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Utteeyo Dasgupta
Lata Gangadharan
Pushkar Maitra
Subha Mani

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Utteeyo Dasgupta
Wagner College

Lata Gangadharan
Monash University

Pushkar Maitra
Monash University

Subha Mani
CIPS, Fordham University,
PSC, University of Pennsylvania and IZA

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IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

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ABSTRACT

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The goal of this paper is to examine stability in preferences using the Stigler-Becker state-dependent framework. Using a randomized intervention that changes the opportunity sets of individuals we construct a unique panel data from an artefactual field experiment and evaluate whether the change in the state space influences our selected indicators of preferences: risk, competitiveness, and confidence. We find that there is considerable heterogeneity of preferences across individuals at a point in time; risk and competitive preferences inter-temporally are consistent with state-dependent preferences, while measures of confidence seem to depend on past experiences.

JEL Classification: C9, D01, D03

Keywords: preference stability, state contingent preferences, artefactual field experiment

Corresponding author:

Subha Mani
Fordham University
Department of Economics
441 East Fordham Road
Dealy Hall, E 520
Bronx, New York 10458
USA
E-mail: smani@fordham.edu

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1. Introduction

The assumption of stable exogenous preferences is crucial for neoclassical economic theory. Without it one cannot infer the causal connection between changes in opportunity sets and predicted changes in choices under the neoclassical framework. Consequently, if preferences are endogenous, and get affected by the very policies themselves, it is not possible to isolate and evaluate the effects of a new policy or a change in a policy.

While the assumptions of homogeneous non-mutable preferences have remained mostly sacrosanct in neoclassical economic theory (barring a few early exceptions as in Pollak (1976), Elster (1979), Winston (1980), West and McKee (1983), Hirschman (1984), and Cowen (1989)), recent years have witnessed a renewed interest in evaluating the plausibility of these assumptions. There is a developing strand of research that claims preferences to be endogenous, acquired, and mutable, amenable to reconstructions and exogenous influence (see Ariely *et al.* (2005)). This literature suggests that there are a number of factors – exogenous as well as endogenous – such as our efforts, experiences, evolving norms, advertising, exposure to different cultures, conflict, and catastrophic events that can strongly influence and modify preferences in many domains of economic choice (see for example Hoeffler and Ariely (1999), Ariely *et al.* (2003), Ariely *et al.* (2005), Bowles (2009), Eckel *et al.* (2009), Castillo and Carter (2011), Voors *et al.* (2012), and Dean and Sautmann (2014) among others).

In their seminal article Stigler and Becker (1977) suggest that an economist faced with evidences of apparent inconsistencies in temporal choices should consider a framework of state contingent preferences rather than rely on the arbitrariness of evolving preferences. They posit that preferences should be defined over different state spaces such that choices can be state specific, and therefore, what appears to be preference inconsistency—and hence changing preferences, can now be rationalized in the familiar framework of stable exogenous preferences when choices are state-contingent. Their proposed framework brings us back to a preference relation that is globally stable but can accommodate differences in choices that are state-specific. In this world, observed

differences in choices, temporally or across individuals need not necessarily lead to an inconsistency in traditional economic theory and observed data anymore.

What constitutes an appropriate state remains an open question. Stigler and Becker (1977) primarily consider prices and income as the state variables. So the states could certainly include the economic opportunities faced by individuals such as employment opportunities. Additionally, Andersen *et al.* (2008) suggest that states can include features as trivial as the weather, or as critical as the individual's mortality risk.[1] The critical assumption here is that the state space must be orthogonal to individuals' choices. Understandably, due to the nature of temporal data that is needed to evaluate such a proposition, the task becomes harder, and consequently the empirical validation for the Stigler-Becker framework remains sparse. Researchers not only need panel observations to test the theory, but also need to be able to identify state spaces that change exogenously.

In this paper we utilize an exogenous change in the state space/opportunity set of individuals that allows us to examine stability in preferences under the Stigler-Becker state-dependent framework. This exogenous change is the result of an experimental intervention. Women residing in specific slums of New Delhi, India were invited to participate in an artefactual field experiment; the first round was conducted in 2010, and the second one a year later in 2011. In the one-year between the two experiments, participants through a public lottery were randomly allocated to receive access to a six-month subsidized vocational training program. This is a clear exogenous change in the opportunity set of a randomly selected set of women. The random assignment of participants to the vocational training program, by its very design, gives us a unique way of providing direct evidence on the issue of temporally stable preferences where the state space is being changed with experimental control, and remains orthogonal to subject decisions. This constitutes an important innovation of our study.

Recent work by Straznicka (2012) uses multiple elicitation methods to test for temporal stability of risk preferences at the individual level as well as at the aggregate

[1] While weather shocks can be a useful backdrop for looking at choices before and after the shock, natural disasters mostly come unannounced. This can restrict the ideal comparison of choices (see Eckel *et al.* 2009 for an interesting examination of risk preferences after a storm).

level controlling for personality traits and performances in a market game. Zeisberger *et al.* (2012) focused on the stability of prospect theory parameters over a one-year period. Both papers find considerable temporal stability in preferences. However, they do not use the state-dependent framework, and instead focus exclusively on reporting subject behavior over time. The paper closest to ours is by Andersen *et al.* (2008), who use the notion of state dependent preferences and examine temporal stability in risk preferences using panel data collected for the Danish population over a 17-month period. In contrast to our approach, they use survey questionnaires as a way to identify state space. Understandably, this can lead to problems of endogeneity and self-reported bias in identifying state spaces. Andersen *et al.* (2008) conclude that although there is some variation in risk attitudes over time, there is no general tendency for the risk attitudes to either increase or decrease temporally.

To the best of our knowledge, this paper is the first to provide evidence relating to the issue of stability in preferences using the state-dependent framework. Specifically we: (*i*) introduce states of nature that are exogenously determined for the subjects with experimental control; (*ii*) use a unique panel data separated by a year, on behavioral choices relating to risk preference, competition, and measures of confidence from low-income households; (*iii*) combine data from an artefactual field experiment and responses from primary surveys; (*iv*) control for changes in the socioeconomic environment; and (*v*) provide alternate empirical strategies to evaluate preference stability in our state-contingent framework.

Our results suggest that risk and competitive preferences are consistent with the Stigler-Becker framework of state dependent choices and are temporally stable. For our measure of confidence we find some influence of experience/task specific learning. Comparing across subjects however, we find considerable variability across subject choices even after controlling for the variation in state space at a point in time. Overall our results lend qualified support to the often-used adage in economic theory **"**De Gustibus Non Est Disputandum!"[2]

[2] "Tastes neither change capriciously nor differ importantly between people. [Tastes] will be there next year, too, and are the same to all men." (Stigler and Becker (1977), page 76).

