Journal of Economic Behavior & Organization 118 (2015) 199–214

Contents lists available at ScienceDirect

Journal of Economic Behavior & Organization

journal homepage: www.elsevier.com/locate/jebo

Catastrophes and time preference: Evidence from the Indian Ocean Earthquake☆

Michael Callen *

Harvard Kennedy School, United States

ARTICLE INFO

Article history:
Received 27 February 2014
Received in revised form
17 November 2014
Accepted 22 February 2015
Available online 18 March 2015

JEL classification:
D91
Q54

Keywords:
Natural disasters
Time preference
Preference stability
Regression discontinuity

ABSTRACT

We provide evidence suggesting that exposure to the Indian Ocean Earthquake tsunami increased patience in a sample of Sri Lankan wage workers. We develop a framework to characterize the various channels through which disaster exposure could affect measures of patience. Drawing on this framework, we show that a battery of empirical tests support the argument that the increase in measured patience reflects a change in time preference and not selective exposure to the event, migration related to the tsunami, or other changes in the economic environment which affect experimental patience measures. The results have implications for policies aimed at disaster recovery and for the literature linking life events to economic preferences.

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, health and education investments made for children, 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 consistent with measured discount factors *increasing* in response to the experience of a major natural disaster. Measures of time preference, in addition to measuring the rate of time preference, also reflect beliefs about the future, background consumption, and intertemporal arbitrage opportunities. Using a simple model that relates underlying preferences and this

☆ I am grateful to Suresh de Mel, David McKenzie, and Christopher Woodruff for kindly sharing their data and to James Andreoni, Eli Berman, Alessandra Cassar, Marjorie Flavin, Gordon Dahl, Peter Kuhn, Leah Nelson, Sarah Reber, Charles Sprenger, Craig McIntosh, Christopher Woodruff, Hal White, and Carrie Wenjing Xu, as well as seminar participants at the University of San Francisco, the 2011 Pacific Development Conference at UC Berkeley, and the 2011 Southern California Conference in Applied Microeconomics at Claremont McKenna for many helpful insights. Callen acknowledges graduate student support from the Air Force Office of Scientific Research (AFOSR) under award FA9550-09-1-0314. All mistakes are my own.

* Correspondence to: Harvard University, Harvard Kennedy School, 79 JFK Street, Cambridge, MA 02138, United States. Tel.: +1 617 495 9965.
E-mail address: michael_callen@hks.harvard.edu

http://dx.doi.org/10.1016/j.jebo.2015.02.019
0167-2681/

set of confounds to revealed laboratory measures, we show that the increase in patience we observe is only consistent with a change in underlying preferences or a large, and implausible, decrease in the return to capital from the tsunami.

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 (Voors et al., 2012; Bchir and Willinger, 2013; Willinger et al., 2013; Cameron and Shah, 2013; Cassar et al., 2013; Malmendier and Nagel, 2011; Meier and Sprenger, forthcoming; Callen et al., 2014) 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 m were recorded and the UN reports 229,866 total individuals lost or missing. In Sri Lanka, 35,322 individuals were killed and 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.[3] 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.

There are two principal issues confronting attempts to establish the causal effect of disasters on economic preferences using data collected after a disaster. The first is that individuals may locate according to their preferences, which we call *selective exposure*. In our case, patient workers may select areas vulnerable to tsunami inundation. Second, in the wake of the disaster, individuals might selectively migrate out of affected zones based on their preferences. We call this *selective migration*.

We lack both longitudinal data tracking individuals before and after the disaster and random assignment and so cannot rule out these confounds. However, several results indicate that these concerns do not account for the increase in patience that we observe. First, the implied magnitude of the increase is robust to using variation only within small arbitrary spatial divisions (fishnet grids) and to a regression discontinuity design based on distance from the high water mark, which should control of additional differences that relate to proximity to the disaster. Second, we find affected and unaffected workers are balanced on a broad range of time-invariant observable characteristics. Third, the share of respondents living in the same community since birth is not related to tsunami exposure, indicating no major effect on migration patterns in our sample two years after the event. Fourth, 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 patient individuals are disproportionately vulnerable (contrary to *selective exposure*) and find comparable shares of individuals living in the same community since birth (contrary to *selective migration*).

To establish that the change we observe reflects a change in preferences we also need to consider a set of competing explanations. To precisely specify potential confounds, we develop a framework to characterize the various channels through which disaster exposure could affect measures of patience. It is possible that because survey questions which ask about consumption trade-offs over time can only measure *time discounting*, which reflects a broader range of considerations than the parameter we are interested – *time preference*, the effect we estimate does not truly reflect a change in tastes. To counter

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

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

[3] The full set of these results are available in Appendix Section A2.

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. In light of the result from the model that an *increase* in measured patience is only consistent with an actual change in *time preference*, we interpret the increase in the measure due to the disaster as reflecting an increase in patience.

