and by as much as 40 percent in the strong Protection Capacity sample.

This result accords with our prediction that strong Protection Capacity candidates prefer DR Form Manipulation. We also document that candidates made efforts to recover votes through Count Manipulation (Temporal Recovery) and by relocating DR Form Manipulations to polling centers where we did not announce monitoring (Spatial Recovery). Last, we find evidence that candidates with weak Protection Capacity prefer Temporal Recovery while candidates with strong Protection Capacity prefer Spatial Recovery. All these results are consistent with the predictions of our model of corrupt transactions between a candidate and an election official capable of selling several substitutable types of illegal votes.

1.5.1 Evidence of Fraud Reduction

Table 1.3 presents estimates of several variants of Equation 1.1 for both the full sample and the strong Protection Capacity sample, sequentially adding stratum fixed effects and covariates to demonstrate robustness. According to our theory, strong Protection Capacity candidates prefer DR Form Manipulation, which the intervention targets. We therefore separately estimate effects for this subsample in columns 4 - 6. Panel A reports our estimates of the effect of announcing Photo Quick Count Monitoring on DR Form Manipulation and Panel B reports estimates of the effect on votes for the Most Connected Candidate.

We report estimates of effects on both measures for two reasons. First, the timing of our intervention made it difficult to quickly redeploy candidate agents to new polling centers to steal materials. This measure therefore provides an estimate of the effect subject to fewer externalities. We include the second measure to obtain a cardinal estimate of treatment effects, which may provide a basis for cost comparisons with other monitoring technologies.[25]

We find that the intervention substantially reduces both measures of fraud. The simple mean difference reported in column 1, Panel A indicates a reduction in

[25] There are at least two concerns about using votes for the Most Connected Candidate as a dependent variable. First, the prediction of Spatial Recovery indicates the presence of spatial externalities, which we discuss in Section 1.5.2 below. Second, the arrival rate for this measure can vary dramatically across polling center for a given candidate and so Ordinary Least Squares may not produce the appropriate specification. We estimate Equation 1.1 using MLE to address this problem in Section 3.7 below. We thank Gordon Dahl for very helpful discussions on this issue.

DR Form Manipulation from 18.9 percent to 7.1 percent. Columns 2 and 3 provide estimates which remain virtually unchanged with the inclusion of stratum fixed effects and covariates. Column 1 in Panel B suggests about a 25 percent reduction in votes for the Most Connected Candidate.[26] Consistent with the model, we find in columns 4 - 6 in both Panels A and B that the largest reductions for both measures occur in the Strong Protection Capacity sample. This is especially true for votes for the Most Connected Candidate, suggesting that connected candidates have additional means of altering the count during aggregation in addition to DR Form Manipulation.

1.5.2 Evidence on Political Connections and the Strategic Response to Monitoring

Temporal Recovery

The first window for recovering votes after our treatment occurs through Count Manipulation. Commonly, this involves taking votes cast for one candidate and attributing them to another. This strategy suffers limited effectiveness because of an adding up constraint (stations have a fixed number of possible votes) and occurs in the presence of observers and candidate agents during the polling center count, who may observe and report on manipulation. Indeed, our data on manipulation in this margin come from such complaints made to the ECC. We focus on two types of complaints in our data. The first includes complaints made to the ECC about the behavior of polling center officials. The second includes complaints made by parliamentary candidates to the ECC about a specific polling stations. The latter complaint usually comes from reports from lesser candidates that votes they know they and their supporters cast on their behalf never appear on DR Forms. For this to happen, manipulation must occur during the count. This strategy has limited effectiveness, but only requires the complicity of a PCM, and not a more senior elections officer.

Table 1.4 reports estimates of Equation 1.2, our test of Temporal Recovery. In Panel A, we measure Count Manipulation as the number of complaints against IEC staff at a given polling station and in Panel B we measure Count Manipulation as the number of complaints filed by candidates. The point estimates in columns 1

[26] Results for all three columns are significant in corresponding negative binomial regressions.

and 2 in Panel A indicate that treatment doubles the number of complaints against IEC officials at the polling center from 1 to 2. Corresponding estimates in Panel B indicate that complaints made by candidates increases even more substantially. Both results are consistent with Temporal Recovery.

Columns 5 and 6 present an additional set of tests using data from FEFA, based on the availability of indelible ink to prevent multiple voting at the polling station. Domestic civil society election monitors working for FEFA gathered the data we use on adherence to anti-fraud election protocols.[27] We focus on the availability of indelible ink, which is applied to voters' fingers after admittance to the polling center to prevent multiple voting, and whether this ink could be washed off as measures of pre-treatment vulnerability to rigging. A lack of truly indelible ink to prevent multiple voting provides information about the pre-monitoring allocation of fraud. This allows us to understand equilibrium allocations of corrupt votes in the absence of any intervention.

The increase in ECC complaints, in this subsample, shown in columns 5 and 6 of both panels, is 2 - 3 times larger. The availability of indelible ink should be correlated with the planned extent of Count Manipulation before the letter announcing Photo Quick Count arrived. This provides additional support for the theory and increases our confidence in our interpretation of the increase in Count Manipulation as evidence of Temporal Recovery.

Spatial Recovery

Table 1.5 reports estimates corresponding to Specification 1.4, with Votes for the Most Connected Candidate as the dependent variable in Panel A and DR Form Manipulation in Panel B. Panel A columns 1 - 3 report estimates on the Weak Protection Capacity subsample. The negative and significant coefficients on the spatial lags are consistent with the caution effect discussed in Section 3.3, resulting from the increase in officials' subjective assessments of the probability of monitoring. It

[27]FEFA visited 201 (89 percent) of the 227 control polling centers from our sample and 202 (85 percent) of the 238 treatment polling centers. We fail to reject the null hypothesis of equality for visits by FEFA monitors with a p-value of 0.25. We observe whether ink is available in 177 (78 percent) of our 227 controls and in 175 (77 percent of our 238 treatments). We also fail to reject the null of differences in this mean with a p-value of 0.25.

appears that news of our letters traveled, causing a reduction in votes for the Most Connected Candidate, even in unmonitored polling centers. The negative coefficients for spatial lags in Panel B, additionally support the presence of a caution effect.

We now test for Spatial Recovery. Columns 4 - 6 of Panel A provide two key insights. First, we see that contamination of our controls, as a result of spatial recovery, requires us to reinterpret the estimates on Votes for the Most Connected Candidate in Table 1.3. Column 4 replicates Column 5 in Panel B of Table 1.3. The effect size is lessened with the inclusion of spatial lags, suggesting that Spatial Recovery increases DR Form Manipulation in unmonitored stations. The second insight is that the negative and signficant estimate for ψ_2, on the strong Protection Capacity sample accords with Spatial Recovery. This is an especially surprising result, given the countervailing Caution Effect we document in columns 1 - 3 of Panel A. Taken together, this suggests candidates may have offset some of the total effects of the intervention through Spatial Recovery.

In Panel B, we see that the negative and significant effects on DR Form Manipulation in Table 1.3 are sustained and are not much affected by the inclusion of spatial lags. The estimates increase slightly, however, when accounting for the Caution Effect. The small point estimates on the spatial lags are consistent with our interpretation of the effect on DR Form Manipulation as reflecting a partial equilibrium response. The effects on Votes for the Most Connected Candidate in Panel A, by contrast, reflect a series of changes that occur later in the aggregation process outside of the polling center. Officials have both more time and face lower costs to spatially coordinating a response, as at this stage DR Forms are in a centralized aggregation center.

The estimates in Table 1.5 support two predictions of our model. First, subjective assessments of monitoring should increase in unmonitored polling centers, creating a Caution Effect. Second, Strong Protection Capacity candidates should prefer Spatial Recovery, because officials still face a low expected cost when engaging in DR Form manipulation on behalf of candidates.

As with the results already discussed, these are consistent with several models. For example, it could be that in our weak Protection Capacity sample, there is a candidate, who is not the Most Connected Candidate, but who is *rigging against* the Most Connected Candidate and causing them to lose votes. This is inconsistent with our interpretation of the negative coefficients on the spatial lags as evidence of a Caution Effect, but still supports the core prediction of Spatial Recovery.

1.5.3 Extention - Further Evidence of Election Official Involvement in Rigging

Table 1.6 reports regressions where we interact treatment status with the availability of ink and with whether the ink could be washed for the sample which reports having ink:

$$DRFormManipulation_{cs} = \beta_1 + \beta_2 T_{cs} + \beta_3 T_{cs} \cdot Ink\ Problem_c + \boldsymbol{\beta}_4' \mathbf{X}_{cs} + \boldsymbol{\beta}_5' \mathbf{D}_c + \nu_{cs} \tag{1.5}$$

Estimates in columns 1 and 2 provide evidence that our researchers successfully observed the stealing of election materials during their investigative work the day after the election. At the point estimate, we find that candidate agents are 25 percent more likely to steal election materials in polling centers with no ink in our control sample. We also find that the effect of the letter on the immediate reaction of candidates is much stronger in polling centers where ink was not available on election day. The results suggest a reduction of materials theft by 33.1 percent in these polling centers. Therefore, the announcement of monitoring has a greater impact in places where fraud is endemic.

If a candidate exerts control over a polling station, they should ensure that PCMs do not enforce safeguards to prevent multiple voting, and additionally alter DR Forms directly or hand them off to someone to adjust later. Our results support this intuition. We also note that having the ink wash predicts votes for the Most Connected Candidate.

1.5.4 Robustness

The results from Table 1.5 constitute the core test of our hypothesis. In this section, we test the sensitivity of these results to various specifications. As mentioned above, one issue concerns the distribution of votes, which may vary dramatically across provinces as we use vote totals for different candidates in different provinces. A related concern addresses the influence of outliers. We use three additional specifications to deal with these concerns. First, we estimate the spatial externalities using a negative binomial model. Second, we transform the dependent variable to the within-sample ascending rank position for the Most Connected Candidate Vote total, which dampens outliers. Last, we run negative binomial regressions using the rank transformation. To provide a further check against outliers, we show that our results are robust to trimming at the 99th percentile of the dependent variable in Panel B. Table 1.7 reports our robustness results. We see that in all cases, our results remain robust to these checks.

1.6 Conclusion

Free and fair elections are critical for democracy to fulfill its key function of empowering citizens to hold politicians accountable. Elections fail in new democracies for a range of reasons, but commonly because of weak institutions with limited constraints on the ability election officials to manipulate on behalf of a candidate. Corruption, traditionally defined as the illegal sale of preferential treatment by government agents, therefore also poses a threat to democracy.

This paper provides results from an experimental evaluation of a novel Photo Quick Count technology intended to reduce the corrupt sale of votes by election officials to candidates. The technology is effective, scalable, well-suited to citizen-based implementation and "viral" adoption, and cost-effective relative to traditional international election monitoring.[28] We exploit the randomized evaluation of this technology, along with unusually rich administrative data on the election process, to test a set of predictions from a model of trade in corrupt votes between a candidate and an election official.

Theoretical treatments of corruption typically consider an official illegally transacting a government good or service, with comparative statics focusing on the determinants of equilibrium prices and quantities(Shleifer and Vishny, 1993; Cadot,

[28] Viral adoption refers to the adoption of new technology based on information that spreads through pre-existing social networks in a self-replication process. Typically viral adoption relies on ICT to spread information about new technologies.

1987; Rose-Ackerman, 1975). More recent empirical work, especially Fisman (2001) and Khwaja and Mian (2005), documents the central role of political connections in determining who gets illegal preferential access to favors from the government. Our results add to this by providing and experimentally testing a logic for the relevance of political connections in determining the pattern of corruption. Politically connected bribe payers can influence the expected cost for officials engaging in corruption, which is reflected in a lower price.

Our results are consistent with a range of models. However, no matter how they are interpreted, they are actionable for policy. First and most importantly, our results indicate that ICT-based corruption monitoring technologies represent a promising and potentially highly cost-effective means of reducing corruption. These results are supported by a standard randomized impact evaluation, and so should be consistent and internally valid. Second, corrupt networks have both incentives and strong means to adapt to changes that result from monitoring. At a minimum, anti-corruption efforts, especially in weakly institutionalized contexts, should attempt to account for these and also remain sensitive to the possibility of perverse allocative consequences. Specifically, resilient corrupt agents might benefit from monitoring as it pushes less powerful individuals out of the market for illegal government goods. Finally, monitoring is likely to be most cost-effective when it is not possible to predict. Foreknowledge may be met by adaptation, undermining effectiveness.

Our findings produce a natural set of questions for future research. First, data on the response of prices for government favors to an unannounced shock to the detection probability would constitute a direct test of the core prediction of our model. This research design would allow our model to be separated from a broader class of theories. Second, exhaustive data on the reallocation of corruption into unmonitored transactions would permit a full accounting of the ultimate equilibrium pattern corruption. Such data would allow definitive statements about the general equilibrium effect of monitoring on total corruption. Third, understanding the long run effects of reducing corruption in an election, or in any other context, is incomplete without an analysis of the welfare consequences. Finally, and perhaps more practically, identifying and operationalizing innovative uses of ICT to quickly gather information on corruption and other types of waste and abuse, in the presence of non-zero punishments, should provide a strong deterrent.

Table 1.1: Summary Statistics for Political Connections Data

	Full Sample	Strong Protection Capacity	Diff. (2) - (1)	p-value
	(1)	(2)	(3)	(4)
Connected to Provincial Elect. Officer (=1)	0.491	0.632	0.140	0.295
	[0.504]	[0.496]	(0.133)	
Connected to District Elect. Officer (=1)	0.228	0.316	0.088	0.451
	[0.423]	[0.478]	(0.116)	
Served in Senior Post Since 2001 (=1)	0.614	0.684	0.070	0.589
	[0.491]	[0.478]	(0.129)	
Served in Junior Post Since 2001 (=1)	0.158	0.263	0.105	0.312
	[0.368]	[0.452]	(0.103)	
Connected Directly to Karzai (=1)	0.298	0.526	0.228*	0.074
	[0.462]	[0.513]	(0.126)	
Indirectly Connected to Karzai (=1)	0.281	0.211	-0.070	0.554
	[0.453]	[0.419]	(0.118)	
Connected to Provincial Governor (=1)	0.632	0.579	-0.053	0.687
	[0.487]	[0.507]	(0.130)	
Connected to Provincial Council (=1)	0.842	0.737	-0.105	0.312
	[0.368]	[0.452]	(0.103)	
Female (=1)	0.123	0.000	-0.123	0.112
	[0.331]	[0.000]	(0.076)	
Pashtun (=1)	0.368	0.316	-0.053	0.683
	[0.487]	[0.478]	(0.128)	
Tajik (=1)	0.246	0.158	-0.088	0.434
	[0.434]	[0.375]	(0.111)	
Hazara (=1)	0.158	0.158	0.000	1.000
	[0.368]	[0.375]	(0.098)	
Uzbek (=1)	0.123	0.158	0.035	0.700
	[0.331]	[0.375]	(0.091)	
Other Ethnicity	0.088	0.211	0.123	0.155
	[0.285]	[0.419]	(0.086)	
Connected to Insurgents (=1)	0.158	0.211	0.053	0.604
	[0.368]	[0.419]	(0.101)	
Connected to Business (=1)	0.316	0.368	0.053	0.677
	[0.469]	[0.496]	(0.126)	
Election Winner (=1)	0.544	0.526	-0.018	0.896
	[0.503]	[0.513]	(0.134)	
# Observations	57	19		

1.7 Acknowledgement

James D. Long is a co-author on this work.

Table 1.2: Randomization Verification

	Control	Treatment	T-C	p-value
	(1)	(2)	(3)	(4)
Plans to turnout during election (=1)	0.788	0.797	0.009	0.682
	[0.237]	[0.232]	(0.022)	
Believes vote is secret (=1)	0.664	0.650	-0.014	0.561
	[0.267]	[0.255]	(0.025)	
Candidate will know how I voted (=1)	0.088	0.090	0.002	0.868
	[0.147]	[0.153]	(0.014)	
Can identify sitting MP (=1)	0.372	0.386	0.013	0.664
	[0.327]	[0.318]	(0.031)	
People in precinct will vote for same cand. (=1)	0.238	0.249	0.010	0.673
	[0.253]	[0.258]	(0.024)	
Problems with ballot transport are likely (=1)	0.533	0.534	0.001	0.974
	[0.304]	[0.302]	(0.029)	
Police in PC help security (=1)	0.738	0.737	-0.000	0.987
	[0.237]	[0.241]	(0.023)	
People like you are threatened to vote one way (=1)	0.217	0.202	-0.015	0.482
	[0.232]	[0.223]	(0.022)	
Local violence likely on elect. day (=1)	0.501	0.483	-0.018	0.570
	[0.317]	[0.347]	(0.032)	
MP Candidate from same Qawm (=1)	0.233	0.232	-0.001	0.973
	[0.221]	[0.227]	(0.021)	
Trad. auth. helps settle disputes (=1)	0.287	0.293	0.006	0.800
	[0.267]	[0.240]	(0.024)	
Pashtun (=1)	0.326	0.318	-0.008	0.830
	[0.388]	[0.407]	(0.038)	
Tajik (=1)	0.426	0.433	0.007	0.858
	[0.383]	[0.390]	(0.037)	
Income generating activity (=1)	0.602	0.607	0.005	0.793
	[0.198]	[0.192]	(0.019)	
Monthly income (1,000 AFs)	10.613	10.553	-0.061	0.910
	[4.817]	[6.356]	(0.540)	
Electrified (=1)	0.726	0.706	-0.020	0.491
	[0.300]	[0.323]	(0.030)	
District Governor keeps elect. fair (=1)	0.111	0.114	0.004	0.814
	[0.170]	[0.169]	(0.016)	
Visited by international election monitors (=1)	0.144	0.174	0.030	0.380
	[0.350]	[0.378]	(0.034)	
# Observations	227	238		

Notes: Standard deviations reported in brackets and standard errors reported in parentheses. Data on election monitoring visits are provided by Democracy International. Polling data are based on 2,904 responses to interviews performed during August 2010 in 450 of the 471 polling center precincts in our experiment sample. Randomization was blocked on province and stratified on shares reporting some electricity and that the District Governor keeps elections fair.

Table 1.3: Evidence of Fraud Reduction

Dependent Variable:	DR Form Manipulation (=1)					
	Full Sample			Strong Protection Capacity		
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
Delivered Letter (=1)	-0.118***	-0.118***	-0.115***	-0.153***	-0.150***	-0.149***
	(0.031)	(0.030)	(0.030)	(0.044)	(0.042)	(0.043)
Constant	0.189***	0.189***	0.162**	0.216***	0.214***	0.353**
	(0.026)	(0.025)	(0.082)	(0.038)	(0.036)	(0.173)
Province+Stratum FEs	No	Yes	Yes	No	Yes	Yes
Full Covariates	No	No	Yes	No	No	Yes
R-Squared	0.031	0.234	0.251	0.049	0.268	0.288
# Observations	465	444	444	243	243	243
Dependent Variable:	Votes for Most Connected Candidate					
	Full Sample			Strong Protection Capacity		
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
Delivered Letter (=1)	-5.923*	-4.729	-4.855*	-11.180**	-9.866**	-7.830*
	(3.303)	(3.053)	(2.867)	(5.139)	(4.218)	(3.982)
Constant	23.262***	23.619***	36.804***	27.703***	27.012***	18.462
	(2.558)	(2.095)	(6.829)	(4.563)	(3.474)	(11.477)
Province+Stratum FEs	No	Yes	Yes	No	Yes	Yes
Full Covariates	No	No	Yes	No	No	Yes
R-Squared	0.006	0.230	0.243	0.018	0.256	0.343
# Observations	1,879	1,786	1,786	873	873	873
# Clusters	437	420	420	232	232	232

Notes: *Level of significance*: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are reported in parentheses (standard errors in *Panel B* are clustered at the the polling center level). DR Form Manipulation is an indicator for whether candidate agents stole materials or damaged Declaration of Results forms. The Most Connected Candidate is identified using the procedure described in Section 3.4. The full set of covariates are the share of respondents who are Pashtun, Tajik, who anticipate violence on election day, and who can identify the sitting member of parliament and whether the polling center was visited by international election monitors (*Panel B* regressions include the total number of votes cast at the station). Strong Protection Capacity corresponds to provinces in which the Most Connected Candidate has a connection to the Provincial Elections Officer.

Table 1.4: Temporal Recovery

Dependent Variable	Count Manipulation 1: Number ECC Complaints Against Polling Official					
	Full Sample				Ink Problems	
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
Letter Delivered (=1)	1.063*	1.016*	1.967	2.106*	3.680*	3.512**
	(0.594)	(0.539)	(1.297)	(1.272)	(1.893)	(1.635)
Letter x Strong Protection Capacity			-1.702	-2.028	-3.493*	-3.497**
			(1.308)	(1.393)	(1.898)	(1.747)
Constant	0.678	0.408	0.769	0.374	0.830	0.448
	(0.736)	(1.270)	(0.720)	(1.243)	(0.840)	(1.624)
R-squared	0.206	0.253	0.213	0.260	0.261	0.324
# Observations	442	442	434	434	339	339
Dependent Variable	Count Manipulation 2: Number of ECC Complaints by Candidates					
	Full Sample				Ink Problems	
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
Letter Delivered (=1)	1.406*	1.313*	2.655	2.765*	4.614*	4.351**
	(0.750)	(0.680)	(1.612)	(1.588)	(2.413)	(2.046)
Letter x Strong Protection Capacity			-2.309	-2.654	-4.320*	-4.282*
			(1.661)	(1.780)	(2.456)	(2.229)
Constant	1.727***	0.523	1.791***	0.387	1.598***	0.788
	(0.289)	(1.398)	(0.287)	(1.362)	(0.336)	(2.230)
R-squared	0.152	0.194	0.159	0.202	0.197	0.260
# Observations	444	444	436	436	341	341

Notes: *Level of significance*: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. All regression include stratum fixed effects. Robust standard errors are reported in parentheses. The full set of covariates are the share of respondents who are Pashtun, Tajik, who anticipate violence on election day, and can identify the sitting member of parliament and whether the polling center was visited by international election monitors. The Ink Problems sample corresponds to polling centers that report at least one station having no indelible ink to prevent multiple voting or that report at least one station where ink is washable.

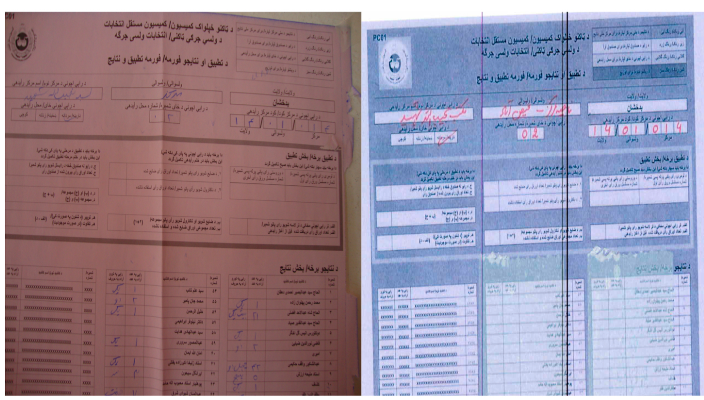

Figure 1.1: Declaration of Results (DR) Form Before and After Aggregation

Table 1.5: Protection Capacity and Spatial Recovery

	Votes for Most Connected Candidate					
	Weak Protection Capacity			Strong Protection Capacity		
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
Letter Delivered (=1)	0.060	-2.317	-3.303	-9.866**	-6.735	-5.466
	(4.351)	(4.093)	(3.709)	(4.218)	(4.278)	(4.364)
Nearest 5 Neighbors Treat (1-5)		-4.967*	-3.794*		1.931	2.915
		(2.611)	(2.090)		(2.249)	(2.566)
Second Nearest 5 Neighbors Treat (1-5)		-5.511**	-5.412**		5.897*	6.988**
		(2.355)	(2.144)		(3.372)	(3.507)
Third Nearest 5 Neighbors Treat (1-5)		-3.555	-3.653		3.022	4.335
		(2.570)	(2.620)		(3.335)	(3.792)
Constant	20.481***	55.303***	60.820***	27.012***	-3.374	1.309
	(2.402)	(13.615)	(15.563)	(3.474)	(19.011)	(25.590)
Full Controls	No	No	Yes	No	No	Yes
R-Squared	0.206	0.226	0.262	0.256	0.264	0.284
# Observations	913	913	913	873	873	873
# Clusters	188	188	188	232	232	232
	DR Form Manipulation (=1)					
	Weak Protection Capacity			Strong Protection Capacity		
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
Letter Delivered (=1)	-0.080*	-0.099**	-0.092*	-0.150***	-0.173***	-0.172***
	(0.044)	(0.048)	(0.048)	(0.042)	(0.049)	(0.051)
Nearest 5 Neighbors Treat (1-5)		-0.039	-0.045		-0.043	-0.039
		(0.032)	(0.033)		(0.030)	(0.031)
Second Nearest 5 Neighbors Treat (1-5)		-0.015	-0.017		-0.045	-0.047
		(0.028)	(0.028)		(0.034)	(0.035)
Third Nearest 5 Neighbors Treat (1-5)		-0.048*	-0.051*		0.021	0.018
		(0.027)	(0.028)		(0.035)	(0.036)
Constant	0.159***	0.414**	0.352*	0.214***	0.404*	0.519*
	(0.034)	(0.175)	(0.195)	(0.036)	(0.221)	(0.275)
# Observations	201	201	201	243	243	243
R-Squared	0.195	0.213	0.253	0.268	0.289	0.307

Notes: *Level of significance*: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Robust standard errors are reported in parentheses (standard errors in *Panel B* are clustered at the the polling center level). DR Form Manipulation is an indicator for whether candidate agents stole materials or damaged Declaration of Results forms. The Most Connected Candidate is identified using the procedure described in Section 3.4. The full set of covariates are the share of respondents who are Pashtun, Tajik, who anticipate violence on election day, and who can identify the sitting member of parliament and whether the polling center was visited by international election monitors (*Panel B* regressions include the total number of votes cast at the station). Strong Protection Capacity corresponds to provinces in which the Most Connected Candidate has a connection to the Provincial Elections Officer.

