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By

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Verifying Reference-Dependent Utility and Loss Aversion with Fukushima Nuclear-Disaster Natural Experiment

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We verify prospect theory with natural experimental data by adopting regression kink design. Our data were collected in 2013 and 2014 from residents displaced by the Fukushima Nuclear Disaster in 2011. We examine how a sudden gain/loss affects stress/utility in four dimensions/resources: family size, health, house size, and income. We find that (i) there is a higher sensitivity to losses from a reference point than to gains (i.e., loss aversion) in house size, and possibly in health and income as well, (ii) the reference point may change over time, and (iii) value function is not separable in the four dimensions/resources. These findings have a few implications. First, in view of the loss aversion, a sufficient—apparently more than enough—compensation should be provided to those who lost so that they can regain the original utility. Second, if the reference point is lowered, the victims must be over-compensated for their loss to recover the original utility. Third, separable value functions should be used with caution.

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1 Introduction

Models of reference-dependent preference and loss aversion in the prospect theory have been tested in many laboratory experiments, but there is little real-world evidence (Köbberling 2006 and DellaVigna 2009). Exceptions are Camerer et al. (1997), Genesove and Mayor (2001), Mas (2006), Crawford and Meng (2011) and Barseghyan et al. (2013), who examined the prospect theory in a variety of contexts such as housing market, finance, labor supply, insurance, and employment. The findings of these studies are largely consistent with prospect theory. Yet, there remain issues such as identification of causal relationship, reference dependence in high dimensions, and generalizability of the laboratory empirical results into a real life experience.

To bridge these gaps in the existing studies, we test basic components of the prospect theory by exploiting a natural experiment generated by the Fukushima Daiichi Nuclear Disaster, March 2011, in Japan. We adopt a regression kink (RK) approach to analyze our unique data, exclusively collected for this study in July 2013 and December 2014 from residents of Futaba who were unexpectedly displaced due to the disaster; Futaba is a town in Fukushima.

Since Futaba is located within a 2-10 km radius from the Fukushima Daiichi nuclear power plant, the government placed an indefinite evacuation order. All Futaba residents were forced to move from their homes, and many residents lost stable incomes and faced a high level of psychological stress (Iwasaki et al. 2017). The Tokyo Electric Power Company (TEPCO) as well as the Japanese government provided a variety of monetary and non-monetary compensations. Specifically, TEPCO provides 100,000 yen per month per person for psychological injuries, the full compensation for the lost assets, and a compensation for the expected income they would get, had they not experienced the displacement. This unforeseen incident provides a natural experiment, with individuals exogenously and unexpectedly losing their home, income, or health.

Applying RK to our natural experimental data, we verify reference dependence and loss aversion, the two basic components of the prospect theory introduced by Kahneman and Tversky (1979). Reference dependence is that individual's value/utility function is defined over the relative difference from a reference point, instead of the absolute level. Loss aversion is that the value function has a kink at the reference point and is steeper for losses than for gains. Thaler (2016) listed loss aversion as one of the three most important concepts of behavioral economics, and stated that most of prospect theory's predictive power comes from its crucial assumptions including reference dependence and loss aversion.

We use pre-disaster levels as reference points for the evacuees' utility function, because the disaster was unforeseen and the rate of migration among the elderly of Futaba who are the major respondents of our surveys is low. According to the 2010 Population Census of Japan, among those aged 65 or above in Futaba, only 4.1% moved out of their town or city, compared with five years ago. Hence we adopt the pre-disaster family size, health status, house size, and income as reference points to test the theory. Although we set our reference point to be pre-disaster levels in our main analysis, we conduct an additional analysis taking into account the possibility of reference point revision as well. For utility, we use 'negative mental stress', as mental stress may be taken as the flip side of utility. We consider four dimensions/resources in the utility function: *family size, health, house size, and income.*

The rest of this paper is organized as follows. Section 2 explains our data. Section 3 lays out our empirical strategy to verify the prospect theory. Section 4 presents the empirical results. Finally, Section 5 concludes our findings. As usual, we often omit the subscript i indexing individuals in variables, because we assume that our observations are independent and identically distributed across $i = 1, ..., N$. Putting some of our main findings in advance, reference dependence and loss aversion hold in house size and possibly in health and income as well, and there is a strong empirical evidence that the utility function is not separable in its arguments, contrary to what is often presumed in economic theory.

2 Data

We conducted surveys in July 2013 and in December 2014, two years and four months and three years and nine month, respectively, after the Great East Japan earthquake. We targeted residents from Futaba in Fukushima which was seriously damaged by the disaster. Futaba has about 6,900 residents and 2,600 households, and Futaba is located within a 2-10 km radius from the Fukushima Daiichi nuclear power plant. Accordingly, the town was placed under a government-mandated evacuation order that residents are forbidden from returning for at least 5 years.

With the support of the Futaba town office, survey questionnaires were sent to 2,900 addresses listed as regular recipients of the town newsletter. Ideally, the survey questionnaires should have been distributed to all residents of Futaba, but due to practical constraints, we addressed the survey only to the heads of household.

The number of households and the number of distributed questionnaires are about 2,600 and 2,900, respectively, which means that the survey forms must have been distributed to most heads of household in Futaba. We received 585 replies for the 2013 survey, and 654 for 2014; the response rate is about 20 % in both surveys. The actual response rate would be higher than 20%, because the 2,900 addresses include the household heads and those who requested the newsletter. The low response rates would be a concern normally, but as long as the selection bias resulting from the response decision is "smooth", it would not bias our RK-based empirical approach, which is further explained below (in Section 3.3).

To measure mental health/stress, we use 'Kessler 6' (K6), a standardized and widely used measure of non-specific psychological distress; Furukawa et al. (2008) confirmed the efficacy of K6 in Japan. K6 introduced by Kessler et al. (2002) is a composite index of six questions on mental health that assigns a maximum of four points to each question for 24 points in total. The six questions are: during the past 30 days, how often did you feel (a) nervous? (b) hopeless? (c) restress/fidgety? (d) so depressed that nothing could cheer you up? (e) everything was an effort? and (f) worthless? The answer is in five point scale (4-0): all of the time, most of the time, some of the time, a little of the time, and none of the time. The threshold for serious mental health problems is usually set at 13 (Kessler et al. 2008).

For the determinants of K6, we asked questions about family size, subjective health status, house size, and income (before and after the disaster). In the income question for the 2013 survey, we asked for categorical household income levels, which were then converted to continuous values with 'interval regression' under the presumption that the minimum income is zero. We also asked about gender, age and other characteristics. The list of variables and their descriptive statistics are in Appendix Tables 1 and 2. Family size ('# family') before and after the disaster is the number of cohabiting family members; it declined after the disaster in both surveys. Health change is the self-reported evaluation of health change after the disaster; the average is negative for both surveys. House size question was included only in the 2014 survey; 'house size' is house size divided by the family size. The average house size change is positive, probably due to the reduced family size. Income is household income divided by the family size. The average income declined from 2.1 million to 1.6 million JPY in the 2014 survey, and from 1.9 to 1.7 millions in the 2013 survey.

Although the population of Futaba as a whole has a balanced male-female ratio according to the 2010 national census, the male percentage is 79 in the 2013 data, and 80 in 2014. Also, the age distribution of the respondents is skewed toward right in both surveys, with a smaller proportion of people under age 50 compared with the census data. These are because household heads tend to be old males in Japan, and thus our data may not be representative.

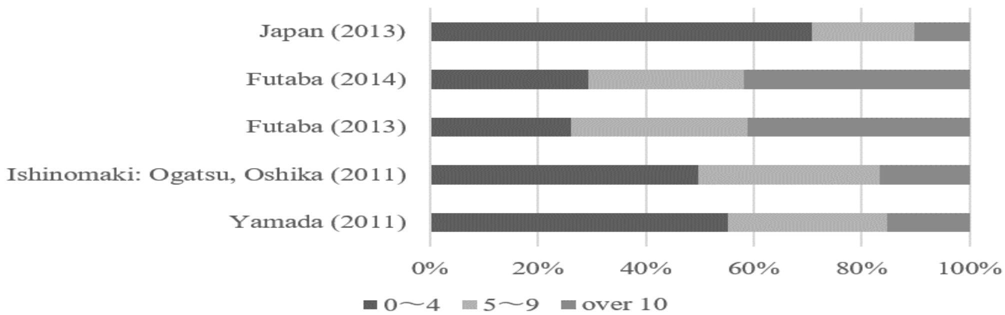

Figure 1: Comparison of K9 across Japan, Futaba and Other Regions

Figure 1 compares Futaba to Japan and two other regions in terms of K6. The information sources on K6 other than for Futaba are three research reports (written in Japanese) for the Japanese government, which are available from the authors upon request. K6 is much higher in Futaba than in the entire Japan. Also, K6 is much higher in Futaba than in two other disaster-struck areas, Ogatsu and Oshika of Ishinomaki city and Yamada town in Iwate prefecture, which were seriously damaged by the tsunami but not directly affected by radiation after the nuclear disaster.

3 Empirical Strategy

3.1 Linear Model for Reference Dependence and Loss Aversion

Reference dependence is that individual's value/utility function is defined over the relative difference from a reference point, not over the level. Loss aversion is that the value function has a kink at the reference point and is steeper for losses than for gains. Figure 2 illustrates reference dependence and loss aversion locally around the reference point; globally, the figure may take a S shape.