The remainder of the paper is organized as follows. Section 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 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 4 provides an overview of our data. Section 5 reviews our empirical strategy. Section 6 documents the effect of the catastrophe on preferences, and Section 7 concludes.

2. Literature

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, 2012). 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.[4] 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 pure rate of time preference.

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.[5] Malmendier and Nagel (2011) 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. Callen et al. (2014) provide evidence that exposure to violence in Afghanistan, while it does not lastingly alter preferences *per se*, does appear to make risk preferences more vulnerable to the recollection of prior violent episodes. Meier and Sprenger (forthcoming) 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 (2010) provide evidence that historical experiences and personal histories shape preferences for social equity. Along these lines, Fisman et al. (2013) find that the "Great Recession" increased both laboratory measures of selfishness and of the relative preference for equity over equality. Castillo and Carter (2011) look at the effects of Hurricane Mitch in rural Honduras. They find that intermediate shocks increase coordination in an anonymous interaction game, while extreme events reduce such coordination.

The second line of research focuses on the economic implications of disasters and of 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

[4] Andreoni and Sprenger (2012) provide an alternative elicitation procedure which can directly measure time preference.

[5] We point interested readers to Chuang and Schechter (2014), who provide an excellent and comprehensive review of this literature. Here, we review contributions most directly linked to our study.

rebuild microenterprises (de Mel et al., 2011). The papers most closely linked to this study are Cameron and Shah (2013), Cassar et al. (2013), and Aubert and Aubert (2013). These three papers look for changes in experimentally elicited preference measures as a result of experiencing disasters. Cameron and Shah (2013) and Cassar et al. (2013) find an increase in risk aversion using measures of risk aversion elicited through incentivized choice experiments. Aubert and Aubert (2013) find a decrease in risk aversion in the losses domain. 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 (2013) and Cassar et al. (2013) to issues of respondent comprehension and to careful measurement, we direct readers interested in the risk preference effects of disasters to those studies.[6]

The primary difference between the current study and both Cameron and Shah (2013) and Cassar et al. (2013) 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 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 (2013) and Cassar et al. (2013), 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 the identifying assumptions common to our paper and to Cameron and Shah (2013) and Cassar et al. (2013) – whether *selective migration* and *selective exposure* are present in our sample. Based on this, we argue for 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. Bchir and Willinger (2013) and Willinger et al. (2013) provide results on the effects of lahars – mudflows that emanate from volcanoes – on risk and time preferences. Bchir and Willinger (2013) examine the effects of lahars in Peru and Willinger et al. (2013) examine the impacts in Java. The approach, in both papers, overcomes the selective migration issue, as they observe preferences both before and after the lahars. Bchir and Willinger (2013) find an increase in risk tolerance and impatience. Willinger et al. (2013) find an overall increase in risk tolerance and both increases and decreases in time preferences within subjects across time.

In addition to changes in trust, trust worthiness, and MPL measures of risk preference, Cassar et al. (2013) 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.[7] The measure which is 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, 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. (2013) 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. 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.

3.1. Predictions of decreased patience

3.1.1. 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

[6] 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 consistent to the lack of a difference found in Hurricane Katrina victims one year after the event in Eckel et al. (2009). A potential explanation for our lack of a result is that both Andrabi and Das (2010) and Cassar et al. (2013) 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.

[7] Given the evidence for random assignment to calamity in this paper as well as in Andrabi and Das (2010), Cameron and Shah (2013), Cassar et al. (2013), 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.

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, principally through parental investments in children. 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.

Another parameter that might be affected by the tsunami are interest rates. We extend our theory to consider interest rates in Appendix Section A4.

3.1.2. *Concave utility and uncertainty – predictions from theory*

As we describe more fully in Appendix A6 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 defined over consumption c and, without loss of generality, $u(0) = 0$ so that the respondent receives no utility if they do not consume. 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 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:

$$u(c_0 + x) + \delta E[u(c_1)] = u(c_0) + \delta E[u(c_1 + m)] \tag{1}$$

which, can be rewritten as

$$u(c_0 + x) - u(c_0) = \delta p[u(c_1 + m) - u(c_1)]. \tag{2}$$

by noting that $u(0) = 0$ and $E[u(c_1)] = pu(c_1) + (1 - p)u(c_0)$. Eq. (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 $(x/m) < 1$ 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:* $(\partial x)/(\partial p) > 0$.

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 Eq. (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 (2013) 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.[8] 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

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

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 (Eq. (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.

3.2. Predictions of increased patience

Prediction 3. *Increases in the pure rate of time preference should increase elicited discount factors:* $(\partial x)/(\partial\delta) > 0$.

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 Eq. (2) with respect to δ.

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

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.[9] 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 1 reports summary statistics of variables that are affected by the tsunami and Table 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.

[8] Our optimism index is the first principal component of subjects' indication of the degree they agree with the following three statements (measured on a five point Likert scale where 5 means "strongly agree" and "1 means strongly disagree"): (1) "In uncertain times I usually expect the best"; (2) "I'm always optimistic about my future"; (3) "Overall I expect more good things to happen to me than bad".