2. Experimental Design and Data

In 2010 two non-governmental organizations based in New Delhi in India (Pratham and SATYA, Social Awakening Through Youth Action) jointly offered a 6-months long, subsidized vocational training program in stitching and tailoring to women who were between the ages of 18 and 39, had completed at least 5 grades of completed schooling, and were residents of specific slums of New Delhi. More information on the actual intervention is presented in Maitra and Mani (2013). The artefactual field experiments that we utilize in this paper were conducted as a part of the impact evaluation of this intervention. A subset of the applicants to the training program (a total of 121 women) participated in the artefactual field experiment both in 2010 (pre-intervention), and in 2011 (5-months post-intervention). Of these participants, 82 women were ultimately assigned to the treatment group (received the training) and the remaining 39 were assigned to the control group (did not receive the training). Assignment to the treatment was determined through a public lottery. The experimental sessions in 2010 (the baseline experiment) were conducted before the lottery to determine treatment status. The follow-up experiments in 2011 were conducted five months after the completion of the program. Since the assignment to the training program was randomized and unknown to the subjects, to the researchers, and the associated non-governmental organizations at the time of the pre-intervention sessions, the change in the opportunity set for those who were included in the training program can be treated as exogenous. To verify that the assumption of exogenous realizations of the state space is indeed valid, we report separately the baseline averages of the outcome variables of interest (see Panel A, Table 1) for those in the treatment and the control group. We discuss these in more detail below.

Attrition is always a concern whenever subjects are followed over time. For example, Andersen *et al.* (2008) are able to track only 38 percent of their baseline participants in subsequent visits, with a consequent attrition rate of 62 percent. In our study the attrition rate from the baseline to the follow-up experiment is 17.12 percent. Importantly, there are no statistically significant differences (*p-value* = 0.98, t-test) in the attrition rates across subjects from the treatment group (17.17 percent) and the control

group (17.02 percent). In our framework attrition rates can result in additional concerns especially if it were found to be endogenous to the treatment status/state space. Unlike in Andersen *et al.* (2008), in which the orthogonality between state space and attrition cannot be fully verified, in our paper we verify statistically the absence of any state specific attrition behavior.

2.1 Experimental Games

In 2010, each subject participated in two games (similar to those reported in Gneezy *et al.* (2009)). The first, an investment game, was designed to evaluate subjects' attitudes towards risk. In this game, participants were endowed with Rs 50 and had the option to allocate any portion of their endowment to a risky asset that had a 50% chance of quadrupling the amount invested. The invested amount could also be lost with a 50% probability. A coin flip decided the outcome. The subjects retained any amount that they chose not to invest.

The second game, designed to investigate the intrinsic competitiveness of subjects (competition game), consisted of a real-effort task that determined payoffs in the competition game. The real-effort task consisted of filling up 1.5 fl oz. zip lock bags with beans in one minute. Our choice of the real-effort task was specific to our field conditions. We chose a real effort task that would be an easily comprehensible task for all our participants. Prior to participating in the real-effort task each subject had to choose one of two possible methods of compensation, a piece-rate compensation method (where payoffs depended solely on their own performance) or a competition-rate compensation method (where the subject's earnings depended on her performance relative to a randomly chosen subject in the same session). In the piece-rate method a subject received Rs. 4 for each correctly filled bag; while in the competition-rate method a subject received Rs. 16 per bag if she filled at least as many bags as her matched opponent, and received nothing if she had filled less bags than the matched opponent. While choosing the compensation method, the subjects were also asked to guess how many bags they expected to fill up, and their expected relative rank based on their bag-filling performance. More discussion of the tasks is presented in Dasgupta *et al.* (2014).

In 2011, we made minor changes to the presentation of games described above in

an attempt to minimize the effect of learning or familiarity with these games. In the investment game, we chose to roll a 6-sided die where '1, 2, 3' determined success of the investment and '4, 5, 6' resulted in failure of the investment (instead of using a coin to demonstrate the 50 percent probability). In the competition game, we changed the size of the zip lock bag and the bean type used in the real effort task to make it less obvious for participants to use their own last years' performances as a benchmark or anchor for their absolute performance. The instructions used in 2010 are available in Appendix 2.

While every subject participated in both the games in a session, only one of the games was chosen for payment purposes. This was explained at the very beginning. Each subject participated in only one session every year where an average session lasted for about 2 hrs. The average payment received from participation was Rs. 203 (including a show-up fee of Rs 150).[3]

We construct four outcome variables of interest based on observed choices**: (***a*) the *CRRA coefficient* derived from choices in the investment game[4]; (*b*) an indicator for *competitiveness* that takes a value 1 if the subject chooses the competitive wage scheme in the competition game, and 0 otherwise; (*c*) a measure for *self ranking* which takes a value between 1 and 5 (5 best, 1 worst) to capture subjects' perceptions of relative rankings of their expected performances in the competition game compared to other subjects in the session; (*d*) a measure of *overconfidence*, computed as the ratio of expected number of bags filled in the competition game divided by the actual number of bags filled in the game. We define a value less than 1 to be reflective of *under-confidence* and a value greater than 1 reflective of *overconfidence*. Panel A in Table 1 presents the baseline averages of these outcomes variables, both for the full sample and also separately by eventual status (treatment or control group) in the randomized training

[3] The official minimum wages for unskilled workers in Delhi was Rs 203 per day at the time of running these experiments (in 2010). The minimum wage legislations are however rarely imposed in India, and most women in our sample would be receiving less than this stipulated amount. Cardenas and Carpenter (2008) in their survey of field experiments in developing countries argue that paying on average one to two days wage for a half-day session creates the necessary salience for participants in the field (page 331). For a two-hour session that we conducted, a day's worth of wages satisfies this criterion. The exchange rate at the time of running these experiments was $1 (US) = Rs 46.

[4] See Appendix 1 for a discussion on the derivation of the CRRA coefficient for our task.

program intervention. Panel B of Table 1 presents the corresponding averages of a set of socioeconomic characteristics (age in years, level of education, dummies for caste (SC, ST, and OBC), marital status, ownership of house, and participation in a Rotating Savings and Credit Association (ROSCA)). These variables are used in the regressions (see below).