To test for reference dependence and loss aversion, we adopt the multi-dimensional

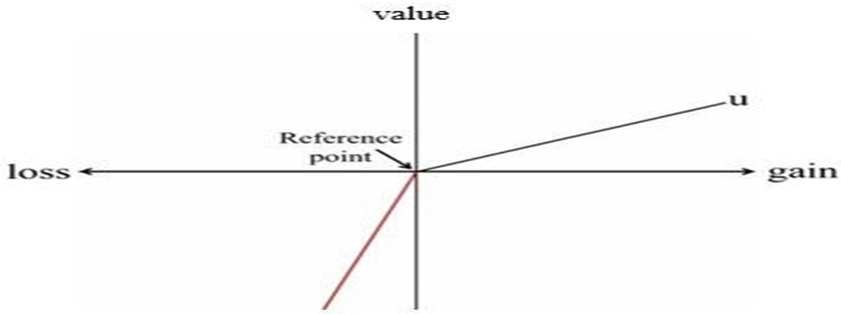

Figure 2: Reference Dependence and Loss Aversion

reference-dependent utility model in Köszegi and Rabin (2006). In their model with separability across "dimensions/resources" assumed, utility V is determined by the level S_j of a monetary or non-monetary resource j, and its gain/loss relative to a reference point r_j:

$$V = \sum_{j=1}^{J} l_j(S_j) + \sum_{j=1}^{J} m_j(S_j - r_j)$$

where $l_j(\cdot)$ and $m_j(\cdot)$ are functions, $j = 1, ..., J$. In our data, (i) S_j is the number of family members, health status, house size, or income, (ii) $V = 24 - K6$, taking on a value in $0 \sim 24$, and (iii) we use the pre-disaster level of S_j as r_j. If $m_j(\cdot) \neq 0$, there is a reference dependence. Because the separability turns out to be empirically rejected, and we extend the V model to include interactions across $S_j - r_j$, $j = 1, ..., J$, below.

Let $1[A] = 1$ if A is true, and 0 otherwise. Define

$$\Delta S_j \equiv S_j - r_j \qquad \text{and} \qquad \delta_j \equiv 1[0 \leq \Delta S_j].$$

To verify reference dependence and loss aversion, we use a linear regression model:

$$V_i = \beta_0 + X_i'\beta_x + \sum_j \beta_j S_{ij} + \sum_j \beta_{j-}(1 - \delta_j)\Delta S_{ij} + \sum_j \beta_{j+}\delta_j \Delta S_{ij} + U_i \qquad (3.1)$$

where X_i is the covariates controlled, the β's are parameters with the slopes β_{j-} and β_{j+} on the left and right side of r_j, and U_i is an error; loss aversion is $\beta_{j-} > \beta_{j+}$.

Since we are interested in the slope differences, (3.1) can be cast in RK framework (see Card et al. 2012, 2015, Lee 2016, Choi and Lee 2017, and references therein), because RK looks at slope change, instead of intercept change as in regression discontinuity (RD). We explore this link next, drawing on Kim and Lee (2016).

3.2 Study Design Based on Regression Kink (RK)

Given a response V, a treatment $D = D(S)$ determined by a 'running variable' S, and an unknown smooth function $\mu(S)$, '(sharp) RK design' refers to $E(V|S)$ and D related through

$$E(V|S) = \mu(S) + \beta_d D \tag{3.2}$$

where β_d is the treatment effect, and the gradient of $D(S)$ with respect to S (denoted $\nabla D(S)$) is discontinuous at the cutoff 0 whereas $\nabla\mu(S)$ is continuous at 0. In general, the cutoff is not zero (say, C), but S can be redefined as $S - C$ to have the normalized cutoff 0. For simplicity, we will call 'running variable' just '*score*'; S_j's are scores.

Define the right and left gradient at $s = 0$:

$$\nabla E(V|0^+) \equiv \lim_{\nu\to 0^+} \frac{E(V|S=\nu) - E(V|S=0)}{\nu}, \ \nabla E(V|0^-) \equiv \lim_{\nu\to 0^+} \frac{E(V|S=0) - E(V|S=-\nu)}{\nu}.$$

The difference between the two one-sided gradients of (3.2) at 0 is, as $\nabla\mu(0^+) = \nabla\mu(0^-)$,

$$\nabla E(V|0^+) - \nabla E(V|0^-) = \beta_d\{\nabla D(0^+) - \nabla D(0^-)\} \Longrightarrow \beta_d = \frac{\nabla E(V|0^+) - \nabla E(V|0^-)}{\nabla D(0^+) - \nabla D(0^-)}. \tag{3.3}$$

As an example for (3.3), D may be a central government subsidy to local governments that is provided only when the local population size S is greater than a cutoff C, and the subsidy amount is proportional to the extra population size $S - C$. Here, $D = \alpha_s 1[C \le S](S - C)$ where $\alpha_s > 0$ is a known proportion. Redefining S as $S - C$ then gives

$$D = \alpha_s 1[0 \le S]S \ \Longrightarrow \nabla D(0^+) - \nabla D(0^-) = \alpha_s - 0 = \alpha_s;$$

$D(S)$ has positive and zero slopes on the positive and negative sides of 0. Hence β_d can be estimated using (3.3): divide a nonparametric estimator for $\nabla E(V|0^+) - \nabla E(V|0^-)$ by α_s.

The advantage of nonparametrically estimating $\nabla E(V|0^+) - \nabla E(V|0^-)$ is that there is no need to specify $\mu(S)$; it is enough to know that $\nabla\mu(S)$ is continuous at the cutoff 0. But estimating a derivative nonparametrically requires a large sample. Hence, if the data size in hand is small as in our case, it is preferable to specify $\mu(S)$ and apply ordinary least squares estimator (OLS) to (3.2). For instance, with $D = \alpha_s 1[0 \le S]S$, we may set up

$$E(V|S) = \beta_0 + \beta_1 S + \beta_2 S^2 \ + \beta_d \cdot \alpha_s 1[0 \le S]S \tag{3.4}$$

and do the OLS of V on $(1, S, S^2, \alpha_s 1[0 \le S]S)$ to estimate $(\beta_0, \beta_1, \beta_2, \beta_d)$. Note that using $(S, 1[0 \le S]S)$ is equivalent to using $(1[S < 0]S, 1[0 \le S]S)$ in OLS. Whereas (3.4) uses a quadratic function to approximate $\mu(S)$, only a linear function appears in (3.1). Higher order polynomial functions of S_j and their interactions can be used in (3.1), if desired.

3.3 Localization and Natural Experiment for Randomization

One notable difference between (3.1) and (3.3) is that we already know that $\nabla D(S)$ has a break in (3.3) to estimate β_d by taking advantage of this known break, whereas in (3.1) we do not know whether $\nabla D(S)$ has a break that is big enough to support the prospect theory. This difference notwithstanding, the main reason to refer to RK is its focus on slope difference and localization (i.e., using a local sample around the cutoff) that apply to both (3.1) and (3.3). This point can be better understood by discussing RD first which is "one degree simpler" than RK, because RD rests on a break in $D(S)$, not in $\nabla D(S)$.

Consider a RD example where S is a test score, $D = 1[C \le S]$ is passing the test or not, and Y is graduation, where the treatment effect of passing the exam on graduation is of interest. In a local sample around C, say $C \pm 1$, those who just passed with $S = C + 1$ is similar to those who just failed with $S = C - 1$ in all covariates, observed or not, because missing a couple of questions among many should be almost random. Hence the treatment effect can be found by the simple group mean difference $E(Y|S = C + 1) - E(Y|S = C - 1)$. This is what localization does in RD, and the same appeal to randomization due to using a local sample works in RK. The only difference is that the RK effect is found by the derivative of the local group mean difference. For our data however, as explained next, localization alone is insufficient to ensure randomization and the ensuing covariate balance, which is why we sought for natural experiment.

In our four scores (family size S_1, health S_2, house size per person S_3, and income per person S_4), $\Delta S_1 = \pm 1$ (losing/gaining a family member) may be random as death/marriage may be so. This randomization becomes less plausible for health change ΔS_2; e.g., those with $\Delta S_2 = 1$ tend to be younger than those $\Delta S_2 = -1$, because the health level is measured in a five point scale and $\Delta S_2 = \pm 1$ are not small changes. For change in house size per person, although changes in family size may be random, changes in house size itself may not be. Randomization is most suspect for income change per person ΔS_4; e.g., during an economic recession, those with a stable job (e.g., civil servants) are likely to have $\Delta S_4 > 0$, and those with an unstable job are likely to have $\Delta S_4 < 0$.

What these show is that, during a normal period, using a local sample alone may not be enough to ensure randomization for covariate balance in our setting. Introducing the natural experiment of the earthquake-tsunami-radiation disaster, we have a better chance of establishing randomization, because all four ΔS_j's are dominated by the changes caused by the disaster to weaken the aforementioned imbalances in age, house size, or job categories. The disaster ensures that, e.g., all individuals lose their income regardless of whether their job is stable or not, and their after-disaster income per person may go back up depending on the number of family members lost. The "double remedy" of localization and natural experiment gives randomization a much higher chance to hold than in the usual RD/RK study with localization alone.

The double remedy is needed, because our sample size is relatively small while our localization dimension is large: we have four scores, differently from most RD/RK studies with just a single score. With the double remedy, our randomization turns out to be adequate even when we use nearly 80% of the observations. Consequently, controlling X makes hardly any difference in our empirical findings, although it may increase the model fitness of (3.1) by reducing the error term variation.

Considering other possible "threats" to our study design, first, a natural disaster may make certain geographic traits unbalanced, because some areas might be more vulnerable to natural disasters (e.g., low lying or coastal regions). In our case, however, the disaster including radiation struck all residents with such an enormity, which makes this type of threat a lesser concern. Second, a selection bias may arise from the low response rates in our survey. Suppose that the decision to respond to our questionnaire adds a selection correction term, say $\lambda(S, X)$, to (3.1). As long as $\lambda(S, X)$ (or $E\{\lambda(S, X)|S\}$) is smooth in S in the sense $\lambda(S, X)$ and $\nabla\lambda(S, X)$ are continuous in S, there is no selection bias. The same reasoning works for other potential biases such as recall bias: as long as they are smooth in the analogous sense, they do not invalidate our approach.

4 Empirical Analysis

4.1 Preliminary Analysis for 2013 and 2014

As a preliminary analysis, we estimate (3.1) separately for each ΔS_j without controlling covariates and the level S_j, using

$$V = \beta_0 + \beta_{j-}(1 - \delta_j)\Delta S_j + \beta_{j+}\delta_j \Delta S_j + error;$$

omitting variables other than ΔS_j facilitates graphical presentation at the expense of possible omitted variable biases. Each model fits a linear line on the positive and negative sides of ΔS_j to allow different slopes.