[9] Suresh de Mel, David McKenzie, and Christopher Woodruff were the Principal Investigators for the project that yielded these data, which they have kindly shared with us. Their study aimed to explore and document the process of post-tsunami reconstruction (de Mel et al., 2011). The survey and laboratory tasks were translated by Professor de Mel, who is a native Sri Lankan. An advantage of using their data is that it provides remarkable geographic coverage and was obtained in the immediate aftermath of the study.

Table 1
Summary statistics for variables related to tsunami impact.

Variable	Full sample	Not affected	Affected	Difference	p-Value
Average discount factor	0.819	0.802	0.853	0.052	0.001
	[0.155]	[0.164]	[0.131]	(0.015)	
Distance to coast (km)	17.685	18.145	16.775	−1.371	0.259
	[12.241]	[12.694	[11.275]	(1.214)	
Elevation (100 m)	1.033	1.038	1.022	−0.016	0.946
	[2.331]	[2.827]	[0.622]	(0.231)	
Share damaged in same grid	0.336	0.172	0.660	0.488	0.000
	[0.33]	[0.223]	[0.262]	(0.023)	
Monthly wage (1000 rupees)	9.970	10.273	9.370	−0.904	0.146
	[6.266]	[6.470]	[5.813]	(0.621)	
Percent of damage repaired	20.349	0.000	60.647	60.647	0.000
	[32.811]	[0.000]	[27.616]	(1.585)	
Damages (1000 rupees)	168.911	0.000	507.851	507.851	0.000
	[727.179]	[0.000]	[1193.174]	(68.470)	
Recovery funds (1000 rupees)	51.203	0.000	152.606	152.606	0.000
	[135.651]	[0.000]	[198.762]	(11.406)	
Coefficient of relative risk aversion	1.154	1.440	0.581	−0.858	0.177
	[6.343]	[6.481]	[6.037]	(0.636)	

Notes: Data are from a survey of wage workers conducted in July 2007. The table reports means, with corresponding standard deviations in brackets and standard errors in parentheses. Elevations are calculated using the United States Geological Survey Center for Earth Resources and Observation Sciences (EROS) 30 arc second × 30 arc second (approximately 1 km) Digital Elevation Model. The point estimate corresponding to the mean difference between subjects affected and not affected by the tsunami for the coefficient of relative risk aversion suggests that the event decreased risk aversion, though this is not statistically significant. Cameron and Shah (2013) and Cassar et al. (2013) find a statistically significant increase in risk aversion. The data reflect 456 subjects in total. 153 of the 456 subjects were affected by the tsunami.

Table 2
t-Tests of equality for affected and unaffected workers.

	Not damaged (ND)	Damaged (D)	Difference $(D)-(ND)$	p-Value $h_0:(D)=(ND)$
In Same Job Since Before Tsunami (=1)	0.683	0.719	0.036	0.434
	[0.466]	[0.451]	(0.046)	
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.

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. We provide a full description of our time preference elicitation procedure in Appendix Section A6.

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 comparable estimates, and, in both cases, we observe an increase in measured patience.

The purpose of the framework developed in Section 3 above is to delineate the mechanisms through which experiencing a disaster could affect measures of patience. Prediction 1 indicates that an increase in p – the subject's belief about the probability that they will receive the payment in one month – should lead to a decrease in our declarative measure of patience. Prediction 2 indicates that if the effect of the tsunami was to reduce consumption and put subjects on a steeper temporal consumption profile as they rebuild their assets, then exposure should also lead to an increase in patience. Prediction 3, by contrast, indicates that if the effect of the tsunami was to increase a subject's discount factor, δ, then this should also increase our measure of patience.[10]

5.1. Approach 1 – Comparing mean differences assuming 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 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 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.

Figs. 1 and 2 provide more intuition for our first approach to identifying the response of preferences to catastrophe. Fig. 1 depicts the location of survey respondents on a topographical map overlaid with 0.07 by 0.07 arc degree fishnet grids.[11] 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 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). Fig. 2 plots

[10] Appendix Section A1 also directly tests Predictions 1 and 2. Only Prediction 2 receives support. Our proxy measure for p, the optimism index, is not significantly correlated with our time preference measure. Income is positively correlated with our time preference measure, suggesting that a reduction in income would create a reduction in measured preference. We interpret this with great caution as these measures are clearly highly endogenous.

[11] 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.