The mean difference between the two groups (reported in Column 4, Table 1) is statistically not significant even at the 10 percent significance level for all outcome variables and socioeconomic characteristics except self-ranking. Further, when we conduct a test of joint significance of differences of all the baseline characteristics (outcomes and socioeconomic characteristics), we cannot reject that the baseline characteristics of women in the treatment and control groups are same (F-stat = 1.25, p-value = 0.25). The balance in characteristics at the baseline indicates that the random assignment into the treatment and control group was successful, thus establishing the fact that the change in opportunity set for the treatment group was indeed exogenous.

3. Conceptual Framework

3.1 State Space and Exogeneity

In this section we introduce the implications of the state dependent choices in the context of our experiment design and outline the associated testable predictions. Stigler and Becker (1977) maintain that preferences are exogenously determined, state-dependent, and homogeneous across individuals. Consequently, any heterogeneity in choices either *within-subjects* temporally, or *across-subjects* should be traced back to any possible difference in state spaces, i.e., underlying opportunity sets. When we assume choices to be state dependent, one needs to question whether changes in state spaces (opportunity sets) are necessary conditions for changes in observed choices or whether they are merely sufficient. Stigler and Becker seem to suggest that they are both.[5] So any change in

[5] "… stable behavior by (say) households faced with stable prices and incomes—or more generally a stable environment—is no contradiction since stability then is implied as much by personal interest theories as by custom and tradition. On the other hand, stable behavior in the face of changing prices and incomes might contradict the approach taken in this essay that assumes utility maximizing with stable tastes" (Stigler and Becker (1977), page 82)

observed choices must be due to underlying changes in states (opportunity sets), and furthermore, a no-change in state space (opportunity sets) must imply a corresponding no-change in observed choices. Note though, while our design allows us to rationalize observed changes via state dependency, the Stigler-Becker framework obviously does not imply that choices will *necessarily* change even if the underlying state space *has* changed. Hence a no-change in observed choices remains consistent with stability in preferences in the Stigler-Becker world even if the underlying state space has changed.

3.2 Testable Implications

Recall that in the baseline experiment, since none of the participants had yet been placed into the Treatment (T) or the Control group (C), there is no difference in the set of opportunities. Hence observed experimental choices should be the same (controlling for differences in any other observable characteristics). In the follow-up experiment in 2011, the opportunity set of T has changed while the opportunity set of C remains the same. Consequently, there should be no change in choices for subjects in C, and if one observes different choices temporally for T, it is due to the change in their opportunity sets. It is useful to note here that this change in opportunity set was considerable – Maitra and Mani (2013) in their evaluation of the training program show that assignment to training/treatment increased individual's monthly income by 150% relative to the control group.

Our above discussion allows us to formulate the five propositions below to evaluate the empirical validity of stable preferences in our experiment.

Testable propositions on homogeneity of subject preferences:

P1: In the 2010 wave, there are no differences in observed choices (using each of the four variables) across subjects.

P2: In the 2011 wave, there are no differences in observed choices (using each of the four variables) across subjects in the treatment group (T).

P3: In the 2011 wave, there are no differences in observed choices (using each of the four variables) across subjects in the control group (C).

Testable propositions on temporal stability of subject preferences:

P4: There are differences in observed choices (using each of the four variables) in the experimental games temporally, for subjects in the treatment group (T).

P5: There are no differences in observed choices (using each of the four variables) in the experimental games temporally, for subjects in the control group (C).

4. Empirical Tests on Preference Stability

In this section we examine the validity of propositions P1 – P5 using our experimental data. We first report tests on homogeneity of subject preferences followed by the analysis of inter-temporal stability of preferences.

4.1 Homogeneity of Subject Preferences

Empirical support of the homogeneity of subject preferences requires that choices are concentrated around the mean. To ascertain whether this is observed in our data we propose two definitions of stability. First, we define subjects to exhibit *strong* preference stability if more than 90 percent of subject choices lie within 0.05 standard deviation of the mean; analogously, we define subjects to exhibit *weak* stability if more than 90 percent of subject choices lie within 0.10 standard deviation of the mean. To examine if preferences are homogenous we construct a z-score for each of the following choice variables: CRRA coefficient, self ranking, and overconfidence. The z-score is constructed by subtracting the mean of the sample from the individual subject choice and this difference is further divided by the standard deviation. The z-score measures of CRRA coefficient, self-ranking, and overconfidence are measured in standard deviation units.[6]

The results on strong and weak preference stability are presented in Table 2. We find substantial heterogeneity in choices across subjects in 2010. Using the weak stability criteria defined above, we find 0%, 27.2%, and 7.4% of subject choices respectively for CRRA coefficient, self-ranking, and overconfidence lie within 0.10 standard deviation of the mean in 2010. The dispersion is starker using the strong stability criterion: we find 0% of subject choices for each of the three variables to lie within 0.05 standard deviation

[6] Since the indicator for competitiveness is not a continuous variable we do not include it in our z-score measures. In our alternative tests of homogeneity we include the choice data on competitiveness.

of the mean in 2010. In year 2011 as well, we observe substantial heterogeneity in subject choices within the T and in the C group separately, using both the strong and the weak stability criteria (see Table 2). A one-tailed standard deviation test rejects the null hypotheses H_0: $\sigma = 0.05$ as well as H_0: $\sigma = 0.10$ against their corresponding alternative hypotheses H_A: $\sigma > 0.05$ and H_A: $\sigma > 0.10$ (*p-value* < 0.01) for each of our measures in 2010 for the pooled sample, and in 2011 separately for the T group and the C group.

We also use a cross-sectional regression framework to test for homogeneity in observed choices. The regression approach allows us to remove other observed sources of variation in choices arising from differences in socioeconomic characteristics. To do this, the four choice variables are regressed on the vector of exogenous regressors reported in Panel B of Table 1. We use the residuals from these regressions, that is, the unexplained variation in choices (removing any explained source of variation arising from variation in possible observable exogenous states of nature) to test for homogeneity in choices in our state dependent framework.[7] If there are no observed variations in the residual terms for each of the outcomes then that is consistent with the idea of homogeneity in state contingent choices. A one-tailed standard deviation test rejects the nulls: H_0: $\sigma = 0.05$ as well as H_0: $\sigma = 0.10$ against their corresponding alternative hypotheses: H_A: $\sigma > 0.05$ and H_A: $\sigma > 0.10$ for each of the residuals capturing unexplained variation in the subject choice measures for the 2010 pooled sample and for the 2011 treatment and control group separately at the 1% level of significance (*p-value* < 0.01) for all tests and outcome measures.

Using our proposed measures we therefore do not find empirical support for propositions P1 – P3 and conclude that there exists significant cross-sectional heterogeneity in choices. This implies that a representative average preference measure for a group can hide considerable heterogeneity across subjects and can therefore be a noisy measure, from a policy perspective.