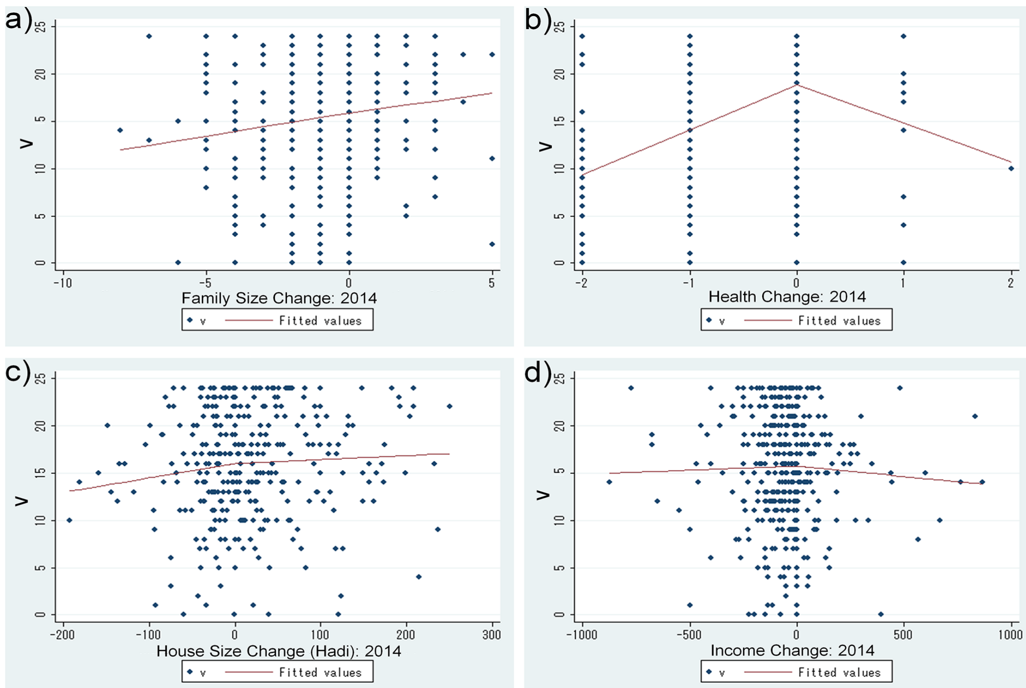

Figure 3: Linear Value Function for 2014 Allowing Different Slopes for Each Score Change; slope difference significant only for health, with the difference -8.8 and standard error 1.5

The estimation results are in Figures 3 and 4. In Figure 3 for the 2014 data, panels (a)-(d) are, respectively, for family size, health, house size after removing 5% outliers by the Hadi (1994) method, and income. In Figure 4 for the 2013 data, panels (a)-(c) are, respectively, for family size, health, and income. Figures 3(b), 3(c), 4(b) and 4(c) support reference dependence and loss aversion while the other panels do not, and remarkably, these findings are more or less borne out by far more sophisticated analyses to be done in the rest of this section. One caveat is that the slope on the positive side in Figure 3(b) is negative mostly due to a single outlier at the right-most position. The single outlier is also troubling in Figure 3(c), which is why we used the Hadi method there; in our main empirical analysis below, outliers do not matter, because they drop out due to localization.

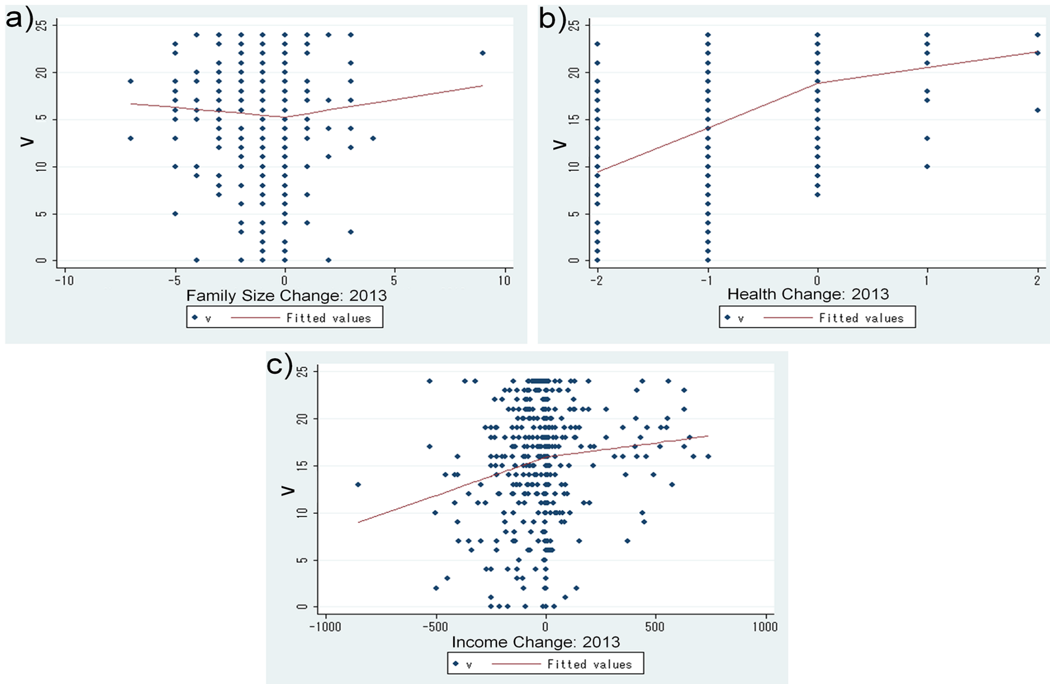

Figure 4: Linear Value Function for 2013 Allowing Different Slopes for Each Score Change; slope difference significant only for health, with the difference -7.7 and standard error 1.3

4.2 Empirical Analysis for 2014

In the 2014 survey with 654 observations, only $N = 279$ observations can be used due to missings in the key variables: V (mental health), and

S_1 : number of family members living together in 2014;

S_2 : health status in 2014;

S_3 : house size (in 100 m^2) per capita in 2014;

S_4 : income (in JPY 1 million) per capita in 2014;

for S_2, only ΔS_2 is observed, not S_2 per se.

With SD denoting standard deviation, define a base bandwidth h_0 for localization, and then bandwidth j, $h_j \equiv h_0 SD(\Delta S_j)$, for score j. Localization is done using only the observations with $\Delta S_j \in \pm h_j = \pm h_0 SD(\Delta S_j)$, $j = 1, 3, 4$ where '$\pm h_j$' means $(-h_j, h_j)$; no localization for health change, because ΔS_2 takes on only $0, \pm 1, \pm 2$. As it turns out, (3.1) is estimable only when $h_0 \geq 1.5$. We use $h_0 = 1.5,\ 2,\ 2.5$, but we present estimation results mainly for $h_0 = 2$ using 86% of $N = 279$. Even the smallest possible bandwidth $h_0 = 1.5$

results in using 73% of the data; i.e., our localization range is large.

For each covariate X_k, we fit a model analogous to (3.1):

$$X_k = \gamma_0 + \sum_j \gamma_{\delta j}\delta_j + \sum_j \gamma_j \Delta S_j + \sum_j \gamma_{j+}\delta_j \Delta S_j + error \quad (4.1)$$

where the γ's are parameters; using ΔS_j and $\delta_j \Delta S_j$ is equivalent to using $(1-\delta_j)\Delta S_j$ and $\delta_j \Delta S_j$ in (3.1). If $\gamma_{\delta j} \neq 0$, then not controlling X_k in the V equation may result in an intercept break due to X_k in the V equation. This is not a concern, however, because we will pick up any intercept break by adding $\sum_j \beta_{\delta j}\delta_j$ into the V model in our empirical analysis below, and $\beta_{\delta j}$'s are not of interest for our RK-based approach. If $\gamma_{j+} \neq 0$, however, then not controlling X_k in the V equation will result in a slope break for ΔS_j in the V model due to X_k, which is a concern because this will bias our slope estimates. Hence, the covariate balance that matters for our RK-based approach is whether $\gamma_{j+} = 0$ or not for $j = 1, 2, 3, 4$.

Table 1. Covariate Balance Test for 2014 with T-Value for $\delta_j \Delta S_j$ & $h_0 = 2$

Age	-0.37 0.58 2.04 -2.64	Unemployed	0.50 0.57 -1.42 -1.69
Female	0.86 -3.68 0.85 1.46	Other job	-0.83 -2.75 0.72 1.63
No family loss	1.90 -0.30 0.72 1.54	Region 2	-0.69 0.71 1.26 -0.66
Family loss	0.61 -1.10 -3.35 -0.06	Region 6	0.76 0.85 0.38 -0.54
NR to family loss	-2.36 1.13 0.59 -1.68	Region 8	-0.24 -0.24 0.80 -0.28
Middle school	-3.13 0.43 -0.61 -0.87	Region 16	0.68 -0.61 -2.02 0.50
High school	0.77 -0.45 -0.22 -0.17	Region 20	0.17 -1.48 -0.14 0.25
Specialty school	0.11 -1.05 1.17 0.45	City 1	-1.94 1.07 0.22 1.73
College	0.54 0.96 0.20 -0.72	City 2	0.42 1.36 -0.87 -1.08
Employee	-0.25 -0.32 -1.81 0.55	City 7	-1.69 -0.09 -1.12 0.71
Civil servant	1.17 -1.60 -2.52 1.65	NR to city	1.56 -0.93 0.13 -1.86
Farmer	-0.42 -1.01 1.61 -0.15	Prefecture 1	-1.66 0.83 0.01 1.85
Self-employed	-1.51 1.94 0.84 1.61	Prefecture 2	-0.57 0.70 0.37 -1.76
Retired	0.50 0.82 0.07 -4.49	Prefecture 11	1.23 -0.61 -1.31 -1.35

Four numbers in each entry are t-values for $\gamma_{j+}= 0$ in (4.1); NR, non-response;

Family loss, family loss by death; Region #, a region in Futaba; City #, a city in Fukushima.