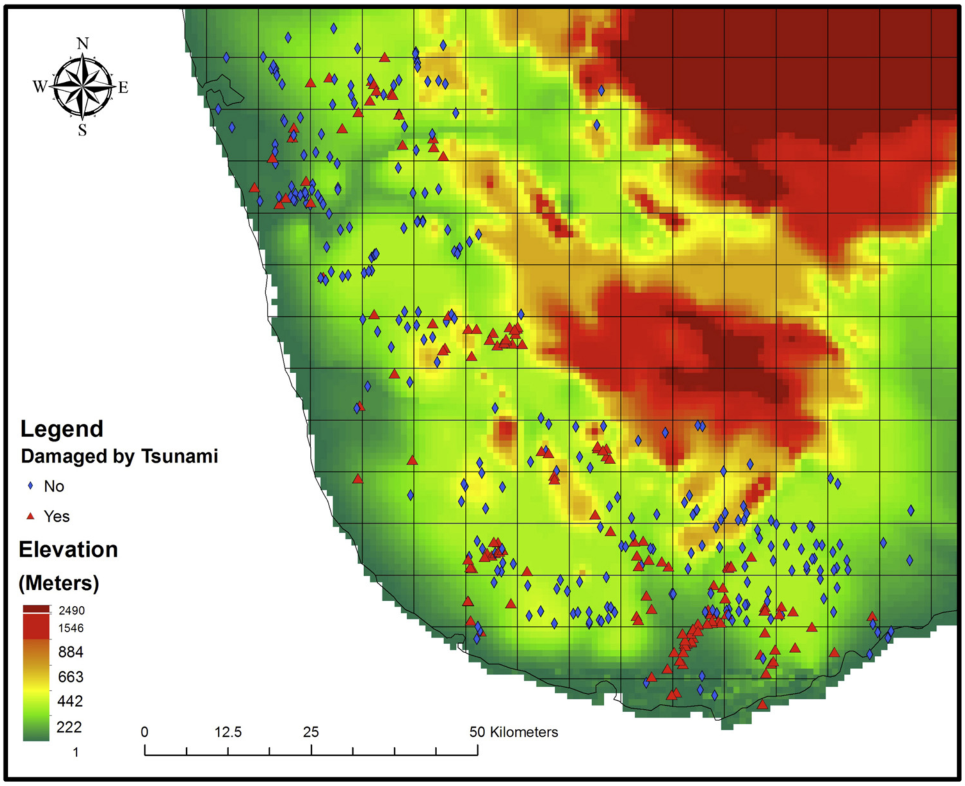

Fig. 1. 0.07 by 0.07 arc degree fishnet grids.

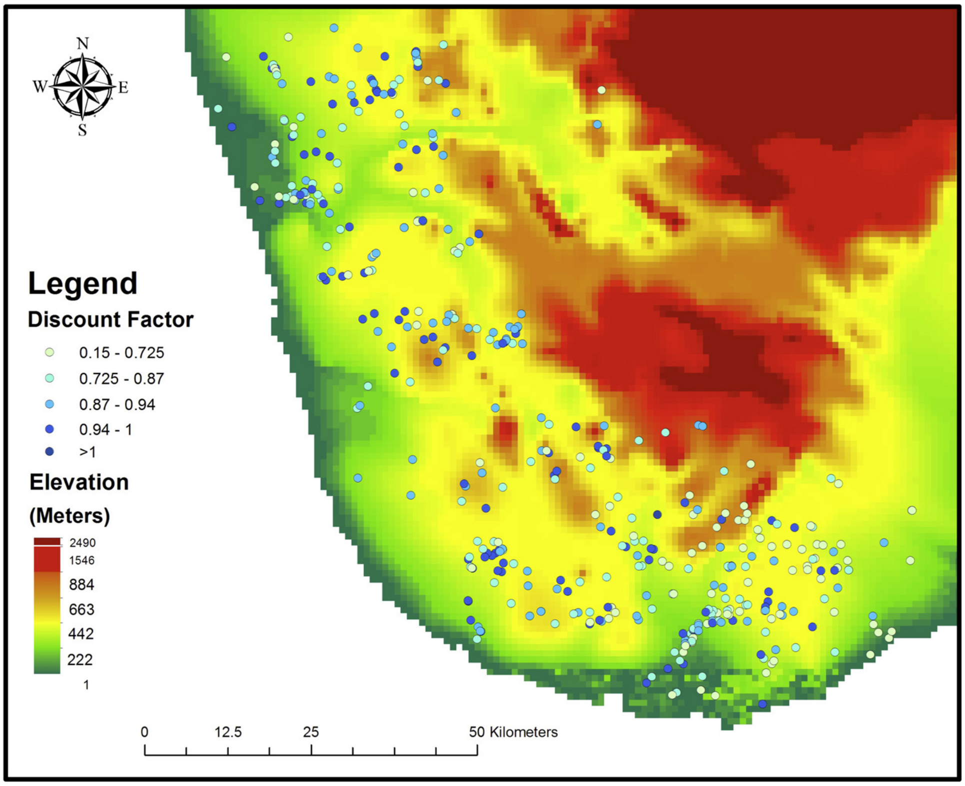

Fig. 2. The spatial distribution of discount factors.