[7] The regression results used to obtain the residual terms are available upon request from the authors.

4.2 Inter-temporal stability of preferences

We present four different approaches to evaluate propositions P4 and P5. First, we compute the within-subject differences in choices for each participant across the two years using a two-sided t-test; the corresponding mean and the standard deviation of the within subject differences in choices are reported in Column 1 of Table 3. If these differences are on average equal to zero, they indicate stable temporal preferences. The associated *p-values* are presented in Column 2 of Table 3 for the control and treatment groups in Panels A and B respectively. These provide some evidence that the average within subject differences in choices is zero for three of the four indicators. The results for overconfidence however are not consistent with stable preferences. We discuss this in more detail later.

Although intuitive, the above technique does not correct for the possibility that large but opposite changes in participant behavior cancel each other out to make the average appear close to zero. Examining the distribution of changes instead, and testing for unimodality addresses this problem. This is our second approach. We examine whether the distribution of the within subject difference in choices (across the two years) is unimodal at zero. Figures 1 and 2 present the corresponding distributions of the within subject differences in choices for each of our four indicators separately for women in the treatment (Figure 1) and control (Figure 2) groups. The associated mean, median, and inter-quartile range (25^{th}-75^{th} percentile) of the within subject differences in choices for the four variables (CRRA coefficient, competitiveness, self ranking, and overconfidence) are also provided in Panels A (for control group) and B (for treatment group) of Table 3. With the exception of overconfidence they support the observations that (1) the distributions have no apparent tendency to be positive or negative; and (2) they peak at zero.

The third approach to evaluate temporal stability is to examine the variance of the distribution of changes and compare the levels of dispersion within and across subjects. For choices to exhibit temporal stability in subject preferences, variation in choices over time should be less than the variation in choices across subjects in a given point in time (see Andersen *et al.* (2008)). That is, an empirical support for temporally

stable choices would require that within-subject variance should be less than the between subject variance (i.e., $\sigma_W^2 < \sigma_B^2$). Table 4 reports the variance decomposition analysis separately for women in the two groups. To evaluate P4 and P5, there are eight necessary comparisons we make using each of our four measures. For the Control group (Panel A, Table 4), we find that the between subject variance is always greater than the within subject variance for three of the four measures. The lone exception is overconfidence where the inequality goes in the opposite direction.

In the Treatment group (Panel B, Table 4) we find that the between subject variance is greater than the within subject variance for competitiveness and self-ranking. For the CRRA and the overconfidence measure we find the inequality go in the opposite direction. Here however, a reversal in the inequality of the between and within subject variance, is consistent with state dependent choices: a situation where within-subject dispersions are relatively larger (than between-subjects) can be attributed to the underlying change in the state space, and is therefore consistent with state dependent choices within a stable preference theory (see Stigler and Becker (1977)). The overconfidence measure however, which exhibits a similar pattern in both the C and the T group needs further explanation. Recall that our measure of overconfidence is constructed as a ratio of expected number of bags to the actual number of bags filled for every subject. The choice data reveals that in the baseline treatment, without any prior experience with the real effort task, all participants systematically overestimated the number of bags they were likely to fill. In the follow up year, the subjects (independent of being in the T or C group) seem to have taken into account their experience from the baseline and considerably tightened their performance estimates.[8] We therefore believe that the observed variation in overconfidence is reflective of participant's experience and learning from the baseline.

Finally, our fourth approach evaluates temporal stability of subject choices controlling for pre-intervention socioeconomic characteristics (collected using primary

[8] The mean level of overconfidence is 2.86 in 2010 and 2.15 in 2011. The standard deviation of overconfidence is 2.16 in 2010 and 1.66 in 2011. The mean and the standard deviation are both considerably lower with time.

surveys). We estimate the following empirical specification controlling for changes in the realization of the exogenous states of nature captured through the treatment dummy.

$$\Delta B_i = \beta_0 + \beta_1 T_i + \gamma_j \sum_{j=1}^{K} X_{ij} + \epsilon_i \quad (1)$$

The dependent variable in the above equation are changes in our four outcome variables of interest over time for each subject. The set of explanatory variables include the treatment dummy ($T_i = 1$ if assigned to the treatment group, 0 otherwise), and a vector of pre-intervention characteristics X_{ij}. Temporal stability in preferences for the control group is captured by the intercept term in the regression, and a failure to reject the null of $\beta_0 = 0$ is consistent with the notion of temporal stability in preferences for the control group. For the Treatment group the constraints are less restrictive. Notice, in our state dependent framework, changes in observed choices temporally can be readily accounted for by the underlying changes in state space, while a no-change in observed choice is also consistent with temporally stable preferences. Failing to reject the joint test on the intercept (β_0) and the treatment dummy (β_1) would be consistent with the latter.

The regression results from equation (1) are provided in Table 5 below. We fail to reject the null that the intercept is equal to zero even at the 10% significance level, consistently across Columns 1 – 4; additionally the joint test $\beta_0 + \beta_1 = 0$ can also never be rejected. This suggests that our subject choices are consistent with temporal stability in preferences for both groups.

5. Conclusion

We introduce a novel design to investigate the empirical plausibility of stable exogenous preferences where choices are assumed to be a function of state space. We make use of a randomized intervention, to examine the impact of exogenously varying opportunity sets faced by participants and evaluate their attitudes towards competitiveness and risk preferences pre-intervention and post-intervention. Using multiple empirical strategies, we find that these two characteristics remain temporally stable for subjects in a state-dependent framework. Measures of confidence however, appear to be task-dependent and can depend on previous experience with the task. In addition, we fail to find empirical

support for homogeneous preferences across individuals controlling for state spaces. This suggests that one needs to be careful in using a representative average measure for preferences when considering policy implementations given the heterogeneity of preferences in the constituent subgroups.

Can preferences then be assumed to be exogenous for all practical purposes? Or, are they mostly mutable? Hirschman (1984) distinguishes between preferences and meta-preferences, where preferences are primary "tastes" and meta-preferences are mutable "values" that can change (as also in Sen (1977)). This provides a useful framework to put into perspective some of the recent results on mutable preferences (see for example, Ariely *et al.* (2003), Ariely *et al.* (2005), Ariely and Norton (2008) and Amir and Levav (2008)) as well as results reported in Meier and Sprenger (2010), and most recently ours, that point towards certain preferences to be more stable than others. While it is plausible and intuitive that economic transactions such as putting effort, repeated exposure, and experience can indeed influence preferences (Hoeffler and Ariely (1999)), it is not an odd observation however, that some are born more courageous, or indulge in greater risk-taking than many others, independent of the state of nature they face. The latter are indeed "tastes" about which you do not argue – De Gustibus Non Est Disputandum! On the other hand, a taste about which one might argue, stops being a taste – by definition it turns into "value" that can be mutable as Ulysses's behavior when exposed to the songs of the sirens!