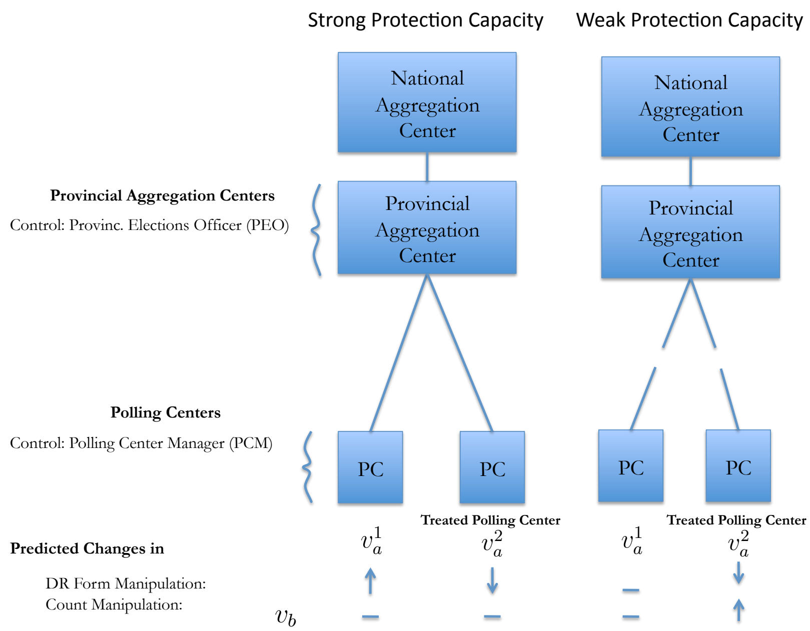

Figure 1.2: The Aggregation Process and Theoretical Predictions

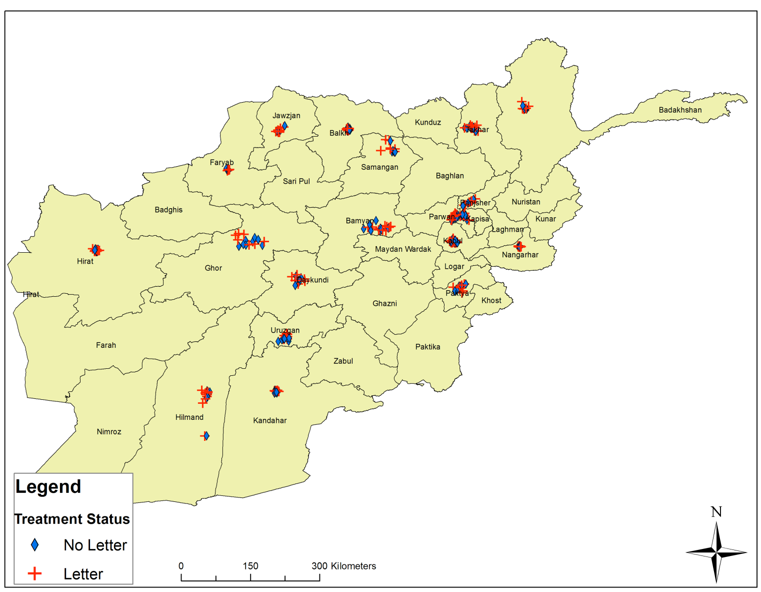

Figure 1.3: Experimental Sample Map

Table 1.6: Polling Centers with Pre-Count Fraud Exhibit the Largest Treatment Effects

	DR Form Manipulation				Votes for Politically Connected MP			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Received Letter (=1)	-0.063*	-0.056*	-0.043	-0.037	-5.706	-5.728	9.656	11.694
	(0.033)	(0.033)	(0.092)	(0.090)	(3.973)	(3.641)	(9.125)	(8.751)
No Ink at PC (=1)	0.247**	0.265**			-7.745	-10.121		
	(0.124)	(0.122)			(7.278)	(7.205)		
Treat x No Ink at PC	-0.268***	-0.276***			9.022	9.540		
	(0.095)	(0.092)			(6.655)	(6.886)		
Ink Washable (=1)			-0.014	-0.024			17.523**	18.000***
			(0.084)	(0.082)			(6.769)	(6.683)
Treat x Ink Washable			-0.037	-0.043		-18.655*	-20.716**	
			(0.098)	(0.096)			(10.018)	(9.606)
Constant	0.117	0.063	0.174*	0.080	28.325***	44.063***	12.040	26.750**
	(0.076)	(0.108)	(0.102)	(0.124)	(7.899)	(11.337)	(7.829)	(10.882)
Sample	Full	Full	Ink	Ink	Full	Full	Ink	Ink
Full Controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.191	0.216	0.165	0.195	0.196	0.218	0.208	0.233
# Observations	387	387	336	336	1,608	1,608	1,387	1,387
# Clusters	-	-	-	-	369	369	319	319

Notes: *Level of significance*: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Robust standard errors are reported in parentheses. Standard errors for estimates in *Panel B* are clustered at the the polling station level. All regressions include province and stratum fixed effects. The full set of covariates are the share of respondents who are Pashtun, Tajik, who anticipate violence on election day, and can identify the sitting member of parliament and whether the polling center was visited by international election monitors. *Panel B* estimates also control for total votes cast at the station. The *Ink* sample corresponds to the set of polling centers in our experimental sample that report having indelible ink to prevent multiple voting on election day. Results in specifications 1 - 4 are robust to negative binomial regression and results in specifications 5 -8 are robust to probit regression.

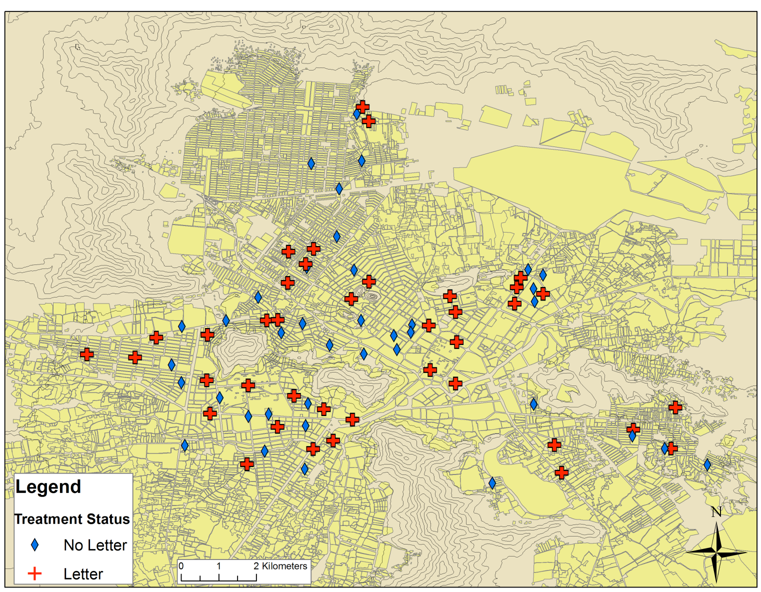

Figure 1.4: Experimental Sample in Kabul

Table 1.7: Robustness - Protection Capacity and Spatial Recovery

	Votes for Most Connected Candidate					
	Weak Protection Capacity			Strong Protection Capacity		
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
Received Letter (=1)	-0.037	-64.338	-0.049	-0.456**	-76.310	-0.116
	(0.174)	(58.626)	(0.076)	(0.181)	(51.160)	(0.076)
Nearest 5 Neighbors Treat (1-5)	-0.190**	-69.835*	-0.085**	0.094	8.664	0.060
	(0.087)	(35.556)	(0.041)	(0.155)	(32.326)	(0.061)
Second Nearest 5 Neighbors Treat (1-5)	-0.321***	-84.993**	-0.106**	0.242*	58.619*	0.101**
	(0.111)	(38.541)	(0.050)	(0.145)	(35.307)	(0.046)
Third Nearest 5 Neighbors Treat (1-5)	-0.173	-80.500**	-0.134***	0.270	13.995	0.045
	(0.108)	(34.350)	(0.047)	(0.170)	(41.851)	(0.069)
Constant	6.461***	1408.485***	8.261***	1.543	1146.590***	6.548***
	(0.835)	(286.598)	(0.376)	(1.434)	(271.927)	(0.493)
$ln(\alpha)$	0.203**		0.218*	0.657***		0.574***
	(0.100)		(0.116)	(0.098)		(0.089)
Estimation	NB	Rank	NB Rank	NB	Rank	NB Rank
# Observations	913	913	913	866	866	866
# Clusters	188	188	188	230	230	230
Log-Likelihood (R-Squared)	-3375.316	(0.378)	-7124.650	-3194.843	(0.398)	-6571.506
	Votes for Most Connected Candidate					
Panel B	Weak Protection Capacity			Strong Protection Capacity		
Trimmed at 99th Pctile	(1)	(2)	(3)	(4)	(5)	(6)
Received Letter (=1)	-0.123	-70.250	-0.053	-0.225	-65.422	-0.088
	(0.162)	(58.554)	(0.076)	(0.161)	(50.361)	(0.078)
Nearest 5 Neighbors Treat (1-5)	-0.224***	-72.550**	-0.087**	0.000	-2.416	0.044
	(0.083)	(35.201)	(0.040)	(0.099)	(29.949)	(0.058)
Second Nearest 5 Neighbors Treat (1-5)	-0.336***	-86.505**	-0.107**	0.215**	45.307	0.082**
	(0.104)	(38.465)	(0.050)	(0.100)	(32.865)	(0.041)
Third Nearest 5 Neighbors Treat (1-5)	-0.116	-72.964**	-0.130***	0.007	-6.873	-0.005
	(0.104)	(34.332)	(0.048)	(0.127)	(37.948)	(0.061)
Constant	3.714***	1360.474***	7.581***	2.773***	1143.890***	6.882***
	(0.741)	(286.182)	(0.374)	(0.783)	(266.474)	(0.338)
$ln(\alpha)$	0.135		0.226*	0.454***		0.575***
	(0.106)		(0.116)	(0.081)		(0.089)
Estimation	NB	Rank	NB Rank	NB	Rank	NB Rank
# Observations	905	905	905	855	855	855
# Clusters	188	188	188	230	230	230
Log-Likelihood (R-Squared)	-3289.262	(0.375)	-7054.750	-3029.159	(0.413)	-6467.521

Notes: *Level of significance*: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. All regressions include province and stratum fixed effects and full covariates. Robust standard errors clustered at the Polling Center level are reported in parentheses. The full set of covariates are the share of respondents who are Pashtun, Tajik, who anticipate violence on election day, and whether the polling center was visited by international election monitors. Estimation is by: NB = Negative Binomial; Rank = OLS with the dependent variable transformed to be the within-sample rank; NB Rank = Negative Binomial with the dependent variable transformed to be the within-sample rank. Panel B is trimmed at the 99th percentile of the dependent variable.

Polling Center Name: …………………..

Polling Center Code:…………………….

Date: ………………………………………

Dear Sir or Madam-

Greetings! I am an official election observer with the Opinion Research Center of Afghanistan (ORCA). My organization is providing this letter to collect some important information about your polling center and share it with our main office. Your polling center has been randomly selected from among polling centers in this province.

In our attempts to help Afghanistan have free and fair elections, I will return to this polling center tomorrow morning in order to take pictures of the results for every candidate in every station on the tally sheets after they have been posted.

The information will be posted on a website that belongs to local and international election observers so that it will be used by the people of Afghanistan, the international community, and local and international media. We will also compare the photos taken with the tally certified by the IEC in Kabul.

As recognition that you have read and understood this letter, please sign here: ___________

Thank you kindly for your help and cooperation.

Sincerely,

Haj Abdul Nabi Barakzai

Deputy Head of ORCA

Name and Signature of manager of polling station:………………………………………

Figure 1.5: Letter Delivered to PCMs

Opinion Research Center of Afghanistan

نام مرکز رائ دهی: ____________________ تاریخ: ____________________

____ مرکز رائ دهی: ________________کود

به حضور محترم آقای / خانم

مسئولیت نظارت 472 مراکز رائ برحسب توافقنامه کمیسیون مستقل انتخابات دفتر اورکا
دهی را بر عهده دارد.

میباشد و برای او (ORCA) دفتربه مربوطیک تن از نظارت کننده گان رسمیدارنده مکتوب
معلومات تا بتواند مرکز رای دهی تسلیم نمودهداین تا این مکتوب را وظیفه سپرده شده است
. این مرکز دفتر مرکزی شریک بسازدجمع آوری نموده و با مرکز رای دهی این و دقیق را ازموثق
این ولایت تمام مراکز رای دهی میانبه صورت تصادفی از گر به شمول چندین مراکز دیرای دهی
انتخاب شده است.

فردا صبح . ناظر ما یک انتخابات آزاد و مشروع در افغانستان کمک خواهیم کرد تقویتبرایما
. نصب میگردد اخذ نمایدمرکز رای دهیاین که در را نتایج کاندیدان لست آمد تا تصاویر از دخواه

گذاشته مربوط به ناظرین انتخاباتی داخلی و خارجی این نتایج در سایت انترنتی تصاویر
از این نتایج ، موسسات خارجی، و مطبوعات داخلی و خارجیخواهد شد تا تمام مردم افغانستان
نتایج را با نتایج که از طرف اینتصاویر حاصله از ناظرحیث مناستفاده کنند. و همچنان ما
انتخابات در کابل نشر میشود مقایسه خواهیم کرد. مستقلکمیسیون

در پائین ایدبرای تائید اینکه این مکتوب بدسترس شما قرار گرفته و شما انرا مطالعه نموده
مضا نمائید. لطف نموده ا

از همکاری شما قبلاً اظهار سپاس.

بااحترام

حاجی عبدالنبی بارکزی

معاون دفتر اورکا

آمر محترم مرکزرائ دهی: ________________ یامضااسم و

Figure 1.6: Dari Translation of Letter Delivered to PCMs

Chapter 2

Towards an Understanding of Violent Trauma and Risk Preference: Artefactual and Experimental Evidence from Afghanistan

Abstract

Trauma has complex and strongly enduring mental, physical, and social consequences, especially among populations with direct exposure to extreme violence. Documenting, understanding, and treating these e?ects lie, appropriately, in the medical and psychiatric elds. Many of the most severe consequences, however, manifest themselves as changes in economic decision-making suggesting that economics may be able to provide a complementary contribution. Using a novel two-part experimental procedure which non-parametrically estimates aversion to risk and the direct preference for certainty, controlled experimental recall based on established methods in psychology, and a correlational study of a population with considerable exposure to violence based on precisely geocoded military records, we establish a link between trauma, risk preference, and the direct preference for certainty. These results suggest consistency between apparently contrasting results from early studies in psychology

and economics: trauma increases risk tolerance but does so disproportionately when choosing between two uncertain gambles, which results in an increased direct preference for certainty. We additionally provide evidence that the e?ect of trauma on economic decision-making is primarily mediated through recall.

2.1 Introduction

Documenting, analyzing, and understanding the effects of trauma lie, appropriately, in the medical and psychiatric fields. Clinicians recognize that exposure to trauma can have complex and lasting effects on both mental and physical health (Boscarino, 2006; Yehuda, 2002). Dramatically, Post-Traumatic Stress Disorder (PTSD), is estimated to affect 5-6 percent of men and 10-14 percent of women in the United States at some point during their lives (Kessler et al., 1995; Yehuda, 2002). Estimates from the National Vietnam Veterans Readjustment Survey indicate that the lifetime PTSD prevalence for veterans is much higher, 30.9 percent for males and 26.9 percent for females (Schlenger et al., 1992). The disorder is linked to depression, generalized anxiety disorder, panic disorder, substance abuse, and a range of health conditions including hypertension, asthma, and chronic pain syndrome (Kessler et al., 1995; Yehuda, 2002).

Social science, and economics in particular, has yet to make substantial contributions to the study of trauma. However, given the volume of trauma-affected individuals in both the civilian and military populations there is reason to be interested in the socio-economic implications of traumatic exposure. Comorbidity studies suggest that trauma leads to severe economic consequences because of associated psychiatric, psychosocial, and occupational impairments (Brunello et al., 2001). Exploring the mechanisms underlying these phenomena may help design policy interventions and lead to greater insight on individual decision-making.

Recent research in both economics and psychology points to a potential relationship between trauma and the economic risk preferences central to decision-making. Early life financial experiences such as the Great Depression are linked to more conservative later life investing behavior (Malmendier and Nagel, Forthcoming), potentially suggesting an increase in risk aversion.[1] Conversely, artefactual field experiments (Harrison and List, 2004) from trauma-affected regions such as zones of natural disaster or conflict suggest that traumatized individuals are more risk seeking (Eckel et al., 2009b; Voors et al., Forthcoming).[2] Though causality is difficult to establish and a central stylized fact has yet to appear, these correlational studies compellingly suggest that individuals exposed to trauma may have their risk preferences *permanently* altered.[3] This sustained change in fundamental economic decision-making carries the implication that economic consequences of broad-based exposure to trauma may be extremely large.

By contrast, psychology has taken a more controlled approach to investigating the relationship between trauma and risk. Though trauma cannot be experimentally administered, it can be experimentally recalled. Fitting in to a broader agenda inducing positive and negative affect and observing effects on decision-making (Isen and Geva, 1987; Johnson and Tversky, 1983; Lerner et al., 2004; Slovic and Peters, 2006), one priming mechanism employed in the literature has been the controlled recollection of traumatic episodes. Lerner and Keltner (2001) and Lerner et al. (2003) ask individuals to recall specific, potentially traumatic events with corresponding emotion, such as anger or fear.[4] These recollections both induce the relevant emotion in self-reports (Lerner and Keltner, 2001; Lerner et al., 2003), and have startling effects on decision-making under uncertainty. Fearful recollections induce more pessimistic likelihood judgements about a variety of events, and, in related studies, self-reported fear and anxiety correlate highly with risk averse choices.[5] Though the type of questions

[1] A clear alternative, however, is that such experiences change beliefs about the process of returns. See Malmendier and Nagel (Forthcoming) for further discussion. Such evidence relates closely to genetic evidence indicating that though some heritability in risk preferences and financial behavior is observed, much of the variation remains unexplained (Cesarini et al., 2009, 2010; Kuhnen et al., 2011).

[2] In contrast, using identical experimental methods to those implemented by Eckel et al. (2009b) in the aftermath of Hurricane Katrina, Cameron and Shah (2010b) show in Indonesia an increase in risk aversion associated with exposure to earthquakes and floods and document long-lasting effects.

[3] Eckel et al. (2009b) note that changed risk preferences appear to attenuate within one year while Cameron and Shah (2010b) document effects up to nine years after exposure.

[4] For example, after Sepember, 11th 2001, Lerner et al. (2003) ask "The terrorist attacks evoked a lot of emotion in Americans. We are particularly interested in what makes you most AFRAID about the attacks. Please describe in detail the one thing that makes you most AFRAID about the attacks. Write as detailed a description of that thing as possible. If you can, write your description so that someone reading it might even get AFRAID from learning about the situation. "

[5] For example, questions such as the Asian disease problem (Lerner and Keltner, 2001, Study 1).

asked differs greatly across experimental economics and psychology, the psychological finding that recalling fearful or anxious events is linked to more risk aversion, and the economic finding that living through such trauma is linked to less risk aversion presents a potential inconsistency.

Behavioral economics is predicated on combining psychological and economic insights and so can bring to bear a variety of useful tools for analyzing the relationship between trauma and risk preferences. In this paper, we combine three critical tools. First, we introduce a novel, field-ready experimental procedure based on the uncertainty equivalents of Andreoni and Sprenger (2011b).[6] This procedure can both non-parametrically measure risk aversion, and test the predictions of a variety of competing decision theories including Expected Utility (EU), Cumulative Prospect Theory (CPT) (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992), Disappointment Aversion (DA) (Bell, 1985; Loomes and Sugden, 1986; Gul, 1991; Koszegi and Rabin, 2006, 2007)[7], and "*u*-*v*" preferences (Neilson, 1992; Schmidt, 1998; Diecidue et al., 2004).[8] Several of these models make predictions of changing patterns of risk aversion. Our design—which measures risk aversion elicited under both certainty and uncertainty—permits an investigation of whether the *prima facie* inconsistency between the psychological and economic investigations discussed above reflects systematically changing patterns of risk aversion.

Second, we conduct our experiments in Afghanistan, a nation with widespread exposure to violent trauma, on a sample of 1,127 Afghan civilians across 12 of the 34

[6]Methods like the uncertainty equivalent were discussed in Farquhar's (1984) excellent survey of utility assessment methods and, to our knowledge, were implemented experimentally in only one study of nine subjects using hypothetical monetary rewards (McCord and de Neufville, 1986), and a number of medical questionnaires (Magat et al., 1996; Oliver, 2005, 2007; Bleichrodt et al., 2007).

[7]We include the Koszegi and Rabin (2006, 2007) model in the broad class of disappointment averse preferences as the model's predictions will closely resemble those of Bell (1985); Loomes and Sugden (1986); Gul (1991) in the present context as well as most other experimental environments (Ericson and Fuster, Forthcoming; Gill and Prowse, 2010; Abeler et al., Forthcoming). For specific evidence distinguishing Koszegi and Rabin (2006, 2007) preferences from Bell (1985); Loomes and Sugden (1986); Gul (1991), see Sprenger (2010).

[8]*u*-*v* preferences are less well-known than the other preference models. For a discussion of the early history of *u*-*v* preferences, see Schoemaker (1982). These models capture the intuition of Allais (1953) that when options are far from certain, individuals act effectively as EU maximizers but, when certainty is available, it is disproportionately preferred. The *u*-*v* model differs in important ways from extreme or even discontinuous probability weighting and prior experiments have demonstrated these differences (Andreoni and Sprenger, 2011a).

provincial capitals.[9] Importantly, we have access to detailed data on violent incidents from the International Security Assistance Force (ISAF), with precise geocoded locations and timestamps of both successful and failed insurgent attacks. The richness of the violence data provides for artefactual analysis based on administrative, as opposed to self-reported data, and for plausible placebo tests based on the correlation between risk preferences and failed and successful attacks.[10]

Third, psychological methods that randomize the controlled recollection of fearful episodes are deployed in the field and stratified geographically to permit estimation of the interaction between exposure to violence and priming. One-third of our sample was asked to recall a fear or anxiety-inducing episode using methods validated in the psychological literature immediately prior to completing the experimental tasks. Administration of the primes is random so that primed and un-primed individuals can be compared to measure the average causal effect of recalling trauma on risky decisions. Critically, the design allows artefactual and experimental data to be combined so that trauma-affected and unaffected individuals can be compared with and without experimental primes. This may provide the necessary machinery to speak to the permanence of the effects of trauma on risk preferences and the potential triggering of irrational economic behavior.

The combination of methods provides a series of interesting results. To begin, we document substantial differences between utility elicited under uncertainty and utility elicited under certainty. Individuals are systematically more risk averse under certainty, in contradiction to both EU and CPT, indicating a specific preference for certainty as in DA and u-v preferences. Interestingly, the preference for certainty, which we term a Certainty Premium, is exacerbated by violent exposure and by recollection of fearful events. The nature of the results suggest that when everything is uncertain, exposure to violence and fearful recollections increase risk tolerance, but

[9] In 2007, 1,523 civilian deaths were recorded in Afghanistan (UNAMA, 2008). This number increased to 2,118 in 2008, 2,415 in 2009 and 2,777 in 2010 (UNAMA, 2010). During this period, instability has spread from the south to the northern, eastern and western regions of the country such that Afghans throughout the country have become exposed to violence.