Table 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 (km)	0.001			0.001	0.004
	(0.001)			(0.001)	(0.006)
	[0.001]			[0.001]	[0.005]
Elevation (100 m)		−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 significance)					
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: This table reports on the relationship between predictors of tsunami exposure and time preferences among wage workers unaffected by the tsunami. All specifications are Ordinary Least Squares. Fishnet grid effects are separate dummy variables for each of the .07 × .07 fishnet grids overlaying our sample. The share damaged in the same grid is calculated as the share of respondents living in the same grid as the respondent who have been impacted by the tsunami. 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. There are 52 Grama Niladara in our data. 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 $m \in \{5000, 10, 000\}$ rupees.
Level of significance:
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

elicited time preferences on a topographical map. In this figure, there does not appear to be an obvious 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 their discount rates. We now turn to a formal test of whether preferences and vulnerability are correlated.

In Table 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.[12] 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:

$$Average\,Discount\,Factor_i = \beta_0 + \beta_1 Damaged_i + \beta_2 X_i + \epsilon_i \tag{3}$$

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

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

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

Table 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)
Coefficient of 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 (m)			0.000			0.000			−0.000
			(0.000)			(0.000)			(0.000)
Distance to coast (km)			0.001			0.001			0.001
			(0.001)			(0.001)			(0.004)
Monthly wage (1000 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 (1000 rupees)			0.000**			0.000**			0.000
			(0.000)			(0.000)			(0.000)
Recovery funds (1000 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: This table reports on the effects of exposure to the tsunami on our average discount factor measure. Data are from a survey of wage workers conducted in July 2007. All specifications are Ordinary Least Squares. Standard errors clustered at the Grama Niladara level are reported in parentheses. There are 52 Grama Niladara in our data. We calculate the optimism index as the first principal component of three questions related to expectations about the future. The fixed effect samples are DS = District (7 divisions) and FN = 0.07 × 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 $m \in \{5000, 10,000\}$ rupees.
Level of significance:
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

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 0.07 by 0.07 arc degree fishnet grid. We show below that our approach is robust to using variation both within pre-existing administrative units (Grama Niladara) and evenly sized arbitrary fishnet grids.

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

$$Average\ Discount\ Factor_i = \gamma_0 + \gamma_1 Damaged_i + \gamma_2 \quad M_i + \gamma_3 M_i^2 + \gamma_4 M_i^3 + \gamma_5 M_i^4 + \gamma_6 X_i + \epsilon_i \tag{4}$$

where M_i is the distance in meters between individual i and the local high water mark and $Damaged_i$ is instrumented with a dummy variable equal to one when respondent i is below the recorded water level ($M_i < 0$) to provide a fuzzy regression discontinuity estimate.[13]

6. Results

6.1. Approach 1 – Estimates of mean differences under random assignment to exposure

Table 4 reports results for Specification 3. 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

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

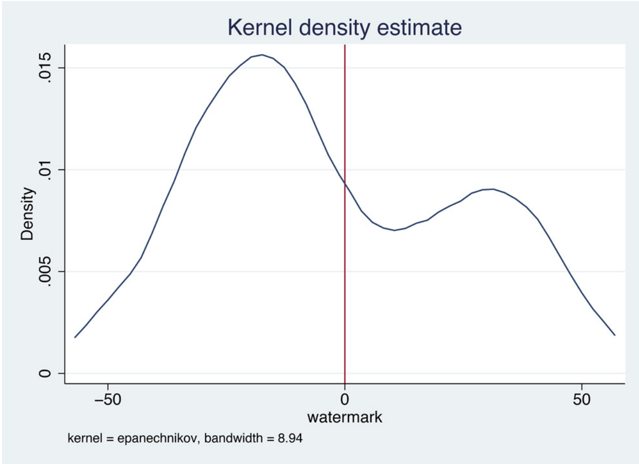

Fig. 3. Regression discontinuity evidence. *Note:* 4th-order polynomial regressions with 95% confidence intervals reported. High water marks for each of the 52 Grama Niladara administrative units in the sample are calculated as the elevation of the highest individual in the sample reporting damage from the tsunami. We exclude from this figure the 52 respondents residing exactly at the high water mark.

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, 2012; 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).[14] 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.

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. Fig. 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. Consistent with the tsunami being an imprecisely predicted disaster, we do not observe any clear bunching near the watermark. Fig. 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 Fig. 3 and the visually salient discontinuity in Fig. 4 suggest that we can estimate the causal effect of the tsunami on time preference using a regression discontinuity design.

Table 5 reports results for specification 4. 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 km by 5 km.[15] Even in column 3, where the result is not significant at conventional levels, we obtain a p-value of 0.114.[16] Fig. 5 depicts the histogram of time preference measures for affected and unaffected samples. Visually, it appears that the affected sample is more patient.

[14] In contrast to the positive correlation between risk and time preference in Column 2, Willinger et al. (2013) find a negative correlation. This correlation, in our data, however, is not very robust. Indeed it is insignificant in all other specifications in Table 5.