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Figure 1: Distribution of Within-subject Difference in Subjects Choices: Treatment Group

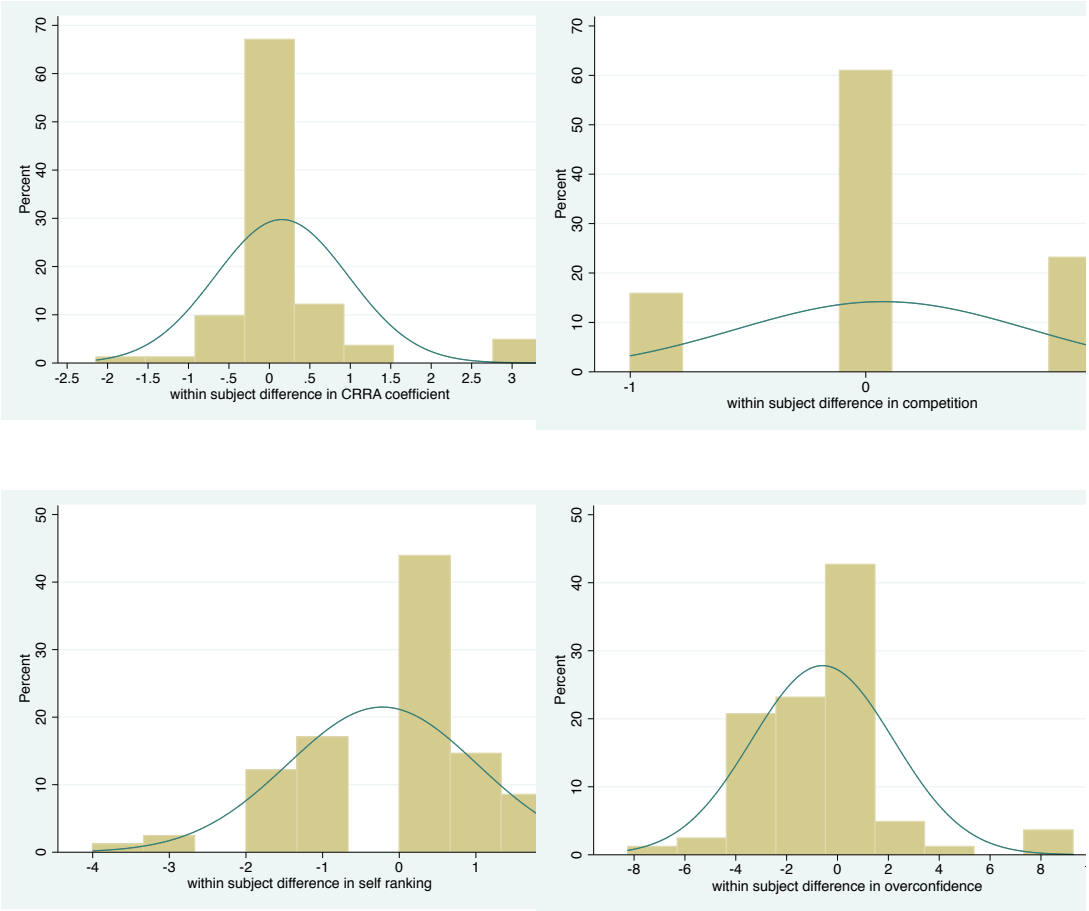

Figure 2: Distribution of Within-subject Difference in Subjects Choices: Control Group