The test results with $h_0 = 2$ are in Table 1. For dummy variables, we test the balance only when its mean is greater than 0.05 and smaller than 0.95, i.e., only when the number of observations for the dummy being one or zero is at least 14, because covariate balance requires a law of large numbers to kick in. There are 28 covariates in Table 1, and thus $28 \times 4 = 112$ t-values. With ± 1.96 as the critical values, we find 10 t-values statistically significant. Given the 5% type-1 error rate, $112 \times 0.05 = 5.6$ tests are bound to reject even when the null is true; i.e., among the 10 rejections, only about four out of 112 are genuine rejections. This means that the covariate balance is adequate, despite that we are using as much as 86% of the data. Since our localization is not really local, we attribute this randomization success to the natural experiment.

Covariate balance becomes a little worse with $h_0 = 1.5$ and 2.5, as they results in 15 and 13 rejections, respectively, which is why we use $h_0 = 2$ mainly. The worse randomization with the smaller $h_0 = 1.5$ than with the larger $h_0 = 2.5$ corroborates our conjecture that the randomization success in Table 1 is mostly due to the natural experiment, not to localization. Since the covariate balance test is done one at a time in Table 1, when covariates appear jointly in the V model, the result may differ; given our small data size, we could not test the covariate balance jointly.

Because covariate balance looks good overall, in the following, first, we omit all covariates to obtain main findings, and then we add covariates deemed to be important a prior (such as age, female, and prefecture 1 that is Fukushima) along with some other covariates to demonstrate that the main findings obtained without those covariates controlled still hold.

After exploring various specifications, we set our first model for 2014 as

$$\begin{aligned} Y = \beta_0 + \{&\beta_{age}\text{Age} + \beta_{civil}\text{Civil} + \beta_{female}\text{Female} + \beta_{nrcity}\text{NRcity} + \beta_{pref1}\text{Pref1} \\ &+ \sum_{j=1}^{4} \beta_{\delta j}\delta_j\} + \beta_1 S_1 + \beta_3 S_3 + \beta_4 S_4 + \sum_{j=1}^{4} \beta_{j-}(1-\delta_j)\Delta S_j + \sum_{j=1}^{4} \beta_{j+}\delta_j \Delta S_j + U \quad (4.2) \end{aligned}$$

where Civil, NRcity, and Pref1 are dummies for civil servant, NR (non-response) to city, and prefecture 1 that is Fukushima (there are 28 prefectures in our data). In (4.2), $\beta_2 S_2$ is missing, because S_2 (health status in 2014) is not available in the data. Call (4.2) without the part in $\{\cdot\}$ (i.e., Age, Civil, Female, NRcity, Pref1 and $\sum_{j=1}^{4} \beta_{\delta j}\delta_j$) "base model".

One caution in estimating (4.2) is that $P(\Delta S_j > 0)$ can be small: without localization

$$P(\Delta S_1 > 0) = 0.12, \quad P(\Delta S_2 > 0) = 0.02, \quad P(\Delta S_3 > 0) = 0.53, \quad P(\Delta S_4 > 0) = 0.25.$$

Positive changes in S_j are unlikely, as the family size and income changes show (only 12% and 25% positive). But house size per person increased (53%), probably because some family members were sent away to stay with relatives. The most problematic is health change with 2% positive (only 6 persons). Hence we should take health change estimates "with a grain of salt". We will have more individuals with positive heath changes in the 2013 data.

Table 2. OLS to Model (4.2) for 2014 ($N = 279$)

	h_0=2: $\hat{\beta}$ (tv)	h_0=1.5: $\hat{\beta}$ (tv)	h_0=2: $\hat{\beta}$ (tv)	h_0=2.5: $\hat{\beta}$ (tv)
Age		-0.03 (-0.88)	-0.04 (-1.65)	-0.05 (-2.10)
Civil servant		-3.16 (-3.39)	-2.68 (-3.18)	-2.66 (-3.19)
Female		-0.03 (-0.03)	-0.28 (-0.31)	-0.40 (-0.43)
NR to city		-12.9 (-13.3)	-12.5 (-13.4)	-12.8 (-14.1)
Prefecture 1		-13.7 (-13.8)	-13.2 (-13.9)	-13.1 (-14.0)
δ_1		-0.48 (-0.29)	-1.28 (-1.14)	-2.06 (-2.04)
δ_2		-2.95 (-1.56)	-2.84 (-1.57)	-2.62 (-1.47)
δ_3		-0.61 (-0.57)	-0.79 (-0.88)	-0.40 (-0.46)
δ_4		-0.12 (-0.10)	-0.37 (-0.40)	0.14 (0.15)
#Family1	0.44 (1.99)	0.63 (2.64)	0.49 (2.07)	0.52 (2.17)
HouseSize1	-0.84 (-0.65)	0.02 (0.01)	0.20 (0.14)	0.97 (0.70)
Income1	0.78 (2.93)	1.11 (3.70)	0.86 (3.12)	0.68 (2.53)
ΔFamily$_-$	0.19 (0.52)	-0.10 (-0.08)	0.72 (1.21)	1.25 (2.47)
ΔFamily$_+$	-0.54 (-0.89)	0.76 (0.77)	-0.46 (-0.78)	-0.63 (-1.27)
ΔHealth$_-$	3.84 (6.22)	5.52 (3.37)	5.54 (3.50)	5.52 (3.51)
ΔHealth$_+$	-2.41 (-1.55)	-2.13 (-1.60)	-2.90 (-2.08)	-2.87 (-1.90)
ΔHouseSize$_-$	1.44 (1.92)	0.88 (0.82)	1.49 (1.80)	0.93 (1.29)
ΔHouseSize$_+$	2.20 (1.30)	1.69 (0.78)	1.34 (0.73)	-0.29 (-0.17)
ΔIncome$_-$	0.33 (0.69)	0.47 (0.56)	0.52 (0.87)	0.26 (0.49)
ΔIncome$_+$	-1.03 (-1.48)	-0.90 (-0.73)	-0.61 (-0.83)	-0.83 (-1.41)
N (%), R^2	239 (86), 0.27	203 (73), 0.36	239 (86), 0.33	255 (91), 0.33
Eq.Slope Pv	0.00, 0.00, 0.67	0.00, 0.00, 0.72	0.00, 0.00, 0.94	0.00, 0.00, 0.53

h_0, base bandwidth; tv, t-value; NR, non-response; #Family1, family size before;

HouseSize1, house size per person before; Income1, income per person before;

Eq.Slope Pv, joint & marginal test p-values for slope equality of ΔHealth & ΔHouseSize.

Table 2 presents four sets of estimates depending on h_0, where #Family1, HouseSize1 and Income1 are the levels in 2014, and $\Delta\text{Family}_- \equiv (1-\delta_1)S_1$ and $\Delta\text{Family}_+ \equiv \delta_1 S_1$; the other variables with $\pm$ are analogously defined. We omit the intercept estimates for this table and all other tables to appear. The row 'N (%), R^2' shows the sample size after the localization, the proportion relative to $N = 279$, and R^2; e.g., '203 (73)' in the $h_0 = 1.5$ column means that 203 observations are used, which is 73% of $N = 279$.

In Table 2, age and female dummy are not significant except for age with $h_0 = 2.5$; both are included nonetheless, as they are important demographic variables. Civil servants were adversely affected by the earthquake. The non-respondents to the city question and individuals from prefecture 1 suffered most with slope $-13 \sim -14$; recall that K6 takes on $0 \sim 24$. The non-respondents to the city question felt a stigma of coming from a ravaged city, and thus did not want to reveal the city identity. Reportedly, almost the same thing happened after World War II to those from Hiroshima and Nagasaki. No intercept break due to $\delta_1 \sim \delta_4$ is significant, except for δ_1 with $h_0 = 2.5$.

Among the three 2014 level variables, both family size and income are significant. Among the four changes, family size change is not significant (except for ΔFamily_- with $h_0 = 2.5$) with some wrong (i.e., negative) signs; health change has significantly positive slopes on the negative side and nearly significant negative slopes on the positive side; house size change has some nearly significant positive slopes on the negative side and insignificant positive slopes on the positive side; and income change has insignificant slopes throughout. These findings suggest that the prospect theory might hold for health and house size changes, but not for family size and income changes.

Each entry in the row 'Eq.Slope Pv' presents three Wald test p-values: the joint test for health and house size change slope-equality, the marginal test for health change slope-equality, and the marginal test for house size change slope-equality. All tests reject the equal slope for health change, but not for house size change. Comparing the four estimate columns to the first column for the base model, controlling covariates and $\delta_1 \sim \delta_4$ makes little difference for our main findings, although it increases R^2 from 0.27 to 0.33$\sim$0.36.

4.3 Extended Empirical Analysis for 2014

A more extensive "full" model than (4.2) is obtained by adding the following 12 interaction terms among ΔS_j's:

$$
\begin{array}{rcll}
\text{Interactions with } \Delta S_1 & : & \beta_{12-}(1-\delta_1)\Delta S_1 \cdot (1-\delta_2)\Delta S_2, & \beta_{12+}\delta_1\Delta S_1 \cdot \delta_2\Delta S_2, \\
 & & \beta_{13-}(1-\delta_1)\Delta S_1 \cdot (1-\delta_3)\Delta S_3, & \beta_{13+}\delta_1\Delta S_1 \cdot \delta_3\Delta S_3, \\
 & & \beta_{14-}(1-\delta_1)\Delta S_1 \cdot (1-\delta_4)\Delta S_4, & \beta_{14+}\delta_1\Delta S_1 \cdot \delta_4\Delta S_4; \\
\text{Interactions with } \Delta S_2 & : & \beta_{23-}(1-\delta_2)\Delta S_2 \cdot (1-\delta_3)\Delta S_3, & \beta_{23+}\delta_2\Delta S_2 \cdot \delta_3\Delta S_3, \\
 & & \beta_{24-}(1-\delta_2)\Delta S_2 \cdot (1-\delta_4)\Delta S_4, & \beta_{24+}\delta_2\Delta S_2 \cdot \delta_4\Delta S_4; \\
\text{Interactions with } \Delta S_3 & : & \beta_{34-}(1-\delta_3)\Delta S_3 \cdot (1-\delta_4)\Delta S_4, & \beta_{34+}\delta_3\Delta S_3 \cdot \delta_4\Delta S_4.
\end{array}
$$

Generalizing this further, we may go for triple or quadruple interactions, which we eschew, however, to keep the number of parameters within a reasonable limit.