[10] Relying exclusively on self-reports would introduce several additional confounds. First, unwillingness to discuss violent episodes is a hallmark symptom of PTSD (Yehuda, 2002). Second, a preference for high probability outcomes might reflect distrust in the experimenter, which may be affected by the general perception of the role of foreigners in causing and in responding to trauma-inducing events (Andrabi and Das, 2010).

that when certainty is available it remains attractive, increasing measured Certainty Premia. When exploring the interaction, we find that violence without fear and fear without violence have no effect on measured risk preferences. Taken together the results indicate that fearful recollections trigger changes in risk and certainty preferences for those exposed to violent trauma. The results are robust to a variety of alternate specifications including exploring issues of selective migration, social cohesion, and changing vintages of violence.

We point to three implications of these findings. First, as we obtain both increased risk tolerance under uncertainty in response to both violence and fearful recollections, and increased preferences for certainty, our findings may help to rationalize inconsistencies across psychological and economic techniques. Second, our data speak to the permanence of traumatic exposure's influence on risk preference. Violence-exposed individuals may be altered, but it is the susceptibility of their risk preferences to priming triggers that is altered, not their risk preferences *per se*. Third, if a specific pattern of risk preferences can be triggered for trauma-exposed individuals, then this information is potentially actionable by both marketers and policy-makers in product design and policy intervention.

The paper proceeds as follows. Section 2 presents our broad research design combining novel utility elicitation procedures, priming mechanisms borrowed from psychology, and objective violence data from ISAF. Section 3 presents results and Section 4 is a discussion and conclusion.

2.2 Research Design

In this section we describe our broad research design exploring exposure to violent trauma and risk preferences in three sub-sections. First, we describe our methodology for utility elicitation. Second, we discuss the priming methods borrowed from psychology for manipulating traumatic recollections. Third, we present our violence data obtained from military records and introduce our artefactual structure.

2.2.1 Utility Elicitation

Researchers in both decision science and experimental economics have long been interested in eliciting utility and measuring risk aversion.[11] A key contribution from experimental economics is the risk preference Multiple Price List (MPL) methodology of Holt and Laury (2002). Subjects make a series of decisions between Option A, a safe binary gamble with outcomes close together, and Option B, a risky binary gamble with more variable outcomes. As subjects proceed through the task, the probability of the high outcome in each gamble moves from zero to one, such that the difference in expected value, $EV(A) - EV(B)$, moves from positive to negative. Therefore, where a subject switches from Option A to Option B in a given Holt and Laury (2002) task carries interval information on their risk aversion. A risk neutral subject will switch from Option A to Option B when the difference in expected value switches from positive to negative, a risk loving subject will switch before, and a risk averse subject will switch after.

The choices in Holt and Laury (2002) tasks are often used to infer a parametric measure of risk aversion such as the coefficient of relative risk aversion. That is, EU is imposed, a functional form for utility is assumed, and the shape of the utility function is calculated or estimated at either the group or individual level. Harrison and Rutstrom (2008) provide a detailed summary of the estimation exercises associated with the Holt and Laury (2002) task, and other tasks similar to those of Voors et al. (Forthcoming), Eckel et al. (2009b), and Cameron and Shah (2010b).[12]

A potential difficulty in parametrically identifying utility values lies in the validity of the underlying EU assumptions. Particular attention should be given to the independence axiom and its implication of linearity-in-probabilities.[13] Beginning

[11] In decision science Farquhar (1984) discusses a variety of techniques for utility assessment and Harrison and Rutstrom (2008) provide a detailed summary of experimental economic methods.

[12] A critical distinction between these tasks and the Holt and Laury (2002) task is the use of certainty. Certainty does not play a role in the Holt and Laury (2002) task as individuals, with the exception of the last row, are always choosing between positive variance gambles. In Eckel et al. (2009b), and Cameron and Shah (2010b) individuals make a choice between six binary gambles, one of which pays the same in both states. Voors et al. (Forthcoming) implement a variant of a task implemented in Harbaugh et al. (2010) where individuals choose make six choice between a changing certain amount and a 30%-70% gamble over a high outcome and zero.

[13] The independence axiom is closely related to the Savage (1954) 'sure-thing principle' for subjective expected utility (Samuelson, 1952). Expected utility became known as von Neumann-Morgenstern (vNM) preferences after the publication of von Neumann and Morgenstern (1944).

with the Allais (1953) common-ratio and common-consequence paradoxes, research consistently demonstrates failures of linearity-in-probabilities (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992; Tversky and Fox, 1995), particularly in decisions involving certainty (Conlisk, 1989; Camerer, 1992; Harless and Camerer, 1994; Starmer, 2000). A number of behavioral decision theories, designed to account for these so-called 'certainty effects' have arisen, including CPT (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992), DA (Bell, 1985; Loomes and Sugden, 1986; Gul, 1991; Koszegi and Rabin, 2006, 2007), and *u*-*v* preferences (Neilson, 1992; Schmidt, 1998; Diecidue et al., 2004). Under these alternative models, behavior in Holt and Laury (2002) tasks, and the tasks of Voors et al. (Forthcoming), Eckel et al. (2009b), and Cameron and Shah (2010b), is not attributable to utility function curvature alone.

We introduce a novel experimental methodology designed to both elicit utility, and test the predictions of EU and competing behavioral models of risk preference designed to accommodate certainty effects. The task is a field-ready, two-question modification of the uncertainty equivalents presented in Andreoni and Sprenger (2011b). Whereas a certainty equivalent identifies the certain amount that generates indifference to a given gamble, the uncertainty equivalent identifies the probability mixture over the gamble's best outcome and zero that generates indifference. For example, consider a $(p, 1-p)$ gamble over X and Y > X, $(p; X, Y)$. The uncertainty equivalent identifies the $(q, 1-q)$ gamble over Y and 0, $(q; Y, 0)$, that generates indifference.[14] The uncertainty equivalent draws its motivation from the derivation of expected utility, where the cardinal index for a gamble is derived as the probability mixture over the best and worst options in the space of gambles.[15] This means that in an uncertainty equivalent measure, the elicited q in $(q; Y, 0)$ can be interpreted as an utility

Independence, however, was not among the discussed axioms, but rather implicitly assumed. Samuelson (1952, 1953) discusses the resulting confusion and his suspicion of an implicit assumption of independence in the vNM treatment. Samuelson's suspicion was then confirmed in a note by Malinvaud (1952). For an excellent discussion of the history of the independence axiom, see Fishburn and Wakker (1995).

[14] We recognize that it is a slight abuse of traditional notation to have the probability refer to the lower outcome in the given gamble and the higher outcome in the uncertainty equivalent. It does, however, ease explication to have p refer to the probability of the low value and q refer to the probability of the high value.

[15] Such derivations are provided in most textbook treatments of expected utility. See, e.g. Varian (1992).

index for the p gamble, $(p; X, Y)$, when $Y > X > 0$.

Task 1: Eliciting Utility Under Uncertainty: In Task 1, we fix $p = 0.5$. Under the EU framework, the uncertainty equivalent establishes the indifference condition

$$0.5 \cdot v(X) + 0.5 \cdot v(Y) = q \cdot v(Y) + (1 - q) \cdot v(0).$$

As EU is unique up to an affine transformation, we fix the values $v(0) = 0$ and $v(Y) = 1$ and rearrange to obtain the utility of X,

$$v(X)_U \equiv \frac{q - 0.5}{0.5}, \tag{2.1}$$

where the U subscript refers to the utility being elicited under uncertainty.[16] Note that $v(X)_U$ can be compared to a risk neutral level, X/Y. A risk neutral individual will exhibit $v(X)_U = X/Y$, a risk averse individual will exhibit $v(X)_U > X/Y$, and a risk loving individual will exhibit $v(X)_U < X/Y$. This is a non-parametric measure of risk aversion as $v(X)_U$ values can be compared across individuals, but no assumptions are necessary for utility's functional form.

Task 2: Eliciting Utility Under Certainty: In Task 2, we fix $p = 1$. Again, under the EU framework the uncertainty equivalent establishes the indifference condition

$$v(X) = q' \cdot v(Y) + (1 - q') \cdot v(0).$$

We make the EU substitutions $v(0) = 0$ and $v(Y) = 1$ to obtain

$$v(X)_C \equiv q', \tag{2.2}$$

where the C subscript refers to the utility being elicited under certainty.[17] This utility value again acts as a non-parametric measure of risk aversion.

Importantly, EU's prediction of linearity-in-probabilities implies the equality

$$v(X)_U = v(X)_C$$

[16]Note that this is a definition. The object $v(X)_U$ will be estimated in subsequent analysis.
[17]Note that this is a definition. The object $v(X)_C$ will be estimated in subsequent analysis.

will hold. This is a critical prediction of EU's independence axiom testable in the uncertainty equivalent environment. Unlike parametric tests of linearity-in-probabilities (Tversky and Kahneman, 1992; Tversky and Fox, 1995), our test does not rely on functional form assumptions for identification. Interestingly, alternative preference models provide differing predictions as to the relationship between $v(X)_U$ and $v(X)_C$. We consider these next.

Alternative Preference Models

CPT, DA, and *u*-*v* preferences make differing predictions in uncertainty equivalents (Andreoni and Sprenger, 2011b). In our two question environment, these arguments reduce to sign predictions for the inequality between $v(X)_U$ and $v(X)_C$.

CPT attributes violations of EU to a non-linear probability weighting scheme.[18] In particular, it is argued that individuals 'edit' probabilities, up-weighting low probabilities and down-weighting high probabilities, giving rise to an inverted S-shaped weighting transformation, $\pi(p)$. In CPT, decision weights are applied to the higher outcome of a binary gamble and probabilities zero and one are unweighted. Identifying the S-shape of the weighting function and determining its parameter values has received significant attention both theoretically and in experiments (Wu and Gonzalez, 1996; Prelec, 1998; Gonzalez and Wu, 1999; Abdellaoui, 2000). Consensus establishes the region of up-weighting to be probabilities less than one-third, with down-weighting thereafter. For Task 1, with $p = 0.5$, the probability of receiving the high outcome, Y, in the $(p; X, Y)$ gamble would be down-weighted. Given that $q \geq 0.5$ is expected in order for the utility function to be monotonic, the probability of receiving Y in the $(q; Y, 0)$ gamble would also be down-weighted. The relative effects of these down-weighting forces, in combination with standard utility function curvature, will determine risk aversion in Task 1.

As the literature has followed a primarily parametric path, we consider one form for CPT probability weighting popularized by Tversky and Kahneman (1992), $\pi(p) = p^\gamma/(p^\gamma + (1-p)^\gamma)^{1/\gamma}$, $0 < \gamma < 1$, where γ represents the intensity of probability weighting. For ease of exposition and to foreshadow our implementation, we assume

[18] We abstract away from loss aversion around a fixed-reference point that is part of the CPT formulation.

risk neutrality in the absence of probability weighting, $X = 1$, and $Y = 3$, and the Tversky and Kahneman (1992) estimate of $\hat{\gamma} = 0.61$ such that the CPT indifference condition in Task 1 is written

$$(1 - \pi(0.5)) \cdot 1 + \pi(0.5) \cdot 3 = \pi(q) \cdot 3 + (1 - \pi(q)) \cdot 0.$$

Hence we can compare the elicited uncertainty equivalent

$$q = \pi^{-1}(\frac{(1 - \pi(0.5)) \cdot 1 + \pi(0.5) \cdot 3}{3}) = 0.81$$

to the risk neutral value

$$\overline{q} = \frac{0.5 \cdot 1 + 0.5 \cdot 3}{3} = \frac{2}{3},$$

noting that for these parameter values CPT predicts risk aversion as $q > \overline{q}$ and hence $v(X)_U > X/Y$.[19]

Interestingly, the risk aversion prediction above is not maintained under certainty. Note that in Task 2 under our assumed forms, the CPT indifference condition is written

$$1 = \pi(q') \cdot 3; \; q' = \pi^{-1}(\frac{1}{3}).$$

Importantly, under most functional forms for $\pi(\cdot)$ considered in the literature, probabilities are neither up-weighted nor down-weighted for probabilities around 1/3. Hence, near risk neutrality should be expected. Indeed under the Tversky and Kahneman (1992) weights, we predict $q' = 0.33$ leading to the observation that under risk-neutral CPT, $X = 1$, $Y = 3$, common functional forms, and parameter values

$$v(X)_C < v(X)_U.$$

The prediction from CPT is in contrast to DA and u-v preferences. These models both feature specific preferences for certainty in order to accommodate certainty effects. Under disappointment averse preferences, the prospect of losses are eliminated at certainty, leading certainty to be disproportionately preferred. Under

[19] Indeed, for all $\gamma < 1$ this relationship is maintained. Andreoni and Sprenger (2011b) provide more detailed discussion as well as model predictions without appeal to specific functional forms and parameter values.

u-v preferences, certainty *per se* yields a utility boost. Though in a richer environment such as that presented in Andreoni and Sprenger (2011b) the two models can be distinguished, in the present environment they both generate the prediction that utility under certainty is greater than utility under uncertainty,

$$v(X)_C > v(X)_U.$$

Because the discussed non-EU models can predict differences between $v(X)_C$ and $v(X)_U$, we define the signed distance between the two as the

$$Certainty\ Premium \equiv v(X)_C - v(X)_U. \tag{2.3}$$

Based on the development above, EU predicts a zero Certainty Premium, CPT predicts a negative Certainty Premium, and DA and u-v preferences both predict a positive Certainty Premium. Note that that Certainty Premium, $v(X)_C - v(X)_U$, is defined in probability units of the high outcome, Y, such that one can refer to certainty of X being worth a specific percent chance of Y relative to its uncertain value.

With our modied two-task uncertainty equivalent, we are able to provide measures of risk aversion as well as test for violations of EU, separate between competing non-EU decision theories, and generate a measure of Certainty Premium.

Implementation and Protocol

In order to implement our two-task uncertainty equivalent, two MPLs were designed. These experimental tasks were placed in fixed order, Task 2 then Task 1, in a broad survey of household experiences, attitudes, and beliefs, administered to 2,027 respondents in 12 of Afghanistan's 34 provincial centers from December 18 - 27, 2010.

In each task, subjects made a series of ten decisions between Option A, a fixed $(p; X, Y)$ gamble, and Option B, a changing $(q; Y, 0)$ gamble. The probability, q, increased from 0.1 to 1 through a task. The values of X and Y were chosen to be 150 and 450 Afghanis, respectively. These values correspond to around 1 and 3 days wages in provincial centers, according to prior household surveys (NRVA, 2005, 2007). Therefore, in Task 1, Option A was a $(0.5; 150, 450)$ gamble and in Task 2, Option A was a $(1; 150, 450)$ gamble, while Option B was a changing $(q; 450, 0)$ gamble for both tasks.

Table 2.1 provides the multiple price lists as they appeared, translated into English.[20] Where an individual switches from Option A to Option B in each task carries interval information on their uncertainty equivalent, and so the utility values, $v(X)_U$ and $v(X)_C$, and their Certainty Premium.

The primary sampling unit for our survey was polling centers open on election day, September 18th 2010, and deemed secure by the International Security Assistance Force (ISAF) and Afghan National Police (ANP) for the safety of our field staff.[21] These polling centers were generally neighborhood landmarks such as mosques, schools or markets. Survey enumerators were told to begin at the polling center and survey 6 to 8 subjects. Enumerators adhered to the right hand rule random selection method and respondents within houses were selected according to a Kish grid (Kish, 1949). In keeping with Afghan custom, men and women were interviewed by field staff of their own gender.

Critical to implementing surveys and experiments with non-standard subject pools, particularly in zones of conflict, are cultural differences, enumerator training, and subject comprehension. One of our largest worries in design was the potential sensitivity of questions involving risk in a predominantly Muslim country. For this reason, we opted only to administer the questions in 12 less conservative provinces of Badakhshan, Balkh, Bamyan, Daikondi, Faryab, Herat, Juzjan, Kabul, Kapisa, Panjshir, Parwan, and Samangan. Additionally, we had our interviewers read a fixed informed consent script, asking individuals if they were willing to answer a few questions about uncertain outcomes.[22] Reflecting sensitivities regarding gambling, of the

[20] The language of experimentation, Dari, reads right to left such that the reading of the task may have differed from standard populations. Enumerators were told to describe each question in turn as a choice between Option A and Option B. Appendix Figure 2.2 shows the original instruments.

[21] Our experiment fit in with a broader survey of Afghan civilians conducted in relation to the Afghan elections.

[22] The script read *"We are interested in understanding how Afghans make decisions involving uncertain outcomes and some normal risks that people face every day. We would like to ask you some hypothetical questions that will help us understand these decisions. There is no real money involved and you will not receive any money for answering these questions. Are you willing to answer*

2,027 respondents contacted only 1,127 respondents consented to participate in the experimental component of the survey in 278 polling centers. Of these 1,127 respondents 977 completed both Task 1 and Task 2.[23]

A second major concern was the use of incentivized methods and safety. We chose to use hypothetical tasks as we viewed it too dangerous for our survey enumerators to carry substantial sums on the street and were particularly worried about the potential for conflicts between respondents receiving different amounts. Though we cannot be sure of any potential bias induced by this choice, it clearly suggests the need for further research with incentivized payments. Importantly, we can compare the hypothetical responses of our subjects with the incentivized responses of Andreoni and Sprenger (2011b) for qualitative differences in behavior.

In order to increase survey quality, 247 enumerators were trained in the experimental methods in a series of 4 training sessions. These sessions provided enumerators with a script for explaining the tasks and a mechanism for visualizing the gambles for subjects.[24] Additionally, the 38 survey supervisors for the project trained for two days with the authors, receiving both translated instruction from the US authors and direct instruction in Dari from the Afghan author. Before deploying to the field, both supervisors and enumerators also carried out group mock elicitations to ensure proper explanation.

The employed methodology appeared to have helped subject comprehension. One potential way of measuring miscomprehension is to measure the proportion of individuals who switch from Option A to Option B more than once in a given price list. Such multiple switching is difficult to rationalize and may indicate subject con-

these questions?"

[23] Attrition from the experiment is discussed in subsection 2.3.3. As in most consented experiments where respondents are allowed to select into the survey, we naturally cannot claim that our sample is representative even of the non-representatively selected neighborhoods where the survey took place.

[24] For example, when describing a gamble, enumerators were told to rip 10 small pieces of paper, number them 1 to 10 and place them in a bag. When discussing say the $(1; 150, 450)$ gamble against the $(q; 450, 0)$ gamble, they would describe it as follows: *First we will ask you a hypothetical question over an amount for certain, or an amount that will be awarded depending on which of ten numbers you draw from a bag. We have deposited 10 cards numbered 1 through 10 into a bag. You have an even chance of drawing any of the 10 numbers. The numbers in parentheses indicate the winning numbers. For each Option No., please indicate whether you would prefer Choice A or Choice B. For each Option No. there will be 10 numbers in the bag and you are only able to draw one. This is not for real money and we are not asking you to make a gamble, we just want to understand how you would respond to naturally occurring risk.*

fusion. About 9.7 percent of our subjects switched more than once in Task 1 and 12.7 percent switched more than once in Task 2, while Holt and Laury (2002) document around 10 percent multiple switching from a standard subject pool. Another way of identifying miscomprehension is identifying individuals with non-monotonic utility functions. That is, individuals for whom $v(X)_U = (q - 0.5)/0.5 < 0$. 12.9 percent of our subjects exhibit such behavior.[25] 87 respondents (8.9 percent of the sample completing the tasks) have both non-monotonic utility functions and exhibit multiple switching on both tasks, consistent with miscomprehension. As these two behaviors both suggest failure to understand the experimental paradigm, our main analysis focuses on the 816 individuals who completed both tasks and did not multiply switch or exhibit non-monotonic utility. The remaining individuals are discussed in the robustness tests in subsection 2.3.3.

2.2.2 Psychological Primes

Psychology has developed a series of methodologies for priming, the objective of which is to cue an emotional state or identification. In a representative survey of 973 U.S. individuals in the aftermath of September 11th conducted by Lerner et al. (2003), one-third were randomly asked to recall and write down the one thing about the event that made them the most fearful or anxious, one-third were asked what made them most angry, and one-third were asked what made them most sad. The evidence suggested that those primed with fear and anxiety in this way both became more fearful in self-reports than the other groups and believed that terrorist attacks were substantially more likely. Given the impressive effects and validation of the fearful recollections of trauma in both Lerner et al. (2003) and Lerner and Keltner (2001), we implemented small modifications of these priming mechanisms.

Just prior to completing the utility elicitation tasks described above 1/3 of subjects at random were asked: *"We are interested in understanding your daily experiences that may make you fearful or anxious. This could be anything, for example getting sick, experiencing violence, losing a job, etc. Could you describe one event in the past year that caused you fear or anxiety?"* (FEAR)

[25] This classification recognizes the interval nature of the uncertainty equivalent data.

Another 1/3 were asked: *"We are interested in understanding your daily experiences that make you happy or joyous. This could be anything, for example birth of child, marriage of a relative, or success in your job. Could you describe an event in the past year that caused you happiness?"* (HAPPY)

And another 1/3 were asked: *"We are interested in understanding your general daily experiences. This could be anything. Could you describe an event from the past year.* (NEUTRAL)

Implementation and Protocol

Primes were given to all individuals who agreed to the consent discussed above asking individuals if they were willing to answer questions on their daily experiences and on risky decisions.

In order to implement the randomized psychological primes several baseline steps were taken to ensure randomness and maximize statistical power. First, following procedures from the field experimental literature (Bruhn and McKenzie, 2009), we stratified our assignment of primes at the polling center level. Hence we are able to provide within-polling center estimates, limiting the set of plausible alternatives for our interacted results to those operating on a small, and precise, geographical scale. Second, we implemented this stratified randomization by setting a random seed for each polling center. That is, the prime of the first survey was randomly set and then a fixed order was followed such that HAPPY followed FEAR, NEUTRAL followed HAPPY, and FEAR followed NEUTRAL. This fixed pattern and random seeding helps to alleviate potential concerns about enumerators selectively altering the order of primes. Additionally, interviews were time stamped with an enumerator self-report. Of course, this does not fully remove threats to the randomization, so we added a standard random monitoring campaign with supervisors present for around 16 percent of all surveys and personally back-checking an additional 11 percent.

Table 2.2 reports summary statistics across the three psychological primes as well as t-tests of means. Note that broad balance is achieved across a variety of demographics including age, income, gender, religion, education, and marriage. Additional variables corresponding to social cohesion, if they think it's important to report insurgent attacks, whether they feel government authority should resolve disputes, are also balanced. One exception is that individuals assigned to the NEUTRAL primes do appear more likely to believe that police should resolve disputes.[26] To increase confidence that the experimental effects on risk preference we observe are the result of the administration of primes and do not reflect pre-existing differences, we additionally asked respondents to indicate their risk tolerance on a 0-10 Likert scale, where 0 is anchored at "unwilling to take risks" and the value 10 means "fully prepared to take risks" at a point in the interview substantially before the administration of primes. Balance is achieved on this measure as well.[27] Importantly, unlike in other correlational analyses where measurements are taken sometime after exposure to trauma, the random primes generate a sample that is balanced on both exposure to violence and the share of respondents who were born in the neighborhood of the survey location. Hence mobility of affected individuals is of less concern than in other environments.

2.2.3 Violence Data

As a country, Afghanistan has suffered 30 years of unabated violence. Following the military coup of the communist People's Democratic Party of Afghanistan in 1978 and the subsequent Soviet invasion in 1979, Afghans have lived through large-scale violence, repression, civil war, and ethnic cleansing.

We focus our analysis on violence from October 2005 to February 2010, a period for which we have precisely geocoded and time-stamped data on violent incidents. Our violence data come from incident records of the International Security Assistance Force (ISAF), a multilateral military body present in Afghanistan since December 2001, obtained through the Combined Information Data Network Exchange (CIDNE).

In addition to geocodes at 5 decimal digit precision (accurate to within one meter at the equator), the CIDNE data provide the time and type of the incident. In effect, the CIDNE data capture all types of violence reported to ISAF, including

[26] Of course, with 30 conducted t-tests, such a result could naturally happen by chance. Consistent with this being a random deviation, the means for the FEAR and HAPPY groups are not significantly different from one another (p-value = 0.498).

[27] We were not able to elicit risk preference at this stage using a full price list, as we had not yet provided individuals who were unwilling to participate for religious reasons the option of discontinuing the survey.

incidents in which ISAF forces were not directly engaged. The CIDNE identify eight types of incidents: direct fire, Improvised Explosive Device (IED) explosions, indirect fire, mine strikes, suicide attacks, IEDs found and cleared, IED hoaxes, and mines found and cleared. A total of 51,098 incidents are identified in the CIDNE data set during our five year window.

We separate the type of incidents into two groups. First, we define a *Successful Attack* as one of: direct fire, IED explosions, indirect fire, mine strikes, and suicide attacks. Second, we define an *Unsuccessful Attack* as one of: IEDs found and cleared, IED hoaxes, and mines found and cleared. The CIDNE data contain 41,842 Successful Attacks and 9,256 Unsuccessful Attacks during our window of observation.