[15] In results available on request, we find that our specification is robust to different fishnet grid sizes

[16] In results available on request, we check the robustness of our estimates to taking a multilevel modeling approach to estimation. Taking this approach, our estimate of the coefficient on the "Damaged (=1)" regressor for specifications (1) to (3) are 0.052 (SE = 0.015), 0.052 (SE = 0.015), and 0.055 (SE = 0.032), respectively.

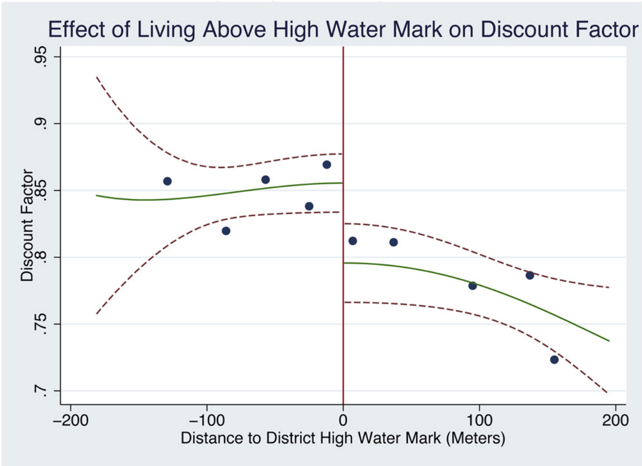

Fig. 4. Regression discontinuity evidence. *Note:* 4th-order polynomial regressions with 95% confidence intervals reported. High water marks for each of the 52 Grama Niladara administrative units in the sample are calculated as the elevation of the highest individual in the sample reporting damage from the tsunami.

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 the degree to which *selective migration* might be driving our results.

Table 5
Fuzzy regression discontinuity results.

Dependent variable	Average discount factor					
	(1)	(2)	(3)	(4)	(5)	(6)
Damaged (=1)	0.089*	0.105*	0.085	0.080	0.110***	0.112***
	(0.053)	(0.058)	(0.062)	(0.060)	(0.038)	(0.042)
Distance to water mark (km)	−0.000	−0.000	0.000	−0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Distance to water mark^2	−0.000	−0.000	−0.000	−0.000	−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)	(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)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.813***	0.774***	0.656***	0.623***	1.002***	0.985***
	(0.020)	(0.027)	(0.023)	(0.041)	(0.005)	(0.027)
R-squared	0.061	0.097	0.213	0.257	0.325	0.358
N	442.000	432.000	442.000	432.000	442.000	432.000
Fixed effects	No	No	GN	GN	FN	FN
Extra controls	No	Full	No	Full	No	Full
First stage *F*-statistic	177.54	98.297	107.774	91.470	94.370	53.773
R-squared	0.061	0.097	0.213	0.257	0.325	0.358
N	442	432	442	432	442	432

Notes: This table provides Regression Discontinuity estimates of the impact of the tsunami on time preferences. All specifications include the full set of interacted between affected and the Distance to Water Mark polynomial terms. Standard errors clustered at the Grama Niladara level reported in parentheses. There are 52 Grama Niladara in our data. *Damaged* is instrumented with a dummy equal to 1 if a respondent lived below high water mark in their Grama Niladara, 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 $m \in \{5000, 10,000\}$ 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 = 0.07 × 0.07 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.

Level of significance:

* $p < 0.1$.

** $p < 0.05$.

*** $p < 0.01$.

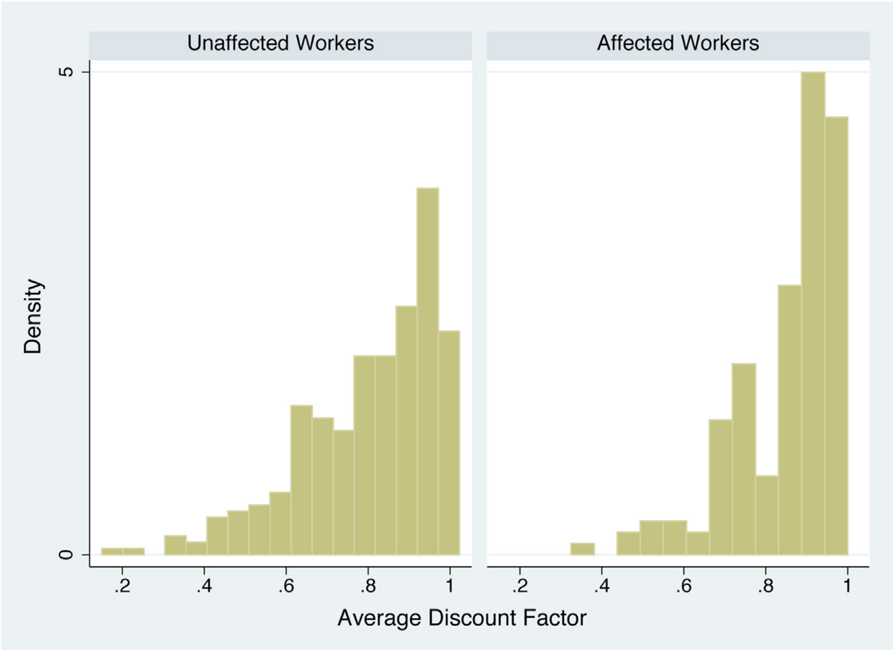

Fig. 5. Tsunami exposure and the distribution of preferences. *Notes*: Data are from a survey of wage workers conducted in July 2007. The average discount factor is the average of responses to four hypothetical survey questions: the amount required today to forego m rupees in one month and the amount required in 5 months to forego m in 6 months where $m \in \{5000, 10, 000\}$ rupees.