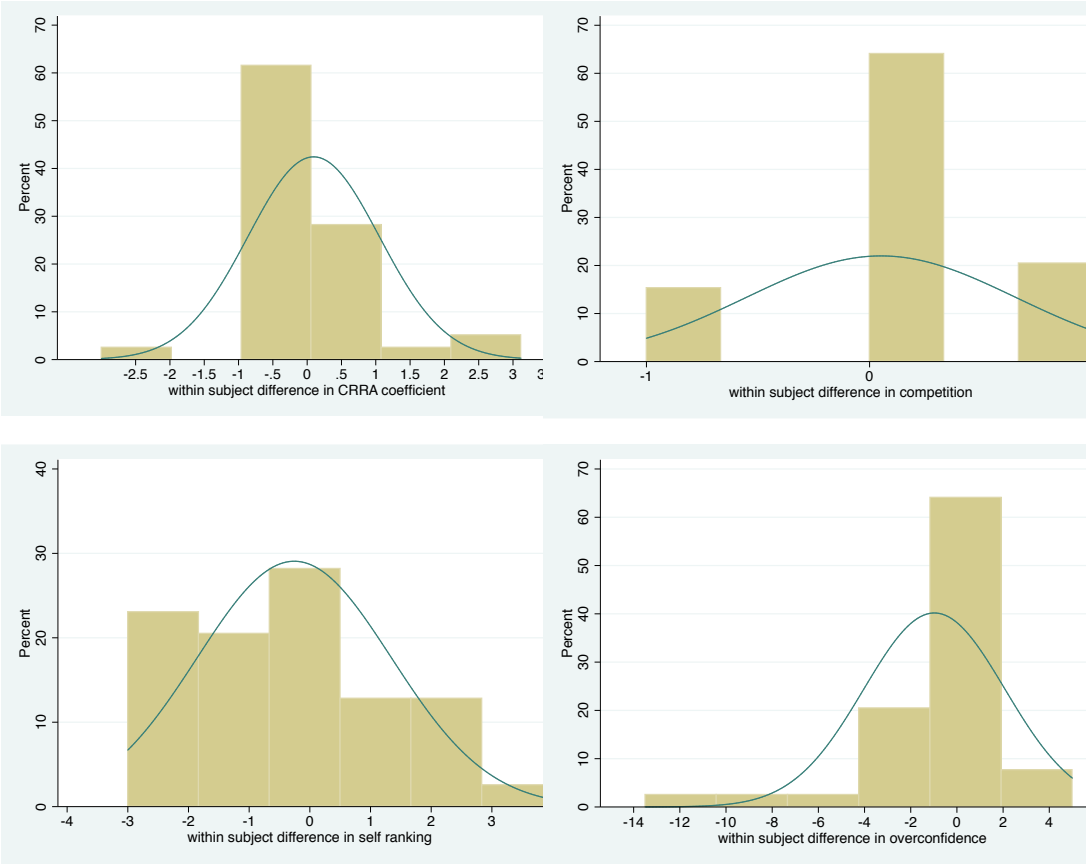

Table 1: Baseline Comparison of Choices and Socioeconomic Characteristics

Variables	Pooled (1)	Treatment (2)	Control (3)	Difference (4 = 2 – 3)
Panel A: Preferences				
CRRA coefficient	0.76	0.74	0.79	-0.046
	(0.44)	(0.39)	(0.54)	[0.08]
Competitiveness	0.38	0.41	0.33	0.08
	(0.48)	(0.49)	(0.47)	[0.09]
Self ranking	4.08	4.23	3.77	0.46**
	(0.98)	(0.88)	(1.11)	[0.18]
Overconfidence	2.86	2.74	3.12	-0.38
	(2.16)	(1.88)	(2.66)	[0.42]
Panel B: Socioeconomic characteristics				
Age in years	23.97	24.47	22.93	1.53
	(6.01)	(6.12)	(5.71)	[1.16]
Schooling (Dummy if completed six or more grades of schooling)	0.86	0.85	0.89	-0.04
	(0.34)	(0.35)	(0.30)	[0.06]
SC (Dummy if belonged to scheduled caste group)	0.60	0.59	0.61	-0.02
	(0.49)	(0.49)	(0.49)	[0.09]
ST (Dummy if belonged to the scheduled tribe group)	0.24	0.28	0.15	0.13
	(0.42)	(0.45)	(0.36)	[0.08]
OBC (Dummy if belonged to the other backward group)	0.06	0.048	0.076	-0.028
	(0.23)	(0.21)	(0.27)	[0.045]
Married	0.48	0.53	0.38	0.15
	(0.50)	(0.50)	(0.49)	[0.09]
Household size	6.00	5.85	6.33	-0.48
	(2.38)	(2.08)	(2.92)	[0.46]
Other income (household income excluding individual's own income) in logs	7.72	7.50	8.18	-0.68
	(7.33)	(7.01)	(8.05)	[1.43]
Own House (Dummy if own a house)	0.88	0.89	0.87	0.02
	(0.32)	(0.31)	(0.34)	[0.06]
ROSCA (Rotating Savings and Credit Association) participation (Dummy if member of a ROSCA)	0.11	0.12	0.10	0.02
	(0.32)	(0.33)	(0.31)	[0.06]
F- value (from the regression of the treatment dummy on all variables reported in Panels A and B)				**1.25**
[p-value]				**[0.25]**
Sample Size	121	82	39	

Notes: Standard deviations in parentheses and standard error brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Strong and Weak Preference Stability

	Percent lying within *x* standard deviation of mean					
	Strong Preference Stability (*x* = 0.05)			Weak Preference Stability (*x* = 0.10)		
	2010	2011		2010	2011	
	All	T	C	All	T	C
CRRA Coefficient	0	0	17.9	0	30.5	17.9
Self Ranking	0	20.7	0	27.2	20.7	0
Over Confidence	0	3.6	0	7.4	19.5	5.1
Sample size	121	82	39	121	82	39

Table 3: Summary Statistics of Within Subject Differences in Choices

Variables	Mean	Null: Mean=0 (p-value)	Median	25th Percentile	75th percentile
	(1)	(2)	(3)	(4)	(5)
Panel A: Control group					
CRRA coefficient	0.09 (0.96)	0.54	0	-0.28	0.28
Competitiveness	0.05 (0.60)	0.59	0	0	0
Self ranking	-0.25 (1.60)	0.32	0	-1	1
Overconfidence	-0.97 (3.06)	0.05	-0.5	-2	0.66
Panel B: Treatment group					
CRRA coefficient	0.15 (0.82)	0.09	0	-0.17	0.28
Competitiveness	0.07 (0.62)	0.29	0	0	0
Self ranking	-0.22 (1.24)	0.11	0	-1	0
Overconfidence	-0.58 (2.80)	0.06	-0.29	-2.33	0.5

Notes: Control group: N = 39, Treatment group: N = 82. Standard deviation in parenthesis.

Table 4: Variance Decomposition

Variables	Total variance	Between subject variation	Within subject variation
	(1)	(2)	(3)
Panel A: Control group			
CRRA coefficient	39.89	22.28	17.61
Competitiveness	17.95	10.95	7
Self ranking	105.95	55.95	50
Overconfidence	351.43	154.72	196.70
Panel B: Treatment group			
CRRA coefficient	53.78	25.43	28.35
Competitiveness	40.61	24.61	16
Self ranking	157.56	93.56	64
Overconfidence	572.85	242.84	330.01

Notes: Control group: N = 39, Treatment group: N = 82.

Table 5: Regression Analysis Controlling for Pre-intervention Socioeconomic Characteristics

	Within subject difference in CRRA coefficient	Within subject difference in Competitiveness	Within subject difference in Self ranking	Within subject difference in Over-Confidence
	(1)	(2)	(3)	(4)
Constant ($\beta_0 = 0$)	-0.82	-0.23	-0.05	-2.28
	(0.57)	(0.44)	(0.80)	(1.98)
Treatment ($\beta_1 = 0$)	0.051	0.05	0.099	0.339
	(0.17)	(0.13)	(0.29)	(0.53)
Treatment + Constant: ($\boldsymbol{\beta_0 + \beta_1 = 0}$)	-0.76	-.0.18	0.045	-1.94
	(0.55)	(0.43)	(0.78)	(1.97)
Age in years	0.027	0.004	0.018	0.072
	(0.02)	(0.015)	(0.026)	(0.08)
Schooling	-0.013	0.022	0.15	0.58
	(0.16)	(0.19)	(0.37)	(0.74)
SC	-0.042	0.072	-0.106	0.026
	(0.19)	(0.15)	(0.33)	(0.75)
ST	0.20	-0.07	-0.84**	-0.82
	(0.24)	(0.15)	(0.39)	(0.89)
Married	-0.223	-0.024	-0.13	0.008
	(0.273)	(0.20)	(0.36)	(0.82)
Household size	-0.005	-0.0003	-0.024	-0.009
	(0.035)	(0.023)	(0.07)	(0.10)
Other income in logs	0.0137	0.0143***	-0.007	-0.090
	(0.0132)	(0.005)	(0.014)	(0.07)
ROSCA participation	-0.292*	0.03	-0.14	-0.24
	(0.15)	(0.16)	(0.35)	(0.89)
Ownership of house	0.32	0.002	-0.24	0.08
	(0.24)	(0.18)	(0.35)	(0.87)
Observations	121	121	121	121
R-squared	0.10	0.066	0.087	0.094