As noted above, our experimental procedures were localized around specific polling centers. Hence, we attach each incident to its nearest polling center with a 1 kilometer halo. That is, if an incident is further than 1 kilometer from any polling center it will not be used in the analysis and if an incident lies within 1 kilometer of two polling centers, it will be attached to the nearer of the two. For our 278 polling centers, we are able to attach a total of 439 incidents, of which 312 were Successful Attacks and the remainder Unsuccessful Attacks.[28]

As our primary measure of exposure to violence we define the indicator *Violence*, which takes the value 1 if there are one or more Successful Attacks attached to the polling center in the window of observation and zero otherwise. 30.31 percent of the polling centers in our sample has Violence = 1.[29]

Unsuccessful Attacks provide the possibility to perform placebo tests. Under the assumption that conditional on intent, the success or failure of an attack is random, then Unsuccessful Attacks can inform researchers as to whether it is the intention of violence or its realization that correlates with experimental behavior. We define the indicator *Failed Violence*, which takes the value 1 if there was one or more Unsuccessful Attacks attached to the polling center in the window of observation and zero otherwise. 23 percent of the polling centers in our sample has Failed Violence =

[28] While our sample has been exposed to considerable trauma, the regions we could safely send interviewers to are among the safer in Afghanistan. This was particularly important given the sensitive nature of the attacks.

[29] In the robustness tests of subsection 2.3.3, we consider alternate definitions of violence changing the halo distance, the observation window, and also consider a continuous measure of the intensity of violence.

1.

The spatial dimension of these violence measures are clearly important for our research design. Given the density of our polling centers, locations that experienced Violence, Failed Violence and neither of each lie geographically close. Figure 2.1 presents a map of Kabul, Afghanistan's capitol, along with the 75 polling centers and their violence classifications. Violence and Failed Violence and neither of each are observed across the city. In the next section, we explore whether this violent exposure, and its recollection, correlate with experimental responses.

2.3 Results

We present results in three sub-sections. We begin by documenting the levels of $v(150)_U$, $v(150)_C$, and Certainty Premium, and explore their relationship with experimental priming manipulations, objective measures of violence and their combinations. Second, we explore alternate explanations for our obtained results. Third, we provide more standard robustness tests related to decision error and definitions of violence.

2.3.1 Main Results

Section 2 provided definitions for non-parametric measures of risk aversion, $v(X)_U$ and $v(X)_C$, and Certainty Premium, $v(X)_C - v(X)_U$. Under our experimental parameters, the defined objects are $v(150)_U$ and $v(150)_C$, and $v(150)_C - v(150)_U$. Note that these values are pinned down by experimental responses in the uncertainty equivalents of Task 1 and 2. Given the interval nature of the experimental data, these values will be interval coded as well. Our analysis accounts for the interval nature of the data by conducting interval regressions (Stewart, 1983), taking as dependent variable the interval of $v(150)_U$, $v(150)_C$, or the Certainty Premium, $v(150)_C - v(150)_U$. Standard errors are clustered at the polling center level.

In Table 2.3, the three dependent variables are organized in columns and three panels are provided corresponding to analysis based on experimental priming results, artefactual violence data, and their combination. In Panel A, Column (1), we present priming results based on interval regressions of $v(150)_U$ on the FEAR prime and a constant. The HAPPY and NEUTRAL primes are grouped together in the constant as no significant differences were obtained. The estimated constant of 0.26 ($s.e.$ = 0.01) in the absence of the FEAR recollections can be compared to the risk neutral benchmark of $X/Y = 150/450 = 1/3$, indicating slight risk tolerance in the absence of the prime. Interestingly, the randomly administered FEAR significantly increases risk tolerance elicited under uncertainty. Individuals asked to recall a fearful episode are significantly more risk tolerant. Consistent with random assignment of the prime, this result is maintained with the addition of the covariates summarized in Table 2.2 characteristics in Column (2).

The risk tolerance elicited under uncertainty is in stark contrast to the preferences elicited under certainty. In Table 2.3, Panel A, Columns (3) and (4), we present results for $v(150)_C$ and note that the estimated constant in Column (3) of 0.62 (0.01) indicates substantial risk aversion relative to the benchmark of 1/3. FEAR is associated with a marginally significant increase in risk tolerance. However, this effect is only one-third the size of the effect of FEAR under uncertainty.

Taken together these data indicate a substantial positive Certainty Premium. In Column (5), we document an average Certainty Premium of 0.37 (0.01), indicating that 150 Afghanis received with certainty are worth an additional 37 percent chance of receiving 450 Afghanis relative to their value under uncertainty.[30] This positive Certainty Premium is at odds with both EU and CPT, but consistent with models such as DA and u-v preferences that feature a specific preference for certainty. This result that individuals prefer certainty when primed with fear resonates closely with the risk aversion results obtained under different metrics in the psychology literature.

Table 2.3, Panel B effectively repeats the above analysis with the key independent variable being the presence of violent activity at the polling center level, Violence (= 1). Virtually identical results obtain albeit with less clear signficance. Similar to recollection of fearful episodes, exposure to violence is linked to increased risk tolerance under uncertainty, a substantially lower change in risk preference under

[30] Andreoni and Sprenger (2011b) also document a sizable positive Certainty Premium were certainty of 10 US Dollars is found to be worth an additional 15 - 20 percent chance of winning 30 dollars relative to its uncertain value.

certainty, and a significant increase in Certainty Premium. Our results indicate that exposed individuals are more risk tolerant under uncertainty, but hold on to certainty when it is available.[31]

Panels A and B together demonstrate that the employed two-task uncertainty equivalent potentially allows us to resolve the above-discussed differences between psychological and experimental economic findings. Consistent results are obtained for both artefactual and priming analyses. We find both increased risk tolerance under uncertainty for exposure to violence, as in economics, and an increased preference for certainty under priming, similar to psychology. As prior work in economics and psychology relied on single task procedures and differing measurement instruments, without being able to test for non-EU certainty effects, such consistent results could not appear.

In Panel C of Table 2.3, we combine our data sources, examining the interacted effect of being exposed to violence and being asked to recall the fear-inducing episodes. Panel C indicates that FEAR without Violence and Violence without FEAR, have limited effect on elicited risk preferences and Certainty Premium. Individuals exposed to violence who are asked to recall these episodes drive the observed effects. It is they who are more risk tolerant under uncertainty and they who have significantly higher Certainty Premia. Violence exposed individuals, primed to recall fearful episodes have Certainty Premia 6 percentage points larger than unexposed, unprimed individuals indicating a 16% greater willingness to pay for certainty.

These results have an interesting interpretaion. The effect of violence on risk preferences may be mediated through recall. If these recollections can be manipulated, then they can be triggered by external cues such as marketing or policy. Based on a benchmark for rationality of the Expected Utility model, this indicates that

[31]It is important to attempt to resolve our artefactual results with those obtained in prior exercises. Columns (1) and (2) of Panel B echo prior results of increased risk tolerance correlating with trauma, but Columns (3) through (6) do not. In Voors et al. (Forthcoming) subjects make a series of choices between a fixed gamble and changing certain amounts. A body of evidence in decision science, behavioral theory, and experimental economics shows that this induces less risk aversion than if the certain amount is fixed and gambles are varied, as individuals may potentially feel endowed with risk (Hershey et al., 1982; Hershey and Schoemaker, 1985; Koszegi and Rabin, 2007; Sprenger, 2010). This may explain the risk tolerance in the Voors et al. (Forthcoming) data, exacerbated by exposure. Eckel et al. (2009b) have individuals choose between 6 binary gambles, of which one is certain. As choosing the certain option requires substantial risk aversion, subjects may view this as a choice between six risky gambles, similar to our results under uncertainty.

irrationality can be exacerbated with recollection triggers. These triggers, if exploited, could potentially lead to detrimental decision-making. In effect, the observed behavior is equivalent to individuals being willing to pay incrementally more for certainty and so has analogs in financial behaviors such as insurance purchasing and portfolio choice.

Because we have interacted our experimental condition assignment with a non-random measure of violence, we cannot interpret the coefficients of Panel C as causal effects. Important to the interpretation that the effect of violence on preference is mediated through recall and so can be triggered by external cues, is the possibility that violent incidents are correlated with some other characteristic of the polling center, such as migration flows or willingness to trust military personnel who can pre-empt an attack by providing a tip, which is actually driving the result. If this is true, it is still the case that the effect of priming on preference is larger for violence-exposed individuals, *but exposure is not the reason why it is larger.* We provide a battery of tests exploring these possibilities in the next sub-section.

2.3.2 Testing Alternative Explanations

This subsection reports a further set of tests of whether the effect of violence on risk preferences is mediated through recall. First, we exploit the stratification of experimental primes at the polling center level to test the sensitivity of the results to estimation using only within-polling center variation. Any omitted correlate of violence causing individuals in violent neighborhoods to exhibit more sensitivity to FEAR, if it is not perfectly spatially correlated with patterns of violence, should cause our estimates to be sensitive to the inclusion of polling center fixed effects.[32] Second, we exploit a feature of our data which allows us to identify attacks which were planned but never succeeded in order to perform placebo tests. The testable prediction is that if the omitted correlate of violence which is driving vulnerability to primes is also correlated with the intention to attack, then we should find differential vulnerability to primes dividing the sample according to this variable as well. Last, we directly

[32]As Violence is measured at the polling center level, we cannot repeat the analysis in Panel B of Table 2.3 as the fixed effects are perfectly collinear with Violence. Additionally, when estimating the relation between Violence x FEAR on Certainty Preference, Violence would be collinear with the polling center fixed effects, and so is removed from regression.

examine a set of plausible omitted correlates, such as migration, willingness to report attacks to military forces, and willingness to use the Afghan government to adjudicate disputes. These results add further support to the findings and interpretation above.

Within-Polling Center Variation

As a first consistency test we present Table 2.4, which corresponds to Panel A and Panel C of Table 2.3. All regressions in Table 2.3 were estimated using within-province variation. However, as described in subsection 2.2.2, we stratified the assignment of primes at the polling center level, which permits estimation of experimental effects using only within-polling center variation. Table 2.4 repeats specifications from Panel C of Table 2.3 adding polling center fixed effects.[33] If potential confounds that prevent interpreting the interaction terms as causal are not perfectly spatially correlated with violent incidents, then they should be sensitive to estimation using variation at a finer degree of spatial granularity. Consistent with spatial stratification permitting the detection of smaller effects, the results grow more significant in Table 2.4.[34]

Placebo Tests of Failed Violence

Our data allows us to identify attacks which were planned but never succeeded in order to perform placebo tests. If unobserved correlates of violence are also correlated with the intention to attack, then we should find differential vulnerability to primes dividing the sample according to this variable as well.

Table 2.5 reports placebo tests regressing $v(150)_C - v(150)_U$ on the full set of interactions for Violence and Failed Violence, separated by FEAR.[35] Consistent with violence causing decision-making to be vulnerable to fearful recollections, it is Violence alone and not Failed Violence or the interaction that is correlated with Certainty Premium in the FEAR condition. Not only does this suggest that it is the

[33] As Bruhn and McKenzie (2009) show, this approach is likely to provide a high powered test of the effect of the prime. Last, because the estimation of effect relies on comparisons between individuals living in the same neighborhood, it is less likely that the result is due to chance.

[34] Additionally, in Column (2) of Table 2.4, we find the coefficient on assignment to the FEAR gains marginal significance, which suggests that the prime has some small effect even outside of violent polling centers which is not detectable in the less powerful specifications in Table 2.3.

[35] Of course, data limitations may restrict detecting effect due to a limited sample size.

realization of traumatic episodes that provide the basis for fearful recollection, but also it narrows the set of potential confounds to correlates of realized violent activity that are not highly correlated with failed attacks.

Measures of Social Cohesion and Selective Migration

A set of possible alternative explanations for our observed effects present themselves. It may be that successful attacks drive migration while planned attacks do not, and that individuals whose preferences are responsive to recall do not migrate. It may be that individuals who are susceptible to primes are less likely to report attacks, and so disproportionately live in neighborhoods where attacks succeed. It may be that individuals who are susceptible to primes disproportionately live in areas that are not under government control, and it is in these areas that attacks are more common.

All three potential confounds share the common feature that individuals selectively migrate or settle depending on violence, so the insensitivity of the results to the inclusion of polling center fixed effects reported in Table 2.4 already provides some evidence against their relevance. Importantly, our data provide additional opportunity to test these hypotheses as we are able to measure social cohesion, the importance of reporting attacks, and migration.

In Table 2.6, we provide tests of these competing accounts. In regressions of Certainty Premium, we sequentially interact FEAR both with Violence and candidate omitted variables including whether individuals feel that reporting a terror attack is important, whether they use the formal police or court system to resolve disputes and whether they were born in the neighborhood of the survey. The coefficient on the interaction between Violence and FEAR remains strongly significant and the magnitude is virtually unchanged, suggesting that the effect of FEAR is different according to exposure to violence and not according to these confounds.

To summarize, the tests to here remain consistent with the conclusion that violent exposure creates a triggerability in decision-making of traumatic recollections. The results are retained when relying on within-polling variation, find support in placebo tests employing Failed Violence, and are robust to potential alternative stories relying on selective migration and settlement. In the following section, we address the issues of decision error, systematic attrition from decision tasks resulting from the prime, and show our results are robust to different definitions of Violence.

2.3.3 Robustness Tests

This section addresses three additional concerns for the main results reported in Table 2.3. First, the recollection of violent trauma may be mentally taxing and so cause respondents to respond erratically to the price lists. Second, it may be that, because the FEAR prime raises a sensitive and uncomfortable issue, more risk averse respondents select to discontinue participation, driving the result. Last, it may be that results hinge on this particular definition of Violence. We test each of these concerns in turn.

Decision Error

Recalling a violent episode may be cognitively difficult and so drive respondents to systematically misunderstand the choice tasks used to elicit risk and Certainty Premium. This problem may be particularly salient in Afghanistan, where literacy, especially for women, is among the lowest in the world.[36] Above, we show that our respondents switch multiple times on a price list about as often as in Holt and Laury (2002), which provides basic indication that our respondents understand the task. We additionally discuss that responses with $v(X)_U < 0$ (i.e. non-monotonic utility) as indicating miscomprehension. Columns 1 - 4 of Table 2.7 report results from regressions of a dummy variable equal to 1 for respondents switching multiple times in either choice Task 1 or 2 on FEAR and FEAR interacted with Violence. Columns 5 - 8 repeat Columns 1 - 4 replacing the left hand side of the regression with a dummy variable equal to 1 for respondents exhibiting non-monotonic utility. We find no significant differences in decision error according to treatment status in any specification. With only about 10 percent of respondents exhibiting such errors, these tests may be underpowered.

[36] According to year 2000 estimates the CIA World Factbook, 43.1 percent of males, 12.6 percent of females, and 28.1 percent of the total population over 15 can read and write. An earlier survey in our sample of polling centers indicate that 30.3 percent of male respondents and 23.21 of female respondents are literate.

Attrition

Of the 1,127 respondents who consented to questioning about risk, 144 (12.7 percent) did not complete the first task in the protocol (Task 2) and an additional 6 respondents failed to complete Task 1. This poses a problem for our result if the attrition is systematically linked to the prime since we would be measuring different parts of the primed and unprimed sample. Table 2.8 demonstrates that attrition is not systematically linked to priming by regressing an indicator for attrition on FEAR, Violence, and their interaction.

Definition of Violence

So far, we have measured violence as whether or not a military record of an attack exists over the period November 2005 to February 2010. To investigate the importance of violence intensity, we construct a new measure as the number of local attacks divided by 36, which is the maximum number of attacks in any polling center in our sample. Table 2.9, Panel A repeats the estimations of Table 2.3 replacing the Violence dummy with our violence intensity measure. As in Table 2.3, for more severely exposed individuals fearful recollections increase Certainty Premia.[37]

Table 2.9, Panel B separates violence by time period, November 2005 to December 2007, and January 2008 to February 2010. Separating the data in this way provides evidence that the effect is coming primarily from recent violent exposure as opposed to older violence.

A final concern may be that our results depend on our decision to code Violence based on events within 1 kilometer of the nearest polling center. To check this concern, Table 2.10 redefines violence as events occuring within 3 kilometers of a polling center. The results remain robust. In unreported results, we find that the correlations break down once we expand the halo radius to 5 kilometers, a distance that could introduce substantial measurement error.

[37] Consistent with the weaker results in Table 2.4, the effect of the prime on individuals in unaffected areas now also achieves significance. However, the effect, measured in units of the probability of receiving 450 Afghanis, for individuals in the most violent precinct is an order of magnitude larger.

2.4 Discussion and Conclusion

Trauma has complex and strongly enduring mental, physical, and social consequences, especially among populations such as military veterans with direct exposure to violence. Clinicians have made remarkable progress on diagnostics, treatment, and analyzing comorbidity of trauma-related disorders. Findings suggest that trauma-exposed individuals can suffer severe economic consequences linked to psychiatric, psychosocial, and occupational impairments (Brunello et al., 2001). Understanding the mechanisms linking trauma to economic outcomes may improve policy interventions, allow for the development of economic diagnostics, and additionally provide deeper insights into individual decision-making.

We present data from a novel three-part study with 1,127 Afghan civilians combining artefactual and experimental techniques from both economics and psychology. First, we implement a novel two-task risk procedure, eliciting utility under both uncertainty and certainty. The procedure can identify risk preferences without functional form assumptions for utility; test competing decision theories including Expected Utility (EU), Cumulative Prospect Theory (CPT), Disappointment Aversion (DA) and u-v preferences; and provide a measure of the specific preference for certainty inherent to non-EU models of decision-making. Second, we borrow priming methods established in psychology, randomly asking 1/3 of subjects to recall a fear or anxiety-inducing episode. Third, we measure exposure to violence in Afghanistan directly by linking experimental data to records of violent incidents recorded by the International Security Assistance Force (ISAF).

The combination of methods provides a series of interesting results. To begin, we document substantial differences between utility elicited under uncertainty and utility elicited under certainty. Individuals are systematically more risk averse under certainty, in contradiction to both EU and CPT, indicating a specific preference for certainty as in DA and u-v preferences. This departure from EU compares with results obtained from a markedly different experimental sample—American college students. Interestingly, the preference for certainty, which we term a Certainty Premium, is exacerbated by violent exposure and by recollection of fearful events. The nature of the results suggest that when everything is uncertain, exposure to violence and fearful recollections increase risk tolerance, but that when certainty is available it remains attractive, increasing measured Certainty Premia. When exploring the interaction, we find that violence without fear and fear without violence have limited effect on measured risk preferences. Taken together the results indicate that fearful recollections trigger changes in risk and certainty preferences for those exposed to violent trauma.

We note three implications of our results. First, an apparent inconsistency exists across prior psychological and economic techniques in obtained findings. Though no stylized fact has emerged, in artefactual field studies in economics, exposure to trauma appears to increase risk tolerance. In priming studies in psychology, however, fearful recollections of traumatic episodes appears to be linked to less risk tolerance. Though the questions asked differ, the dual finding that living through trauma increases risk tolerance, while recalling it decreases risk tolerance presents a potential inconsistency. Importantly, our findings may help to rationalize this inconsistency. In both our artefactual and priming analysis we find that exposure and fearful recollection increase risk tolerance under uncertainty and increase measured Certainty Premia. As prior work relies on single task procedures and uses differing measurement instruments, such consistent findings could not appear.

Second, a further inconsistency across psychology and economics lies in the permanence of observed effects. Artefactual field studies carry with them the implication that individual risk preferences are potentially *permanently* altered, while psychology priming studies suggest that risk preferences may be changed only temporarily. Our work demonstrates the importance of both recency of violence and of recollections. Individuals with violent exposure may be changed for a substantial period of time (though potentially not forever), but what changes is not necessarily their risk preferences, but rather the susceptibility of their preferences to fearful recollections.

Third, if fearful recollections of traumatic epsiodes trigger a specific profile of risk preferences, then our results point to the actionability of recall mechanisms. Marketers, policy makers, and others interacting with trauma-affected individuals may then be able to trigger irrational (or rational) behavior in trauma-affected individuals. As our observed behavior is equivalent to individuals being willing to pay incrementally more for certainty when triggered, one can imagine close analogs in financial decision-making such as insurance purchasing and portfolio choice. Trauma-affected individuals are known to have broadly worse economic outcomes, and so future research should explore both triggering mechanisms that may generate such phenomena and policy interventions that may reduce negative outcomes.

Table 2.1: Multiple Price Lists

Task 1		
$q' \in$	Option A	Option B
$[0, 0.1]$	10% chance of 450 Afs, 90% chance of 0 Afs	50% chance of 450 Afs, 50% chance of 150 Afs
$[0.1, 0.2]$	20% chance of 450 Afs, 80% chance of 0 Afs	50% chance of 450 Afs, 50% chance of 150 Afs
$[0.2, 0.3]$	30% chance of 450 Afs, 70% chance of 0 Afs	50% chance of 450 Afs, 50% chance of 150 Afs
$[0.3, 0.4]$	40% chance of 450 Afs, 60% chance of 0 Afs	50% chance of 450 Afs, 50% chance of 150 Afs
$[0.4, 0.5]$	50% chance of 450 Afs, 50% chance of 0 Afs	50% chance of 450 Afs, 50% chance of 150 Afs
$[0.5, 0.6]$	60% chance of 450 Afs, 40% chance of 0 Afs	50% chance of 450 Afs, 50% chance of 150 Afs
$[0.6, 0.7]$	70% chance of 450 Afs, 30% chance of 0 Afs	50% chance of 450 Afs, 50% chance of 150 Afs
$[0.7, 0.8]$	80% chance of 450 Afs, 20% chance of 0 Afs	50% chance of 450 Afs, 50% chance of 150 Afs
$[0.8, 0.9]$	90% chance of 450 Afs, 10% chance of 0 Afs	50% chance of 450 Afs, 50% chance of 150 Afs
$[0.9, 1]$	100% chance of 450 Afs, 0% chance of 0 Afs	50% chance of 450 Afs, 50% chance of 150 Afs
Task 2		
$q \in$	Option A	Option B
$[0, 0.1]$	10% chance of 450 Afs, 90% chance of 0 Afs	150 Afghanis
$[0.1, 0.2]$	20% chance of 450 Afs, 80% chance of 0 Afs	150 Afghanis
$[0.2, 0.3]$	30% chance of 450 Afs, 70% chance of 0 Afs	150 Afghanis
$[0.3, 0.4]$	40% chance of 450 Afs, 60% chance of 0 Afs	150 Afghanis
$[0.4, 0.5]$	50% chance of 450 Afs, 50% chance of 0 Afs	150 Afghanis
$[0.5, 0.6]$	60% chance of 450 Afs, 40% chance of 0 Afs	150 Afghanis
$[0.6, 0.7]$	70% chance of 450 Afs, 30% chance of 0 Afs	150 Afghanis
$[0.7, 0.8]$	80% chance of 450 Afs, 20% chance of 0 Afs	150 Afghanis
$[0.8, 0.9]$	90% chance of 450 Afs, 10% chance of 0 Afs	150 Afghanis
$[0.9, 1]$	100% chance of 450 Afs, 0% chance of 0 Afs	150 Afghanis

Notes:

Table 2.2: Summary Statistics

	Neutral Prime	Anxiety Prime	Happiness Prime	t-test of: (P-value)	
	(1)	(2)	(3)	(2) - (1)	(3) - (1)
Socio-demographics					
Age	29.520	29.592	28.926	0.937	0.495
	(0.648)	(0.648)	(0.576)		
Income (1,000 AFs)	12.994	12.303	12.395	0.409	0.445
	(0.536)	(0.648)	(0.573)		
Female (=1)	0.370	0.438	0.407	0.108	0.370
	(0.029)	(0.031)	(0.030)		
Shia (=1)	0.157	0.140	0.130	0.578	0.368
	(0.022)	(0.021)	(0.020)		
Education (Years)	9.719	9.796	10.004	0.834	0.442
	(0.259)	(0.261)	(0.264)		
Married (=1)	0.626	0.619	0.593	0.858	0.418
	(0.029)	(0.030)	(0.030)		
Social Cohesion					
Reporting Insurgent Activity Important	0.496	0.525	0.474	0.521	0.637
	(0.032)	(0.032)	(0.033)		
Police Resolve Disputes	0.249	0.189	0.189	0.089	0.088
	(0.026)	(0.024)	(0.024)		
Courts Resolve Disputes	0.135	0.174	0.178	0.215	0.170
	(0.020)	(0.023)	(0.023)		
Violence and Mobility					
Violence (=1)	0.345	0.358	0.381	0.746	0.377
	(0.028)	(0.030)	(0.030)		
Failed Violence (=1)	0.292	0.253	0.241	0.308	0.176
	(0.027)	(0.027)	(0.026)		
Respondent Born Locally	0.786	0.781	0.800	0.880	0.696
	(0.024)	(0.025)	(0.024)		
Baseline Risk					
Baseline Risk (0-10)	2.246	2.015	2.296	0.263	0.810
	(0.142)	(0.149)	(0.158)		
# Observations	281	265	270		

Notes: Standard errors reported in parentheses.