With the interaction terms, the slopes of ΔS_1 on the negative and positive sides vary across individuals and they are

$$
\begin{aligned}
&\beta_{1-} + \beta_{12-}(1-\delta_2)\Delta S_2 + \beta_{13-}(1-\delta_3)\Delta S_3 + \beta_{14-}(1-\delta_4)\Delta S_4, \\
&\beta_{1+} + \beta_{12+}\delta_2\Delta S_2 + \beta_{13+}\delta_3\Delta S_3 + \beta_{14+}\delta_4\Delta S_4.
\end{aligned} \tag{4.3}
$$

The slopes of ΔS_2 on the negative and positive sides are

$$
\begin{aligned}
&\beta_{2-} + \beta_{21-}(1-\delta_1)\Delta S_1 + \beta_{23-}(1-\delta_3)\Delta S_3 + \beta_{24-}(1-\delta_4)\Delta S_4, \\
&\beta_{2+} + \beta_{21+}\delta_1\Delta S_1 + \beta_{23+}\delta_3\Delta S_3 + \beta_{24+}\delta_4\Delta S_4.
\end{aligned} \tag{4.4}
$$

The slopes of ΔS_3 on the negative and positive sides are

$$
\begin{aligned}
&\beta_{3-} + \beta_{31-}(1-\delta_1)\Delta S_1 + \beta_{32-}(1-\delta_2)\Delta S_2 + \beta_{34-}(1-\delta_4)\Delta S_4, \\
&\beta_{3+} + \beta_{31+}\delta_1\Delta S_1 + \beta_{32+}\delta_2\Delta S_2 + \beta_{34+}\delta_4\Delta S_4.
\end{aligned} \tag{4.5}
$$

The slopes of ΔS_4 on the negative and positive sides are

$$
\begin{aligned}
&\beta_{4-} + \beta_{41-}(1-\delta_1)\Delta S_1 + \beta_{42-}(1-\delta_2)\Delta S_2 + \beta_{43-}(1-\delta_3)\Delta S_3, \\
&\beta_{4+} + \beta_{41+}\delta_1\Delta S_1 + \beta_{42+}\delta_2\Delta S_2 + \beta_{43+}\delta_3\Delta S_3.
\end{aligned} \tag{4.6}
$$

Table 3 provides the estimates for the extended model with $h_0 = 2$. Among the slopes of ΔS_j's, only health change slopes are significant, but among the newly added 12 interaction terms, as many as six are significant, which is an overwhelming evidence against the separability of the value function, despite that separable ones are often used in economics. These terms collectively increased R^2 from 0.33 in Table 2 to 0.41 in Table 3.

With interactions terms in, the slopes of ΔS_j's are not constant; rather, they vary across individuals. Hence, to assess prospect theory, we use figures. Figure 5 presents (4.3)-(4.6) with $h_0 = 2$ obtained from the base model with the interaction terms added extra, and Figure 6 does the same using the estimates in Table 3; i.e., the covariates and $\delta_1 \sim \delta_4$ are used as regressors for Figure 6, but not for Figure 5. In each panel, we plot the individual negative side slope on the horizontal axis, and the positive side slope on the vertical axis. Under the prospect theory, individual slopes should fall in the first quadrant below its 45^o diagonal line for a larger positive slope on the horizontal axis than on the vertical axis.

Table 3. OLS to Interaction Model & $h_0 = 2$ for 2014 (R^2=0.41)

	$\hat{\beta}$ (tv)		$\hat{\beta}$ (tv)
ΔFamily$_-$	0.85 (0.95)	ΔFamily$_-\Delta$HouseSize$_-$	-3.99 (-2.62)
ΔFamily$_+$	0.03 (0.04)	ΔFamily$_+\Delta$HouseSize$_+$	-0.30 (-0.20)
ΔHealth$_-$	4.09 (2.14)	ΔFamily$_-\Delta$Income$_-$	1.92 (3.74)
ΔHealth$_+$	-5.47 (-5.60)	ΔFamily$_+\Delta$Income$_+$	5.07 (3.76)
ΔHouseSize$_-$	-0.89 (-0.55)	ΔHealth$_-\Delta$HouseSize$_-$	0.10 (0.07)
ΔHouseSize$_+$	1.84 (0.98)	ΔHealth$_+\Delta$HouseSize$_+$	-1.69 (-0.87)
ΔIncome$_-$	0.12 (0.16)	ΔHealth$_-\Delta$Income$_-$	-1.17 (-1.55)
ΔIncome$_+$	-0.13 (-0.20)	ΔHealth$_+\Delta$Income$_+$	12.08 (4.27)
ΔFamily$_-\Delta$Health$_-$	-0.52 (-0.75)	ΔHouseSize$_-\Delta$Income$_-$	-2.39 (-1.98)
ΔFamily$_+\Delta$Health$_+$	-1.69 (-2.28)	ΔHouseSize$_+\Delta$Income$_+$	-2.13 (-1.91)

(4.2) with interaction terms added; the same covariates and $\delta_1 \sim \delta_4$ used; only the terms with Δ are presented; tv, t-value.

The purpose of presenting both Figures 5 and 6 is to demonstrate that, regardless of using covariates and $\delta_1 \sim \delta_4$ as regressors, the main findings from Figures 5 and 6 do not change, as Figures 5 and 6 are little different. Changing h_0, however, makes some difference as the two figures in the appendix with $h_0 = 2.5$ show; the extended model is not estimable with $h_0 = 1.5$. In the following, we interpret only Figure 5.

When interaction terms making slopes heterogeneous are omitted, the resulting constant

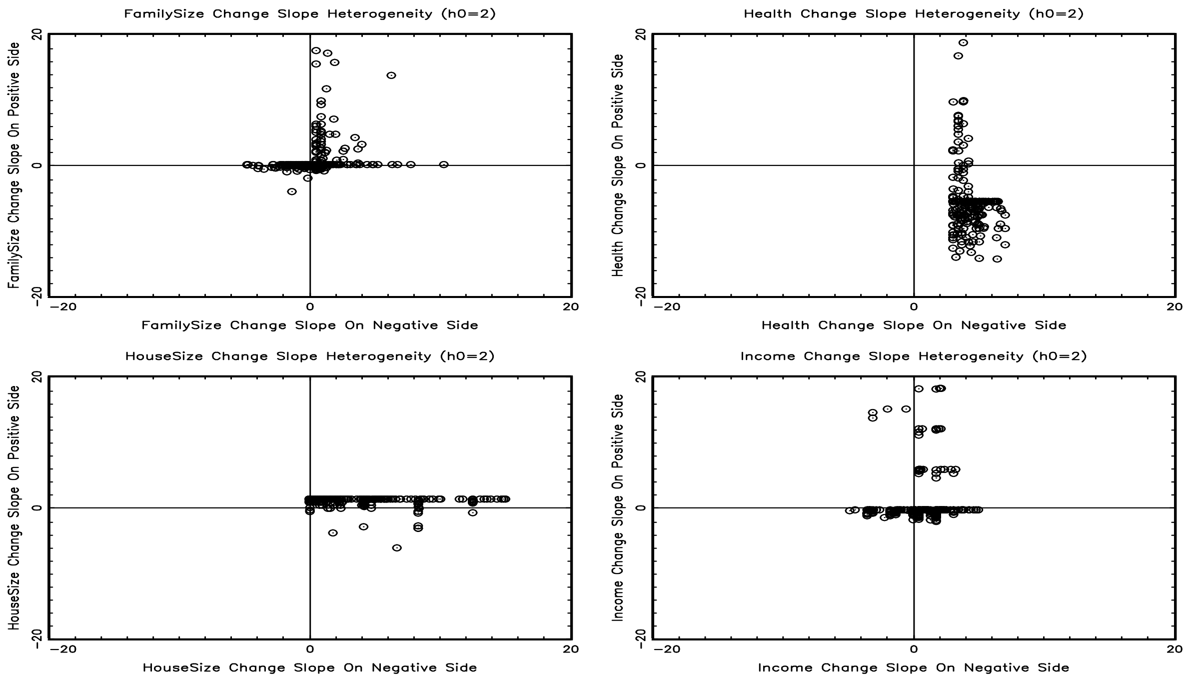

Figure 5: Slopes in Base Model: Circles in 1st Quadrant below 45^o Support Prospect Theory

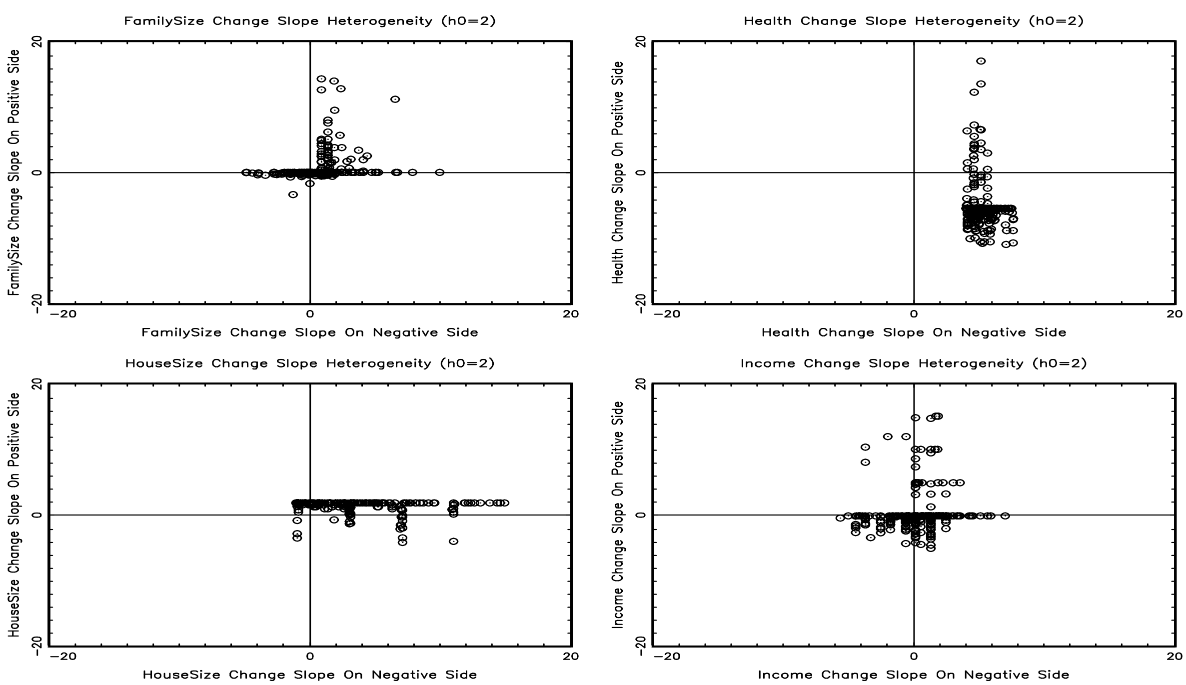

Figure 6: Slopes in Full Model: Circles in 1st Quadrant below 45^o Support Prospect Theory