6.4. Selective migration

The survey was conducted 2 and a half years after the event. An alternative explanation therefore is that the impatient individuals may have migrated out in response to the event, creating a difference in means between affected and unaffected samples that does not reflect a causal change. While we cannot rule this out conclusively, several findings suggest that it may not be a major issue. We address this potential concern using detailed data on the job histories of wage workers in our sample. These histories cover periods prior to the tsunami. With these data, we can check whether the tsunami appears to have affected a proxy measure of migration, whether a respondent has been in the same job since prior to the tsunami, and the distribution of workers across jobs.

In Table 2, we see that the share of workers in the same job since before the tsunami is the same for both affected and unaffected populations. Table 3A reports regressions of a dummy variable equal to one on an indicated for whether a respondent experienced the tsunami. In columns (1) to (3), we find no significant differences. In column (4), we find, using variation within arbitrary 0.07×0.07 fishnet grids, that respondents who are damaged are 15 percentage points *more* likely to have been in the same job since before the tsunami. We obtain a similar, though insignificant, result in column (6) using a fuzzy regression discontinuity approach. This indicates that, right at the high water mark, it appears that unaffected individuals were more likely to switch jobs. An alternative interpretation is that individuals in the same job since prior to the tsunami are more likely to have actually experienced it than others living in their immediate vicinity.

To provide an additional, possibly more convincing, check, we create a simple sectoral employment Herfindahl index defined as $\sum_j s_j^2$ where s_j is the share of workers employed in a given category j at the time of the survey. We have data on the following categories: wage work, casual work, self-employed, and apprenticeship. If the tsunami created large-scale migration either out (because of people leaving decimated areas) or in (as wage workers came to seek new employment opportunities), then we would expect that the distribution of workers across these positions would shift due to economic changes created by the tsunami. Running a simple regression of the sectoral employment Herfindahl on whether a dummy variable equal to one for affected Grama Niladaras suggests no relationship.[17] In results available from the author, I also do not find any difference in average employment within each category.

[17] The Herfindahl in unaffected Grama Niladaras is 0.797 (se = 0.087) and in affected Grama Niladaras is 0.806 (se = 0.051). The difference between these two means is −0.009 (se = 0.101) with a corresponding p-value of 0.930. In results available on request, I find no relationship using a battery of regressions, including fuzzy RD specifications run at the respondent level.

6.5. Alternative explanations

In order to establish that our result reflects an increase in pure time preference, 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 Appendix Section A3, we consider four alternative explanations and provide evidence consistent with our interpretation. In that section, we address the following potential confounds: (i) the selective migration of exposed respondents; (ii) increases in the demand for savings from exposure; and (iii) problems arising from the hypothetical nature of the measurement protocol.

7. 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.

Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.jebo.2015.02.019.