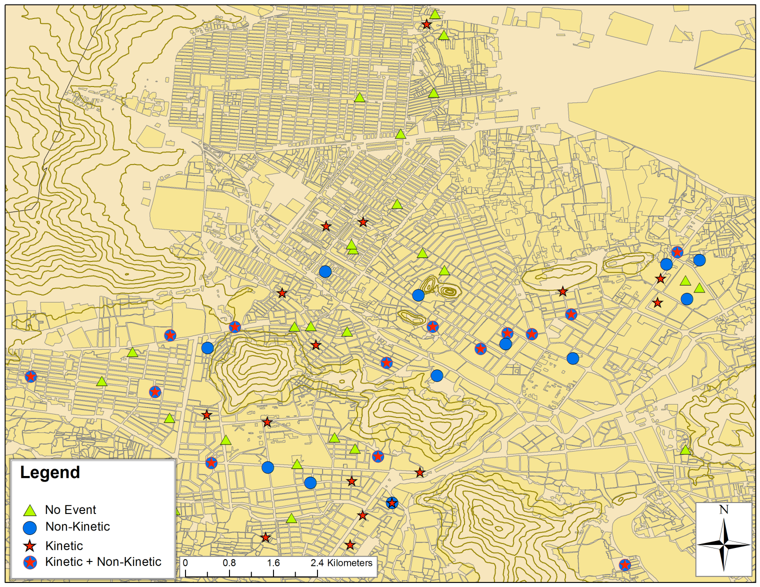

Figure 2.1: Successful Attacks and Failed Attacks in Kabul

2.5 Robustness Questions

1. How willing are you to take risks regarding your households finances? Please tick a box on the scale, where the value 0 means: "unwilling to take risks" and the value 10 means: "fully prepared to take risks."

2. In your opinion, how important is it for you to share information about insurgents to ISAF (for example, pending IED attacks or the location of weapons caches): is it very important, somewhat important, or not at all important? 1.Very important 2. Somewhat important 3. Not at all important 98.Dont know 99.RTA

3. If you had a dispute with a neighbor, who would you trust to settle it (randomize ordering): head of family, police, courts, religious leaders, shura, elders, ISAF, or other?

   1. Head of family
   2. Police
   3. Courts
   4. Religious leaders
   5. Shura
   6. Elders

Figure 2.2: Dari Translations of Decision Tasks 1 and 2

Panel A : Task 1

نمبر سوالات	بخش 1	بخش 2	انتخاب شما (1 یا2)
1	10% چانس بردن 450 افغانی (شماره 10)، 90% چانس بردن 0 افغانی (شماره های 1 الی 9)	50% چانس بردن 450 افغانی (شماره های 6 الی 10)، 50% چانس بردن 150 افغانی(شماره های 1 الی 5)	(WWQ7_1)_____
2	20% چانس بردن 450 افغانی (شماره های 9 الی 10)، 80% چانس بردن 0 افغانی (شماره های 1 الی 8)	50% چانس بردن 450 افغانی (شماره های 6 الی 10)، 50% چانس بردن 150 افغانی (شماره های 1 الی 5)	
3	30% چانس بردن 450 افغانی (شماره های 8 الی 10)، 70% چانس بردن 0 افغانی (شماره های 1 الی 7)	50% چانس بردن 450 افغانی (شماره های 6 الی 10)، 50% چانس بردن 150 افغانی (شماره های 1 الی 5)	
4	40% چانس بردن 450 افغانی (شماره های 7 الی 10)، 60% چانس بردن 0 افغانی (شماره های 1 الی 6)	50% چانس بردن 450 افغانی (شماره های 6 الی 10)، 50% چانس بردن 150 افغانی (شماره های 1 الی 5)	
5	50% چانس بردن 450 افغانی (شماره های 6 الی 10)، 50% چانس بردن 0 افغانی (شماره های 1 الی 5)	50% چانس بردن 450 افغانی (شماره های 6 الی 10)، 50% چانس بردن 150 افغانی (شماره های 1 الی 5)	
6	60% چانس بردن 450 افغانی (شماره های 5 الی 10)، 40% چانس بردن 0 افغانی (شماره های 1 الی 4)	50% چانس بردن 450 افغانی (شماره های 6 الی 10)، 50% چانس بردن 150 افغانی (شماره های 1 الی 5)	
7	70% چانس بردن 450 افغانی (شماره های 4 الی 10)، 30% چانس بردن 0 افغانی (شماره های 1 الی 3)	50% چانس بردن 450 افغانی (شماره های 6 الی 10)، 50% چانس بردن 150 افغانی (شماره های 1 الی 5)	
8	80% چانس بردن 450 افغانی (شماره های 3 الی 10)، 20% چانس بردن 0 افغانی (شماره های 1 الی 2)	50% چانس بردن 450 افغانی (شماره های 6 الی 10)، 50% چانس بردن 150 افغانی (شماره های 1 الی 5)	
9	90% چانس بردن 450 افغانی (شماره های 2 الی 10)، 10% چانس بردن 0 افغانی (شماره 1)	50% چانس بردن 450 افغانی (شماره های 6 الی 10)، 50% چانس بردن 150 افغانی (شماره های 1 الی 5)	
10	100% چانس بردن 450 افغانی (شماره های 1 الی 10)، 0% چانس بردن 0 افغانی	50% چانس بردن 450 افغانی (شماره های 6 الی 10)، 50% چانس بردن 150 افغانی (شماره های 1 الی 5)	(WWQ7_10)_____

Panel B : Task 2

گزینه شماره	انتخاب 1	انتخاب 2	انتخاب شما (1 یا2)
1	10% چانس بردن 450 افغانی (شماره 10)، 90% چانس بردن 0 افغانی (شماره های 1 الی 9)	150 افغانی	(WWQ6_1)________
2	20% چانس بردن 450 افغانی (شماره های 9 و10)، 80% چانس بردن 0 افغانی (شماره های 1 الی 8)	150 افغانی	(WWQ6_2)________
3	30% چانس بردن 450 افغانی (شماره های 8 الی 10)، 70% چانس بردن 0 افغانی (شماره های 1 الی 7)	150 افغانی	(WWQ6_3)________
4	40% چانس بردن 450 افغانی (شماره های 7 الی 10)، 60% چانس بردن 0 افغانی (شماره های 1 الی 6)	150 افغانی	(WWQ6_4)________
5	50% چانس بردن 450 افغانی (شماره های 6 الی 10)، 50% چانس بردن 0 افغانی (شماره های 1 الی 5)	150 افغانی	(WWQ6_5)________
6	60% چانس بردن 450 افغانی (شماره های 5 الی 10)، 40% چانس بردن 0 افغانی (شماره های 1 الی 4)	150 افغانی	(WWQ6_6)________
7	70% چانس بردن 450 افغانی (شماره های 4 الی 10)، 30% چانس بردن 0 افغانی (شماره های 1 الی 3)	150 افغانی	(WWQ6_7)________
8	80% چانس بردن 450 افغانی (شماره های 3 الی 10)، 20% چانس بردن 0 افغانی (شماره 1 الی 2)	150 افغانی	(WWQ6_8)________
9	90% چانس بردن 450 افغانی (شماره های 2 الی 10)، 10% چانس بردن 0 افغانی (شماره 1)	150 افغانی	(WWQ6_9)________
10	100% چانس بردن 450 افغانی (شماره های 1 الی 10)، 0% چانس بردن 0 افغانی	150 افغانی	(WWQ6_10)________

Table 2.3: Attacks, Primes, and Elicited Utility

Panel A - Priming Results						
Dependent Variable:	$v(150)_u$		$v(150)_c$		$v(150)_c - v(150)_u$	
	(1)	(2)	(3)	(4)	(5)	(6)
FEAR (=1)	-0.052***	-0.068***	-0.018*	-0.024**	0.034***	0.043***
	(0.018)	(0.018)	(0.009)	(0.009)	(0.011)	(0.011)
Constant	0.256***	0.071	0.622***	0.517***	0.367***	0.442***
	(0.011)	(0.050)	(0.005)	(0.026)	(0.009)	(0.032)
Covariates	No	Yes	No	Yes	No	Yes
# Observations	816	718	816	718	816	718
# Clusters	278	267	278	267	278	267
Log-Likelihood	-1285.164	-1105.430	-1300.302	-1123.941	-572.954	-467.789
Panel B - Artefactual Violence Results						
Violence (=1)	-0.031**	-0.028*	-0.003	-0.004	0.028**	0.023**
	(0.014)	(0.016)	(0.007)	(0.008)	(0.011)	(0.011)
Constant	0.254***	0.062	0.619***	0.512***	0.364***	0.446***
	(0.012)	(0.051)	(0.006)	(0.027)	(0.009)	(0.032)
Covariates	No	Yes	No	Yes	No	Yes
# Observations	816	718	816	718	816	718
# Clusters	278	267	278	267	278	267
Log-Likelihood	-1288.140	-1111.353	-1302.161	-1127.088	-574.690	-473.178
Panel C - Exposure to Violence and Prime Sensitivity						
FEAR (=1)	-0.021	-0.035	-0.008	-0.013	0.012	0.020
	(0.023)	(0.023)	(0.011)	(0.011)	(0.014)	(0.014)
Violence (=1)	-0.003	0.001	0.005	0.005	0.009	0.004
	(0.018)	(0.020)	(0.009)	(0.010)	(0.013)	(0.014)
FEAR x Violence	-0.086**	-0.092***	-0.026	-0.028	0.059***	0.063***
	(0.034)	(0.035)	(0.019)	(0.019)	(0.022)	(0.022)
Constant	0.261***	0.072	0.621***	0.516***	0.360***	0.440***
	(0.013)	(0.050)	(0.006)	(0.026)	(0.011)	(0.033)
Covariates	No	Yes	No	Yes	No	Yes
# Observations	816	718	816	718	816	718
# Clusters	278	267	278	267	278	267
Log-Likelihood	-1280.994	-1101.211	-1299.279	-1122.732	-566.863	-461.940

Notes: Robust standard errors clustered at the Polling Center level reported in parentheses. All regressions include province fixed effects. Violence data are from ISAF CIDNE. Violence is defined as a violent event occurring within one kilometer of interview location over the period October 2005 - February 2010. Sample: 816 individuals with positive $v(150)$ and no multiple switching. $v(150)_C$ refers to elicited utility under certainty while $v(150)_U$ refers to elicited utility under uncertainty. The differences $v(150)_C - v(150)_U$ is the measured Certainty Premium. Violent incident data are from ISAF CIDNE. The covariates are pre-prime risk (0 - 10), female (=1), shia (=1), years of education, born locally (=1), reporting insurgent activity important (=1), prefer police resolve disputes (=1), prefer courts resolve disputes (=1), married (=1), age, and log(income).

7. ISAF
8. Other (record verbatim)
98. Dont know

Table 2.4: Attacks and Elicited Utility - Including PC Fixed Effects

Panel A - Priming Results						
Dependent Variable:	$v(150)_u$		$v(150)_c$		$v(150)_c - v(150)_u$	
	(1)	(2)	(3)	(4)	(5)	(6)
FEAR (=1)	-0.059***	-0.077***	-0.020**	-0.030***	0.037***	0.047***
	(0.018)	(0.018)	(0.009)	(0.009)	(0.011)	(0.010)
Constant	0.320***	0.029	0.624***	0.507***	0.305***	0.463***
	(0.084)	(0.111)	(0.043)	(0.057)	(0.051)	(0.066)
Covariates	No	Yes	No	Yes	No	Yes
# Observations	816	718	816	718	816	718
Log-Likelihood	-1173.515	-985.089	-1193.460	-1005.554	-423.597	-317.696
Panel B - Exposure to Violence and Prime Sensitivity						
FEAR (=1)	-0.025	-0.038*	-0.010	-0.018	0.017	0.023*
	(0.022)	(0.023)	(0.011)	(0.012)	(0.013)	(0.013)
FEAR x Violence	-0.086**	-0.100***	-0.027	-0.030	0.052**	0.062***
	(0.036)	(0.037)	(0.018)	(0.019)	(0.022)	(0.022)
Constant	0.337***	0.058	0.629***	0.516***	0.296***	0.449***
	(0.084)	(0.111)	(0.043)	(0.057)	(0.052)	(0.067)
Covariates	No	Yes	No	Yes	No	Yes
# Observations	816	718	816	718	816	718
Log-Likelihood	-1170.615	-981.391	-1192.395	-1004.298	-420.661	-313.555

Notes: Robust standard errors clustered at the Polling Center level reported in parentheses. All regressions include Polling Center fixed effects. There are 278 polling centers in our sample. We do not include Violence in Panel B as it is measured at the polling center level and so is perfectly collinear with the polling center fixed effects. Violence data are from ISAF CIDNE. Violence is defined as a violent event occurring within one kilometer of interview location over the period October 2005 - February 2010. Sample: 816 individuals with positive $v(150)$ and no multiple switching. $v(150)_C$ refers to elicited utility under certainty while $v(150)_U$ refers to elicited utility under uncertainty. The differences $v(150)_C - v(150)_U$ is the measured Certainty Premium. Violent incident data are from ISAF CIDNE. The covariates are pre-prime risk (0 - 10), female (=1), shia (=1), years of education, born locally (=1), reporting insurgent activity important (=1), prefer police resolve disputes (=1), prefer courts resolve disputes (=1), married (=1), age, and log(income).

99. RTA

4. Were you born in this village, or did you move here from somewhere else?

   1. Born here
   2. Moved
   98. Dont know
   99. RTA

Table 2.5: Placebo Tests Using Planned But Unsuccessful Attacks

Dependent Variable:	Certainty Premium: $v(150)_C - v(150)_U$			
	Fear Prime (=1)		Fear Prime (=0)	
	(1)	(2)	(3)	(4)
Violence (=1)	0.065***	0.072***	0.020	0.017
	(0.019)	(0.019)	(0.016)	(0.017)
Failed Violence (=1)	0.002	0.007	-0.022	-0.011
	(0.029)	(0.030)	(0.028)	(0.024)
Violence x Failed Violence	0.022	0.002	-0.018	-0.032
	(0.041)	(0.044)	(0.033)	(0.030)
Female (=1)		-0.005		0.007
		(0.021)		(0.014)
Age		-0.002***		-0.001
		(0.001)		(0.001)
Shia (=1)		-0.051*		-0.018
		(0.027)		(0.021)
Education (Years)		-0.001		-0.004**
		(0.002)		(0.002)
Ln(Income + 1)		0.003		0.001
		(0.003)		(0.002)
Baseline Risk (0 - 10)		-0.007*		-0.001
		(0.004)		(0.003)
Respondent Born Locally		0.011		-0.026
		(0.022)		(0.018)
Reporting Insurgent Activity Important		-0.009		-0.006
		(0.017)		(0.013)
Police Solve Disputes		0.015		0.013
		(0.026)		(0.016)
Courts Solve Disputes		-0.019		0.006
		(0.028)		(0.019)
Constant	0.374***	0.441***	0.365***	0.441***
	(0.018)	(0.053)	(0.014)	(0.042)
# Observations	265	238	551	480
# Clusters	196	181	258	239
Log-Likelihood	-168.699	-139.228	-389.977	-313.641

Notes: Standard errors clustered at the polling center level reported in parentheses. All regressions include province fixed effecs. The differences $u(150)_C - u(150)_U$ is the measured certainty premium. Violent incident data are from ISAF CIDNE.

Table 2.6: Robustness - Does Violence Affect Vulnerability to Primes?

Dependent Variable:	$v(150)_c - v(150)_u$							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fear Prime (=1)	0.021	0.023	0.016	0.017	0.014	0.016	-0.014	-0.013
	(0.018)	(0.018)	(0.015)	(0.015)	(0.015)	(0.015)	(0.027)	(0.027)
Violence (=1)	0.003	0.004	0.009	0.009	0.009	0.008	0.009	0.008
	(0.013)	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Fear x Violence	0.065***	0.064***	0.059***	0.056**	0.060***	0.057***	0.061***	0.058***
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
Report. Ins. Act. Imp.	-0.008	-0.009						
	(0.013)	(0.013)						
Police Solve Disputes (=1)			0.014	0.014				
			(0.015)	(0.015)				
Court Solve Disputes (=1)					0.003	0.008		
					(0.018)	(0.018)		
Born Locally (=1)							-0.026	-0.022
							(0.018)	(0.017)
Fear x Report	-0.003	-0.005						
	(0.020)	(0.020)						
Fear x Police			-0.020	-0.013				
			(0.029)	(0.028)				
Fear x Court					-0.011	-0.014		
					(0.031)	(0.031)		
Fear x Local							0.032	0.033
							(0.028)	(0.027)
Constant	0.360***	0.435***	0.357***	0.415***	0.360***	0.420***	0.379***	0.434***
	(0.012)	(0.030)	(0.012)	(0.032)	(0.011)	(0.031)	(0.018)	(0.034)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Fixed Effects	Prov.	Prov.	Prov.	Prov.	Prov.	Prov.	Prov.	Prov.
# Observations	718	718	816	816	816	816	816	816
# Clusters	267	267	278	278	278	278	278	278
Log-Likelihood	-472.955	-462.536	-566.404	-557.038	-566.791	-557.313	-565.422	-556.282

Notes: Robust standard errors clustered at the polling center level reported in parentheses. Violence is defined as a violent event occurring within one kilometer of interview location over the period October 2005 - February 2010. Sample: 816 individuals with positive $v(150)$ and no multiple switching. $v(150)_C - v(150)_U$ is the measured Certainty Premium. Violent incident data are from ISAF CIDNE. The covariates are pre-prime risk (0 - 10), female (=1), shia (=1), years of education, born locally (=1), reporting insurgent activity important (=1), prefer police resolve disputes (=1), prefer courts resolve disputes (=1), married (=1), age, and log(income).

2.6 Screen and Instructions

THE FOLLOWING QUESTIONS SHOULD ONLY APPEAR FOR THE FOLLOWING PROVINCES: BADAKSHAN, BALKH, BAMYAN, DAIKONDI, FARYAB, HERAT, JUZJAN, KABUL, KAPISA, PANJSCHER, PARWAN, SAMANGAN

D1. We are interested in understanding how Afghans make decisions involving uncertain outcomes and some normal risks that people face every day. We would like to ask you some hypothetical questions that will help us understand these decisions. There is no real money involved and you will not receive any money for answering these questions. Are you willing to answer these questions?

1. Yes (Proceed with survey))

Table 2.7: Robustness - Controlled Recall and Decision Error

Dependent Variable:	Multiple Switcher (=1)				Non-monotonic Utility (=1)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fear Prime (=1)	-0.022	-0.009	-0.011	-0.029	-0.011	-0.001	-0.007	-0.017
	(0.021)	(0.028)	(0.026)	(0.036)	(0.022)	(0.029)	(0.027)	(0.037)
Violence (=1)		-0.015	-0.015			-0.026	0.002	
		(0.027)	(0.026)			(0.026)	(0.025)	
Fear x Violence		-0.039	-0.035	-0.025		-0.028	-0.025	0.002
		(0.042)	(0.041)	(0.055)		(0.044)	(0.038)	(0.052)
Constant	0.134***	0.139***	0.184***	0.166	0.132***	0.142***	0.121*	0.086
	(0.013)	(0.017)	(0.064)	(0.107)	(0.013)	(0.017)	(0.064)	(0.120)
Covariates	No	No	Yes	Yes	No	No	Yes	Yes
Fixed Effects	No	No	Province	PC	No	No	Province	PC
# Observations	977	977	858	858	977	977	858	858
# Clusters	286	286	277	277	286	286	277	277
R-Squared	0.001	0.003	0.277	0.535	0.000	0.003	0.263	0.526

Notes: Robust standard errors clustered at the polling center level reported in parentheses. Violence data are from ISAF CIDNE. Violence is defined as a violent event occurring within one kilometer of interview location over the period October 2005 - February 2010. Sample: 816 individuals with positive $v(150)$ and no multiple switching. $v(150)_C$ refers to elicited utility under certainty while $v(150)_U$ refers to elicited utility under uncertainty. The differences $v(150)_C - v(150)_U$ is the measured Certainty Premium. Violent incident data are from ISAF CIDNE. The covariates are pre-prime risk (0 - 10), female (=1), shia (=1), years of education, born locally (=1), reporting insurgent activity important (=1), prefer police resolve disputes (=1), prefer courts resolve disputes (=1), married (=1), age, and log(income).

Table 2.8: Robustness - Controlled Recall and Attrition from Decision Tasks

Dependent Variable:	Failed to Complete Choice Task 1 or Choice Task 2 (=1)				
	(1)	(2)	(3)	(4)	(5)
Fear Prime (=1)	0.011	0.015	0.015	0.012	0.008
	(0.023)	(0.030)	(0.030)	(0.031)	(0.040)
Violence (=1)		0.021	0.021	0.024	
		(0.029)	(0.029)	(0.024)	
Fear x Violence		-0.012	-0.012	-0.012	-0.009
		(0.046)	(0.046)	(0.046)	(0.058)
Constant	0.130***	0.122***	0.122***	0.022	0.000
	(0.014)	(0.017)	(0.017)	(0.016)	(0.014)
Covariates	No	No	No	Yes	Yes
Fixed Effects	No	No	No	Province	PC
# Observations	1127	1127	1127	1127	1127
# Clusters	287	287	287	287	287
R-Squared	0.000	0.001	0.001	0.122	0.275

Notes: Robust standard errors clustered at the polling center level reported in parentheses. Violence data are from ISAF CIDNE. Violence is defined as a violent event occurring within one kilometer of interview location over the period October 2005 - February 2010. Sample: 816 individuals with positive $v(150)$ and no multiple switching. $v(150)_C$ refers to elicited utility under certainty while $v(150)_U$ refers to elicited utility under uncertainty. The differences $v(150)_C - v(150)_U$ is the measured Certainty Premium. Violent incident data are from ISAF CIDNE. The covariates are pre-prime risk (0 - 10), female (=1), shia (=1), years of education, born locally (=1), reporting insurgent activity important (=1), prefer police resolve disputes (=1), prefer courts resolve disputes (=1), married (=1), age, and log(income).

2. No (Conclude Surevy)

65a: " We are interested in understanding your daily experiences that may make you fearful or anxious. This could be anything, for example getting sick, experienc-

Table 2.9: Effects by Intensity and Time Period of Violence

Panel A - Effects by Intensity of Violence						
Dependent Variable:	$v(150)_u$		$v(150)_c$		$v(150)_c - v(150)_u$	
	(1)	(2)	(3)	(4)	(5)	(6)
FEAR (=1)	-0.040**	-0.055***	-0.013	-0.018*	0.027**	0.036***
	(0.019)	(0.019)	(0.009)	(0.010)	(0.012)	(0.012)
Violence Intensity (0-1)	0.083	0.043	0.044	0.025	-0.037	-0.016
	(0.061)	(0.060)	(0.034)	(0.034)	(0.044)	(0.037)
Fear Prime x Violence Intens.	-0.463**	-0.504**	-0.184*	-0.215**	0.277***	0.288***
	(0.195)	(0.211)	(0.098)	(0.099)	(0.103)	(0.108)
Constant	0.254***	0.063	0.621***	0.514***	0.368***	0.446***
	(0.012)	(0.049)	(0.005)	(0.026)	(0.009)	(0.032)
Covariates	No	Yes	No	Yes	No	Yes
# Observations	816	718	816	718	816	718
# Clusters	278	267	278	267	278	267
Log-Likelihood	-1283.150	-1102.654	-1299.130	-1122.104	-571.119	-465.402
Panel B - Recent Violence and Old Violence						
FEAR(=1)	-0.021	-0.035	-0.008	-0.013	0.012	0.021
	(0.023)	(0.023)	(0.011)	(0.011)	(0.014)	(0.014)
Violence 11/05 - 12/07	-0.019	-0.008	-0.004	0.001	0.016	0.009
	(0.022)	(0.026)	(0.012)	(0.013)	(0.016)	(0.017)
FEAR x Violence 11/05 - 12/07	-0.064	-0.077	-0.028	-0.034	0.032	0.039
	(0.050)	(0.054)	(0.027)	(0.030)	(0.029)	(0.029)
Violence 1/08 - 2/10	0.017	0.012	0.019	0.011	0.002	-0.001
	(0.032)	(0.033)	(0.014)	(0.015)	(0.022)	(0.022)
FEAR x Violence 1/08 - 2/10	-0.130***	-0.137***	-0.039	-0.038	0.095***	0.104***
	(0.046)	(0.043)	(0.028)	(0.027)	(0.033)	(0.035)
New Violence x Old Violence	-0.005	-0.003	-0.015	-0.009	-0.009	-0.007
	(0.043)	(0.048)	(0.021)	(0.023)	(0.030)	(0.031)
Fear x New X Old	0.135*	0.160*	0.061	0.061	-0.078	-0.102**
	(0.081)	(0.082)	(0.047)	(0.047)	(0.051)	(0.051)
Constant	0.261***	0.071	0.621***	0.516***	0.360***	0.441***
	(0.013)	(0.051)	(0.006)	(0.027)	(0.011)	(0.033)
Covariates	No	Yes	No	Yes	No	Yes
# Observations	816	718	816	718	816	718
# Clusters	278	267	278	267	278	267
Log-Likelihood	-1280.258	-1100.407	-1298.193	-1122.272	-565.680	-460.251

Notes: Robust standard errors clustered at the Polling Center level reported in parentheses. All regressions include province fixed effects. Violence data are from ISAF CIDNE. Violence is defined as a violent event occurring within one kilometer of interview location over the period October 2005 - February 2010. Sample: 816 individuals with positive $v(150)$ and no multiple switching. $v(150)_C$ refers to elicited utility under certainty while $v(150)_U$ refers to elicited utility under uncertainty. The differences $v(150)_C - v(150)_U$ is the measured Certainty Premium. Violent incident data are from ISAF CIDNE. The covariates are pre-prime risk (0 - 10), female (=1), shia (=1), years of education, born locally (=1), reporting insurgent activity important (=1), prefer police resolve disputes (=1), prefer courts resolve disputes (=1), married (=1), age, and log(income).

ing violence, losing a job, etc. Could you describe one event in the past year that caused you fear or anxiety?" — record verbatim.