slope becomes an weighted average of the heterogeneous slopes. First, family size change plots in Figure 5(a) are scattered in all quadrants, which is why insignificant mixed signs appeared in Table 2; 30% of the circles fall in the desired region (i.e., below the 45^o line in the first quadrant). Second, there is nobody on the second and third quadrants for health change, which is why ΔHealth$_-$ is significant with positive slopes in Table 2; only 3.3% of the circles fall in the desired region. Third, for house change, *74% of the circles for house size change fall in the desired region* (and 83% and 80% do with $h_0 = 2.5$ in the two figures of the appendix). Fourth, for income change, almost 0% fall in the desired region.

Overall, the prospect theory seems to hold for house size change. The interaction-augmented non-separable model and Figure 5 are helpful, as they reveal that the prospect theory is most plausible for house size change. In contrast, (4.2) and Table 2 suggest that the prospect theory holds for health change, which is, however, due to wrongly omitting the interaction terms.

4.4 Empirical Analysis for 2013

For the 2013 data, we proceed analogously to what we did for the 2014 data. One big difference is though that there is no house size information in the 2013 survey, which is unfortunate because house size change is the only variable for which the prospect theory seems to hold in the 2014 data. For 2013, hence, there are only three scores (*family size* S_1*, health* S_2*, and income* S_3). The baseline health level information is not available either. Despite these, an advantage in the 2013 data is that there are more people reporting positive health changes: the proportion of ΔS_j being positive before localization is

$$P(\Delta S_1 > 0) = 0.088, \quad P(\Delta S_2 > 0) = 0.044 \text{ (18 persons)}, \quad P(\Delta S_3 > 0) = 0.38$$

which is based on $N = 408$ after removing the observations with missings in the key variables. There are 18 individuals with $\Delta S_2 > 0$, which is a three-fold increase from only six in 2014.

In the following empirical findings for 2013, we omit the covariate balance check that turns out to be somewhat worse than in the 2014 data, and we also simplify our presentations by skipping some of the detailed discussions done for the 2014 data. Our main findings do not change regardless of whether we use covariates and δ_j's as regressors or not as in the 2014 data. Also as in the 2014 data, we do not localize health change that takes on only 0, ± 1 and ± 2.

Our first model for 2013 is

$$Y = \beta_0 + \{\beta_{age}\text{Age} + \beta_{female}\text{Female} + \beta_{nrcity}\text{NRcity} + \beta_{pref1}\text{Pref1}$$
$$+ \sum_{j=1}^{3}\beta_{\delta j}\delta_j\} + \beta_1 S_1 + \beta_3 S_3 + \sum_{j=1}^{3}\beta_{j-}(1-\delta_j)\Delta S_j + \sum_{j=1}^{3}\beta_{j+}\delta_j\Delta S_j + U; \quad (4.7)$$

differently from the 2014 data, 'civil servant' is not available as a job category. In fact, the entire education and job categories are slightly different between the two surveys.

Table 4. OLS to Model (4.7) for 2013 ($N = 408$)

	h_0=2: $\hat{\beta}$ (tv)	h_0=1.5: $\hat{\beta}$ (tv)	h_0=2: $\hat{\beta}$ (tv)	h_0=2.5: $\hat{\beta}$ (tv)
Age		-0.02 (-0.77)	-0.03 (-1.77)	-0.03 (-1.55)
Female		0.48 (0.70)	0.33 (0.48)	0.41 (0.62)
NR to city		-5.58 (-1.40)	-5.56 (-1.34)	-5.72 (-1.39)
Prefecture 1		-6.09 (-1.53)	-6.12 (-1.48)	-6.20 (-1.51)
δ_1		-1.58 (-1.14)	-1.90 (-1.94)	-1.84 (-1.91)
δ_2		-0.29 (-0.23)	0.14 (0.11)	0.20 (0.16)
δ_3		-1.19 (-1.41)	-0.67 (-0.87)	-0.59 (-0.82)
#Family1	0.57 (2.57)	0.40 (1.58)	0.37 (1.51)	0.39 (1.63)
Income1	0.19 (0.83)	-0.02 (-0.06)	0.06 (0.24)	0.12 (0.53)
ΔFamily$_-$	-0.23 (-0.66)	0.52 (0.49)	0.73 (1.27)	0.60 (1.06)
ΔFamily$_+$	0.44 (0.49)	1.98 (2.24)	0.52 (0.61)	0.01 (0.02)
ΔHealth$_-$	4.46 (10.0)	4.51 (4.34)	4.20 (3.97)	4.17 (4.07)
ΔHealth$_+$	2.48 (3.33)	2.34 (3.21)	2.33 (3.26)	2.34 (3.26)
ΔIncome$_-$	0.52 (1.41)	1.21 (2.28)	0.92 (2.19)	0.95 (2.87)
ΔIncome$_+$	0.38 (0.53)	1.58 (1.76)	0.66 (0.90)	0.10 (0.19)
N (%), R^2	358 (88), 0.32	324 (79), 0.35	358 (88), 0.34	270 (0.91), 0.34
Eq.Slope Pv	0.13, 0.045, 0.99	0.23, 0.093, 0.93	0.33, 0.15, 0.95	0.13, 0.15, 0.36

h_0, base bandwidth; tv, t-value; NR, non-response; #Family1, family size before; Income1, income before; Eq.Slope Pv, joint & marginal test p-values for slope equality of ΔHealth & ΔIncome.

In Table 4, the effects of NR to city and prefecture 1 are much weaker than in Table 2. The δ's are not significant, except possibly for δ_1, which was also the case in Table 2. As for the three scores, family size change slopes have mostly positive signs with all being insignificant as in Table 2 except for ΔFamily$_+$ with $h_0 = 1.5$; health change slopes are all significantly positive on both sides—recall that the slopes on the positive side were negative in Table 2; and the income slopes are positive, with the negative side slopes significant. Overall, the prospect theory might hold for health and income changes. In the last row with the p-values for the joint and marginal equal slope tests for health and income changes, the test nearly rejects only for health change with $h_0 = 1.5$ and with $h_0 = 2$ in the base model.

A more extensive model than (4.7) is obtained by adding 6 interaction terms:

$$
\begin{aligned}
\text{Interactions with } \Delta S_1 \quad &: \quad \beta_{12-}(1-\delta_1)\Delta S_1 \cdot (1-\delta_2)\Delta S_2, \quad \beta_{12+}\delta_1\Delta S_1 \cdot \delta_2\Delta S_2, \\
&\quad\ \ \beta_{13-}(1-\delta_1)\Delta S_1 \cdot (1-\delta_3)\Delta S_3, \quad \beta_{13+}\delta_1\Delta S_1 \cdot \delta_3\Delta S_3; \\
\text{Interactions with } \Delta S_2 \quad &: \quad \beta_{23-}(1-\delta_2)\Delta S_2 \cdot (1-\delta_3)\Delta S_3, \quad \beta_{23+}\delta_2\Delta S_2 \cdot \delta_3\Delta S_3;
\end{aligned}
$$

The slopes of ΔS_1, ΔS_2 and ΔS_3 are obtained as in (4.3)-(4.6) by dropping all terms with subscript 4. Table 6 provides the estimates using the extended model with $h_0 = 2$. Among the slopes of the ΔS_j's, only the health change slopes are significant. Among the newly added six interaction terms, two involving Δincome are significant.

Table 5. OLS to Interaction Model & $h_0 = 2$ for 2013 (R^2=0.36)

	$\hat{\beta}$ (tv)		$\hat{\beta}$ (tv)
ΔFamily$_-$	0.64 (0.95)	ΔFamily$_-\Delta$Health$_-$	0.11 (0.20)
ΔFamily$_+$	0.13 (0.15)	ΔFamily$_+\Delta$Health$_+$	0.40 (0.42)
ΔHealth$_-$	3.28 (2.77)	ΔFamily$_-\Delta$Income$_-$	-0.43 (-0.85)
ΔHealth$_+$	2.49 (2.91)	ΔFamily$_+\Delta$Income$_+$	3.94 (2.35)
ΔIncome$_-$	-0.71 (-1.26)	ΔHealth$_-\Delta$Income$_-$	-1.62 (-2.88)
ΔIncome$_+$	0.46 (0.60)	ΔHealth$_+\Delta$Income$_+$	-0.71 (-0.64)

(4.7) with interaction terms; the same covariates & $\delta_1, \delta_2, \delta_3$ used; only the terms with Δ are presented; tv, t-value.