References

Alesina, A., Giuliano, P., 2010. Preferences for redistribution. Handbook of Social Economics, vol. 1A. North Holland, pp. 93–131.
Andrabi, T., Das, J., 2010. In Aid We Trust: Hearts and Minds and the Pakistan Earthquake of 2005. Working Paper.
Andreoni, J., Sprenger, C., 2012. Estimating time preferences from convex budgets. Am. Econ. Rev. 102 (7), 3333–3356.
Ashraf, N., Karlan, D., Yin, W., 2006. Tying Odysseus to the mast: evidence from a commitment savings product in the Philippines. Q. J. Econ. 121 (May (2)), 635–672.
Aubert, R., Aubert, C., 2013. Do Natural Disasters Modify Risk Preferences? Evidence from a Field Experiment in Vietnam. Working Paper.
Augenblick, N., Niederle, M., Sprenger, C., 2013. Working Over Time: Dynamic Inconsistency in Real Effort Tasks.
Bauer, M., Chytilova, J., Morduch, J., 2012. Behavioral foundations of microcredit: experimental and survey evidence from rural India. Am. Econ. Rev. 102 (November (2)), 1118–1139.
Bchir, M.A., Willinger, M., 2013. Does the exposure to natural hazards affect risk and time preferences? Some insights from a field experiment in Peru. Working Paper.
Becker, G.S., Mulligan, C., 1997. The endogenous determination of time preference. Q. J. Econ. 112 (3), 729–758.
Ben-Ner, A., Putterman, L., 2001. Trusting and trustworthiness. Boston Univ. Law Rev. 81, 523–551.
Blanchard, O.J., 1985. Debt, deficits, and finite horizons. J. Polit. Econ. 93 (2), 223–247.
Blattman, C., Annan, J., 2010. The consequences of child soldiering. Rev. Econ. Stat. 92 (4), 882–898.
Böhm-Bawerk, E.V., 1889. Capital and Interest. Libertarian Press, South Holland.
Callen, M., Isaqzadeh, M., Long, J.D., Sprenger, C., 2014. Violence and risk preference: experimental evidence from Afghanistan. Am. Econ. Rev. 104 (1), 123–148.
Cameron, L., Shah, M., 2013. Do Natural Distasters Shape Risk Attitudes? Working Paper.
Cassar, A., Healy, A., von Kessler, C., 2013. Trust Risk and Time Preferences After a Natural Disaster: Experimental Evidence from Thailand. Working Paper.
Castillo, M., Carter, M., 2011. Behavioral Responses to Natural Disasters. Working Paper.
Chuang, Y., Schechter, L., 2014. Stability of Experimental and Survey Measures of Risk, Time, and Social Preferences Over Multiple Years. Working Paper.
de Mel, S., McKenzie, D., Woodruff, C., 2008, February. Who are the Microenterprise Owners? Evidence from Sri Lanka on Tokman v. de Soto.
de Mel, S., McKenzie, D., Woodru, C., 2011. Enterprise recovery following natural disasters. Econ. J. 122, 64–91.
Dominey-Howes, D., Papathoma, M., 2007. Validating a Tsunami Vulnerability Assessment Model (the PTVA Model) using field data from the 2004 Indian Ocean Tsunami. Nat. Hazards 40, 113–136.
Eckel, C.C., El-Gamal, M.A., Wilson, R.K., 2009. Risk loving after the storm: a Bayesian-Network study of Hurricane Katrina evacuees. J. Econ. Behav. Organ. 69 (February (2)), 110–124.
Finlay, J.E., 2009, March. Fertility response to natural disasters: the case of three high mortality earthquakes. Policy Research Working Paper Series 4883, The World Bank.
Fisman, R., Jakiela, P., Kariv, S., 2013. How Did Distributional Preferences Change During the Great Recession.
Frederick, S., Loewenstein, G., O'Donoghue, T., 2002. Time discounting and time preference: a critical review. J. Econ. Lit. 40 (June (2)), 351–401.
Harrison, G.W., et al., 2008. Eliciting risk and time preferences. Econometrica 76 (3), 583–618.
Jayachandran, S., Lleras-Muney, A., 2009. Life expectancy and human capital investments: evidence from maternal mortality declines. Q. J. Econ. 124 (February (1)), 349–397.

Jevons, H.S., 1905. Essays on Economics. Macmillan, London.
Jevons, W.S., 1888. The Theory of Political Economy. Macmillan, London.
Karlan, D., 2005. Using experimental economics to measure social capital and predict real financial decisions. Am. Econ. Rev. 95 (5), 1688–1699.
Koshimura, S., Oie, T., Yanagisawa, H., Imamura, F., 2008. Vulnerability Estimation in Banda Aceh Using the Tsunami Numerical Model and the Post-Tsunami Survey Data.
Laffont, J.J., Matoussi, M.S., 1995. Moral Hazard, Financial Constraints and Sharecropping in El Oulja. Rev. Econ. Stud. 62 (3), 381–399.
Malmendier, U.M., Nagel, S., 2011. Depression babies: do macroeconomic experiences affect risk taking? Q. J. Econ. 126 (1), 373–416.
Meier, S., Sprenger, C., 2015. Temporal stability of time preferences. Rev. Econ. Stat. 97 (2), 273–286.
Mischel, W., Shoda, Y., Rodriguez, M., 1989. Delay of gratification in children. Science 244 (4907), 933–938.
Olson, M., Bailey, M.J., 1981. Positive time preference. J. Polit. Econ. 89 (1), 1–25.
Pörtner, C., 2008. Gone With the Wind? Hurricane Risk, Fertility and Education. Working Papers UWEC-2006-19-R, University of Washington, Department of Economics.
Rae, J., 1834. The Sociological Theory of Capital. Macmillan, London.
Senior, N.W., 1836. An Outline of the Science of Political Economy. Clowes and Sons, London.
Stigler, G.J., Becker, G.S., 1977. De Gustibus Non Est Disputandum. Am. Econ. Rev. 67 (March (2)), 76–90.
Voors, M., Nillesen, E., Verwimp, P., Bulte, E., Lensink, R., van Soest, D., 2012. Does conflict affect preferences? Results from field experiments in Burundi. Am. Econ. Rev. 102 (2), 941–964.
Willinger, M., Bchir, M.A., Heitz, C., 2013. Risk and Time Preferences Under the Threat of Background Risk: A Case Study of Lahars Risk in Central Java. Working Paper.
Yaari, M.E., 1965. Uncertain lifetime, life insurance, and the theory of the consumer. Rev. Econ. Stud. 32 (2), 137–150.