65b: " We are interested in understanding your daily experiences that make you happy or joyous. This could be anything, for example birth of child, marriage of a relative, or success in your job. Could you describe an event in the past year that

Table 2.10: Robustness - Redefining the Polling Catchment from 1km to 3 km

Dependent Variable:	$v(150)_u$		$v(150)_c$		$v(150)_c - v(150)_u$	
	(1)	(2)	(3)	(4)	(5)	(6)
Fear Prime (=1)	-0.024	-0.024	-0.009	-0.009	0.014	0.014
	(0.024)	(0.024)	(0.012)	(0.012)	(0.015)	(0.015)
Constant	0.264***	0.264***	0.623***	0.623***	0.359***	0.359***
	(0.014)	(0.014)	(0.007)	(0.007)	(0.011)	(0.011)
Violence 3km (=1)	-0.013	-0.013	0.000	0.000	0.014	0.014
	(0.019)	(0.019)	(0.009)	(0.009)	(0.013)	(0.013)
Violence 3km x FEAR	-0.070**	-0.070**	-0.021	-0.021	0.047**	0.047**
	(0.034)	(0.034)	(0.018)	(0.018)	(0.022)	(0.022)
# Observations	816	816	816	816	816	816
# Clusters	278	278	278	278	278	278
Log-Likelihood	-1281.281	-1281.281	-1299.351	-1299.351	-567.509	-567.509

Notes: Robust standard errors clustered at the Polling Center level reported in parentheses. All regressions include province fixed effects. Violence data are from ISAF CIDNE. Violence is defined as a violent event occurring within three kilometers of interview location over the period October 2005 - February 2010. Sample: 816 individuals with positive $v(150)$ and no multiple switching. $v(150)_C$ refers to elicited utility under certainty while $v(150)_U$ refers to elicited utility under uncertainty. The differences $v(150)_C - v(150)_U$ is the measured Certainty Premium. Violent incident data are from ISAF CIDNE. The covariates are pre-prime risk (0 - 10), female (=1), shia (=1), years of education, born locally (=1), reporting insurgent activity important (=1), prefer police resolve disputes (=1), prefer courts resolve disputes (=1), married (=1), age, and log(income).

caused you happiness?” — record verbatim.

65c “We are interested in understanding your general daily experiences. This could be anything. Could describe an event from the past year. — record verbatim

[Show Card] First we will ask you a hypothetical question over an amount for certain, or an amount that will be awarded depending on which of ten numbers you draw from a bag. We have deposited 10 cards numbered 1 through 10 into a bag. You have an even chance of drawing any of the 10 numbers. The numbers in parentheses indicate the winning number. For each Option No., please indicate whether you would prefer Choice 1 or Choice 2. For each Option No. there will be 10 numbers in the bag and you are only able to draw one. This is not for real money and we are not asking you to make a gamble, we just want to understand how you would respond to naturally occurring risk.

2.7 Acknowledgement

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Chapter 3

Catastrophes and Time Preference: Evidence From the Indian Ocean Earthquake

Abstract

We provide evidence that exposure to the Indian Ocean Earthquake tsunami increased an experimental patience measure in a sample of Sri Lankan wage workers. Regression discontinuity estimates based on proximity to the high water mark confirm this finding. We show theoretically and use a battery of empirical tests to argue that this reflects a change in preference and not other changes in the economic environment which affect experimental patience measures. The effects are largest for less educated workers, shorter workers, and for the left-tail of the distribution consistent with tsunami exposure being a substitute for other inputs to patience formation.

3.1 Introduction

Time preference features centrally in theories of consumer optimization, economic growth, and interest rate determination. Intertemporal preferences, additionally, are of key relevance for development economics. Capital investments, time spent learning, and other accumulation decisions directly affect economic success. Evaluating whether time preference responds to disasters may therefore be important in understanding the economic legacies of these events. Additionally, if major life events impact preferences, then understanding this linkage is relevant to efforts to account for the sources of heterogeneity in economic taste. In this paper, we provide evidence that measured discount factors increase in response to experiencing a natural disaster reflecting a change in the rate of time preference and not other shifts in the economic environment.

In the set of economic motivations for discounting identified in early research (Rae, 1834; Jevons, 1888, 1905; Senior, 1836; Böhm-Bawerk, 1889), the specific taste for trading off utility over time is sharply distinguished by modern theory from considerations which affect expectations and the intertemporal budget constraint.[1] Olson and Bailey (1981), provide a clear articulation of this broadly accepted view, which we follow, terming the complete set of reasons for impatience *time discounting* and the specific taste for intertemporal utility tradeoffs *time preference*.

Stigler and Becker (1977) emphasize the importance of this distinction as it allows the "economist [to continue] to search for differences in prices or income to explain any differences or changes in behavior," without having to account for differences in taste. More recently, economists have begun to investigate whether tastes respond systematically to economic shocks (Malmendier and Nagel, 2007; Meier and Sprenger, 2011) and also if preferences develop as a result of utility-maximizing investments in preference formation (Becker and Mulligan, 1997).

Our data, collected in regions affected by the Indian Ocean Earthquake formally called the Sumatra-Andaman earthquake) in Sri Lankan are well-suited to testing whether economic preference responds to catastrophe. The event, which took place on December 26, 2004 off the west coast of Sumatra, Indonesia devastated household assets. In our sample, 153 of 456 wage workers (33.55%) report at least some damage and the median affected individual reported suffering approximately 200,000 rupees worth of damage, which is approximately 23.7 times the median monthly wage. The tsunamis triggered by the earthquake significantly damaged the coasts bordering the Indian Ocean. Waves up to 30 meters were recorded and the UN reports 229,866 total individuals lost or missing. In Sri Lanka, 35,322 individuals were killed and

[1] Frederick et al. (2002) provide an authoritative review of the intellectual history of intertemporal choice.

516,150 people were displaced.[2] The economic and psychological damage wrought by the tsunami was unexpected and severe. It likely affected horizons, consumption streams, and made salient the possibility of extreme unanticipated losses. Using a standard measure of willingness to delay consumption, we find that workers affected by the tsunami are more patient than unaffected workers.

More specifically, our estimates are consistent with the tsunami creating an increase in the average monthly discount factor from about 0.8 to about 0.85 or to about 0.9 depending on the specification. This represents a 1/3rd to 2/3rd of a standard deviation increase in our patience measure. These estimates are robust to a broad set of specifications and are very similar to regression discontinuity results. Additionally, the effect appears to be enduring. Our data reflect preferences elicited two and a half years after the Indian Ocean Earthquake. We also find that this effect is largest for individuals who: have no secondary or higher education, are shorter and so are less likely to have received sufficient caloric nourishment as children, perform worse on a cognitive test and, who are in the left tail of the patience distribution. While this only lets us begin to speculate as to the reasons that patience appears to have increased as a result of the tsunami, these results are consistent with tsunami exposure being a substitute for other inputs which are argued to develop patience (Becker and Mulligan, 1997). The direction of the effect, when contrasted against the predictions of our simple theory, and the richness of our data allow us to argue that our results are not driven by liquidity constraints and other considerations that affect elicited discount rates but that are not part of standard theoretical concept of the rate of time preference.

The principal threat to our identification strategy is that patient workers may select areas vulnerable to tsunami inundation. Without pre-tsunami measurements, we cannot establish that affected and unaffected workers had similar preferences prior to the event. Contrary to this, we find affected and unaffected workers are balanced on a broad range of variables which should not respond to tsunami exposure and that the results are robust to including fixed effects for small arbitrary spatial divisions (fishnet grids). Moreover, looking at the part of our sample which is vulnerable to inundation but escaped exposure because of the wave's direction, we find no evidence that more

[2] See http://en.wikipedia.org/wiki/2004_Indian_Ocean_earthquake

patient individuals are disproportionately vulnerable. Last, regression discontinuity estimates based on the high water mark, which should not reflect locational sorting prior to the event, show a positive effect on patience that is almost identical to the simple mean difference in discount factors between affected and unaffected workers.

To establish that the change we observe reflects a change in preferences we also need to rule out a set of competing explanations. First, it is possible that because survey questions which ask about consumption trade-offs over time can only measure *time discounting*, which is reflects a broader range of considerations that the parameter we are interested–*time preference*, the effect we estimate does not truly reflect a change in tastes. To counter this concern, we model the survey respondents' problem and show that an *increase* in patience is consistent only with a change in *time preference*. Second, it may be that the revealed willingness to delay consumption merely reflects a demand to replace assets demolished during the disaster. We find that the effect of the disaster on preferences does not differ by age, childhood poverty status, debt levels, or wages and we do not find any effect of the percentage of assets replace or the intensity of the damage on preferences after controlling for whether an individual was affected at all. Moreover, a 1 month delay to a payment may be negligible in terms of life cycle saving two and one-half years after the event. Last, it may be that the large-scale aid response to the tsunami may have either *relaxed* credit constraints or may have flattened the time path of consumption for beneficiaries by making them richer. However, we find a similar increase for individuals even after restricting our sample to individuals who received less than a day's wage worth of aid support. The evidence against these potential explanation supports our interpretation of the increase in measured patience that results from disaster exposure as reflecting an increase in patience.

The remainder of the paper is organized as follows. Section 3.2 reviews the distinction, discussed above, between *time discounting* and *time preference* and also relates our study to the empirical literature that relates intertemporal decision-making to economic success. Section 3.3 builds on the potential links identified by the literature and derives theoretical predictions about how changes in survival expectations and differential marginal utility from changing consumption levels will affect responses to the survey question we use to elicit time preference. Section 3.4 provides an overview of our data. Section 3.5 reviews our empirical strategy. Section 3.6 documents the effect of the catastrophe on preferences. Section 3.7 provides evidence that the experience substituted for other inputs to preference formation such as education, and Section 3.8 concludes.

3.2 Literature

3.2.1 Time Discounting and Time Preference

Olson and Bailey (1981) and Frederick et al. (2002) draw a sharp theoretical distinction between *time discounting* and *time preference*. This distinction is key to our analysis because a well-known criticism of experimental time preference elicitation techniques is that they confound several factors which affect intertemporal decisions (Andreoni and Sprenger, 2010). Specifically, standard preference elicitation techniques measure *time discounting* or the complete set of reasons for discounting the future, including uncertainty, changing tastes, and differential marginal utility arising from changing consumption levels. By contrast, *time preference* refers exclusively to the preference for immediate utility over delayed utility. Frederick et al. (2002) review the set of considerations which cause elicited preferences to diverge from the rate of time preference. These are (a) intertemporal arbitrage; (b) diminishing marginal utility; (c) uncertainty about the future; (d) price-level inflation; (e) expectations of changing utility; and (f) considerations of habit formation. Harrison et al. (2008) confirm the empirical relevance of diminishing marginal utility and show that it generates a large downward bias in measured discount factors.[3] For the purpose of clarity, we use the terms discounting, patience, impatience, discount factors, and measured discount factors to refer to *time discounting* and we reserve *time preference* specifically for the inverse of the intertemporal elasticity of substitution.

[3] Andreoni and Sprenger (2010) provide an alternative elicitation procedure which can directly measure time preference.

3.2.2 Life Experience, Preferences, and Economic Success

Our findings relate to two additional lines of research. The first is the empirical literature linking life experience to fundamental and enduring changes in preferences and behavior. Malmendier and Nagel (2007) show that individuals who have lived through periods of exceptional stock market performance exhibit less risk aversion and that birth cohorts that experienced the Great Depression are much less likely to participate in the stock market. Meier and Sprenger (2011) show that measured time preferences do not change in response to more common shocks, such as the loss of employment, among users of a volunteer tax assistance site in Boston. Blattman and Annan (2010) show that childhood conscripts in Uganda who experience the most severe violence are more likely to report psychological distress as adults, and Alesina and Giuliano (2009) provide evidence that historical experiences and personal histories shape preferences for social equality.

The second line of research focuses on the economic implications of disasters and responses to disasters. Economists have investigated, for example, how fertility responds to disasters (Finlay, 2009; Pörtner, 2008), whether aid in response to a disaster improves perceptions of the aid provider (Andrabi and Das, 2010) and how to best target reconstruction aid to rebuild microenterprises (de Mel et al., 2010). The papers most closely linked to this study are Cameron and Shah (2010a) and Cassar et al. (2011), both of which look for changes in experimentally-elicited preference measures as a result of experiencing disasters. Both papers find that exposure to natural disasters increase measures of risk aversion elicited through incentivized choice experiments. Andrabi and Das (2010), in an ancillary result, find a similar change as a result of the 2005 earthquake in Northern Pakistan for measurements that are not incentivized. While we have a similar measure of risk-aversion elicited through incentivized Holt-Laury Multiple Price Lists, our focus, for reasons we describe below, is on time preference. Given the attention paid by both Cameron and Shah (2010a) and Cassar et al. (2011) to issues of respondent comprehension and to careful measurement, we direct readers interested in the risk preference effects of disasters to those studies.[4]

[4] In results available on request, we cannot reject no change in risk preference from the disaster using the same identification strategies we use for our result on time preference. This result is

The primary difference between the current study and both Cameron and Shah (2010a) and Cassar et al. (2011) is that our focus is on time preference rather than risk preference. Our focus on time preference, as we discuss in detail in section 3.3 below, permits our focus on separately identifying changes in time preference from other changes to the economic environment, such as changes to the time path of consumption and to subjective survival expectations. (Those two papers acknowledge the relevance of this distinction.)

As in Cameron and Shah (2010a) and Cassar et al. (2011), we establish a change in preference measures using a t-test of means between affected and unaffected survey respondents. A secondary difference, however, is that our data on precise respondent locations allow us to additionally identify the effect using regression discontinuity and also allow us to directly test an identifying assumption common to our paper and to Cameron and Shah (2010a) and Cassar et al. (2011)—that ex ante disaster vulnerability is not selected based on economic preference–which allows a causal interpretation of the difference in means. Our information on respondent locations also allows us to establish that our result is robust to within variation for small arbitrary fishnet grids. We interpret the robustness of the core result, the similarity of the result estimated through regression discontinuity, and our failure to reject selection of vulnerability based on preferences as strongly supporting the empirical strategy used in all three papers.

In addition to changes in trust, trust worthiness, and MPL measures of risk preference, Cassar et al. (2011) find modest decreases in patience as a result of the Indian Ocean Earthquake in Thailand for individuals who experience injuries or a death in the family after controlling for risk preferences.[5] The measure which is

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consistent to the lack of a difference found in Hurricane Katrina victims one year after the event in Eckel et al. (2009a). A potential explanation for our lack of a result is that both Andrabi and Das (2010) and Cassar et al. (2011) find increases in trust as a result of experiencing disaster and that Ben-Ner and Putterman (2001) and Karlan (2005) find that risk aversion and trust are negatively correlated so that the trust effects of the disaster are offsetting the risk preference effects. However, given the broad range of extra-preference confounds that may enter into price list elicitations of risk preference, we can only speculate as to the cause of the difference.

[5]Given the evidence for random assignment to calamity in this paper as well as in Andrabi and Das (2010), Cameron and Shah (2010a), Cassar et al. (2011), it seems likely that the effect on time preference is only significant in specifications which include risk preference measures as a covariate because of the importance of risk preference in soaking up noise in measures of time preference and not because risk preference is needed to satisfy conditional independence.

significally correlated with time preference in this paper is personal injury or family death, which is distinct from our measure, which is any exposure at all. As we show in section 3.3 below, this result is consistent both with a change to respondents' subjective survival probabilities and with an increase in the marginal utility of consumption due to a decrease in consumption levels resulting from losing a family source of income. We therefore view our results as consistent with those of Cassar et al. (2011) as they support the same model. Their measure of damage—injuries or a death in the family—should, according to our theory, generate a different result.

3.3 Theory

To assess whether the increase in our survey measure of patience reflects a change in time preference and not a response to the set of extra-time preference considerations listed above, we develop theoretical predictions regarding how these considerations can be expected to change our measure as a result of the tsunami. This distinction is critical given that our survey question does not measure time preference directly.

3.3.1 Predictions of Decreased Patience

Concave Utility and Uncertainty

In our sample, the median affected individual reports suffering approximately 200,000 rupees worth of damage, which is approximately 23.7 times the median monthly wage. We expect the devastation and the rapid subsequent aid response to put individuals on a steeper (though dramatically lower) consumption path and thereby create incentives to move consumption to the present. Below, we show how this intuition plays out by considering the optimization problem faced by a survey respondent.

Additionally, we expect that surviving the tsunami had some effect on individuals subjective beliefs about future payoffs. Yaari (1965) and Blanchard (1985) provide finite-horizon models that link expected survival probabilities to time discounting. Survival expectations are modeled as a per period probability of dying that is added directly to the rate of time preference. Similarly, Jayachandran and Lleras-Muney (2009) provide a model where the stream of future consumption utility has some positive probability of not realizing because of death during childbirth which depends, in turn, on the prevailing rate of maternal mortality. Jayachandran and Lleras-Muney (2009) show additionally that a large exogenous reduction in maternal mortality in Sri Lanka increased female education and literacy. Given this evidence, it is clear that we should take seriously the implications of changing uncertainty and differential marginal utility for the discount rate we impute.

Concave Utility and Uncertainty - Predictions from Theory

As we describe more fully below, we measure discounting using responses to questions of the form:

"Suppose someone was going to pay you Rupees m six months from now. He/she offers to pay you a lower amount x in five months time. What amount in five months would make you just as happy as receiving Rupees m in six months?"

To see how concave utility and uncertainty affect our imputed measure, consider the respondent's problem. Because our predictions are general with respect to the functional form of the discount factor over time, we consider only the exponential discounting case. Let $u(c)$ be a concave utility function and, without loss of generality, $u(0) = 0$ so that the respondent receives no utility if they do not survive. For simplicity, we assume a one month interest rate of zero, but show below that an increase in the marginal return to capital from asset devastation should decrease discount factors. Additionally, let c_0 and c_1 be equilibrium consumption today and in one month, m be a fixed reward to be provided in the future, p be the probability that an individual is alive one month from today, δ be the discount factor, and x be compensation that is required to forego some reward in the future. We measure the individual discount factor in our data $\hat{\delta}$ as $\frac{x}{m}$. Because m is fixed, it is sufficient to consider the predictions for x to summarize the expected response of measured patience to catastrophe. The respondent selects x to satisfy the marginal condition:

$$u(c_0 + x) + \delta E[u(c_1)] = u(c_0) + \delta E[u(c_1 + m)] \tag{3.1}$$

which, can be rewritten as

$$u(c_0 + x) - u(c_0) = \delta p[u(c_1 + m) - u(c_1)]. \tag{3.2}$$

by noting that $u(0) = 0$ and $E[u(c_1)] = pu(c_1) + (1 - p)u(c_0)$. Equation 3.2 summarizes how expectations and curvature should affect the respondents' selection of x. If we take as a baseline $\delta = 1$ and $p = 1$ and linear utility, then choice that satisfies the marginal condition is $x = m$. If any of the conditions $\delta < 1$, $p < 1$, or with concave utility $c_0 < c_1$ hold, then $\frac{x}{m} < 1$ and our matching task will code an individual as impatient. If $\delta < 1$, individuals are truly impatient. If $p < 1$, individuals apply a discount that reflects their beliefs about survival. Last, if more consumption will be available in the future ($c_0 < c_1$), then, due to concave utility, the benefit from additional future consumption is less than the benefit from additional current consumption. We use this indifference condition to develop predictions for how the disaster should affect our measure of time preference.

Prediction 1 - *Decreasing survival expectations reduces measured patience:* $\frac{\partial x}{\partial p} > 0$.

If the tsunami caused individuals to assign a lower subjective probability to receiving future consumption, then, to satisfy their indifference condition, individuals should move consumption from the future to the present. Using the implicit function theorem to differentiate Equation 3.2 with respect to p we see that:

$$\frac{\partial x}{\partial p} = \frac{\delta[u(c_1 + m) - u(c_1)]}{u'(c_0 + x)} > 0$$

If the effect of the tsunami was to decrease the subjective probability of survival for the affected, then our indifference condition predicts a decrease in p and, consequently, in measured patience. It is worth noting, however, that it is possible that respondents believe there is some arrival rate for catastrophe and having survived the tsunami makes respondents feel as though they are much less likely to encounter calamity in the future. However, Cameron and Shah (2010a) show that the tsunami greatly increased individuals assessments that another tsunami would occur in the future so, if anything, we should expect p to decrease. To provide additional evidence against the relevance of p, we control for this in our regressions using measures of individuals' subjective expectations about the future. Introducing measures of subjective expectations on the right hand side does not affect our result.

An additional confound for measures of patience is difference in marginal utility of consumption over time. To derive predictions, we let consumption and the survey response x depend on whether or not an individual experienced a disaster so that $\{c_t(1), x(1)\}$ is consumption and the desired payment in period $t \in \{0, 1\}$ in the affected state and $\{c_t(0), x(0)\}$ is consumption and the desired payment in the unaffected state.

Prediction 2 - *If pre-event consumption is flat or increasing over time* ($c_0(0) \leq c_1(0)$)*, more consumption is lost in period 0 than is lost period 1* ($c_0(1) - c_0(0) \leq c_1(1) - c_1(0)$)*, and the individual is weakly patient* ($x(0) \leq m$)*, then concavity of the utility function implies that x should decrease in response to disaster.*

This result follows directly from the concavity of the utility function. To see this, note that

$$
\begin{aligned}
&[U(c_0(1)) + x(0)) - u(c_0(1))] - [U(c_0(0) + x(0)) - u(c_0(0))] \geq \\
&[U(c_1(1)) + m) - u(c_1(1))] - [U(c_1(0) + m) - u(c_1(0))] > \\
&\delta p\,[U(c_1(1)) + m) - u(c_1(1))] - [U(c_1(0) + m) - u(c_1(0))]
\end{aligned}
$$

where the first weak inequality follows from our assumptions and the concavity of the utility function and the second holds if either p or δ is less than 1. Canceling equal terms based on our marginal indifference condition (Equation 3.2) from the first and the third expression in the inequality, we have that

$$
[U(c_0(1)) + x(0)) - u(c_0(1))] > \delta p\,[U(c_1(1)) + m) - u(c_1(1))]
$$

which directly implies $x(1) < x(0)$ to satisfy indifference. Thus we have that x will adjust downward to satisfy our marginal indifference condition in response to a catastrophe. This corresponds to our intuition that, if the effect of the tsunami is to move respondents with concave utility functions to a lower and weakly steeper consumption path, the resulting increase in the marginal utility of current consumption should encourage affected respondents to have an increased taste for current consumption.

3.3.2 Predictions of Increased Patience

Prediction 3 - *Increases in the pure rate of time preference should increase elicited discount factors:* $\frac{\partial x}{\partial \delta} > 0$.

This result simply establishes that some of what our elicitation procedure measures is what we are interested in. To show that the relationship between time discounting and time preference is positive and linear we take the partial derivative with respect to the rate of pure time preference. To obtain this result, we just need to differentiate Equation 3.2 with respect to δ.

$$\frac{\partial x}{\partial \delta} = \frac{p[u(c_1 + m) - u(c_1)]}{u'(c_0 + x)} > 0.$$

3.3.3 Intertemporal Arbitrage

So far, we have assumed that the monthly interest rate is zero. However, if our respondents are not credit constrained, then for an optimizing respondent the rate of time preference imputed from a matching task should collapse to the prevailing interest rate. To see this, consider the standard intertemporal Euler for a respondent at period t,

$$u'(c_t) = (1 + r)\delta E[u'(c_{t+1})]. \tag{3.3}$$

Under linear utility, the Euler equation is the no arbitrage condition a respondent faces when we impute discount rates by asking individuals how much they require in the current period to forego some fixed consumption in the future. For small degrees of curvature in the utility function, we expect that individuals should be indifferent between roughly $\frac{1}{1+r}$ rupees today and 1 rupee in the next period. If our measured rate of time preference is just the interest rate, an increase in returns to capital will work to decrease measured patience. Moreover, as Frederick et al. (2002) emphasize, most studies find that measured preferences are largely unaffected by interest rates. Last, the heterogeneity of measured discount factors in our data, as is visible in Figure 3.5, would imply an implausible number of different interest rates.