Figures 7 and 8 present three individual slope plots for ΔS_j's; Figure 8 uses (4.7) with interactions, whereas Figure 7 does not use covariates and $\delta_1, \delta_2, \delta_3$ as regressors. Recall that circles should be on the first quadrant below the 45^o line under the prospect theory. Since the two figures are close, we interpret only Figure 7. *The percentages of the slopes in the desired region are, respectively, 1%, 99% and 62% for family size, health and income changes, providing evidence for prospect theory in health and income changes in the 2013 data; also,*

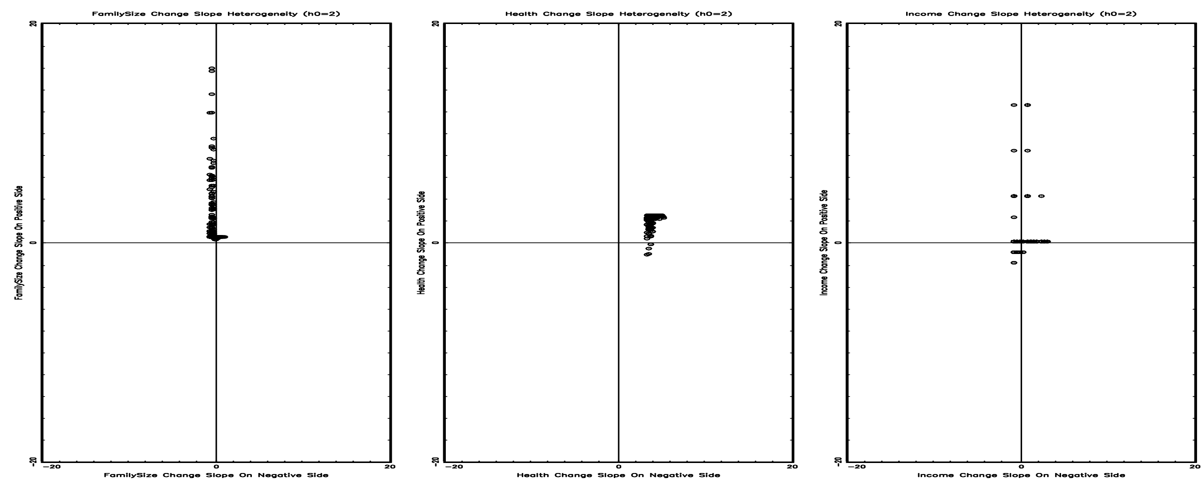

Figure 7: Individual -&+ Side Slopes in Base Model for ΔFamilySize (left), ΔHealth (middle) and ΔIncome (right): Circles in 1st Quadrant below 45^o Support Prospect Theory

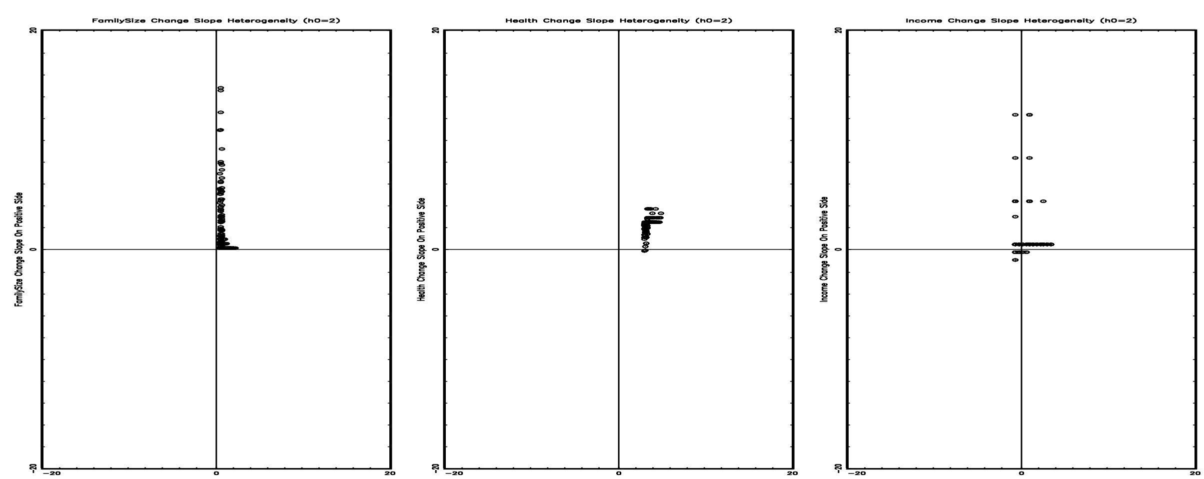

Figure 8: Individual -&+ Side Slopes in Full Model for ΔFamilySize (left), ΔHealth (middle) and ΔIncome (right): Circles in 1st Quadrant below 45^o Support Prospect Theory

the value function is not separable. It should be borne in mind, however, that house size change was omitted, and only 18 persons reported health increase.

4.5 Remarks on Quality Control and Reference Point Change

A caution is warranted that quality/trait is not controlled in our analysis. For instance, a decrease in family size may be the death of a spouse or a grandma, which are qualitatively different. Also the house quality may be different when house size per person changes. The house quality is not necessarily lower when house size per person decreases, because when individuals moved out after the disaster, there were various options (relative' houses, company-provided housing, temporary public housing, new rent-supported apartment, etc.) whose quality is not necessarily lower. House size per person can decrease as well by somebody moving in, in which case there is no quality change. As long as these unobserved covariates for quality/trait are balanced by randomization, they would not affect our findings.

It is possible that the reference point changes over time. For instance, for income change, the government fully compensated for the pre-disaster income in 2013, no matter how much they earned after the disaster. However, when the second survey was conducted in 2014, the TEPCO's compensation policy changed, and TEPCO covered only the lost income, taking the current income into account. Such policies and surrounding circumstances might have changed reference points of some residents for income change.

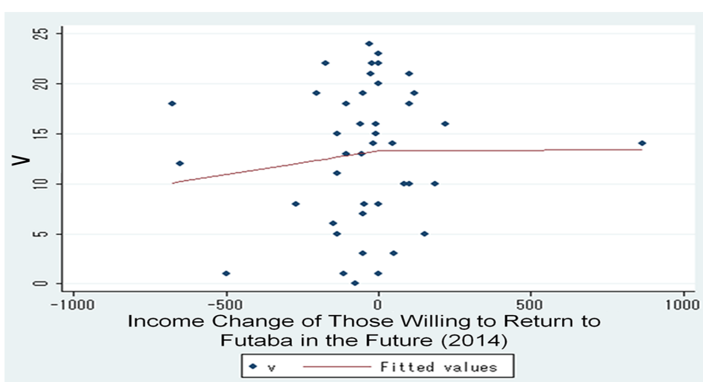

Figure 9: Utility v. Income Change for Those Willing to Return to Futaba (N=42)

In our 2014 survey, the question for whether the residents would go back to Futaba or not is included. We use this question to separate those with reference point change and those without, because the reference point of those who answered yes to indicate the willingness to return to Futaba (66 persons) may be the same as the pre-disaster level whereas the reference point of those who answered no could have changed. The OLS of regressing utility on income change for only those who answered yes is in Figure 9 (sample size 42 with no missings), which suggests loss aversion.

It is important to be aware that if the reference point was lowered, the victims must be overcompensated to recover their original utility; Figure 12 in the appendix explains this point succinctly. Also, varying reference points may be the reason why we could not find evidence for prospect theory in income with the 2014 data, although we did with the 2013 data to an extent. Empirical study on endogenous determination of reference point would be an interesting topic for future research.

5 Conclusions

The conventional expected utility theory explains how individuals facing uncertain outcomes make decisions. There are, however, many individual behaviors that cannot be understood with the expected utility theory. For those, prospect theory provides alternative explanations. Does the prospect theory work? There are several studies reporting positive laboratory experimental evidence, but no evidence based on real world data exists as far as we know. This paper provides probably the first such evidence by testing the two main tenets of prospect theory: reference dependence and loss aversion.

Following the well known Tsunami and Fukushima Nuclear Disaster in March 2011, we conducted surveys in 2013 and 2014 for the residents from Futaba (a town in Fukushima prefecture), because this disaster provides a natural experiment where losses/gains were not self-selected. We took mental distress measured by Kessler 6 (K6) as the dependent variable, and used 'negative K6' as value/utility to be explained by four important factors: family size, health, house size, and income. The pre-disaster level serves as a reference point and we measured changes in these four variables. Understandably, there were far more losses than gains, but except for health, the proportion of gains was not too small (8% or higher) to enable our empirical analysis. For house size per person, there were almost equal numbers of losses and gains.

Loss aversion combined with reference dependence implies a regression function with different slopes around the reference point, with prospect theory predicting the steeper slope on the negative (i.e., loss) side than on the positive (i.e., gain) side. This is related to the recent 'regression kink (RK) design' in the treatment effect literature. We adopted the RK approach in that we used a local sample around the reference point so that we could save on the number of control variables, given a relatively small sample size (a couple of hundreds) compared with many parameters to estimate. Such a localization combined with the natural experiment ensured that most covariates, observed or unobserved, are balanced as in randomized experiments. We ended up using $73 \sim 91\%$ of the data, which means that not much localization was done. Nevertheless, the covariate balance was still adequate, and our main findings hardly changed with or without the covariates controlled, which we attribute to the natural experiment.

Our main empirical finding is that there is evidence for prospect theory in the 2014 data with house size. In the 2013 survey, house size variable is not available unfortunately, but we still found evidence for prospect theory with health and income; our evidence with health is qualified though, because the number of persons with positive health changes was rather small. Why then was there no evidence for prospect theory in the 2013 data with income? One possible reason is a revised (i.e. lowered) reference point of income in 2014. We also found that Fukushima prefecture residents, civil servants, and residents who did not answer the original residential place question in our survey have substantially low mental health status.

Our findings have a few important implications. First, a sufficient, apparently more than enough, compensation should be provided to those who suffered a loss so that they can recover their original utility level. Second, if the reference point is lowered after the disaster, then the victims should be over-compensated for their loss to recover the original utility. Third, utility/value functions imposing separability should be used with caution. Additionally, given the multi-dimensionality of loss aversion, intervention programs should be also multi-dimensional including health care as well as individual/group counseling.