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix 1: Derivation of the CRRA coefficient from allocation in the Investment Game

Assume that individual i divides her budget B between the risky and the safe asset. Assume that L_i is the amount invested in the risky asset. Then the amount invested in the safe asset $S_i = B - L_i$. Assume that I_i is the permanent income of the individual, which can be assumed to be zero without loss of generality. Individual i then maximizes her expected utility, and chooses L_i.

$$\begin{aligned} Max\colon E\big(U_i(L_i, S_i|B)\big) &= 0.5U_i(S_i) + 0.5U_i(S_i + 4L_i) \\ &= 0.5U_i(B - L_i) + 0.5U_i(B + 3L_i) \end{aligned} \quad (1)$$

The first order condition for this maximization problem is:

$$\begin{aligned} -0.5U_i'(B - L_i) + 1.5U_i'(B + 3L_i) &= 0 \\ U_i'(B - L_i) &= 3U_i'(B + 3L_i) \end{aligned} \quad (2)$$

Assuming a CRRA utility function, equation (2) implies

$$(B - L_i)^{-\rho} = 3(B + 3L_i)^{-\rho}$$

Algebraic manipulation yields

$$L_i^* = \left(\frac{3^{1/\rho} - 1}{3^{1/\rho} + 3}\right)B \quad (3)$$

So the optimal investment in the risky asset (L_i^*) is a function of the coefficient of relative risk aversion (ρ) and the endowment (B). Given L_i^*, we can solve for ρ as

$$\rho = \frac{ln(3)}{ln\left(\frac{B + 3L_i^*}{B - L_i^*}\right)} \quad (4)$$

Given B and the optimal investment in the risky asset one can obtain the coefficient of relative risk aversion.

Appendix 2: English Version of the Subject Instructions[9]

General Instructions
Player ID #: _____
Thank you for your participation. You will be paid Rs. 150 for your participation. There are 2 tasks that we will ask you to participate in. Performing each task can win you more money in cash, in addition to the guaranteed Rs. 150.
Although, each of you will complete both the tasks, only one of them will be chosen for payments. I will toss a coin at the end of the two tasks in front of everyone to determine the task you will be paid for. Note that everyone will be paid according to their performances in the task determined by the coin toss.
We are about to begin the first task. Please listen carefully. It is important that you understand the rules of the task properly. If you do not understand, you will not be able to participate effectively. We will explain the task and go through some examples together. There is to be no talking or discussion of the task amongst you. There will be opportunities to ask questions to be sure that you understand how to perform each task. At any time whilst you are waiting during this experiment, please remain seated, and do not do anything unless instructed by the experimenter. Also do not look at others responses at any time during this experiment.
Finally, each page has an ID# on it. Do not show this ID# to any other participant or allow it to be visible to anyone during or after this experiment.
If you are ready, then we will proceed.

[9] These are experiment instructions for the 2010 wave. Instructions for the 2011 wave are very similar (as explained in the text) and can be requested from the authors.

Instructions for the Investment Game
Player ID #: _____

We are about to begin the first task. Please listen carefully to the instructions.
In this task, you are provided Rs.50. You have the opportunity to invest a portion of this amount (between Rs.0 and Rs.50). No money will be given at this point. All actual payments will be made at the end of the experiment if this task is chosen as the one that you will be paid for.

The investment:
There is an equal chance that the investment will fail or succeed. If the investment fails, you lose the amount you invested. If the investment succeeds, you receive 4 times the amount invested.

How do we determine the outcome of the investment:
After you have chosen how much you wish to invest, you will toss a coin to determine whether your investment has failed or succeeded, if this task is chosen for payment. If the coin comes up heads, you win four times the amount you chose to invest. If it comes up tails, you lose the amount invested. You will toss the coin at the end of the experiment, when you come to collect your payment.

Here are some examples:

1. You choose to invest nothing. You will get Rs.50 for sure if this task is chosen for payment.
2. You choose to invest all of the Rs.50. Then if the coin comes up heads, you get Rs.200. If the coin comes up tails, you get Rs.0.
3. You choose to invest Rs.30. Then if the coin comes up heads, you get 30x4=120 from your investment, plus Rs. 20 left from your initial amount. So you will receive a total of Rs.140. However, if the coin comes up tails, you will get nothing from the 30 rupees that you invested. So in this situation you will only get Rs.20 left from the initial amount that you chose not to invest.

Do you have any questions? If you are ready, we will proceed.
We will call each of you one at a time in the adjoining areas where you will be asked a few questions and participate in the described task.
Once you have finished the task, you will go back to your sitting area. Please make sure that you do not converse with anyone. If we find you conversing you will be disqualified from further participation and escorted out by one of the experimenters.

Player ID #:

Decision Sheet for the Investment Game

Please complete the example below:

1. If you choose to invest Rs 15 and the coin toss comes up heads, what will you receive?
   Rs_____ x _____ = Rs_____

Actual Decision:

2. Amount that I wish to invest: _____

3. Reason for this decision:

Instructions for the Competition Game
Player ID #: _____
We are about to begin the next task. Please listen carefully to the instructions. All the money that you earn from this task is yours to keep and will be given to you at the end of this experiment if this task is chosen as the one that you will be paid for.

For this Task, you will be asked to fill bags with Rajma beans and seal it so its contents remain securely inside. We will give a demonstration before you start the task.
You will be given 1 minute to fill up as many bags as you can. Only bags filled and properly sealed will be counted towards your payments.
You can choose one of two payment options for this task.
Option 1:
If you choose this option, you get Re. 4 for each bag that you fill properly in 1 minute.
Option 2:
If you choose this option, you will be randomly paired with another person and your payment depends on your performance relative to that of the person that you are paired with. If you fill up more bags properly than the person you are paired with, you will receive Rs.16 per bag that you filled. If you both fill the same number of bags you will receive Rs. 16 per bag. If you fill up less number of bags than the person you are paired with, you will receive Rs. 0.
Note that what you will earn does not depend on the decision of the person that you are paired with; it only depends on your own choice of payment, your performance and their performance.
Here are some examples of what could happen:

1) You choose option 1. You fill 10 bags properly. You will receive 10xRe. 4 = Rs. 40.
2) You choose option 2. You fill 3 bags properly. The person that you are paired with fills 2 bags properly. You will receive 3xRs.16 = Rs. 48.
3) You choose option 2. You fill 3 bags properly. The person that you are paired with fills 4 bags properly. You will receive 3 x 0 = Rs. 0.