3.4 Data and Preference Measurement

Our data come from a survey of Sri Lankan workers residing in regions affected by the Tsunami undertaken two and a half years after the tsunami in July 2007. 456 wage workers comprise our sample. 155 (34%) of the workers in our survey lost some household assets as a result of the Tsunami. Table 3.1 reports summary statistics of variables that are affected by the tsunami and Table 3.2 provides evidence for balance on endline observables and retrospective variables which should be orthogonal to tsunami exposure if our identifying assumption is correct. The survey of wage workers we use, which elicited measures of patience using a matching task, are described in detail in de Mel et al. (2008). The data were collected two and a half years after the event, which suggests that the changes in preferences we observe are enduring.

Our data suggest that economic recovery from the tsunami was well underway but not complete. The median affected respondent indicates having repaired about 50% of the damage resulting from the Tsunami. The data suggest that part of the recovery resulted from the rapid large scale international aid response. Of the 155 individuals who suffered some damage, 133 received a relief grant and 140 received some type of recovery aid. On average, aid to the affected workers in our sample was equal to 87.2% of reported losses. We strongly reject, however, that received aid was equal to the value of the damages (p-value=0.0002). We additionally check a range of specifications which include both the rupee value of damages and the estimated percentage of assets replaced as controls and find that the effect of disasters on preference remains robust.

The workers in our sample are young, predominately male, Sinhalese and Buddhist. They appear to have been impoverished during their childhood. The average monthly wage in our sample is about 10,000 rupees which, at exchange rates contemporary to the survey, translates to roughly US $3.30 a day. Our results apply only to this demographic. However, a key focus of the development literature are the reasons individuals transition from wage work to entrepreneurship, and so it may be valuable to understand the responsiveness of time-preference to shocks in this population. Given the range of studies that suggest that life experience can permanently alter behavior, we believe these results have some generalizability. Wage workers, moreover, may be constrained with regard to selecting where they live, which allays some concern that pre-tsunami time preference influences exposure to the Tsunami. Before turning to the effects of exposure, we review our method for eliciting measures of patience and confirm that it reflects, to some degree, preferences over intertemporal tradeoffs.

3.5 Empirical Strategy

We take two approaches to identifying the effect of disasters on time preference. First, we provide evidence that exposure to the tsunami is exogenous with respect to pre-event time preference and then estimate the mean difference between affected and unaffected workers, which, if our identifying assumption is valid, can be interpreted as causal. Second, we use a natural discontinuity created by the high water mark, GPS coordinates for our respondents, and GIS data to estimate the effect using a regression discontinuity approach. Both strategies provide highly significant and very similar estimates.

3.5.1 Approach 1 - Comparing Mean Differences Under Random Assignment to Exposure

In this approach, our identifying assumption is that suffering damage from the tsunami is random. This assumption has two testable implications. First, if exposure is random, then our sample should be balanced on retrospective variables and on fixed variables which should not be altered by tsunami exposure. If these observables are correlated with time preference, then the absence of any statistical difference between affected and unaffected workers we observe in Table 3.1 supports exogeneity. Second, there should be no correlation between preferences and tsunami vulnerability in places that were not affected by the tsunami because of topography and the direction of the wave, but could have been had the wave had a different point of origin.

Our identifying assumption will be violated if individuals select where they live based on their preferences. This may plausibly happen if preferences influence selection into different lines of work and if the spatial distribution of jobs creates differences in exposure probability. Table 3.2 provides evidence in support of our identifying assumption. We find that our sample is strongly balanced for variables that are plausibly exogenous to exposure or that are retrospective. Age, gender, and education, which are among the strongest and most consistent predictors of patience across studies, appear to be balanced. It is also important to note that we find no statistical difference for the type of pre-tsunami employment contract. Laffont and Matoussi (1995) provide evidence that the selection of employment contracts depends on preferences. Moreover, a plausible explanation for selection into tsunami vulnerability according to preferences is through job selection. We interpret the absence of any statistically significant difference in the type of employment contract prior to the Tsunami as evidence suggesting that average time preference was identical in both groups prior to the Tsunami.

Figures 3.1 and 3.2 provide more intuition for our first approach to identifying the response of preferences to catastrophe. Figure 3.1 depicts the location of survey respondents on a topographical map overlaid with 0.07 by 0.07 arc degree fishnet grids.[6] The fishnet grids allow us to test whether our result is robust to using only within variation for small regions less subject to concerns about migration and pre-event selection of location on preference. In the next section, we also make use of the fishnet grids to implement a regression discontinuity test of the response of preferences to the tsunami. We see that, while damages were highly concentrated, they do not appear to be consistently correlated with distance to the coast or elevation owing to the origination of the wave off the coast of Sumatra to the northeast. In the eastern parts of the sample, for example, there are clusters of respondents who live next to the coast at low elevation who were not affected because of the wave's

[6]Dividing the space where this survey was administered into 0.07 arc degree fishnet grids creates 87 units with an average of 5.52 respondents per grid.

direction. This is in line with the emphasis placed on bathymetry and topography in the literature that tries to model and predict tsunami inundation (see e.g. Dominey-Howes and Papathoma (2007) and Koshimura et al. (2008)). Figure 3.2 plots elicited time preferences on a topographical map. In this figure, there does not appear to be an obvious correlation correlation between elevation or distance to the coast among unaffected populations, which is consistent with the idea that individuals do not select vulnerability based on time preference. We now turn to a formal test of whether preferences and vulnerabilty are correlated.

In Table 3.3, we report the results of a regression of our elicited discount factor on the straight-line distance between the individual and the coast, the elevation of the respondent's household, and the average number of respondents in the same 0.07 by 0.07 Arc Degree fishnet grid affected by the tsunami. We run this test only for unaffected individuals, as we argue that the experience of the tsunami creates a relationship between patience and vulnerability in the affected sample. A failure to reject the null hypothesis that these measures do not describe any differences in elicited patience in our sample is evidence in support of our identifying assumption. To make this test as stringent as possible, we report both standard errors clustered at the Grama Niladara level and standard errors with no clustering and additionally report the p-values corresponding to an F-test for joint significance for both sets of standard errors.[7] In no cases do we find evidence that discount factors and vulnerability are correlated in our unaffected sample.

Given this evidence, there is a case, which we reinforce using regression discontinuity in the next section, that unaffected workers provide a valid counterfactual for workers affected by the tsunami.

Thus, we can compare the average time preference of workers affected by the Tsunami with those of workers unaffected by the Tsunami to determine the causal effect of exposure on time preference. This motivates the specification:

$$Average\ Discount\ Factor_i = \beta_0 + \beta_1 Damaged_i + \beta_2 X_i + \epsilon_i \qquad (3.4)$$

where $Average\ Discount\ Factor_i$ is the discount factor described in the previous

[7] There are an average of 15.2 respondents pre Grama Niladara and 52 Grama Niladara units in our data

section, $Damaged_i$ is a dummy variable equal to 1 if the Tsunami destroyed household assets belonging to worker i, and X_i are controls.

3.5.2 Approach 2 - Regression Discontinuity

While the assumptions necessary to identify a causal effect by comparing means are supported by the data, because we lack experimental assignment to catastrophe and pre-event baseline data to check balance, we additionally estimate the effect of the tsunami on time preference using regression discontinuity. This approach ameliorates some additional threats to our identification, such as whether differential exposure to aid workers affected how individuals respond to a survey. Assuming that individuals were not able to perfectly anticipate the high water mark, the individuals who live either immediately above or below the water mark should be balanced on unobservables.

Regression discontinuity estimates, therefore, are unlikely to represent pre-existing differences in preferences or in other unobservables. To obtain the estimate, we first calculate the elevation for each of the respondents in the sample based on their GPS coordinates. Because data on the precise high watermark in all locations do not exist, we identify the high-water mark as the highest elevation at which a respondent reports being affected both in each Grama Niladara and in each 0.05 by 0.05 arc degree fishnet grid. We show below that our approach is robust to estimating the high water mark using both pre-existing (Grama Niladara) units and evenly-sized arbitary fishnet grids.

We estimate the effect of tsunami exposure on time preference using the following regression:

$$Average\ Discount\ Factor_i = \gamma_0 + \gamma_1 Damaged_i + \gamma_2 Water\ Mark_i^2 \tag{3.5}$$

$$+\gamma_3 Water\ Mark_i^3 + \gamma_4 Water\ Mark_i^4 + \gamma_5 X_i + \epsilon_i \tag{3.6}$$

where $Water\ Mark_i^4$ is the distance in meters between individual i and the local high water mark and $Damaged_i$ is instrumented with $Water\ Mark_i$ to provide a fuzzy regression discontinuity estimate.[8]

[8] In results not reported here, we show that our results are not changed using a sharp discontinuity

3.6 Results

3.6.1 Approach 1 - Estimates of Mean Differences Under Random Assignment to Exposure

Table 3.4 reports results for specification 3.7. We cluster standard errors at the Grama Niladara level to account for the high degree of spatial correlation in tsunami exposure. Our estimates are consistent with the tsunami creating an increase in the average monthly discount factor from about 0.8 to about 0.85 or to about 0.9 depending on the specification. This represents roughly a 1/3rd to 2/3rd standard deviation increase in our patience measure. While problems with taking the cardinality of this measure seriously are well-known (see e.g. Andreoni and Sprenger, 2010; Frederick et al., 2002), this provides evidence of patience increasing as a result of the event.

We attempt to control for locational sorting first by including elevation and distance to the coast as proxies for vulnerability in the regression and by using two different sets of fixed effects. In some specifications we add our measure of risk preference elicited using incentivized Holt-Laury MPLS as these are argued to describe some of the variation in time preference (Harrison et al., 2008). Columns 1 - 3 report results with no fixed effects, columns 4 - 6 provide results when dummies are added for the 7 districts represented in the data and columns 7 - 9 include dummies for 87 arbitrary evenly-sized 0.07 arc degree fishnet grids. We find that our result is robust except for in column 9, which is quite demanding on the data as we are attempting to estimate 9 parameters using within variation for geographic divisions that have an average of 5.2 respondents.

3.6.2 Approach 2 - Regression Discontinuity Estimates

Because we lack pre-event data and random assignment, we use data on respondent locations and an estimate of the high water mark to estimate the effect of the tsunami on time preference using regression discontinuity. Figure 3.3, which depicts the distribution of respondents' elevation difference from the high watermark excluding the one worker per GN who resides at exactly the high water mark. Consis-

design.

tent with the tsunami being an imprecisely predicted disaster, we do not observe any clear bunching near the watermark. Figure 3.4 graphs the average elicited discount factor and the estimated 4th-order polynomial along with 95% confidence intervals when the high water mark is calculated within Grama Niladara unit. The continuous distribution in Figure 3.3 and the visually salient discontinuity in Figure 3.4 suggest that we can estimate the causal effect of the tsunami on time preference using a regression discontinuity.

Table 3.5 reports results for specification 3.5. Columns 1 and 2 provide estimates based on calculating the high water mark as the highest elevation within a Grama Niladara division in which a respondent reports damage. Hydrological models of inundation and using satellite imagery to estimate the extent of damage are known to be highly imperfect and highly sensitive to assumptions about wave sizes at landfall (see e.g. Dominey-Howes and Papathoma, 2007; Koshimura et al., 2008). To check the robustness of our result to additional estimations of the high water mark, columns 3 and 4 report results where the high water mark is calculated as the elevation of the highest respondent reporting tsunami damage within small fishnet grids (0.05 by 0.05 arc degrees) of about 5 kilometers by 5 kilometers.[9]. Even in column 3, where the result is not significant at conventional levels, we obtain a p-value of 0.114.

3.6.3 Interpretation

The increase in patience estimated using a comparison of means ranges from 0.048 to 0.072 (ignoring column (9) where we lack the power to obtain precise estimates) and the increase estimated using regression discontinuity ranges from 0.077 to 0.108. The similarity of the coefficients obtained using two different approaches, as well as the evidence supporting the identifying assumption for the first approach and the robustness of those estimates to using within variation from 87 different geographic units provides evidence that exposure to the tsunami increased patience. We now review and test a series of competing explanations to attempt to validate our interpretation of this change as an increase in the taste for delaying gratification.

[9] In results not reported here, we find that our specification is robust to different fishnet grid sizes

3.6.4 Alternative Explanations

In order to establish that our result reflects an increase in the intertemporal elasticity of substitution, we need to show that the effect we find is not the result of some other change in the economy due to the tsunami. The theory we develop above allows us to narrow the list somewhat. In this section, we consider four alternative explanations and provide evidence consistent with our interpretation.

Increases in Savings Demand

It is possible that an increased willingness to delay a payment reflects a desire to replace assets devastated by the tsunami. There are at least four reasons to doubt such an interpretation. First, if the increase in discount factors just reflects an increased demand for savings, then we should observe a larger effect for older individuals who have a shorter remaining working life to replace assets, and, because debt-levels and wages should be correlated over short time spans, individuals who are in debt, and for individuals who currently have a lower wage. Similarly, the effect should be smaller for respondents with other sources of insurance such as large households and spouses who can provide some insurance. Results reported in Table 3.6 show no evidence of heterogeneous effects along any of these dimensions. Second, if an asset replacement motive is dominant, then the effects of being damaged should be on the intensive and not the extensive margin. We find in columns 8 and 9 that including self-reports of the intensity of the damage and on the amount of total asset damage that has been repaired does not affect our result. In Table 3.7, we provide a more rigorous test for activity on the intensive margin by separating our data into bins by levels of aid received and then testing whether the sum of the coefficients on these categories adds to the coefficient on whether an individual was damaged. We reject equality of these effects in all specifications but one where the p-value is 0.108.

Third, the optimal response if the savings motive dominates, as we discussed in section 3.3 is to arbitrage the elicited discount factor to the prevailing interest rate, which does not seem to occur in our data. Last, the question asks about delaying consumption for one month, which should be a negligible horizon two and a half years after the event if the motive is to replace assets to be used much later in life.

The Importance of Incentivized Experiments

A general criticism of our study is that respondents were not paid and so the data may imprecisely measure discount factors. There are two reasons we do not believe that this affects our inference. First, for the lack of incentivization to explain our result, the error in measurement induced in our experimentally-elicited discount factor must be correlated with tsunami exposure. Otherwise, the measurement error resulting from the lack of incentivization should be classical, which should bias us against find significant results by increasing our standard errors. We find this a priori implausible.[10] Second, given the clear effects of disaster exposure on trust document by Cassar et al. (2011) and specifically toward aid providers in Andrabi and Das (2010), it is possible that incentivization might induce an artificial increase in discount factors which just reflects trust that the later payment will in fact be delivered.[11] Given this, we think a hypothetical measure is desirable. We have evidence that this confound does not affect our hypothetical measure because aid provision would have to be perfectly demarcated at our high water mark to explain our result.

Humanitarian Aid and Consumption Paths

To allay concerns that the result is due to the large scale aid response either relaxing liquidity constraints or raising the consumption path of affected workers, Table 3.7 estimates Equation 3.7 on the subset of workers who report receiving less than one days' wage worth of aid support. The effect for this subset is, if anything, larger than for the entire sample and still highly robust. We additionally check this by separating the data into categories by aid levels. Again, we find no evidence that the humanitarian response increased consumption levels and so created an increase in patience.

[10] The risk preference measure in these data was incentivized and in results available on request we find no effects on this measure.

[11] A front-end payment delay design can be used to mitigate this concern somewhat, but it cannot eliminate the concern entirely.

3.7 Why Does Exposure Increase Patience?

The capacity to be patient may require investments over time. While many studies show that preferences are remarkably stable (see Mischel et al. (1989) for the best-known example), it may be that small increases can be made through investments in the capacity to visualize the future and by learning through experience the benefits that come from delaying gratification as suggested by Becker and Mulligan (1997). Moreover, studies which show extreme stability are not focused on populations experiencing dislocations of a comparable magnitude. Our finding, which we discuss below, that the tsunami appears to have generate the largest change for the least educated and the shortest, for instance, suggests that on a separate richer and more educated sample, the tsunami may have had no detectable effect at all. We examine heterogenous exposure effects by running regressions of the form:

$$\begin{aligned} Average\ Discount\ Factor_i &= \psi_0 + \psi_1 Damaged_i + \psi_2 Input_i \qquad (3.7) \\ &+ \psi_3 Damaged_i \cdot Input_i + \psi_4 X_i + \epsilon_i \qquad (3.8) \end{aligned}$$

Where $Input_i$ are variables that may plausibly generate patience over time such as education, performance on a test of cognitive skill, and height, which is a well-documented biometric measure of lifetime health. Table 3.8 reports the results of this regression. In column 1, we see that, relative to uneducated workers and workers with only a primary education, workers with a secondary and tertiary education are much less likely to exhibit a change in patience due to tsunami exposure. Likewise, we find that workers with a z-score of 1 (based on the distribution in our sample) have response that is half as large as average workers with a z-score of 0. Column 8, which interacts damage with a self-assessment of meticulousness, provides some weak evidence that the heterogeneous chance is not a result of less educated and shorter workers being less well-prepared for the event. We take this evidence as being consistent with the view that the tsunami increased patience by increasing respondents awareness of the importance of delaying gratification.

We can also directly test the prediction that the least patient are driving the result by noting that if our identifying assumption holds, we can view the unaffected distribution as the counterfactual to the affected distribution. In Appendix B, we review the assumptions that need to hold in order for us to interpret our estimates as Quantile Treatment Effects (QTEs). We see in Figures 3.5 and 3.6 that the most dramatic increases are for individuals in the left tail. Quantile regression allows us to estimate these effects directly. Moreover, as we now detail, the first-order stochastic dominance that we observe when we plot the CDF of preferences for affected an unaffected workers, the figure suggests that the QTE may be a lower bound estimate of the treatment effect for individuals.

In Table 3.9, we see that the difference between affected and unaffected workers is indeed monotonically decreasing in quantiles. The effect for the 10th percentile is as large as 0.178, or a full standard deviation. Provided that we an interpret these as QTEs, this suggests that patience has increased most dramatically for individuals in the left-tail of the distribution, consistent with the event being a substitute for other inputs which contribute to preference formation.

3.8 Conclusion

Explanations for the discounting of future utility have a long history in economic research. However, only recently have researchers had the data to test for systematic causes of heterogeneity in economic preference. This approach, rather than relying on introspection, forces the researcher to provide credible documentation that the hypothesized determinants of preference create differences *causally*.

In this paper we provide evidence that, contrary to a standard modeling assumption, preferences are systematically affected by life experience. Our data provide measures of time preference for a population that was severely affected by a major disaster and also permit us to exploit exogenous variation in exposure to that disaster to estimate its' effect on preferences. These data therefore allow us to attempt a stringent test of whether preference parameters can be linked in a systematic way to life experience.

We find that exposure increases patience using two different empirical strategies. A battery of tests supports that these changes are indeed attributable to a change in preferences and not to other changes in the economic environment. Complementing this, we find preliminary evidence that experiencing the event appears to substitute for other inputs to preference formation such as education. It is our hope that these results continue to open the door for a broader agenda which seeks to understand sources of heterogeneity in economic preference and that relies not only on differences in prices and budgets, but also on systematic and well-understood differences in preferences to explain differences in economic behavior.

Table 3.1: Summary Statistics for Variables Related to Tsunami Impact

Variable	Average Full Sample	Average Affected	# Obs. Full sample	# Obs. Affected
Average Discount Factor	0.819	0.853	456	153
	[0.155]	[0.131]		
Distance to coast (KMs)	17.685	16.775	456	153
	[12.241]	[11.275]		
Elevation (100M)	1.033	1.022	456	153
	[2.331]	[0.622]		
Share Damaged in Same Grid	0.336	0.66	456	153
	[0.33]	[0.262]		
Monthly Wage (1,000 rupees)	9.970	9.370	456	153
	[6.266]	[5.813]		
Percent of Damage Repaired	20.349	60.647	456	153
	[32.811]	[27.616]		
Damages (1,000 rupees)	168.911	507.851	454	151
	[727.179]	[1193.174]		
Recovery funds (1,000 rupees)	51.203	152.606	456	153
	[135.651]	[198.762]		
Coef. Relative Risk Aversion	1.154	0.581	448	149
	[6.343]	[6.037]		
Recovery funds (1,000 rupees)	51.203	152.606	456	153
	[135.651]	[198.762]		

Notes: Data are from a survey of wage workers conducted in July 2007. Standard deviations are in brackets. Elevations are calculated using the United States Geological Survey Center for Earth Resources and Observation Sciences (EROS) 30 arc second x 30 arc second (approximately 1KM) Digital Elevation Model. In results available on request we show that risk preference does not appear to respond to the tsunami in our sample, though Cameron and Shah (2010a) and Cassar et al. (2011) do find an increase in risk aversion.

Table 3.2: T-tests of Equality for Affected and Unaffected Workers

	Not Damaged (ND)	Damaged (D)	Difference (D) - (ND)	p-value $h_0 : (D) = (ND)$
Gender (Female=1)	0.370	0.333	-0.036	0.446
	[0.484]	[0.473]	(0.048)	
Years of education	10.403	10.510	0.107	0.719
	[3.107]	[2.770]	(0.297)	
Household size	4.495	4.549	0.054	0.741
	[1.715]	[1.504]	(0.163)	
Marital status (Married=1)	0.690	0.752	0.062	0.170
	[0.463]	[0.433]	(0.045)	
Age	37.096	38.497	1.401	0.231
	[11.797]	[11.726]	(1.168)	
Digit span recall	6.602	6.376	-0.226	0.135
	[1.571]	[1.368]	(0.151)	
Father's years of education	7.543	7.976	0.433	0.242
	[3.504]	[3.171]	(0.369)	
Sinhalese (=1)	0.937	0.961	0.023	0.299
	[0.243]	[0.195]	(0.023)	
English speaker (=1)	0.142	0.124	-0.018	0.603
	[0.350]	[0.331]	(0.034)	
Tamil speaker (=1)	0.069	0.059	-0.010	0.671
	[0.254]	[0.236]	(0.025)	
Hindu (=1)	0.007	0.000	-0.007	0.315
	[0.081]	[0.000]	(0.007)	
Muslim (=1)	0.056	0.033	-0.023	0.271
	[0.231]	[0.178]	(0.021)	
Buddhist (=1)	0.911	0.922	0.011	0.701
	[0.285]	[0.270]	(0.028)	
Wage work pre-tsunami	0.604	0.667	0.063	0.192
	[0.490]	[0.473]	(0.048)	
Casual worker pre-tsunami	0.370	0.314	-0.056	0.238
	[0.484]	[0.466]	(0.047)	
Self-employed pre-tsunami	0.066	0.052	-0.014	0.565
	[0.249]	[0.223]	(0.024)	
Apprentice pre-tsunami	0.033	0.033	-0.000	0.985
	[0.179]	[0.178]	(0.018)	
Worked overseas pre-tsunami	0.023	0.026	0.003	0.842
	[0.150]	[0.160]	(0.015)	

Notes: Data are from a survey of wage workers conducted in July 2007. Standard deviations are in brackets and standard errors are in parentheses.