APPENDIX

Appendix Table 1. Descriptive Statistics 2014 ($N = 654$)

Variable	Obs	Mean,SD,Min,Max	Variable	Obs	Mean,SD,Min,Max
K6 (Kessker 6)	594	8.6, 6.0, 0, 24	junior high	654	0.09, 0.28, 0, 1
#Family after	643	2.7, 1.5, 1, 12	high sch.	654	0.53, 0.50, 0, 1
#Family before	625	3.4, 1.8, 0, 10	specialty sch.	654	0.11, 0.31, 0, 1
Δ#Family	621	-0.75, 1.7, -8, 5	vocational	654	0.01, 0.08, 0, 1
ΔHealth	638	-0.68, 0.68, -2, 2	com. college	654	0.03, 0.18, 0, 1
House size after	403	2.1, 22, 0.04, 451	college	654	0.15, 0.36, 0, 1
House size before	523	0.69, 0.72, 0, 8	graduate sch.	654	0.01, 0.10, 0, 1
ΔHouse size	365	0.24, 1.6, -7.1, 19	other sch.	654	0.03, 0.18, 0, 1
Income after	477	1.6, 1.8, 0, 13	NR to sch.	654	0.04, 0.20, 0, 1
Income before	465	2.1, 1.5, 0, 10	employee	654	0.36, 0.48, 0, 1
ΔIncome	451	-0.46, 1.8, -8.8, 8.7	civil servant	654	0.08, 0.27, 0, 1
Age	641	63, 14, 26, 96	doctor/lawyer	654	0.00, 0.06, 0, 1
Female	654	0.20, 0.40, 0, 1	farmer	654	0.11, 0.31, 0, 1
No family loss	654	0.24, 0.43, 0, 1	fisherman	654	0.00, 0.04, 0, 1
Some family loss	654	0.13, 0.33, 0, 1	self-employed	654	0.12, 0.32, 0, 1
NR to family loss	654	0.62, 0.49, 0, 1	part time	654	0.02, 0.14, 0, 1
			housework	654	0.02, 0.15, 0, 1
			retired	654	0.12, 0.33, 0, 1
			none	654	0.07, 0.26, 0, 1
			others	654	0.07, 0.25, 0, 1
			NR to job	654	0.02, 0.14, 0, 1

#Family, cohabiting family member #; Δ, change; House size, house size per person in 100 m^2; Income, income per person in JPY 10^6; NR, non-response; com., community; sch, school; Obs, #observed; NR regarded as an observed category for family loss, school, job.

Appendix Table 2. Descriptive Statistics 2013 ($N = 585$)					
Variable	Obs	Mean,SD,Min,Max	Variable	Obs	Mean,SD,Min,Max
K6 (Kessker 6)	524	8.6, 6.0, 0, 24	junior high	585	0.08, 0.27, 0, 1
#Family after	544	2.3, 1.4, 1, 10	high sch.	585	0.56, 0.50, 0, 1
#Family before	555	3.0, 1.7, 1, 10	com. college	585	0.08, 0.28, 0, 1
Δ#Family	535	-0.72, 1.6, -7, 9	college	585	0.12, 0.33, 0, 1
ΔHealth	525	-0.73, 0.75, -2, 2	other sch.	585	0.07, 0.26, 0, 1
Income after	489	1.7, 1.8, 0, 9.9	NR to sch.	585	0.08, 0.27, 0, 1
Income before	501	1.9, 1.4, 0, 10	employee	585	0.38, 0.49, 0, 1
ΔIncome	471	-0.17, 1.9, -8.6, 7.4	farmer	585	0.10, 0.31, 0, 1
Age	575	63, 14, 24, 94	fisherman	585	0.00, 0.04, 0, 1
Female	585	0.21, 0.41, 0, 1	doctor/lawyer	585	0.01, 0.07, 0, 1
No family loss	585	0.26, 0.44, 0, 1	self-employed	585	0.10, 0.30, 0, 1
Some family loss	585	0.10, 0.31, 0, 1	part time	585	0.03, 0.18, 0, 1
NR to family loss	585	0.64, 0.48, 0, 1	housework	585	0.02, 0.14, 0, 1
			retired	585	0.12, 0.33, 0, 1
			none	585	0.03, 0.17, 0, 1
			others	585	0.03, 0.17, 0, 1
			NR to job	585	0.14, 0.35, 0, 1

#Family, cohabiting family member #; Δ, change; Income, income per person in JPY 10^6;

NR, non-response; com., community; sch, school; Obs, #observed;

NR regarded as an observed category for family loss, school, job.

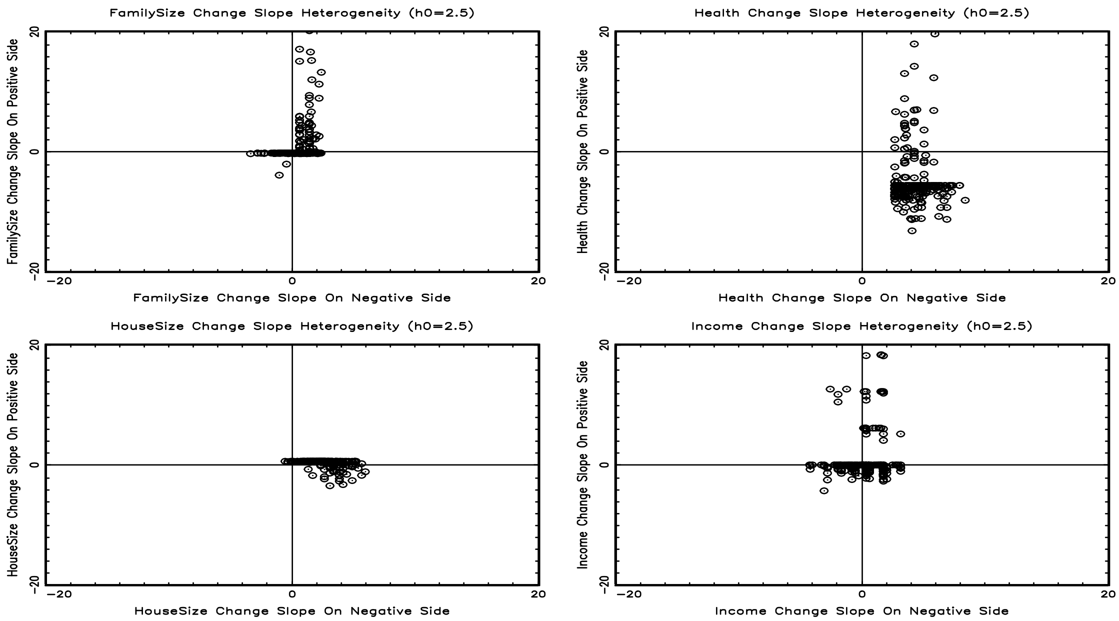

Figure 10: Slopes in Base Model: Circles in 1st Quadrant below 45^o Support Prospect Theory

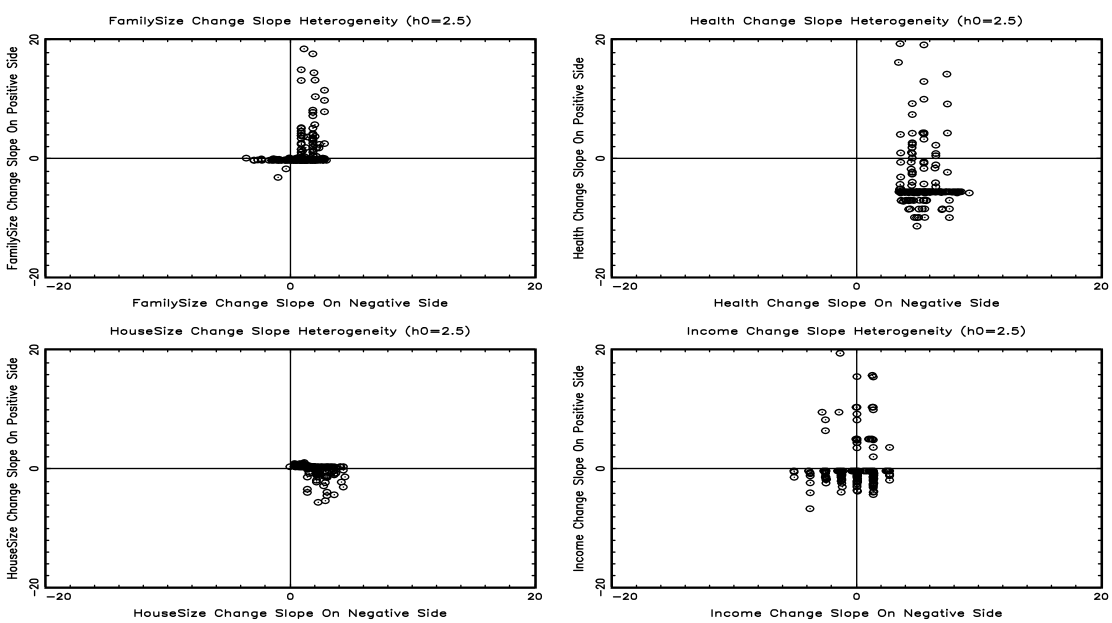

Figure 11: Slopes in Full Model: Circles in 1st Quadrant below 45^o Support Prospect Theory

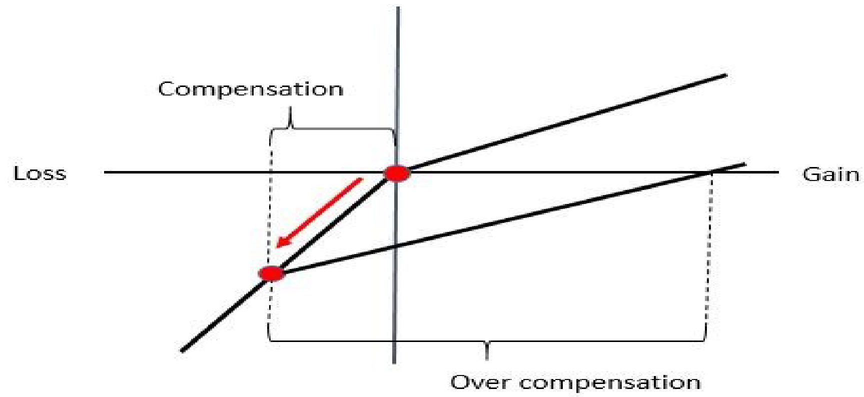

Figure 12: Over-Compensation is Necessary if the Reference Point is Lowered

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