Note that these are examples only. The actual decision is up to you.
The rest of the task will proceed as follows:
Next, we will call each of you one at a time in the adjoining area where you will be asked a few questions and choose your preferred option in the above described task. Once you have answered the questions and indicated your preferred option, you will come back to your sitting area. Please make sure that you do not converse with anyone at this time. If we find you conversing you will be disqualified from further participation and escorted out by one of the experimenters.
Once everyone is back to the seating area we will announce the start of the task and you can start filling up the bags. We will make an announcement when there are 30 seconds remaining. When time is up, we will say, “Stop the task now”. You should **immediately** stop filling the bags. Please make sure that your hands are in your lap now and not touching any of the bags that you filled up. If you do not do this within 2 seconds, you will receive Rs. 0 for the entire experiment.
We will come around and inspect the bags and record the number of bags filled each of you managed to fill up.
Once all counting is done we will flip a coin to decide which of the two tasks will be chosen for payments.
After the coin toss, each of you will be again called one at a time to the adjoining area for the final payment procedures.

Are there any questions before we begin? If you are ready, we will proceed.

Player ID #:

Decision Sheet for the Competition Game

For experimenter use only
Paired Player ID #: _____

Questions for Task # 2

Please answer the following questions:

1. Suppose you choose Option 1. You complete 11 bags correctly at the end of 1 minute. How much money do you receive?

_____ x Rs_____ = _____

2. Suppose you choose Option 2. You complete 7 bags correctly. The person you are paired with completes 6 bags correctly. How much money do you receive?

_____ x Rs_____ = _____

3. How many bags do you think you can fill properly in 1 minute?

_____

4. If we were to rank everyone's performance, in the group of people in this room, from best to worst, where do you think you would fall compared to the average person? *Please place a tick next to the rank that you think applies to you.*

__ 1-4 (very above average)
__ 5-8 (above average)
__ 9-12 (average)
__13-16 (below average)
__ 17-20 (very below average)

5. We now ask you to choose how you want to be paid: according to option 1 or option 2?

_____

6. What was your decision based on?

7. If you chose Option 1, did your decision depend on the payment rate under Option 2? If so, what payment rate would have convinced you to choose Option 2?

Instructions for Final Payment Determination

We will now determine what task to pay you for. We will flip a coin; you will all be paid for task 1 if Heads come and task 2 if Tails come up.

If Head comes up, then Task 1 is chosen: Each one of you will flip a coin to determine whether your investment succeeded or not. If the coin comes up heads, you win four times the amount you chose to invest. If it comes up tails, you lose the amount invested.

If Tail comes up then Task 2 is chosen: We will pay you according to the choice you had indicated earlier.

If you had chosen option 1, we will pay you according to your performance.

If you had chosen option 2, we will ask you to pick one chit amongst several chits of paper on the front desk. Each chit contains an id number of one of the participants. Your performance will be matched with the performance of the participant whose ID number you picked. You will be paid according to your relative performance as described earlier.

Now we will call each of you one at a time like before. Please take your decision sheets with your ID# written on it when you come.

Visual Charts

Figure A1: Slides used in the *Investment Game* in conjunction with the oral instructions

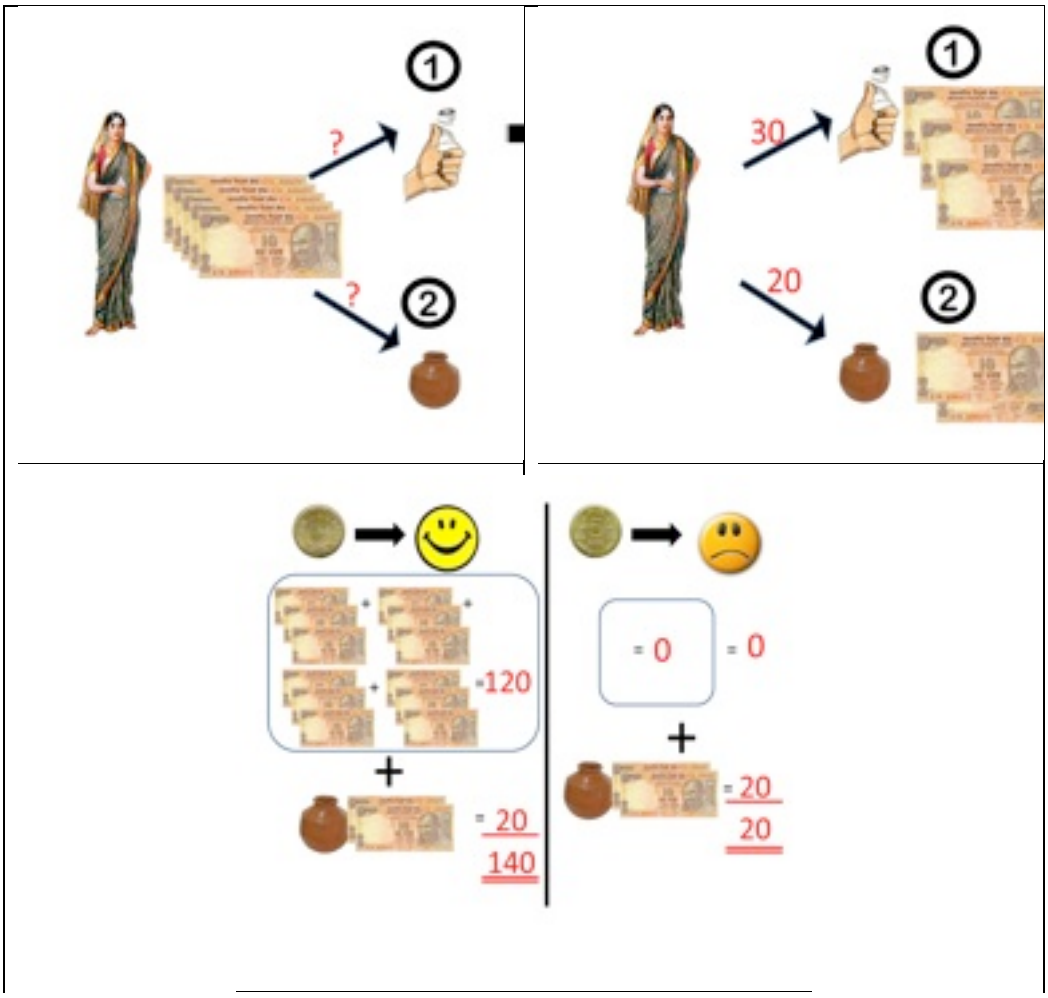

Figure A2: Visual slides used in the *Competition Game* in conjunction with the oral instructions