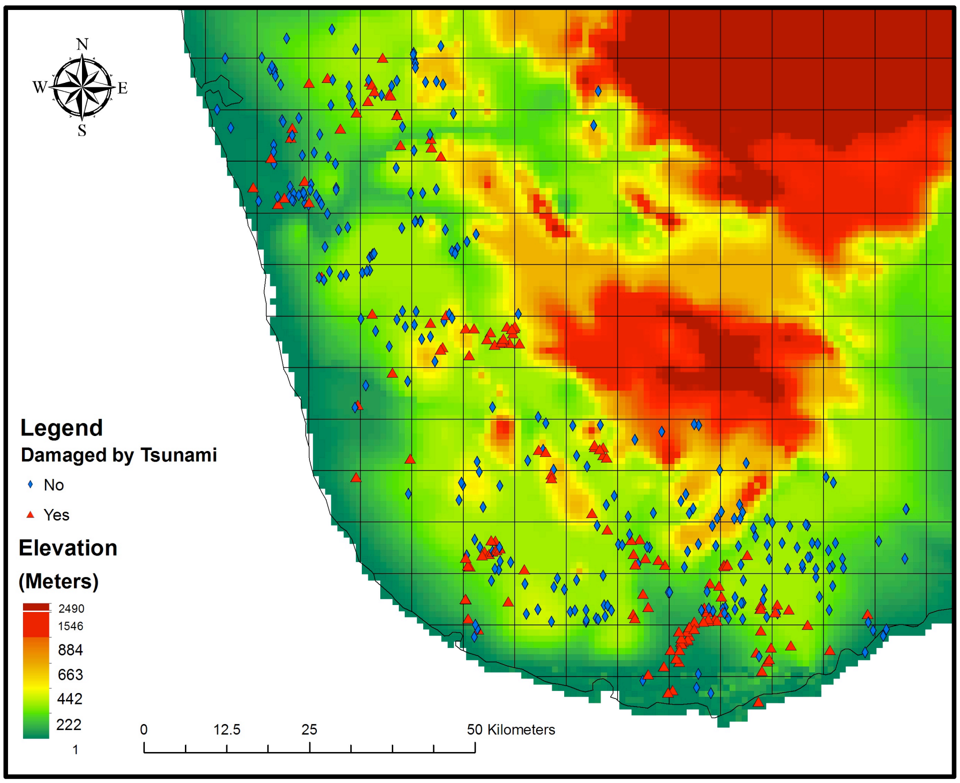

Figure 3.1: 0.07 by 0.07 Arc Degree Fishnet Grids

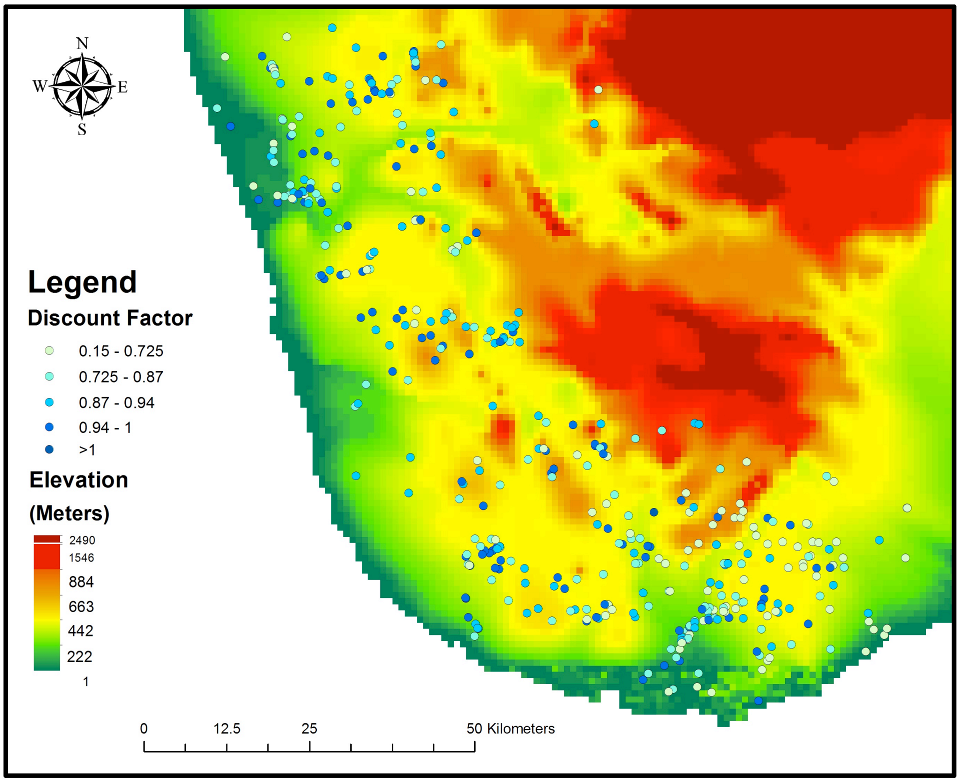

Figure 3.2: The Spatial Distribution of Discount Factors

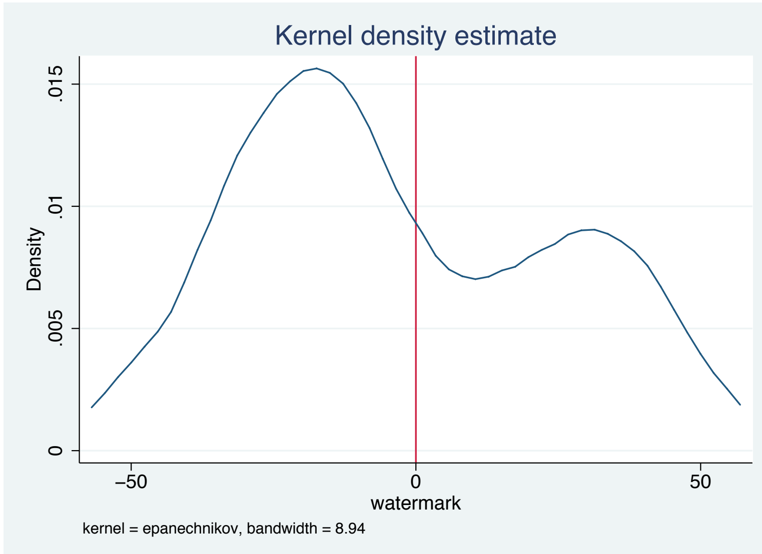

Figure 3.3: Regression Discontinuity Evidence 1

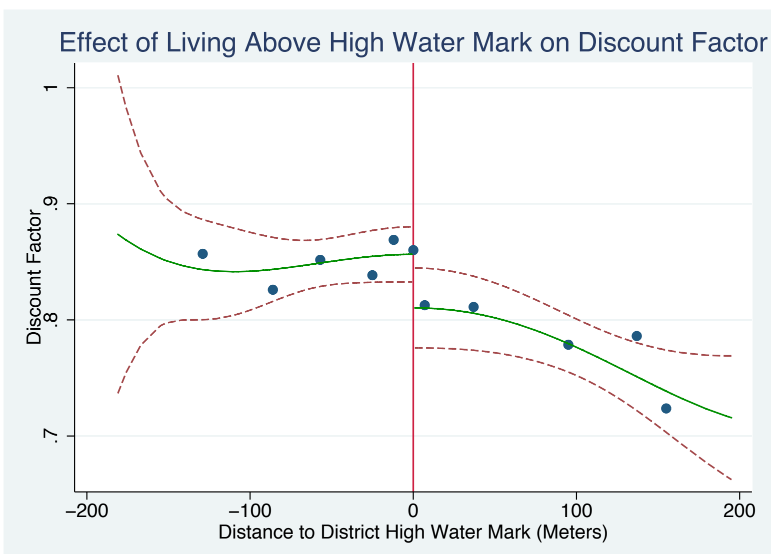

Figure 3.4: Regression Discontinuity Evidence 2

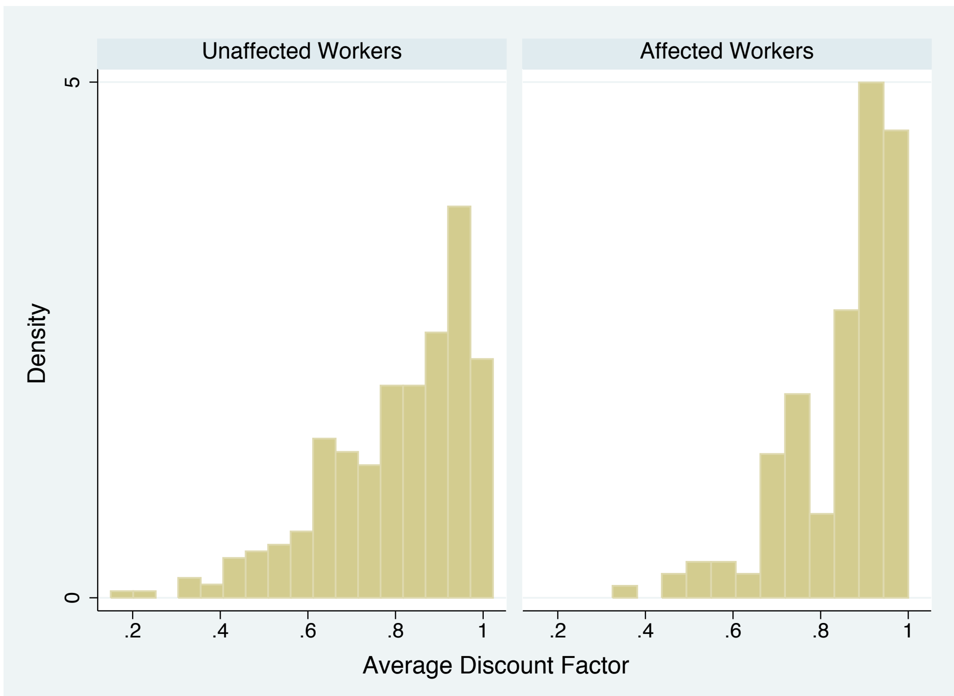

Figure 3.5: Tsunami Exposure and the Distribution of Preferences 1
Notes: Data are from a survey of wage workers conducted in July 2007. The average discount factor is the average of responses to four hypothetical survey questions: the amount required today to forego m rupees in one month and the amount required in 5 months to forego m in 6 months where $m \in \{5000, 10000\}$ rupees.

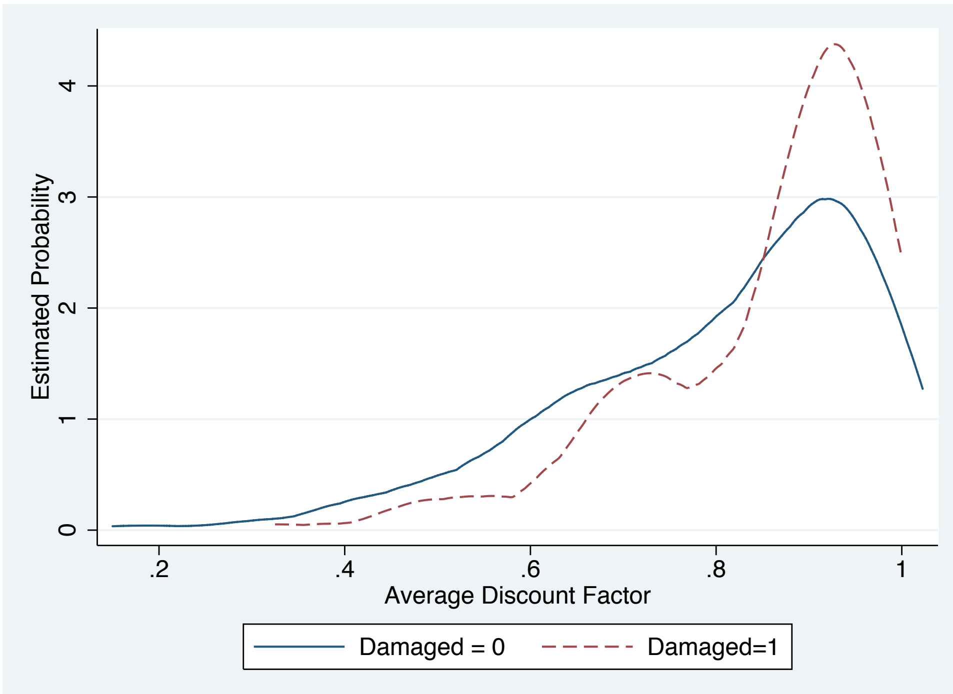

Figure 3.6: Tsunami Exposure and the Distribution of Preferences 2

Notes: Data are from a survey of wage workers conducted in July 2007. The average discount factor is the average of responses to four hypothetical survey questions: the amount required today to forego m rupees in one month and the amount required in 5 months to forego m in 6 months where $m \in \{5000, 10000\}$ rupees.

Table 3.3: Tsunami Vulnerability and Discounting for Unaffected Workers

Dependent Variable	Average Discount Factor, Not Damaged (ND) Sample				
	(1)	(2)	(3)	(4)	(5)
Distance to coast (KMs)	0.001			0.001	0.004
	(0.001)			(0.001)	(0.006)
	[0.001]			[0.001]	[0.005]
Elevation (100M)		-0.000		0.001	-0.026
		(0.001)		(0.002)	(0.028)
		[0.001]		[0.002]	[0.029]
Share Damaged in Same Grid			0.016	0.020	
			(0.039)	(0.039)	
			[0.041]	[0.041]	
Constant	0.787***	0.802***	0.799***	0.783***	0.761***
	(0.016)	(0.009)	(0.012)	(0.019)	(0.100)
	[0.027]	[0.016]	[0.017]	[0.029]	[0.083]
Fishnet Grid Effects	No	No	No	No	Yes
R-squared	0.004	0.000	0.000	0.004	0.407
# Observations	303	303	303	303	303
p-value (joint signficiance)					
No Clustering	0.258	0.952	0.676	0.466	0.617
GN Clustered SEs	0.373	0.954	0.687	0.674	0.645

Notes: *Level of significance*: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Data are from a survey of wage workers conducted in July 2007. Robust standard errors reported in parentheses and robust standard errors clustered at the Grama Niladara (GN) level reported in brackets. The sample is restricted to individuals reporting no damage. The average discount factor is the average of responses to four hypothetical survey questions: the amount required today to forego m rupees in one month and the amount required in 5 months to forego m in 6 months where $m \in \{5000, 10000\}$ rupees.

Table 3.4: The Effect of Tsunami Exposure on Elicited Discount Factors

Dependent Variable	Average Discount Factor								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Damaged (=1)	0.052**	0.052***	0.054*	0.072***	0.073***	0.063**	0.048**	0.050**	0.022
	(0.020)	(0.019)	(0.032)	(0.018)	(0.018)	(0.031)	(0.019)	(0.019)	(0.025)
Coef. Relative Risk Aversion		0.002**	0.002		0.001	0.001		-0.001	-0.001
		(0.001)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)
Optimism Index		-0.012	-0.017*		-0.010	-0.015*		-0.007	-0.012
		(0.010)	(0.009)		(0.009)	(0.008)		(0.008)	(0.007)
Elevation (Meters)			0.000			0.000			-0.000
			(0.000)			(0.000)			(0.000)
Distance to coast (KMs)			0.001			0.001			0.001
			(0.001)			(0.001)			(0.004)
Monthly Wage (1,000 rupees)			0.003**			0.003**			0.002
			(0.001)			(0.001)			(0.001)
Percent of Damage Repaired			0.000			0.000			0.000
			(0.000)			(0.000)			(0.000)
Damages (1,000 rupees)			0.000**			0.000**			0.000
			(0.000)			(0.000)			(0.000)
Recovery funds (1,000 rupees)			-0.000			-0.000			-0.000
			(0.000)			(0.000)			(0.000)
Constant	0.802***	0.799***	0.746***	0.795***	0.793***	0.749***	0.803***	0.803***	0.773***
	(0.016)	(0.016)	(0.030)	(0.013)	(0.013)	(0.027)	(0.009)	(0.009)	(0.069)
Fixed Effects	No	No	No	DS	DS	DS	FN	FN	FN
R-squared	0.025	0.042	0.076	0.073	0.088	0.116	0.345	0.360	0.377
# Observations	456	448	446	456	448	446	456	448	446
# Clusters	52	52	52	52	52	52	52	52	52

Notes: *Level of significance*: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Data are from a survey of wage workers conducted in July 2007. Standard errors clustered at the Grama Niladara level reported in parentheses. We calculate the optimism index as the first principal component of four questions related to expectations about the future. The fixed effect samples are DS = District (7 divisions) and FN = 0.07 x 0.07 Arc Degree Fishnet Grids (87 divisions). The coefficient of relative risk aversion is elicited using incentivized Holt-Laury price lists. The average discount factor is the average of responses to four hypothetical survey questions: the amount required today to forego m rupees in one month and the amount required in 5 months to forego m in 6 months where $m \in \{5000, 10000\}$ rupees.

Table 3.5: Fuzzy Regression Discontinuity Results

Dependent Variable	Average Discount Factor			
	(1)	(2)	(3)	(4)
Damaged (=1)	0.108***	0.089**	0.077	0.092*
	(0.038)	(0.036)	(0.049)	(0.050)
Distance to Water Mark^2	-0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Distance to Water Mark^3	-0.000*	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Distance to Water Mark^4	0.000**	0.000**	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.790***	0.692***	0.794***	0.672***
	(0.019)	(0.051)	(0.028)	(0.052)
Division	GN	GN	FN	FN
Extra Controls	No	Full	No	Full
Estimation	TSLS	TSLS	TSLS	TSLS
First Stage (F-Stat)	81.587	80.08	46.747	45.63
R-squared	0.009	0.091	0.021	0.077
N	442	434	442	434

Notes: *Level of significance*: $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Robust standard errors reported in parentheses. *Damaged* is instrumented with the distance to the high water mark, which provides the fuzzy regression discontinuity estimate. The average discount factor is the average of responses to four hypothetical survey questions: the amount required today to forego m rupees in one month and the amount required in 5 months to forego m in 6 months where $m \in \{5000, 10000\}$ rupees. The high water mark is calculated as the highest elevation at which someone within the cluster reports being hit where the clusters are GN = Grama Niladara and FN = 0.05 x 0.05 Arc Degree Fishnet Grids. The full set of controls are elevation, distance to the coast, monthly wage, CRRA, optimism index, gender, marriage status, years of education, household size, and age.

Table 3.6: Do Increases in Discount Factors Reflect a Change in the Demand for Savings?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Damaged (=1)	0.052**	0.042*	0.053**	0.058*	0.079**	0.065**	0.051**
	(0.020)	(0.022)	(0.024)	(0.034)	(0.035)	(0.032)	(0.020)
Currently has a loan (=1)	0.007						
	(0.017)						
Damaged x Currently has a loan	-0.003						
	(0.028)						
Over 35 years old		-0.019					
		(0.023)					
Damaged x over 35		0.019					
		(0.030)					
More than 4 HH Members			0.006				
			(0.020)				
Damaged x More the 4 HH members			-0.003				
			(0.029)				
Marital status (Married=1)				0.032*			
				(0.018)			
Damaged x Married				-0.011			
				(0.033)			
Monthly Wage (1,000 rupees)					0.004***		
					(0.001)		
Damaged x Monthly Wage					-0.003		
					(0.002)		
Percent of Damage Repaired						-0.000	
						(0.000)	
Damages (1,000 rupees)							0.000
							(0.000)
Constant	0.800***	0.811***	0.799***	0.780***	0.759***	0.802***	0.802***
	(0.016)	(0.017)	(0.017)	(0.020)	(0.026)	(0.016)	(0.016)
R-squared	0.025	0.027	0.025	0.032	0.046	0.025	0.029
# Observations	456	456	456	456	456	456	454
# Clusters	52	52	52	52	52	52	52

Notes: *Level of significance*: $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Data are from a survey of wage workers conducted in July 2007. Standard errors clustered at the Grama Niladara level reported in parentheses. The average discount factor is the average of responses to four hypothetical survey questions: the amount required today to forego m rupees in one month and the amount required in 5 months to forego m in 6 months where $m \in \{5000, 10000\}$ rupees.

Table 3.7: Do Savings Demand or Decreases in the Marginal Utility of Consumption Explain the Result?

Dependent Variable	Average Discount Factor					
	(1)	(2)	(3)	(4)	(5)	(6)
Damaged (=1)	0.101***	0.082**	0.101***	0.099***	0.101***	0.099***
	(0.035)	(0.039)	(0.036)	(0.036)	(0.036)	(0.036)
Coef. Relative Risk Aversion		0.002		0.002		0.002*
		(0.001)		(0.001)		(0.001)
Optimism Index		-0.020*		-0.014		-0.013
		(0.011)		(0.010)		(0.010)
Recovery funds b/w 3,000 Rs. and 9,000 Rs.			-0.021	-0.002	-0.032	-0.013
			(0.037)	(0.038)	(0.039)	(0.041)
Recovery Funds b/w 9,000 Rs. and 100,000 Rs.			-0.061*	-0.050	-0.065*	-0.054
			(0.036)	(0.035)	(0.035)	(0.035)
Recovery Funds b/w 100,000 Rs. and 150,000 Rs.			-0.089*	-0.093*	-0.052	-0.056
			(0.049)	(0.048)	(0.039)	(0.040)
Recovery Funds Greater Than 150,000 Rs.			-0.039	-0.031	-0.045	-0.038
			(0.038)	(0.040)	(0.041)	(0.042)
Constant	0.802***	0.657***	0.802***	0.742***	0.802***	0.743***
	(0.016)	(0.058)	(0.017)	(0.032)	(0.017)	(0.031)
Sample	Low Aid	Low Aid	Aid$\leq$Damage	Aid$\leq$Damage	Full	Full
p-value: $\beta_{damaged} = \beta_2$			0.019	0.032	0.016	0.027
p-value: $\beta_{damaged} = \sum_{i=1}^{4} \beta_i$			0.051	0.095	0.061	0.108
R-squared	0.019	0.103	0.031	0.076	0.030	0.072
# Observations	320	315	430	423	456	448
# Clusters	48	48	52	52	52	52

Notes: *Level of significance*: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Data are from a survey of wage workers conducted in July 2007. Standard errors clustered at the Grama Niladara level reported in parentheses. The average discount factor is the average of responses to four hypothetical survey questions: the amount required today to forego m rupees in one month and the amount required in 5 months to forego m in 6 months where $m \in \{5000, 10000\}$ rupees. The low aid sample reports receiving less than 3,000 Rs. The aid $\leq$ damage sample reports receiving tsunami relief that is less than or equal to the amount they report losing in the tsunami. The omitted category in columns 3 - 6 corresponds to the low aid sample.

Table 3.8: Why Do Preferences Change in Response to Catastrophe?

	(1)	(2)	(3)	(4)	(5)
Damaged (=1)	0.130***	0.052**	0.046**	0.125*	0.276***
	(0.043)	(0.020)	(0.020)	(0.071)	(0.096)
Secondary Education	0.081				
	(0.058)				
Higher Education	0.091**				
	(0.045)				
Damaged x Secondary Education (=1)	-0.082				
	(0.066)				
Damaged x Higher Education (=1)	-0.089*				
	(0.049)				
Height (z-score)		0.026**			
		(0.011)			
Damaged x Height (z-score)		-0.030*			
		(0.017)			
Had a dirt floor growing up (=1)			-0.039*		
			(0.021)		
Damaged x Had a Dirt Floor			0.022		
			(0.024)		
Digit span recall				0.010	0.031***
				(0.006)	(0.011)
Damaged x Digit Span				-0.012	-0.039**
				(0.011)	(0.016)
Constant	0.720***	0.801***	0.813***	0.738***	0.624***
	(0.043)	(0.017)	(0.015)	(0.041)	(0.068)
Sample	Full	Full	Full	Full	Digit Span < 8
R-squared	0.042	0.046	0.045	0.030	0.049
# Observations	456	456	448	448	334
# Clusters	52	52	52	52	48

Notes: *Level of significance*: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Robust standard errors reported in parentheses. The sample is restricted to individuals reporting no damaged. The probability of tsunami exposure for a respondent is calculated as the share of damaged respondents in the same Grama Niladara exlcuding the respondent. The coefficient of relative risk aversion is elicited using incentivized Holt-Laury price lists. The average discount factor is the average of responses to four hypothetical survey questions: the amount required today to forego m rupees in one month and the amount required in 5 months to forego m in 6 months where $m \in \{5000, 10000\}$ rupees.

Table 3.9: Tsunami Exposure and the Distribution of Preferences

	Desc. Stats		Quantile Regression Estimates					OLS Est.
Dependent Variable	Mean	SD	0.1	0.25	0.5	0.75	0.9	
Panel A - No Controls								
Average Discount Factor	0.819	0.155	0.100***	0.073**	0.050***	0.020**	0.000	0.052***
			(0.033)	(0.036)	(0.017)	(0.008)	(0.007)	(0.014)
Current Discount Factor	0.803	0.184	0.050	0.100**	0.050***	0.015	0.000	0.046***
			(0.037)	(0.044)	(0.013)	(0.012)	(0.011)	(0.018)
Future Discount Factor	0.834	0.164	0.150***	0.150***	0.050**	0.000	0.000	0.057***
			(0.052)	(0.029)	(0.024)	(0.010)	(0.010)	(0.014)
Panel B - Controls								
Average Discount Factor	0.819	0.155	0.178**	0.044	0.065*	0.025*	0.009	0.069**
			(0.088)	(0.061)	(0.034)	(0.014)	(0.017)	(0.029)
Current Discount Factor	0.803	0.184	0.120	0.102	0.070**	0.025	0.024*	0.083**
			(0.091)	(0.080)	(0.034)	(0.023)	(0.013)	(0.035)
Future Discount Factor	0.834	0.164	0.163	0.113*	0.038	0.010	-0.005	0.056*
			(0.139)	(0.060)	(0.029)	(0.015)	(0.016)	(0.032)

Notes: *Level of significance*: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Bootstrapped standard errors based on 1000 iterations and robust to clustering at the Grama Niladara reported in parentheses. The controls in our second specification are elevation, distance to the coast, monthly wage (1,000 rupees), the percentage of tsunami damages repaired, and years of education. The current (future) discount factors are calculated as the average of the amount required today (in five months) to forego 5000 rupees in one month (six months) and the average required today (in five months) to forego 10000 rupees in one month (six months) each divided by the respective foregone amounts. The average discount factor is the average of current and future discount factors.

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